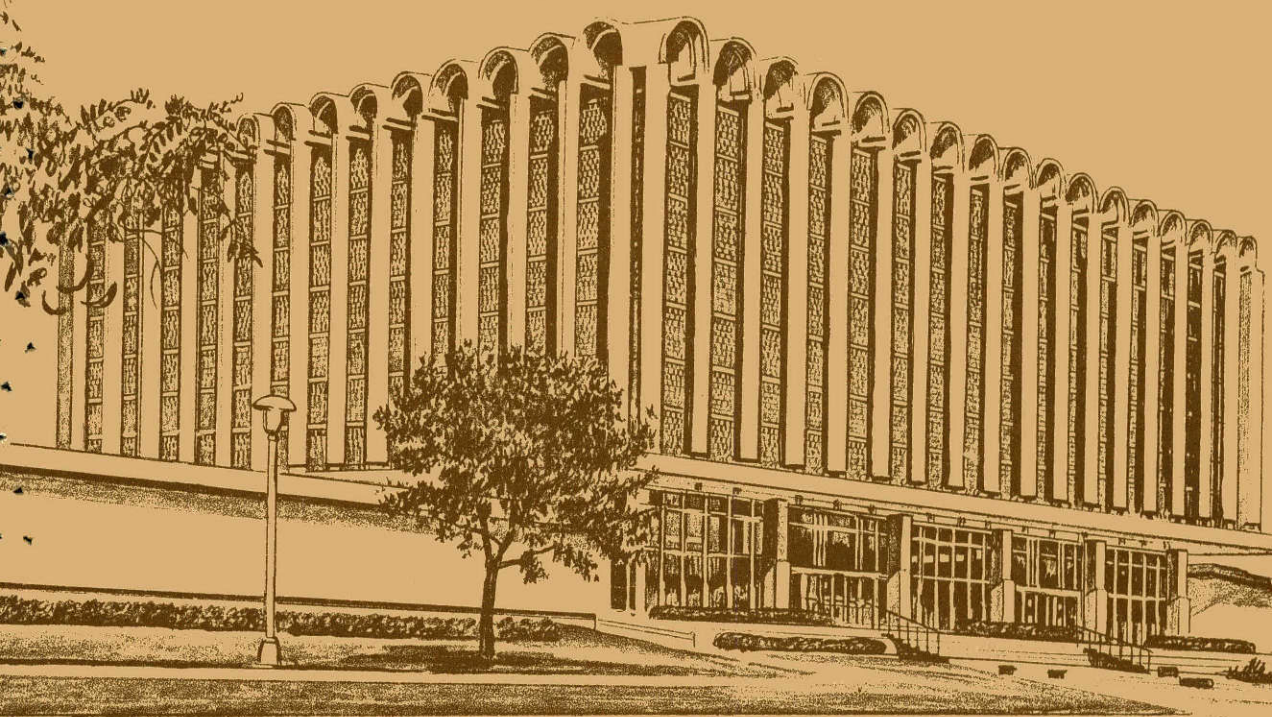
GRADUATE STUDIES
TEXAS TECH UNIVERSITY

Charles Sanders Peirce: Contributions to *The Nation*
Part One: 1869-1893

Compiled and Annotated by
Kenneth Laine Ketner and James Edward Cook



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INTRODUCTION

Charles Sanders Peirce's contributions to *The Nation* represent not only a valuable gift to philosophy, but also an encyclopedic intellectual time-capsule for the latter part of the nineteenth century. It is our goal to make public a second time, in a convenient format, all of the verified Peirce contributions along with those articles that we or other Peirce scholars believe to be his work. So that discussions presented in these materials may be followed to the fullest, relevant items by other authors have also been included. The actual contributions will be issued in three separate parts, this volume being the first. 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

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* (Philip P. Weiner and Frederic H. Young, eds.), Cambridge: Harvard University Press, 1952] cite additional evidence that Garrison pruned Peirce's reviews (see especially pp. 376-377).

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 biographical 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. 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 to the staff of the Houghton Library at Harvard University who have assisted us in obtaining access to materials in the Peirce papers. The editors of the *Antioch Review* generously gave permission to reprint Richard Bernstein's article on Peirce and *The Nation*. That essay provides the reader with a fine general overview of Peirce's collaboration. Finally, to Joseph Morton Ransdell, Ketner inscribes his part in the making of this collection in a spirit of friendliest affection and esteem.

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Kenneth Laine Ketner

James Edward Cook

CHARLES SANDERS PEIRCE AND *THE NATION*[1]

RICHARD J. BERNSTEIN

If we were to draw up a list of ideal requirements for a philosopher, we would certainly want to include an intimate knowledge of the empirical sciences and the formal disciplines such as logic and mathematics. Our philosopher should also have a subtle knowledge of the philosophic tradition, preferably one gained from original sources. But knowledge of the sciences, logic, and the history of philosophy is not enough. A philosopher must also combine careful analysis with a curiosity and imagination that ranges over the totality of human experience. The degree to which Charles Sanders Peirce was proficient in all these respects (and other equally impressive ways, too) is unique, and it would be difficult to name another American who was as erudite and original as Peirce.

Peirce spent most of his professional career as a practicing scientist, "swinging pendulums," as he liked to phrase it, for the United States Coast and Geodetic Survey. As the outstanding American logician of his day, his contributions helped to initiate the exciting advances in mathematical logic. He wrote with great perspicacity about the complex issues of medieval philosophy at a time when most of the intellectual world was ignorant of the subtlety and vitality of medieval thought. His studies in the history and development of science were fresh work in a "new" field of scholarship. Peirce is most popularly known as a founder of pragmatism, though his variety of pragmatism, which he later redubbed "pragmaticism," is radically different from many of the doctrines associated with this movement. William James described Peirce's Harvard lectures of 1903 as "flashes of brilliant light relieved against Cimmerian darkness." But reading now through Peirce's published papers (no complete philosophical treatise of his was published during his lifetime) is more like having a powerful searchlight turned on the dark recesses of human knowledge. Sometimes the light passes too rapidly, before we get a chance to see clearly what Peirce is exploring, while at other times the multifarious facets of a problem are brilliantly illuminated. Always there is a sense of exciting adventure in following the intricate network of problems with an incisive inquirer.

There are still mysteries concerning Peirce. Important questions about the man and his work have not been satisfactorily answered. No full-length intellectual biography of Peirce exists, though there is now a noted scholar working on one. However, enticing glimpses of the man can be perceived from his writings and the letters of his contemporaries, including both William and Henry James. The following letter to Charles Eliot from William James is indicative of James' constant efforts to help his close friend. The letter reveals the admiration that Peirce could inspire and the difficulties that he encountered throughout his life.

Cambridge, March 3, 1895

Dear President,

I hate to hunt you down with disagreeable college problems, but how is a Supreme Being to hide from his creatures? The problem is this. The Philosophic Department has met to arrange the courses for next year, and my taking charge of psychology means . . . the important course in "Cosmology" or "Philosophy of Nature" . . . must either be dropped for next year or given to some outsider. Now I want to propose to you no less a person than Charles S. Peirce, whose name I don't suppose will make you bound with eagerness at first, but you may think better of it after a short reflection. . . . He is the best man by far in America for such a course, and one of the best men living. The better graduates would flock to him—his name is one of mysterious greatness for them now—and he would leave a wave of influence, tradition, gossip, etc. that wouldn't die away for many years. *I* should learn a lot from his course. Everyone knows of Peirce's personal uncomfortableness; and if I were President I shouldn't hope for a harmonious wind-up to his connection with the University. But I should take that as part of the disagreeableness of the day's work, and shut my eyes and go ahead, knowing that from the highest intellectual point of view it would be the best thing that could happen for the graduates of the Philosophical Department. . . . Always truly yours,

Wm. James

Eliot, acknowledging that "all that you say of C. S. Peirce's remarkable capacities and acquisitions is true," flatly refused to offer Peirce the appointment. The episode was not unique. Many of the best thinkers of the time, including James, Royce, and Dewey, recognized Peirce's genius. Dewey, who was twenty years younger than Peirce, had been a student at Johns Hopkins during the brief period that Peirce taught logic there. At the time that the above letter was written, Dewey was already the leader of the "Chicago school" of philosophers. Daniel C. Gilman, president of Johns Hopkins, and G. Stanley Hall, a colleague of Peirce at Hopkins and later president of Clark, also recognized Peirce's talents. Both these institutions were leaders in developing the spirit of a university as a community of serious inquirers. Gilman did arrange for a temporary lectureship at Hopkins from 1879 to 1884. And Hall had written of Peirce in *Mind*, 1879, "The author is a distinguished mathematician, and this discussion [a series of articles in the *Popular Science Monthly*] in which he long ago interested himself, promises to be one of the most important of American contributions to philosophy." Peirce was never offered a chair at these institutions or any other university. Why was Peirce so completely ostracized from the academic community? Most explanations revolve around what James discretely calls Peirce's "personal uncomfortableness." Peirce had a reputation for being arrogant, intolerant towards what he considered intellectually inferior, and somewhat crotchety. His divorce and personal idiosyncracies were sufficient to cut him off from conservative academic circles. Yet here too there are puzzles. W. B. Gallie (a Scotchman who has written the only popular introduction to Peirce) has pointed out that the Peirce who comes through in his writings and correspondence is very different from the legend that encompassed him. It is true that Peirce could be wilfully obscure and relentlessly sardonic. But there is a passionate integrity that infuses his work. He courageously struggled against disease and extreme poverty and he was capable of great warmth and loyalty to his few friends.

Peirce's isolation is not only a sad commentary on the provincialism of American intellectual life; it had a profound effect on his work. One of the fundamental principles of his philosophy is that

> the real, then, is that which, sooner or later, information and reasoning would finally result in, and which is therefore independent of the vagaries of me and you. Thus, the very origin of the conception of reality shows that this conception essentially involves the notion of a COMMUNITY, without definite limits, and capable of a definite increase of knowledge.

The community of inquirers is the basis for defining both truth and reality. Peirce insisted that truth is public and that it is only through such a community that one can escape the pitfalls of private prejudices and idiosyncrasies. But he himself lacked such a community and the marks are evident throughout his work.

While Peirce was undoubtedly the victim of provincialism and stupidity, the image of a man prevented from completing his life's work because of the harassment by a hostile or indifferent audience isn't completely accurate. Peirce was philosophically erratic and frequently failed to follow out some of his boldest suggestions.

Peirce has never had an extensive audience. However, he might have become better known to the intellectual community of the late nineteenth century. During a forty year period, he was a reviewer for *The Nation*. *The Nation*, like many other periodicals, did not carry signatures of its reviewers. But the scholarship, the range, and the originality of these reviews are an excellent guide to Peirce's thought. During this period (1869-1908), *The Nation* combined in its pages what is now only to be found in several different types of journal: commentary on recent political events; reflective articles on current intellectual issues; notices and reviews of specialized books and articles. Peirce primarily reviewed scientific and philosophic literature, though occasionally he had an opportunity to indulge his interest in intellectual biography and even reviewed some books on wines. (He reports that he was "for six months under the tutelage of the sommelier of Voisin in Paris . . . to study the red wines of Medoc, and became quite an expert.") There is plenty of evidence that Peirce looked upon his reviewing as hack work, that he felt restrained in what he could say, and that his reviews were severely edited. But at the same time, Peirce could use his reviews for exploring some intricate problem.

His first review for *The Nation* set the style for what were to be his more interesting pieces. The book reviewed was Noah Porter's *The Human Intellect*. Porter, Professor of Philosophy at Yale and soon to become Yale's president, was typical of philosophy teachers in nineteenth-century America. A staunch representative of Christian orthodoxy, steeped in the Scottish school of philosophy which was taught in the theological seminaries, he announced that "the philosophy taught in this volume is pronounced and positive in the spiritual and theistic direction, as contrasted with the materialistic and anti-theistic tendency which is so earnestly defended by its advocates as alone worthy to be called scientific." Peirce was cool in his comments. He accused the Scottish school of

common sense philosophy of being too old to bear fruit. Porter's treatment of medieval philosophy was singled out for criticism and Peirce used the occasion to develop his own ideas on the controversy of nominalism versus realism. Throughout his writings, Peirce returned to this issue which he took to be one of the most crucial in philosophy. At first we might suspect him of reopening dead and fruitless disputes, but his handling of the controversy and his claim that he was a Scotist realist were intimately tied up with his understanding of scientific inquiry. In a letter to *The Nation* (1871), he wrote,

> the only feature of the controversy which has appeared to me to need more emphasis than has hitherto been put before upon it is that each party had its own peculiar ideas of what it is that is real, the realists assuming that reality belongs to what is present to us in true knowledge of any sort, the nominalists assuming that the absolutely external causes of perception are the only realities.

Interpreted in this way, the dispute is quite independent of whether the mind is capable of directly grasping universals or forms. Peirce, as a realist, was not arguing that we have an immediate intuition of abstract entities; he consistently opposed any doctrine of infallible intuition. It was the role of scientific law that demanded realism. Laws are real, and for the scientist they are as real and more reliable than individual insistent facts. But if scientific laws are general and real elements of the universe, we are compelled, so Peirce argued, to adopt a realist outlook. The following passage from a review of some logic books (1895) illustrates the typical movement in Peirce's thought from his daily experience in the laboratory to the abstract issues of philosophy.

> A pendulum has been drawn to one side 86,400 times daily for twenty years, and every time it has returned to its position, and that at almost the same rate of speed. Was that chance-coincidence? If not, there was a really operative *law*. That law is general. It is not only general itself, but it applies to a general class of things; and if the law is real, the class is real.

There is a curious twist in Peirce's attack on the nominalistic attempt to reduce laws and universals to convenient fictions. Nominalists have always looked upon themselves as the tough-minded champions of science. The most vehement attacks upon nominalism have come from those who look upon it as a threat to the moral and spiritual life of man. But this is not the basis of Peirce's attack. It is rather that nominalism is based on a misconception of science and is at the core anti-scientific. In a review of Thomas Huxley (1894), Peirce writes, "He adheres to the sect of English nominalism—the school of Ockham, Hobbes, Locke, Hartley, Berkeley, Hume, Bentham and the Mills—without perceiving how antagonistic they are, upon the whole, to the spirit of science."

Peirce, in defending the reality of scientific laws as the object of true judgments, was not advocating a strict mechanistic determinism. This doctrine, too, which was accepted as dogma in many scientific circles, was persistently attacked by Peirce. It was another instance of a false metaphysical doctrine forced on the facts rather than based on a sensitive understanding of living scientific inquiry. Long before the beginning of quantum physics and its interpretations, Peirce argued that the picture of the universe as consisting only of matter, where

"matter itself is capable of no alteration but motion in space and these motions are modified by fixed attraction and repulsions," is a gross oversimplification. It leaves out of account the irreducible inexactitude that the scientist constantly encounters in his investigations. Peirce's theory of tychism claims that chance, indeterminacy, spontaneity are operative factors in both mental and physical life. Since chance is real and not merely a sign of our ignorance, scientific laws can never have the absolute rigidity claimed by the mechanistic determinists.

Peirce in his reviews returned again and again to distinguish carefully the actual practices of science from the pseudo-scientific theories which were supposedly "proven" by science. This is illustrated in his incisive and persistent criticism of Spencer and his followers. At a time when the country was being swept by social Darwinism—an ideology suited to the needs of rapid industrial expansion and so appealing because of its "scientific" character—Peirce was among the first in America to attack it. While *The Nation* in its front pages was absorbing and favorably acknowledging Spencer's interpretation of social evolution, its back pages carried occasional discordant reviews of the growing literature of social Darwinism. In a short notice of a popular condensation of Spencer, Peirce sardonically remarked

> and to persons who read and reread those thick volumes, not because they believe in them, but only because they want to know what it is that so many others believe, and to whom the writings of the driest scholastic doctor are less heartbreakingly tedious, this one volume of 500 pages in place of a library of 5,000 pages is like a balm of Gilead. Would it only embraced an introduction boiling the whole thing down to 50 pages.

Peirce developed his criticism of Spencer more fully when he wrote, Spencer "quotes with admiration Huxley's fine saying 'Science commits suicide when it adopts a creed.' That is just the principle of death lurking in Spencer's philosophy." Creeds, whether that of Spencer's or any others, cannot be established once and for all by scientific inquiry. Thinkers of conflicting viewpoints have always claimed that their particular dogma is "scientifically proven." But in the doctrine of fallibilism, Peirce argued that every claim of knowledge, no matter how basic, is subject to possible revision. "Science is incomplete, essentially incomplete." To argue from a scientific theory that has been empirically confirmed to a creed that is supposed to be eternally true is to commit a blatant fallacy. Fallibilism is not to be confused with scepticism. Peirce is not saying that since we can never know anything with *absolute* certainty, therefore we can never *know* anything. A theory of knowledge where we can only say that we know something when we know it absolutely is rejected as specious; it is one of the ghosts that has haunted philosophic speculation.

Perhaps the most entrenched dogma concerning the pragmatists is that they are obsessed with utility and conventional practicality. Peirce, himself a victim of intolerance to pure inquiry, severely criticized this tendency in American life. He used the pages of *The Nation* to plead for a greater encouragement of a disinterested pursuit of truth.

> The true devotee of science so long as he enacts the role, never thinks of Philistine Utility. In his mind, to learn the ways of Nature and the reasonableness of things and to be

> absorbed as a particle in the rolling wave of reasonableness, is not useful, but is the *summum bonum* itself towards which true usefulness tends.

Though scientific inquiry is typically Peirce's starting point in thinking about any problem, he was continually reaching out in new directions. In the battle that was raging between science and religion, especially in light of the evolutionary and pseudo-evolutionary theories which were in the air, Peirce sought to disentangle the claims of both. Peirce's suspicion of the attempts to prove scientifically any creed led him to reject the claim that the new science established the truths of religion as well as the equally popular myth that evolutionary theory had finally disproven religious claims. But he was also uneasy about superficial attempts to reconcile science and religion. When Paul Carus' journal, the *Open Court*, announced its intention to make an "effort to conciliate religion with science," Peirce asked "Is this wise? Is it not an endeavor to reach a foredetermined conclusion? And is not *that* an anti-scientific and anti-philosophical aim? Does such a struggle imply a defect of intellectual integrity?" Peirce did believe that there was an authentic religious dimension to experience—one that satisfies a genuine human need that cannot be satisfied by science. But "an accord between scientific and religious thought must come about, when it comes, chiefly by the natural unforced development of each."

Ethics, too, was intimately bound up with science. Contrary to the Spencerians who talked as if new scientific conclusions finally put ethics on an indisputable foundation, Peirce subtly argued that it was the habits and dispositions required for, and encouraged by, scientific inquiry that had the most significant ethical consequences.

> The prosecution of scientific research necessarily requires and strengthens certain moral qualities, quite independently of what the results of that research may be. . . . The first of these teachings is perfect fairness and moral indifference as to the outcome of any inquiry.

He goes on to say that

> Perfect candor in recognizing facts and their bearings, without trying to explain away real difficulties so as to make out a decided conclusion, is the very first point of scientific morals.

Though never fully developed, one of Peirce's more interesting speculative suggestions is that logic itself is based on ethics and ethics is ultimately based on aesthetics.

Peirce's reviews did not provide an opportunity for him to exhibit some of his own technical work in logic, though he did frequently comment on the new developments in logic. Much that went under the name of logic in the late nineteenth century was actually bad epistemology and psychology, and Peirce chided his contemporaries for their failure to study carefully and discriminate the proper domain of logic. In his review of Dewey's *Studies in Logical Theory*, he suggested that Dewey and his school were really interested in a "natural history of thought" rather than logic as a normative science. It was Boole, De Morgan, and Schröder that Peirce praised as true logicians, and from our perspective today, we can see how right he was in detecting the more important strains in logical theory. It is

difficult to summarize succinctly the nature of the new logic, though Peirce in a review of Schröder described it as follows:

> What is the Logic of Relatives? It is a subject treated in all the more complete medieval handbooks, and hinted at by Aristotle. But it was Robert Leslie Ellis, the editor of Bacon's philosophical works, who first got some idea of how it ought to appear in a modern shape. Namely, instead of analyzing a proposition into subject and predicate, it analyzes it into subject, predicate, and objects—which last it conceives as so many additional subjects. In 1858 Augustus DeMorgan published a long memoir on the subject, in which besides establishing many important truths, he clearly showed that, instead of being a special branch of logic, it is, in fact, a great generalization of the old conceptions.

Though few modern logicians would characterize modern symbolic logic in this slightly archaic manner, there are two important points noted here. The first is that propositions no longer are to be analyzed in one set way, but could be analyzed in many new ways in order to develop a great range and flexibility to formal logical analysis. The second is that experimenting with these new techniques of analysis led to a much greater generality and formal simplicity. Diverse areas of logic could be unified in a single system. The innovations in logic were comparable to the discovery of non-Euclidean geometries which Peirce also discussed in *The Nation*, for it opened up previously unknown and unsuspected areas of investigation.

Our remarks thus far give something of the flavor of Peirce's thought as revealed through his reviews, though they do not exhaust the areas of investigation or touch on some of the topics that preoccupied him in his more technical writings, such as his elaborate theory of signs and his cosmological speculations. We can, however, draw together the various strands in his thought by reference to his theory of categories which pervades all his thinking. Writing to Lady Welby, Peirce tells of his "discovery" of the categories:

> I was long ago (1867) led, after only three or four years study, to throw all ideas into the three classes of Firstness, of Secondness, and of Thirdness. This sort of notion is as distasteful to me as to anybody; and for years, I endeavored to pooh-pooh and refute it; but it long ago conquered me completely. Disagreeable as it is to attribute such meanings to numbers, & to a triad above all, it is as true as it is disagreeable.

Peirce, who was always attempting to get to the rock bottom of whatever he studied, became more and more convinced that three fundamental notions or categories were needed for an adequate account of experience and reality. Neglect or exaggeration of any category inevitably leads to distortion. By Firstness, Peirce intends to call attention to the genuine spontaneity, freshness, uniqueness, or qualitative immediacy of experience and reality. But the world does not consist of pure qualitative immediacy; there is also an element of resistance, effort, opposition of brute existence which Peirce labels Secondness. Thirdness is the name used to refer to generality, law, and continuity which Peirce argued, in his pragmatic realism, is just as basic and as irreducible as the other two categories. There have been philosophies that have emphasized one of these categories at the expense of the others. He criticized Hegel, as Kierkegaard did from a similar point of view, for allowing brute existence (Secondness) to be swallowed up in

the dialectic of universals (Thirdness). "The capital error of Hegel which permeates his whole system in every part of it is that he almost altogether ignores the Outward Clash." (Today Peirce might well object to existentialism for exaggerating Secondness and neglecting the element of generality and universality in human life.) The attack on nominalism can also be restated from this categorical perspective; the nominalists concentrated their attention on the first two categories and mistakenly attempted to eliminate Thirdness. The mechanists, on the other hand, failed to take seriously the spontaneity (Firstness) in the universe. Using Peirce's categorical scheme to evaluate strengths and weaknesses of varying philosophies follows from something more basic. It can be used in this way, because if he is right, a complete and accurate account of any phenomenon will necessitate reference to these three categories. Peirce is suggesting, and it runs all through his thinking, a new theory of experience and reality, one which assimilates the insights of traditional positions and does justice to the novelty, the brute compulsion, and lawful regularity of the universe.

What distinguishes Peirce from so many of his nineteenth-century contemporaries is the vitality and relevance of his thought for us today. Spencer, Fiske, and others who enjoyed an enormous popularity are now dated; their only place is in the history of ideological movements. But Peirce's conception of scientific inquiry with its openness and fallibility, his suspicion of scientism, and his insistence that there are other areas of human experience besides science that ought to be the concern of philosophers are strikingly appropriate today. I suggest that in temper and approach Peirce represents what is best in American philosophy. A popular mode of interpretation has spoken of the *two* Peirces; the tough-minded, scientific, cautious Peirce, and the tender-minded, wildly speculative, and imaginative Peirce. But this dichotomy tells more about the interpreters than about Peirce himself. He constantly opposed such a simple-minded dualism. Scientific inquiry and logic are not isolated disciplines; they inevitably raise philosophical questions. And metaphysical and cosmological speculations must be informed with a subtle understanding of living scientific inquiry if they are ever to become more than idle fancies. Peirce sought to reconcile these two tendencies without recourse to "pat" solutions. If he did not always succeed, he has left us a legacy of insights and suggestions which are still a rich source for any philosophic investigator.

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8 (18 March 1869) 211-213

PROFESSOR PORTER'S "HUMAN INTELLECT"

The Human Intellect; with an Introduction upon Psychology and the Soul.
By Noah Porter, D.D., Clarke Professor of Moral Philosophy and Metaphysics in Yale College. New York: Charles Scribner & Co. 1868. 8vo, pp. 673.

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

Noah Porter (1811-1892) was a Congregational clergyman and educator. He was called to Yale College in 1846 as Clarke professor of moral philosophy and metaphysics. He became president of the college in 1871 after having attained an international reputation as a scholar. During Porter's lifetime, *The Human Intellect* became an influential book that was reissued many times.

The Rev. Dr. Porter, of Yale College, has published an important work upon that branch of psychology which relates to the faculties of cognition. Whatever be the judgment pronounced upon this treatise, no man can withhold his respect for the self-denying labor, both in the way of study and of composition, which has been devoted to its production. The size of the book is something stupendous. It is a large octavo of nearly seven hundred pages (printed, we regret to say, upon that harsh, cottony paper in which New York publishers seem to delight), in three sizes of print, of which the largest would not be unusual for a duodecimo while the smallest is painful to read. The work is designed primarily for a text-book, and the part in the largest type "is somewhat technically phrased and formally propounded in order that it may be learned more readily for the examinations of the class-room." But as the philosophical world was also to be addressed and the discussion must accordingly be carried in many places beyond the depth of learners, and inasmuch also as the author wisely thought it well to put more information into the hands of his scholars than they were to be positively required to master, the book has been more than doubled by the addition of matter in two sizes of small print, that in the middle-sized type being suitable for general students, and that in the smallest consisting chiefly of historical and critical notices.

General readers in metaphysics will hardly find the book to their taste. The appearance of it is not inviting; the type is too small, the volume too large, and the paper disagreeable. A style studiously technical and formal, even if it were not stiff and awkward and of a magisterial tone, would not attract them. Nor is a compendium of 699 numbered sections, with scarcely any unity of conception developing through them all, precisely what such readers desire. But it is admirably fitted for a college text-book. The formal and bald manner in which the arguments on either side are laid down is eminently adapted to nourish the logical power of the student. Great pains have been taken to give a full and rigidly precise account of the meaning of the principal terms employed, thus inculcating

one of the most essential requisites for accurate thinking upon abstract subjects. The author's talent for explaining words is well illustrated in the chapter upon consciousness. He shows somewhat more favor to modern German terminology than we should approve. For example, "sense-perception," instead of external perception, seems to us to have little to recommend it. The scholastic terminology forms a system at once precise and elastic. New terms can be constructed in accordance with the principles of it which may be understood by any one who is acquainted with these principles. This system, together with the accretions which it received in the seventeenth century, has the character of a somewhat obsolete but yet universal language; it is not confined to the philosophers of any particular nation, but is equally the possession of all. It is the basis of the actual English terminology, and has even passed in great degree into ordinary English speech. The modern German terminology, on the other hand, is unsettled and unsystematic; most of its single words correspond precisely to no single English words, and its method of compounding them is foreign to our conceptions of grammar. For these reasons, we think that the basis of English terminology should be allowed to remain as it actually is, scholastic; and certainly no one who favors a movement in the direction of Aristotelianism, as Dr. Porter partly does, should oppose this position. But once admit that such should be the basis of our terminology, and no doubt we should adhere to it consistently, except in cases in which it altogether fails us. In the present case it has not failed us. The phrase "external perception" would be quite intelligible to any educated person, even if it were a newly invented term. But in point of fact it is quite familiar both in English and in German. If it be objected that some persons believe in an external perception not through the senses, still Dr. Porter is not one of these; but even if it were judged proper to take account of that mystical and fictitious faculty, the term external sensuous perception might be adopted. Dr. Porter's using "representation" for imagination and memory appears to be another case of borrowing from the German. Representation is wanted in a general unpsychological sense, and as a psychological term it has already been used in two other senses besides that in which Dr. Porter takes it. Either "the representative faculty" or the "imagination" might have been employed advantageously in the last sense, as they were, in fact, by Hamilton. In using words cognate with "activity" we are inclined to suspect that Dr. Porter has been somewhat influenced by German usage, although we do not find that he anywhere defines any of these words, the ambiguity of which has often led writers into fallacies.

Another character of the work which makes it suitable for purposes of instruction is the impartiality with which the whole ground is gone over, no one or more faculties or phenomena being dwelt upon at such inordinate length as to encroach upon the space due to the others. The student will consequently receive the best armor against plausible theories which answer well for the facts that concern one mental process, but which may conflict with those that concern another. Another merit is that in the smaller type the student will generally find some notice of doctrines not contained in the text he is required to learn, and some references to the books in which those doctrines are maintained. Accordingly, when he has

once become thoroughly familiar with this treatise by a year's study of it, it will always serve him as an invaluable index of reference in any further psychological studies which he may choose to pursue. We must not omit to say that the doctrines which it teaches are entirely conformable to orthodox theology, and quite free from any materialistic leanings. A young mind thoroughly imbued with Dr. Porter's teachings will be likely to get its philosophy so bound up with its religion that it cannot part with either unless it parts with the other.

The historical notices are full and valuable. They do not cover every important question, and in some places, as where psychology trends upon logic, are comparatively meagre; but some account is given of most of the more prominent discussions. These notices, considered as criticisms, will be thought by some to carry but little weight and to present no very noticeable characteristics. Considered as statements of fact, they are learned. The accounts of ancient opinions have evidently not been written without a study of the latest commentaries. In what relates to the history of the Scotch and English schools, even professed students of philosophy will find much that is fresh and instructive. The great defect of this part of the book is that, as a general rule, no account whatever is given of recent works, these being cited only by title. This omission detracts very seriously in some cases from the value of the book. Twenty-five pages of the finest print are devoted to an account of the various theories of perception without the least mention, except by title, of the writings of Fechner, Wundt, Trendelenburg, George, Lotze, and others, whose investigations may truly be said to be of more value than all the others put together.

Mediæval doctrines, which are seldom intelligibly treated, are not treated intelligibly here. The reader is for the most part expected to gather the opinions of the masters and doctors from single quoted sentences, which are often utterly meaningless or even misleading to those who have not given special attention to scholastic philosophy. Take for example the account of nominalism and realism on pages 405-407. What is a person not already acquainted with the subject to make of the statement that a certain master taught that a universal is "indifferenter" in all the singulars under it? How correct a notion is he likely to form of Abelard's doctrine from being told that he "*sermones* intuetur et ad illos detorquet quicquid alicubi de universalibus meminit scriptum"? Will he understand, as he should, that the *sermo* means a word actually in application by the mind as a predicate? Considering the historical importance of Roscellin, and considering the fact that, though an extreme nominalist, his doctrines were associated with those of Scotus Erigena, who was a sort of Platonistic idealist, is it quite sufficiently explaining his views to quote that sentence of Anselm's in which he is said to have thought that universal substances are the breath of the voice, that the wisdom of man is the soul, and that color is the colored body? It would have been easy to explain, first, that the *vox* was regarded by grammarians of that age as something incorporeal, because it is *produced* by the percussion of the palate and the air, but *is* not either, and because a natural motion cannot produce a new body, and also because the *vox* is in several ears at once, whereas a body can only be in one place at one time; that we have positive reason to think

that Roscellin believed this; that, in the second place, reasoning (as we may suppose) like others in that age from such facts as that the same line which, when measured by one measure (a foot) is equal to *two*, when measured by another (an inch) is equal to *twenty-four*, and that the wall of a house is on the one hand a whole in itself and on the other a part of a house, he came to believe (as we are positively informed) that all mathematical relations—that is, all relations of parts and whole—exist not in the body itself, but only in the incorporeal words which may be applied to it; and that, thirdly, he thence inferred that those universal essences of things, genera and species, since they essentially have parts and are parts, themselves are not things, but incorporeal *voces*. Of any interruption in the course of the controversy between the twelfth and thirteenth centuries our author tells us nothing although the discovery of all the works of Aristotle except the two short treatises already known, and of the writings of the Arabian commentators, had in the interval between Abelard and Albertus so changed the whole face of scholasticism that it is rarely indeed that any writer of the twelfth century except Peter Lombard and Gilbertus Porretanus is quoted at all in the thirteenth. The facts that Albertus had properly no opinion of his own and that that of St. Thomas was very vacillating (as was notorious in the fourteenth century) are not mentioned. Scotus's realism is said to be identical with that of these writers except as to the *hœcceity*; but the difference is more important. The Thomistic view was that of the two elements of the individual thing—that is to say, the matter and the form, or that which makes it to be, and that which makes it, if it is to be, to be *as* as it is—the form is always universal, the matter, or at least signate matter (this or that matter), is always singular. Their union is an individual, but it is a union in which the form is as such actually universal in itself. Scotus admitted that in the singular thing there is nothing actually universal; all generality results from a relation of reason. Nevertheless, when a general predicate is attached by the mind to a thing, the proposition so formed may be true, and since the *same* predicate may also be truly asserted of other things, it is true that there is something in the thing which, though actually contracted to the grade of singularity, is in its own nature not repugnant to being predicated of many. There is, then, a distinction between a predicate predicated of many and the singular forms in the several things by virtue of which the same general predicate is true. Yet since this general predicate *is* true, it really is in the several things, although it is there in the grade of singularity and identified with these singular forms. Thus there is a really, but only potentially, general form in the singular thing which yet in that thing in itself does not differ from the singular thing. This is the famous doctrine of formal distinctions, which is the central idea of the whole Scotistic philosophy. This formed also the very point of Occam's attack, for his whole notion of a reality was that of a thing which is in itself whatever it really is. This he was able to see must be something devoid of all quality and all relations. All qualities and relations, according to him, are *terms*, subjects and predicates of written, spoken, or thought propositions; and the qualities and relations of things can consist of nothing except that the mind naturally applies to them such and such *terms*. Prof. Porter says the controversy

came to a close early in the fourteenth century, but Occam did not die until 1347, and it certainly raged with the greatest fury after his death.

The Scotch school of philosophy, to which this work belongs is too old a tree to bear good fruit. Its method consists in an appeal to consciousness—that is to say, to what all men know and know that they know (p. 113)—supported by some familiar facts and occasional anecdotes. Such a procedure is not wholly useless. The common sense of mankind has so little impulse to seek explanations of facts that it is hardly tempted to twist them, and he who busies himself with reproducing ordinary beliefs is free from so deep an absorption in laborious experiments and observations as to overlook what lies upon the surface. The great mistake of writers of this sort has been that they have had an ambition to be more than accurate describers of common beliefs and unanalyzed facts. That natural self-consciousness, when heightened by direct effort, becomes a scientific knowledge of the soul, is not the doctrine of modern psychology. This opinion is disappearing, and with it will probably disappear some of that morbid tendency to introspection, the prevalence of which justified the advice given by the editor of a magazine to a contributor, "Should you ever be drowned or hung, be sure and make a note of your sensations; they will be worth to you ten guineas a sheet." The efforts which Dr. Porter recommends, "to hope and fear again and again, simply that we may know more exactly how it seems or what it is to perform [*sic*] or experience these states," to say nothing of their double futility (for we cannot so hope and fear, and if we could it would teach us little of the essence of these emotions), are very unwholesome.

Within the Scottish school we should suppose that this book must take a very high rank. Indeed, as long as Mr. Mansel (even if he properly belongs to that school) produces nothing more, we do not see what living writer, unless it be Dr. McCosh, is to dispute with Dr. Porter the honor of the very first place. In the character of his genius and learning more like Dugald Stewart than any of the other *coryphœi* of that philosophy, Dr. Porter's relation to Scotch psychology is somewhat similar to that of Hamilton, inasmuch as he modifies the pure Scotch opinions by an admixture of the prevalent German views. As Hamilton treated high metaphysics upon modified Kantian principles, so Porter imports into the same branch of philosophy considerations which have been derived in large measure from the study of Trendelenburg. His metaphysic starts, as it ought, with a theory of inductive reasoning. He holds that the reason why an innumerable number of instances will not justify the inference that all swans are white, while a single instance would suffice to show that all men's heads are placed upon their shoulders, is because a failure of the latter induction, unlike a failure of the former, would be "entirely incompatible with the ideal of beauty and convenience to which we assume that nature would certainly conform." Since then the validity of induction rests upon certain assumptions of this sort, these assumptions are not themselves demonstrable either by induction or otherwise, but are original and self-evident truths. These intuitions are as follows: 1st, that an object is either substance or attribute; 2d, that objects originate by a causative energy; 3d, that objects are in space and time; 4th, that properties and laws which

are known *indicate* and *signify* other properties and laws; 5th, that *nature adapts objects and powers to certain ends*; and 6th, that the *rational methods of the divine and human minds are similar.* These ultimate facts and relations are not learned by the ordinary processes of thought, imagination, and perception. They are "not *apprehended by*, but *involved in*, these processes," and must, therefore, be referred to a separate faculty. They are first apprehended in a concrete, not in an abstract, form. We do not set out with the universal belief that every event has a cause, but as we apprehend each separate object by perception or consciousness we apprehend it as caused. Such apprehension is a proposition, and from such propositions are derived the various concepts, substance and attribute, cause and effect, means and end, etc. These concepts being apprehended abstractly and compared with the processes of cognition are found to be essentially involved in them all. Finally, it is perceived that over against all objects of experience, as having these various relations of dependence, there must be some independent correlates upon which they depend. Thus all things being extended, there must be a space; in correlation with all things as being caused there must be a First Cause, etc. The whole argument upon this subject, which occupies some two hundred pages, is followed out with great ability. It will be perceived that this theory of intuition has a general resemblance to that of Dr. McCosh.

It is easy to see upon what side such a theory may expect attack. Its essence is that the process by which we attain our first knowledge of these fundamental ideas is essentially different from the other processes of the mind. Now, if it were shown that all the other mental processes, whether of cognition, emotion, or action, were essentially one, it would be hard to prevent men from believing that this process alone did not conform to their common formula. Accordingly, it is not surprising that we find throughout Dr. Porter's work a tendency to exaggerate the distinctions between the faculties and to overrate the importance of these distinctions, and to explain facts by the general supposition of a peculiar faculty even when such a supposition requires it to be as complex as the facts themselves, in order to explain them in detail. But though the reader of this book would scarcely suspect it, there is a movement which is steadily coming to a head towards identifying all the faculties. It is the motive of all sensualism, it is the latest mood of psycho-physical inquirers, and it is beginning to be consciously felt even in this country. If that doctrine should once be established, it would not avail Dr. Porter's theory that he had correctly answered the question why the inference that all men carry their heads upon their shoulders is so strong, because it would appear that the principle of design which effects this inference is only a derivative one, and that the only assumption which can enter into every induction is no assumption about the things reasoned upon at all. Dr. Porter's opinion is, that the assumptions involved in induction are the only basis of religion; but the only assumption which can be essentially involved in scientific inference is the assumption of the validity of scientific inference. But to make the validity of scientific inference the only possible basis of religion approaches very near to pure rationalism—a doctrine that is not in the interest of religion, because it subordinates religion to science. We are inclined to suspect that the metaphysician,

whether spiritualist or materialist, is in this dilemma; either he must look upon his problems with the cold eye of science, and have no other feeling for the eternal interests of man than the curiosity with which he would examine a trilobite; and then, being in a state of mind essentially irreligious, he can arrive at no result that would really help religion, for at most he can only say to mortal man that it is most likely that there is a God, which is no assurance; or he must bring the feelings of a religious man into the inquiry, and then he is as incompetent to treat the problem as a physician is to judge of his own case. Can it possibly be, that the directest and most uncritical faith in the object which commands one's adoration —the faith of a little child—is the only actual motive to religion which there ever has been or ever will be, and that all reasonings *pro* or *con*, upon the fundamental proposition of religion must be entirely irrelevant and unsatisfactory?

9 (22 July 1869) 73-74

ROSCOE'S SPECTRUM ANALYSIS

Spectrum Analysis. Six Lectures delivered in 1868, before the Society of Apothecaries of London.

By Henry E. Roscoe, B.A., Ph.D., F.R.S., Professor of Chemistry in Owens College, Manchester. New York: D. Appleton & Co. 1869.

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

Sir Henry Enfield Roscoe (1833-1915) was a chemist of great renown, having been graduated with honors from University College, London, in 1852, at which time he undertook work with R. W. von Bunsen in Heidelberg, an association which resulted in important scientific advances. In 1857 he was elected to the chair of chemistry at Owens College, Manchester. He was knighted in 1884, and elected Member of Parliament for South Manchester in 1885. While in Parliament, he supported and sponsored many articles of industrial reform legislation.

The sudden impulse which spectroscopic researches received in 1860, and which has resulted in several brilliant discoveries in chemistry and astronomy, affords a singular problem in the history of scientific progress. There was nothing absolutely new in the method of Kirchhoff and Bunsen. It consisted essentially in observing the spectra of the colorations imparted by different substances to the non-luminous gas-flame generally used in laboratories. Colored flames had been used since an early period in the history of chemistry for distinguishing the different alkalies and alkaline earths; and J. F. W. Herschel in 1822, H. F. Talbot in 1826, and W. A. Miller in 1845, had made some study of the spectra of these flames with reference to chemical analysis. The black lines of the spectra of some of the stars had been examined by Fraunhofer, and found to differ from those of the spectrum common to the sun, moon, and planets. The absorption-lines produced by some gases had been studied by Brewster; and Stokes had pointed out the use of absorption-bands in detecting certain metals in solution. The coincidence of the bright line of incandescent sodium vapor with the D line of the solar spectrum had been noticed by Fraunhofer; and Stokes and William

Thomson thence inferred that sodium was contained in the atmosphere of the sun, because a substance can only emit what it is capable of absorbing.

These investigations appertain to all parts of spectral analysis. Why, then, did they remain comparatively unfruitful while the very first memoir of Kirchhoff and Bunsen created a sensation such as the scientific world had not felt since the discovery of Neptune? Kirchhoff himself seems to think that it was because he and Bunsen first clearly showed that the positions of the spectral lines depend solely upon the chemical constituents of the glowing gases. No doubt, the effect upon the imagination of so broad a proposition upon a new matter of science is great, yet the habitual reliance by chemists upon the flame reaction of sodium seems to show that this law had been implicitly assumed upon all hands to be true in practice. Perhaps the chief causes of the profound impression produced by Kirchhoff and Bunsen's papers were these three: 1st, The flame of the Bunsen burner, which was employed by them, was capable from its intense heat and small lighting power of giving much more satisfactory results than the alcohol flames used by the early experimenters; 2d, The new investigations were conducted with a tact and thoroughness which commanded admiration; and 3d, Bunsen had the good fortune and the skill to detect by the new method two metals—rubidium and caesium—before unknown, in some mineral water he was analyzing, the mixed chlorides of these metals being contained in the proportion of about a drachm in twenty tons of the water.

Bunsen not only discovered these elements, but studied them so well (working partly in company with Kirchhoff) that they are now among those whose chemical relations are the best understood. They have been found to be somewhat widely distributed through the mineral kingdom in very small quantities. An Italian mineral, which had formerly been analyzed by the celebrated mineralogist Platner, has been found to contain 34 per cent of the oxide of caesium, which had been mistaken for potassa. Platner's analysis did not add up 100 per cent at all correctly, owing to the great difference in the combining numbers of potassa and caesium. Many a chemist would have been ashamed to own such an analysis; Platner was willing to publish a work which there was no other reason for condemning than one which was perfectly patent, and the result is that time has shown that his experiments were correctly performed. In 1861, an English chemist, Crockes, hardly known before, discovered by means of the spectroscope another metal (thallium) of very singular chemical characters; and this is a discovery which may lead to others, for with thallium a glass has been made which is reported as wonderfully adapted for prisms. In 1863, a fourth metal—indium —resembling zinc was discovered by means of the spectroscope in the zincblende of Freiberg.

The study of the celestial spectra has afforded important information concerning the sun, the stars, the nebulas, some comets, and the aurora borealis. We have learned that many chemical elements which are found upon the earth exist in the atmosphere of the sun, including nearly all of those which form a large proportion of the earth's crust. We have also ascertained, what might have been known *a priori*, that the most elastic of the gases (hydrogen) extends higher from the sun's

centre than any of the other substances. The solar spots are getting examined; and if some observations lately reported are confirmed, we shall have some of the theories upon this subject brought to a test. In the stars have been recognized a number of the chemical elements which we know; yet in many of them some of the commonest substances here, and those most essential to life as we know it, are altogether wanting. A displacement of one of the hydrogen lines in the spectrum of Sirius is held to prove that that star is moving rapidly towards our system. The nebulas have been found to be of two entirely different kinds; for the spectra of some of them have been found to consist of isolated bright lines, showing that these nebulas are gaseous, while by far the larger proportion show the continuous spectrum which is seldom produced by an incandescent gas. This difference between the spectra corresponds strictly to a difference between the ordinary telescopic appearances of the nebulas. This is the more interesting, as the first proposition upon which Sir William Herschel founded his nebula hypothesis was that there was no natural classification among nebulas. None of the nebulas have been proved to contain any substance otherwise known to us. Several minute comets have been subjected to spectroscopic examination, and two of them have been shown to contain carbon in some gaseous state. The spectrum of the aurora, as usually seen, consists of a single yellowish-green line, which belongs to no substance with which we are acquainted. As the aurora is held to be above the ordinary atmosphere (and this is confirmed by its showing no nitrogen lines), it follows that there is some unknown gas reaching above the other constituents of the atmosphere. According to the laws of gravity and of diffusion of gases, this substance must extend down to the surface of the earth. Why, then, have not chemists discovered it? It must be a very light elastic gas to reach so high. Now, the atomic weights of elementary gases are proportional to their density. It must, then, have a very small atomic weight. It *may* be as much lighter than hydrogen as hydrogen is than air. In that case, its atomic weight would be so small that, supposing it to have an oxide on the type of water, this oxide would contain less than one per cent of it, and in general it would enter into its compounds in such small proportions as almost infallibly to escape detection. In addition to the green line usually seen in the aurora, six others were discovered and measured at the Harvard College Observatory during the brilliant display of last spring, and four of these lines were seen again on another occasion. On the 20th of June last, a single narrow band of auroral light extended from east to west, clear over the heavens, at Cambridge, moving from north to south. This was found to have a continuous spectrum; while the fainter auroral light in the north showed the usual green line.*

Professor Roscoe's book contains an interesting and very thorough account of spectrum analysis. The paper, ink, type, and plates are beautiful. In his style, Mr. Roscoe neither aims at sensational effect, nor so strains after simplicity as to verge upon baby talk. And these are the two commonest faults of popular science. The only exaggeration which we have noticed is in the chromo-lithograph

*We have received permission from Prof. Winlock to state this singular fact, which has not been published before.

of the spectrum of nebula. If the book be taken into a nearly dark room, so that at first glance nothing is seen but the dark oblong shapes of the whole spectra of that plate, the figure in question *will* "serve to give some idea of the peculiar beauty of the phenomenon in question." The lines in the spectrum of Sirius, on the same plate, are made much too distinct, both absolutely and relatively to the other stars.

The practical spectroscopists will find here an exceedingly convenient repertory of facts. Kirchhoff's chart of the solar spectrum, with the extension of Angström and Thalen, is very beautifully reproduced in miniature. Huggin's maps of the metal lines are given in a form far more convenient for use at the spectroscope than the two folding sheets in a huge quarto in which alone they have hitherto been published. The numerical tables in full accompany both sets of maps. It is much to be regretted that Dr. Gibb's important tables for the comparison of Kirchhoff's, Huggins's, and the Normal scales have not been given. We should also have been glad to have Thalen's metallic spectra. At the end of the book there is a "List of Memoirs, etc., upon Spectrum Analysis." This is certainly valuable, and appears to be full. We observe, however, the omission of Stoke's paper upon the absorption-bands as a reagent, and also of Secchi's catalogue of the spectra of the stars. As the work contains little about the spectra of particular celestial objects, the last-named paper might well have been translated and inserted in full, with notes.

Professor Roscoe's book may truly be said to be popular and scientific at the same time. And we call it scientific, not only because it is a thorough account of the facts, but also because it contains long extracts from the original memoirs of the serious workers in this branch of science. There is, doubtless, a vast difference between that knowledge of scientific research which comes of actual practice and that which recommends this book to general readers. No one need be scared by a fear that it is mathematical, for everything which borders upon that subject is omitted. There is nothing about the angles of prisms, the theory of exchanges, or the theory of the displacement of lines owning to the motion of the source of light.

9 (25 November 1869) 461-462

THE ENGLISH DOCTRINE OF IDEAS

Analysis of the Phenomena of the Human Mind.

By James Mill. A new edition, with notes, illustrative and critical, by Alexander Bain, Andrew Finlater, and George Grote; edited with additional notes by John Stuart Mill. 2 vols. 8vo. London: Longmans. 1869.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*. The title by Wundt that Peirce mentions in his note is more fully described as: Wilhelm Wundt, *Vorlesungen über die Menschen und Thierseelen*, Leipzig, 1863, 1st ed.

James Mill (1773-1836) entered the University of Edinburgh in 1870. There he was influenced by the Scottish philosophy as presented by Dugald Stewart, who was lecturing in

Edinburgh at that time. In 1802 he moved to London where he became involved in politics and various literary projects. Around 1808 he formed a friendship with Jeremy Bentham, later becoming a very close disciple. Mill is known for his works in philosophy, history, education, and economics, as well as for his participation in the political life of his day.

George Grote (1794-1871) was an English historian and brother of the Cambridge philosopher John Grote. In his early years, Grote was a friend of Ricardo, James Mill, and Bentham. He was one of the founders of London University, and served as trustee for the British Museum. Grote's most famous work is his *History of Greece*, which has enjoyed several editions and translations into French and German.

James Mill's "Analysis of the Human Mind" has long been known as one of the most original and characteristic productions of English thought. It now appears in a second edition, enlarged by many long notes by the author's disciples, who are to-day the most eminent representatives of the English school. These notes are chiefly of interest as forming the clearest exposition of the present state of opinion in that school, and of the changes which it has undergone since 1829.

It is a timely publication, because the peculiarities of the English mind are so sharply cut in James Mill that it will help to awaken that numerous class of general readers who have become impregnated with the ideas of Stuart Mill's logic into self-consciousness in reference to the intellectual habit which they have contracted. A philosophy or method of thinking which is held in control—the mind rising above it, and understanding its limitations—is a valuable instrument; but a method in which one is simply immersed, without seeing how things can be otherwise rationally regarded, is a sheer restriction of the mental powers. In this point of view, it is a fact of interest to the adherent of the English school that it is not a particularly learned body, and that its more modern leaders at least have not generally been remarkable for an interior understanding of opposing systems, nor even for a wide acquaintance with results the most analogous to their own which have been obtained in other countries. It is a familiar logical maxim that nothing can be comprehended without comparing it with other things; and this is so true in regard to philosophies that a great German metaphysician has said that whoever has reached a thorough comprehension of a philosophical system has outgrown it. Accordingly, we think that we discern in English philosophers an unconsciousness of their own peculiarities, and a tendency to describe them in language much too wide; in consequence of which the student has to gather the essential characters of their thought by a comparison with different systems, and cannot derive any real understanding of them from anything which lies wholly within their horizon alone.

This somewhat insular group of thinkers are now often called Positivists. If this means that they are the philosophers of exact experience, it is too much to say of them; if it means that they are followers of M. Comte, it is too little. They seem to us to be what remains of that *sacra schola invictissimorum nominalium*, of which the English Ockham was the "venerable beginner." Many pages of this "Analysis" might, if somewhat changed in language, easily be mistaken for Ockham's.

The chief methodical characteristic of their thought is "analysis." And what is analysis? The application of Ockham's razor—that is to say, the principle of re-

ducing the expression of the nature of things and of the mind to its simplest terms by lopping off everything which looks like a metaphysical superfluity. By mental analysis the English mean the separation of a compound idea or sensation into its constituent ideas or sensations. Thus, they would say that the sensation of white had no distinct existence; it is merely the concurrence of the three sensations of blue, red, and yellow. So, James Mill says that virtue is the habit of associating with the actions from which men derive advantage the pleasures which result from them. It is plain that such analysis reduces the number of distinct constituents of human nature. The same thinkers reason in a manner entirely analogous when they are not dealing with the mind at all; and in general their method may be described as simplifying existing hypotheses and then endeavoring to show that known facts may be accounted for by these simplified hypotheses. In this way, a highly elegant and instructive system has been created; but it is not pre-eminently scientific. It might be scientific if these philosophers occupied themselves with subjecting their modified theories to the test of exact experience in every possible way, and spent their time in a systematic course of observations and measurements, as some German psychologists have done. But that is not their business; they are writers. Their energies are occupied in adjusting their theories to the facts, and not in ascertaining the certainty of their theories. This cannot be said to hold good fully in the case of Mr. Bain; his books are largely occupied with correcting and limiting theories; but so far he appears quite different from the English school generally, to which, however, he certainly belongs. Desultory experience is what they all build on, and on that basis no true science can be reared.

James Mill's psychological theory is this: All that is in the mind is sensations, and copies of sensations; and whatever *order* there is in these copies is merely a reproduction of the order which there was in their originals. To have a feeling (a sensation, or the copy of one), and to know that we have it, and what its characters are; or to have two feelings, and to know their mutual relations and agreements, are not two things, but one and the same thing. These principles are held to be sufficient to explain all the phenomena of mind.

The beauty of this theory appears when we consider that it is as much as to say simply that *ideas* in *consciousness* are concrete images of *things* in *existence.* For a thing to exist, and for it to have all its characters; or for two things to exist, and for them to have all their relations of existence to each other, are not two facts, but one. A book which thoroughly follows out such a hypothesis is a great contribution to human knowledge, even if the hypothesis does not satisfy the facts. For it clears up our conceptions greatly to understand precisely how far a simple, single supposition like this will go, and where it will fail.

The theory is of the most markedly English character. Though it is a single supposition which cannot logically be broken, yet we may say that its chief points are these three:

1. Every idea is the mere copy of a sensation.
2. Whatever is in the mind is known.
3. The order of ideas is a mere reproduction of the order of sensations.

That every idea is the copy of a sensation has always been recognized as the chief point of English psychology. Hume expresses it in the clearest language, saying that the difference between an idea and a sensation is, that the former is faint and the latter lively. This involves the opinion that all our ideas are singular, or devoid of generality; that is, that just as every existing thing either has or has not each conceivable quality, so every idea is an idea of the presence or absence of every quality. As Berkeley says, my idea of a man "must be either of a white or a black or a tawny, a straight or a crooked, a tall or a low or a middle-sized man." Accordingly, it is obvious that one of the difficulties in the way of these philosophers is to explain our seeming to attach a general meaning to words; for if we have nothing in our minds but sensations and ideas, both of which are singular, we cannot really take a word in a general sense. So, if I compare a red book and a red cushion, there is, according to them, no general sensation *red* which enters into both these images, nor is there any idea of a general respect, color, in which they agree; and their similarity can consist in nothing whatsoever, except that they have the same general name attached to them; and there is no possible reason for their being associated together under one name (which these philosophers can consistently give) than one at which James Mill hints, and which follows from his principles—namely, that the corresponding sensations have been frequently associated together in experience. This was perfectly appreciated in the days when nominalism was actively discussed, but now the nominalists do not seem to look it in the face. We will, therefore, put some passages from the present work in juxtaposition, to show that James Mill did feel, obscurely perhaps, this difficulty. "Every color is an individual color, every size an individual size, every shape an individual shape. But things have no individual color in common, no individual shape in common, no individual size in common; that is to say, they have neither shape, color, nor size in common" (vol. i., p. 249). He here speaks of things; but as things are only sensations or ideas with him, all this holds good of ideas. "It is easy to see, among the principles of association, what particular principle it is which is mainly concerned in classification. . . . That principle is resemblance." "Having the sensation. . . . what happens in recognizing that it is similar to a former sensation? Besides the *sensation*, in this case, there is an *idea*. The idea of the former sensation is called up by, that is, is associated with, the new sensation. As having a sensation, and a sensation, and knowing them, that is, distinguishing them, are the same thing; and having an idea, and an idea, is knowing them; so, having an idea and a sensation, and distinguishing the one from the other, are the same thing. But to know that I have the idea and the sensation, in this case, is not all. I observe that the sensation is like the idea. What is this observation of likeness? Is it anything but that distinguishing of one feeling from another which we have recognized to be the same thing as having two feelings? As change of sensation is sensation; as change from a sensation to an idea differs from change to a sensation in nothing but this, that the second feeling in the latter change is an idea, not a sensation; and as the passing from one feeling to another is distinguishing, the whole difficulty seems to be resolved, for undoubtedly the distinguishing differences and similarities is the

same thing—a similarity being nothing but a slight difference" (vol. ii., p. 15). Evidently, if a similarity is a difference, the line of demarcation between the two is to be drawn where our language happens to draw it. But to ascertain why two similar sensations are associated under one name, we must recur to his general law of association, which is given in these words: "Our ideas spring up or exist in the order in which the sensations existed, of which they are the copies. This is the general law of the 'Association of Ideas' " (vol. i., p. 78). "Resemblance only remains as an alleged principle of association, and it is necessary to enquire whether it is included in the laws which have been above expounded. I believe it will be found that we are accustomed to see like things together. When we see a tree, we generally see more trees than one; when we see an ox, we generally see more oxen than one; a sheep, more sheep than one; a man, more men than one. From this observation, I think we may refer resemblance to the law of frequency, of which it seems to form only a particular case" (vol. i., p. 111). This is what he says upon the subject of similarity. As an attempt at analyzing that idea, it is a complete failure, and with it the whole system falls. Stuart Mill is gravely mistaken in supposing that his father's rejection of resemblance as a guiding principle of association was an unimportant part of his theory. Association by resemblance stood in the way of his doctrine that the order of ideas is nothing but the order of sensations, and to grant the mind a power of giving an inwardly determined order to its ideas would be to grant that there is something in the mind besides sensations and their copies. Moreover, upon nominalistic principles similarity can *consist* in nothing but the association of two ideas with one name, and therefore James Mill must say, with Ockham, that such association is without any reason or cause, or must explain it as he attempts to do. The doctrine that an idea is the copy of a sensation has obviously not been derived from exact observation. It has been adopted because it has been thought that it *must be so*; in fact, because it was a corollary from the notion (which its authors could not free themselves from) that ideas were in consciousness just as things are in existence. It thus forms a striking illustration of Wundt's remark that the chief difference between modern attempts to put psychology upon a basis like that of the physical sciences and earlier speculative systems, is that speculations are now put forth as results of scientific research, while formerly facts of observation were frequently represented as deductions of pure thought.

The same thing may be said of the doctrine that to feel and to be aware of the feeling are the same thing. James Mill plainly cannot cenceive of the opposite supposition. With him, therefore, it is a mere result of defective reading. It is not only not supported by exact observation, but it is directly refuted in that way.

The English school are accustomed to claim the doctrine of the association of ideas as their own discovery, but Hamilton has proved that it is not only given by Aristotle, but that, as to its main features, the knowledge of it by the English was derived from him. This, therefore, does not constitute a valid claim to the scientific character; yet it is the only claim they have. At present, the doctrine has received a transformation at the hands of Wundt of the most fundamental description. He has solved the perplexing questions concerning the principles of associa-

tion by showing that every train of thought is essentially inferential in its character, and is, therefore, regulated by the principles of inference.* But this conception is also found in Aristotle.

The "Analysis" is written in an unusually forcible, perspicuous, and agreeable style—a character which belongs to most of the English philosophers more or less, but to none in a higher degree than to James Mill. One wishes that such a master of language had a doctrine to enunciate which would test his powers more than this simple English psychology. The fewer elements a hypothesis involves, the less complication and consequent obscurity will appear in its development.

*This idea is fully explained in his very important and agreeably written "Vorleungen über die Menschen- und Thierseelen."

1870

11 (4 August 1870) 77-78

BAIN'S LOGIC

Logic.

By Alexander Bain, LL.D., Professor of Logic in the University of Aberdeen. Part First, Deduction. Part Second, Induction. 2 vols. 8vo. London: Longmans. New York: D. Appleton & Co.

We have discovered no manuscript sources that suggest that Peirce wrote this review. Fisch, in *First Supplement*, attributes this to Peirce, but as "uncertain." Two kinds of internal evidence, however, do suggest that Peirce is the author. First, he wrote a great many logic reviews for *The Nation*. Second, there are a few themes in the review that are characteristic of Peirce. In the first paragraph, there is a slap at "English narrowness." The discussion of chemistry, plus the example from mathematics on parallels, taken in conjunction with the fact that the review concerns a logic book, constitutes a constellation of topics that is distinctly Peircean. Another characteristic theme is antinominalism, which appears here in the claim that Bain is associated with the nominalists. This review is unassigned in Haskell's *Index to* The Nation, vol. 1.

Alexander Bain (1818-1903) studied at Marischal College, Aberdeen. In 1848 he moved from Scotland to London where he held various posts in education and civil service. He returned to Aberdeen in 1860 to a chair of logic and English. He resigned this professorship in 1880, but in later years twice served as rector of his university. He authored many books in philosophical psychology, logic, and ethics. He also founded the distinguished philosophical periodical, *Mind*. John Stuart Mill was a close friend, Bain being the biographer of James Mill.

Many works on logic have lately appeared in our language, and a few of them are of considerable importance. The one before us is a school-book of the dryest description, but it is impossible that the best living English psychologist should produce any book which has not the stamp of originality, and which is not deserving of attention. In point of fact, Mr. Bain distinctly proclaims himself a rival, although also a follower, of Mr. Mill. The first thing that we notice in all the English logicians, and Mr. Bain is no exception, is their ignorance or ignoring of all logical writings not English. This is the more reprehensible, as logic has by no means received its greatest development in England. Nothing in the present work will lead the student to suspect that there are any such writers as Trendelenburg or Beneke, although the latter entertains opinions which are more or less in harmony with Bain's own. Trendelenburg has made an elaborate study of Aristotle's categories, the results of which are undeniably of high importance, even if they are not to be regarded as fully established. But Professor Bain does not find it worth while so much as to mention them in his account of the same subject. The exclusively English character of Mr. Bain's work is well illustrated by his making the old distinction of extension and comprehension belong to Hamilton, and by his giving the same writer credit for the symbols S, M, and P, for the three terms of a syllogism.

The chief peculiarity of this treatise is its elaborate treatment of applied logic. One-fourth of the whole book is taken up with "Logic of Mathematics," "Logic

of Physics," "Logic of Chemistry," "Logic of Biology," "Logic of Psychology," "Sciences of Classification," "Logic of Practice," "Logic of Politics," and "Logic of Medicine." The word logic in these phrases is taken in a very much wider sense than that in which Dr. Whewell spoke of the logic of induction. Logic in general is defined by Mr. Bain as "a body of doctrines and rules having reference to truth." He regards logic, therefore, not merely as the *via veritatis*, but as including everything which bears upon truth, whether it relates to the investigation of it or to the testing of it, or simply to what may be called its statical characters. Accordingly, the logic of a particular science is the general description of the nature of that science, including not merely its methods, but also its fundamental conceptions and doctrines. As an example, let us take the logic of chemistry. The author begins by stating the essential characters of chemical attraction. They are three: first, that the proportions (misprinted *properties*; the book is full of misprints) are definite; second, that in combination heat is evolved; third, that the chief properties of the elements disappear. He next divides the propositions of chemistry into two classes; first, those which relate to the general conditions of chemical change; second, those which relate to the chemical changes of special substances. He next divides chemistry into organic and inorganic. (Few chemists would now maintain that this division has more than a temporary validity.) He then proceeds to the classification of the elements. The first great division is into metals and non-metals (this is antiquated). The general properties of each group are enumerated, as, for example, that no opaque non-metal has lustre except selenium (forgetting iodine and carbon). He then gives a classification (very unscientific) of the non-metals. He then says how he thinks a chemical substance should be described in a text-book. He seems to be thinking all along of how a text-book should be written, and not of how the subject should be investigated or conceived in the mind of the chemist, for he urges it as a recommendation to the uniting of oxygen and nitrogen in one class that it gives an opportunity for dwelling on the mechanical peculiarities of gaseous elements. He then states the characters of chemical laws. They are two. The first is that such laws are empirical. As an example, he cites the so-called law of Berthollet, in evident ignorance that this law has been entirely disproved. The other property of chemical laws is that they must express the most general conditions of the redistribution of chemical force. He next remarks that most of the hypotheses of chemistry are representative fictions, and concludes with a few elementary observations upon chemical notation. Such an account of a science as Mr. Bain here attempts would certainly be of the greatest value. It is very unlikely that any one man could successfully accomplish the task for all the sciences. At any rate, he must be profoundly versed in them, and must have quite another than a schoolmaster's conception of science in order to make his work of any use at all. But to attempt to write the logic of mathematics, for example, when one is so ignorant of the work of mathematicians as to be capable of saying that the celebrated axiom concerning parallels is "deducible from the definition of parallel lines, and ought to appear among the theorems of the first book," we must say, smacks of conceit.

Another principal feature in the book is the treatment of definition. Like many of the old logicians, the author separates the process of forming a definition

from reasoning, a separation which ought not to be made, because analysis of the former proceeding shows it to contain the same elements as the latter. His attaching a very high importance to definition is more in accordance with the tendencies of natural science than it is with the doctrines of that nominalistic school of metaphysics with which Mr. Bain is affiliated. He rightly insists that the characters of the object which are enumerated in the definition should be such as are *important*, but his analysis (usually weak) fails to detect in what the *importance* of a character consists. A sentence which he has quoted from Sir George Cornewall Lewis might have furnished him with a hint. "By including in monarchies," says that writer, "and excluding from republics, every government of which a king is the head, *we make every true general proposition respecting monarchies and republics impossible.*" An *important* character is obviously one upon which others depend, that is, one the inclusion of which in a definition renders true general propositions concerning the object defined possible; and the more such propositions a character renders possible, the more important it is. In the same way, a natural class is one which can be so defined that something can be predicated of it which cannot be predicated of the genera included in its definition. Mr. Bain endeavors to make the logical definition identical with the scientific definition—a most worthy aim; but we fancy that zoölogists and botanists are already so much advanced in the knowledge of classification beyond the mere logician, that Mr. Bain's maxims will have little weight with them.

In treating of causation, Mr. Bain includes in the pure logical principle the law of the conservation of force, which according to him, in opposition to the physicists, refers not to *vis viva* but to *momentum.*

He gives a long account of the systems of De Morgan and Boole, but not such a one as they would approve, and he makes some serious mistakes.

As a school-book the work has some advantages, but even where the author's thought is perhaps not itself vague, his manner of expressing it is not calculated to inculcate precision in the mind of the pupil.

1871

12 (13 April 1871) 258

NOTES

This obituary notice is mentioned in the note that immediately follows—12 (20 April 1871) 276—which Fisch attributes to Peirce. Therefore, the foregoing notice is included here in order to complement comments in the next item. This piece is unassigned in Haskell's *Index to* The Nation, vol. 1.

—A scarcely less voluminous writer was Professor De Morgan, who was born at Madura, in Southern India, in June, 1806, of a family distinguished in the military service. His mother's grandfather, however, who was a mathematical teacher of some eminence, may be supposed to have predetermined his career. In 1827, he gained at Cambridge the first place in the mathematical tripos of that year, but declined to subscribe to the religious tests necessary to obtain either the degree of M.A., or a college fellowship. In 1828, he accepted the professorship of mathematics in the London University, the principles on which that institution was founded being in accord with his religious independence; and he abandoned this position in 1866 when, as he thought, in violation of those principles, James Martineau was refused a professorship on account of his theological opinions. In the service of the London insurance companies, "he raised the actuary's vocation to the dignity of a profession," and was almost to his last day the confidential adviser of several associations. His "Essay on Probabilities," "Elements of Algebra," "Formal Logic, or the Calculus of Inference Necessary and Probable," and "Differential and Integral Calculus," are among the works which made him distinguished, but which show but a small part of his intellectual activity. He was a constant contributor to various periodicals, to the *Athenæum* from 1840; and by no means on mathematical subjects alone. "His contributions to Knight's *Penny Cyclopædia* are a considerable proportion of the entire work. "He passed for diversion's sake from one arduous study to another;" but found time to acquire a good degree of proficiency as an instrumental performer, and was a habitual and eager reader of novels, especially of humorous novels. As a mathematician he had the rare merit of not overestimating his favorite science, though he proved by his "Formal Logic" that it was not incompatible for a mathematician to be also a logician; and he was accordingly one of the weightiest adherents that Spiritualism has ever won over. A treatise of his on these manifestations, entitled "From Matter to Spirit," was written in 1863. As a writer and a teacher, he was one of the clearest minds that ever gave instruction, while his genial and hearty manners in private and in the school-room strongly attached to him all who came in contact with him. He was a man of full habit, much given to snuff-taking; and those who have seen him at the blackboard, mingling snuff and chalk in equal proportions, will not soon forget the singular appearance he often presented.

12 (20 April 1871) 276

NOTES

Attributed to Peirce by Fisch in *First Supplement* (internal evidence). This notice is unassigned in Haskell's *Index to* The Nation, vol. 1. Peirce met De Morgan in 1870.

—We need not apologize for adding to the sketch we gave last week of the late Professor De Morgan a few remarks of a more critical nature. Among mathematicians he was distinguished more for the completeness of his logic than for analytical facility. His pupils speak of him with warm admiration, but it may be presumed that they gained from him even more of general skill in accurate reasoning than of specific mathematical power. His elementary books, which are not enough known, are excellent, especially for students who have no natural turn for mathematics; and his work on the calculus is unusually complete, and its demonstrations particularly instructive. Of his researches, one of the most noticeable is his paper on triple algebra, which traces out the consequences of certain definitions of symbols in a manner much like that of his formal logic; but for this difficult subject De Morgan's analysis was not sufficiently subtle and he can only be said to have started the enquiry without having arrived at any valuable results. His best contributions were to mathematical logic. In his controversy with Sir William Hamilton, in 1847, both disputants fought in the dark, because Hamilton's system had never been published, and Hamilton had never patiently examined De Morgan's. All the points of Hamilton's attack were, however, completely disproved. Upon the publication of Hamilton's works, De Morgan renewed the controversy with Mr. Spencer Baynes, who, after an unconditional pledge to produce proof of his position, was compelled to abandon the field. Since that time Hamilton's once celebrated system has fallen into neglect, while De Morgan's commands more and more respect. In point of fact, Hamilton's system, like De Morgan's, is mathematical, but is the work of a mind devoid of mathematical training. It would be premature to try to say what the final judgment of De Morgan's system will be, but it may at least be confidently predicted that the logic of relatives, which he was the first to investigate extensively, will eventually be recognized as a part of logic. The best statement of De Morgan's system is contained in his "Syllabus of a Proposed System of Logic," but his fourth and fifth papers on the syllogism are of later date. De Morgan was a deep student of the history of the sciences to which he was devoted. He wrote many biographical notices of mathematicians in the "Penny Cyclopædia," and the "English Cyclopædia," as well as a bibliography of arithmetic. Indeed, the amount of his writing upon various subjects in the two cyclopædias, in the *Athenæum*, in the *Companion to the British Almanac*, in seventeen or more separate books, and in various scientific periodicals, including the *Journal of the Philological Society*, is enormous, and it is all very pleasant reading for its perspicacity, vigor of thought, wit, and a certain peculiar flavor of style. The last qualities are well seen in his "Budget of Paradoxes," published in the *Athenæum*.

13 (2 November 1871) 294

NOTES

This is probably by Chauncey Wright, inasmuch as the comments on Peirce's review of Fraser's *Berkeley*—see 13 (30 November 1871) 355-356—are by Wright, according to Haskell in *Index to* The Nation.

There are six critical notices this month, and they compare favorably, for weight and learning, with the rest of the number, which, taken altogether, is a very good one, with nothing bad in it, and much that is very good, and having, indeed, no fault except the good-sized fault, that it is deficient, almost to destitution, in purely literary matter, and that, for a "Review," it notices not many books. Those which it does notice, however, it treats with all the customary care. They are these: Delbruck's "Uses of the Conjunctive and Optative in Sanskrit and Greek"; Dr. J. F. Clarke's "Ten Great Religions of the World"; the sixth edition of Professor Max Müller's "Lectures on the Science of Language"; the second and third volumes of Greene's "Life of Major-General Nathanael Greene"; Professor A. C. Fraser's edition of "Berkeley's Works"; and the "Battle of Dorking" —to the remarks upon which we have already referred. The initials "C.S.P." are appended to the review of Berkeley, and, doubtless, they stand for Mr. Charles S. Peirce, who, it is probable, has of all men paid most attention to the subject which he handles in this essay. It is much more than a mere notice of Mr. Fraser's volumes, and we must reserve till next week what we have to say about it.

13 (30 November 1871) 355-356

NOTES

Chauncey Wright, identification: Haskell, *Index to* The Nation, vol. 2.

Chauncey Wright (1830-1875) was graduated from Harvard College in 1852. He was known primarily as a philosopher, having contributed several important essays in that subject to the *North American Review*. In addition to working in philosophy, he made contributions to mathematics and biology, his essays in defense of the evolution of species being reprinted in England at Darwin's insistence. He became a regular member of the Harvard faculty in 1874, where he taught for one year until his untimely death.

—Mr. Charles S. Peirce, in his review of Berkeley in the last *North American*, to which we promised to return, takes the occasion to trace out in the history of philosophical thought in Great Britain the sources of Berkeley's doctrines and of later developments in English philosophy. These he traces back to the famous disputes of the later schoolmen on the question of realism and nominalism—that question on which each new-fledged masculine intellect likes to try its powers of disputation. But the motive of the schoolmen who started this question or gave it prominence, was not in any sense egotistical, however pugilistic it may have been, but was profoundly religious—more religious, in fact, than anything modern, and, perhaps, more fitly to be compared to the devotion that produced the Gothic architecture than to anything else. The most remarkable thing in the essay is Mr. Peirce's interpretation of the actual question so earnestly agitated.

This, it should seem, is not at all what has become the universally accepted account of this voluminous dispute—an account derived, it appears, from Bayle's Dictionary. The realistic schoolmen were not such dolts as to contend for an incognizable reality beyond any powers we have for apprehending it, nor for the existence of universals as the objects of general conceptions existing outside of the mind. They only contended (against the sceptical or nominalistic tendency) that reality, or the truth of things, depends on something besides the actual courses of experience in individual minds, or is independent of differences and accidents in these; and that truth is not determined by the conventions of language, or by what men choose to mean by their words. So far from being the reality commonly supposed—that is to say, the vivid, actual, present contact with things—the reality of the realists was the final upshot of experience, the general agreement in all experience, as far removed as possible from any particular body's sight, or hearing, or touch, or from the accidents which are inseparable from these. Yet it is essentially intelligible, and, in fact, is the very most intelligible, and is quite independent of conventions in language. The faith of the realists (for theirs was a philosophy of faith) was that this result of all men's experience would contain agreements not dependent on the laws and usages of language, but on truths which determine these laws and usages. Modern science affords ample evidence of the justness of this position.

—That this truly was the position of the realistic schoolmen, Mr. Peirce contends; and he bases his opinion and belief on an original examination of their works, such as has not, we venture to say, been undertaken, outside of Germany, for a very long time. In spite of the confirmation of this position which modern science gives, the course of the development of modern science has, nevertheless, as Mr. Peirce points out, been closely associated with the opposite doctrine—nominalism, the representative of the sceptical spirit. This appears in Berkeley's philosophy, who is a nominalist, notwithstanding his *penchant* for Platonic ideas or spiritual archetypes. Hume, a complete representative of the nominalistic and sceptical spirit, is an historical product of Berkeley's nominalism; and, though commonly regarded as the author of modern philosophical movements, was not, historically considered, so different from Berkeley but that Mr. Peirce regards the latter as entitled to "a far more important place in the history of philosophy than has usually been assigned to him." So far as Berkeley was a link in the chain, this is undoubtedly true. So far as Hume (in common with all independent thinkers of the sceptical type) was not such a link, he was, we think, a starting-point in the movement of thought which has resulted in English empiricism, or the so-called "Positivism" of modern science, which Mr. Peirce seems inclined to attribute to a regular development of philosophical thought. Scepticism, though perhaps never original, as we are taught by orthodoxy, and only a revival of old and the oft-exploded errors, is, nevertheless, by its criticism, the source of most of the impulses which the spirit of inquiry has received in the history of philosophy. The results of modern science, the establishment of a great body of undisputed truths, the questions settled beyond debate, may be testimony in favor of the realistic schoolmen; but this settlement was the work, so far as it depended on

the impulse of philosophy, of the nominalistic or sceptical tendencies of modern thought, which has put itself in opposition, not to the faith of the realists, as Mr. Peirce understands them, but to their conservatism and dogmatism, to their desire to agree with authority—that admirable devotion of theirs. It is curious that these things, the most certain of all on which the actual arts of life are now dependent, should be the results equally of the faith of the realists and the sceptical inquiries of the nominalists. But this is enough to account for the gratitude and the indifference which we owe to both of them, especially as the confirmation which science has afforded is not of the sort which the realists anticipated. It is the empirical conjectures of the visionary, not the inspired teachings of the wise, that have established realities for themselves and for truth in general. There are many other curious points of history and criticism in this article which will engage the scrutiny of the student of metaphysics, and doubtless afford him great delight. We are afraid to recommend it to other readers, as Mr. Peirce's style reflects the difficulties of the subject, and is better adapted for persons who have mastered these than for such as would rather avoid them.

13 (14 December 1871) 386

MR. PEIRCE AND THE REALISTS

TO THE EDITOR OF THE NATION:

SIR: In your far too flattering notice of my remarks upon mediæval realism and nominalism, you have attributed to me a degree of originality which is not my due. The common view that realism is a modified Platonism has already been condemned by the most thorough students, such as Prantl and Morin. The realists certainly held (as I have said) that universals really exist in external things. The only feature of the controversy which has appeared to me to need more emphasis than has hitherto been put upon it is that each party had its own peculiar ideas of what it is that is real, the realists assuming that reality belongs to what is present to us in true knowledge of any sort, the nominalists assuming that the absolutely external causes of perception are the only realities. This point of disagreement was never argued out, for the reason that the mental horizon of each party was too limited for it to comprehend what the conception of the other side was. It is a similar narrowness of thought which makes it so hard for many persons to understand one side or the other, at this day. C. S. PEIRCE.

WASHINGTON, D. C., Dec. 10, 1871.

1872

14 (4 April 1872) 222

EDUCATIONAL TEXT-BOOKS. I.

This reference to Fowler's book on logic appeared among notices of several textbooks. We include it because it is mentioned later in *The Nation*, 14 (11 April 1872) 244-246, a set of notices that is attributed to Peirce. Haskell, in *Index to* The Nation, identifies the author of this review of "Educational Textbooks" as being William Francis Allen.

Thomas Fowler (1832-1904) was an English educator and logician. He took his B.A. from Merton College, Oxford, in 1854, and acquired several honorary degrees throughout his career. He held the post of professor of logic at Oxford from 1873 until 1889, and during this period advocated the teaching of natural science and abolition of tests at that university. He became president of Corpus Christi College, Oxford, in 1881, and later vice-chancellor of that college in 1901. He authored several books on logic and ethics, also editing Bacon's *Novum Organum* (1878) and Locke's *Conduct of the Understanding* (1881).

. . .The best logic for instruction in colleges is, in our judgement, Fowler's ("Elements of Deductive Logic"—New York: Macmillan). A young man who has been through it under a teacher of power will have had his mind enlightened and strengthened, and will be better prepared for life. In short, it to some extent fulfills the function of an elementary logic, a thing which most text-books do not begin to do. Mr. Fowler closely follows Mill's work, of which this must be allowed, that it represents the best scientific thought of the age more nearly than any other systematical exposition of the subject. It contains, however, in our opinion various important errors not only upon its philosophical side, but also in its relation to practice, against which the student ought to be put upon his guard. To these we have not space here to refer; but as they are of interest we shall take an early opportunity to recur to them. . . .

14 (11 April 1872) 244-246

EDUCATIONAL TEXT-BOOKS. II.

These comments on the work of Proctor, Maxwell, Wilson, and Fowler are attributed to Peirce by Fisch in *First Supplement*. This review of "Educational Textbooks" continues with some additional remarks on "the metaphysical part of logic." These additional comments easily could have been authored by Peirce, but we have seen no evidence that might confirm that hypothesis. Garrison wrote to Peirce on 10 January 1872 (see MS L 159.1) commissioning the review of two unidentified books. This review could be the result (at least, in part) of that letter. Moreover, a letter dated 9 May 1872 from Peirce to E. L. Godkin (see MS L 248) is conclusive for the Wilson item. Also, Peirce owned a copy of Maxwell's tenth edition (see MS 1598), which he could have acquired (as he acquired many of his books) from Garrison as a review copy. Haskell, in vol. 1 of his *Index to* The Nation, assigns no author for this piece.

Richard Anthony Proctor (1837-1888) was an English astronomer and mathematician. In 1873 he proposed that lunar craters arose through meteoric bombardment, the theory that is held today. In 1881 he moved from England to America, where he remained for the last years of his life.

James Clerk Maxwell (1831-1879) was a Scottish mathematician and physicist. He entered Cambridge in 1850, and was graduated second in his class in mathematics, as Kelvin had

done before him and J. J. Thomson was to do after him. In 1857, Maxwell proposed his revolutionary theory of the planetoid nature of the rings of Saturn, and in 1860 arrived at the Maxwell-Boltzmann theory of gases, a kinetic-particle theory. In 1871 he was appointed professor of experimental physics at Cambridge, the first person ever to hold a professorship in that subject. Maxwell's electromagnetic equations are perhaps his greatest gift to science.

We do not know when a respectable publication has been prefaced with more boastful words than Mr. Proctor's "Star-Atlas" (London: Longmans). In a previous publication, Mr. Proctor had announced that all such works hitherto had been constructed on radically wrong principles, and had put forth a demonstration that there was only one proper way of making a star-atlas. This he repeats in the "Letterpress Introduction" to the present book, only it is a different manner of construction which he demonstrates to be the right one. A regular dodecagon is inscribed in the sphere, and then each face is produced so as to cut off a part of the sphere, and that part is represented on one map. There are, therefore, twelve equal circular maps which overlap each other slightly, except in five points on the circumference of each. The North Pole is made the centre of one of the maps. But after all this theorizing about the method of projection, Mr. Proctor fills in with stars in a very simple manner. He has apparently merely entered them from the British Association Catalogue. The result, at any rate, is that the magnitudes are so extremely inaccurate that there are many parts of the heavens which are perfectly unrecognizable; and on every map the errors are a source of great inconvenience. Let any one who possesses this atlas compare, for example, the Little Bear in the map with the heavens, and he will find that a bare majority of the stars are rightly inserted or omitted. When the author says, "I believe no atlas was ever constructed in which more pains were taken than in the present to avoid errors," he clearly forgets that stars exist in the sky as well as in the B.A. Catalogue, and that some makers of atlases have taken the trouble to examine them. Argelander's "Uranometria" is justly regarded as one of the most perfect works of observation, perhaps in fulfilling its purpose *the* most perfect ever executed. Its atlas is renowned in all lands for its resemblance to the heavens and for its convenience in use. Its accuracy is such that its scale of magnitudes has been everywhere adopted as the standard. But Mr. Proctor has apparently never heard of it. England is eminent in astronomical observation—the Greenwich Observatory alone would suffice to make it so. But Englishmen are generally so naively ignorant of what takes place in the great world of science (which does not centre in London, as they seem to imagine) that it is possible for a respectable man to publish a book there the existence of which depends on such ignorance as would disgrace him in Sicily or in Spain. As for the method of dividing the sphere upon which Mr. Proctor prides himself so much, it is exceedingly inconvenient in practice. It cuts Gemini, Orion, the Great Bear, Hercules, all in two. In short, if anybody interested in the stars has not Argelander's incomparable work, then let him take Elihu Burritt's or any other, but not this new one. We speak from experience.

Heat is still the most interesting part of physics, for the time; and we have devoured Mr. Clerk Maxwell's "Theory of Heat" (London: Longmans). It is not intended, however, primarily to amuse, as Tyndall's was; and it also differs from

that work in giving a correct idea of the mechanical theory of heat. It is intended for a class-book, and is the very best text-book of physics which has been published for some years. Its study will demand some thought from the student, which will be a fatal objection to its extensive use in this country. It is not made with reference to satisfying examining committees, and to getting boys over the ground with the least possible trouble to them. It discusses a good many subjects not strictly a part of the theory of heat, and we could have wished that some things which do belong here had been enlarged upon more, and that more special facts and tables had been given. Yet it must be allowed that within these 300 pages a more beautiful and perfect account of the theory could not have been given.

The old sensationalists, Hartley, Brown, and the Mills, never wrung many admissions from the advocates of *a-priority*. But Dr. Wilson's "Lectures on the Psychology of Thought and Action, Comparative and Human" (Ithaca: Audrus, McChain & Lyons) is evidence that the new physiological materialists are making more impression. The author gives up the whole of sensation as involving no mind or consciousness, and hopes by that admission to strengthen spiritualism in reference to the other parts of the intellect. But though the new position has strength, yet the retreat will encourage the auti-supernaturalists and will make for them new converts. Respectable writers cannot long defend a theory which involves such suppositions as that animals and men acquire a knowledge of external things by an immediate action of the spinal cord without the agency of any external organs, as Dr. Wilson does on pp. 249 and 250.

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

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

We promised last week to discuss some of the errors, as they seem to us to be, of Mill's theory of logic which Mr. Fowler adopts. But we have only space here to refer to Mill's doctrine of scientific hypotheses. This was doubtless suggested by a doctrine of Auguste Comte, who divides the sciences into five classes having different degrees of certainty; and by a hypothesis means a proposition which is not proved with the degree of certainty which belongs to the order of science to which it relates. His maxim of hypothesis is, that such a proposition may be allowed a provisional and secondary place in science, provided it is capable of being proved (or disproved) with the degree of evidence appropriate to its order of science. But Comte's conception of a hypothesis is a peculiar one. A scientific hypothesis is usually defined (and is defined by Mr. Mill) as the supposition of a circumstance which, by the action of known laws (or a generalization of known laws), would result in facts such as have been observed. It is also common to use the term scientific hypothesis to denote a very doubtful conclusion of science. These two meanings are apt to be confounded, and Mill has plain-

ly confounded them when he says that the one condition of the admissibility of a hypothesis is "that it be not destined always to remain a hypothesis, but be of such a nature as to be either proved or disproved by comparison with observed facts." Here, being proved has not the definite meaning that it has in Comte's maxim. There is no absolute distinction to be drawn anywhere between the probability of that which has a bare possibility of truth and that which has a bare possibility of falsehood. A supposition which by the known action of the laws of nature will explain a single known fact, thereby gains some slight probability. This is susceptible of exact demonstration. As the number of facts which the hypothesis explains increases, and as their variety (depending on the laws their explanation involves, and the elements of the hypothesis upon which they depend) increases, the probability of the hypothesis increases indefinitely, until it becomes as certain as any fact we know. But, as a general rule, that which was a hypothesis at first, remains a hypothesis to the last. All that we receive upon testimony is hypothesis; it explains the fact that the witnesses agree. The existence of the relation of space among things, and all that we remember, are hypotheses in the same sense in which it is a hypothesis to say that Marshal Bazaine surrendered Metz treacherously. Between these extremes, hypotheses of every degree of probability may exist, and no absolute line is to be drawn among them. A hypothesis, therefore, does not differ from any other inferential proposition; and the only thing to be considered in reference to its admissibility is the actual evidence upon the matter. Mr. Mill's view is that a hypothesis is not something inferred, but something taken as the basis of enquiry; so that the question is not what the existing evidence is, but what evidence is forthcoming. Here two questions must be distinguished: the first, in reference to what a man may logically do; the second, as to how he may best economize his scientific energies. Now a man may investigate the truth of any proposition whatever, and if he makes no false inference there is nothing illogical in his procedure. But he will be very unwise to spend a large portion of his life in putting anything to the test which can hardly be true or which can hardly be false. When the questions put to nature will only be answered by yes or no, he will advance with the greatest rapidity (as in the game of twenty questions) by asking questions an affirmative answer to which is equally probable with a negative one. He must, however, consider what degree of certainty the answer will have, and the rule will be, among questions of equal importance, to make that investigation which will have the greatest effect in altering existing probabilities. Mr. Mill seems to suppose an absolute distinction between the adoption and the rejection of a hypothesis; but every scientific man has passed that rude state of mind, and takes into account, in every case, as well as he can, the degree of evidence. Making distinctions absolute which are really only relative is the source of most of the errors in Mill's system of philosophy.

There are various other modern schools of logic besides those to which we have referred. In the first place, Boole, De Morgan, and others have made a more exact investigation into purely formal logic, and have greatly advanced the subject. Their researches are still in a very immature state, but they have already succeeded in throwing much light upon the subject. The metaphysical part of logic

has been chiefly prosecuted in Germany. Such questions as these: What is the connection between the following of a conclusion from its premises and the following of an effect from its cause? and what is the connection between the relation of a subject to its predicate and the relation of a substance to its attributes? have a high philosophical importance. Hegel considers the real relations of existing things and the formal relations of thought to be strictly identical; but he is led to modify profoundly the usual views regarding the maxims of reasoning in making out his point. His philosophy is now exploded; that is to say, hardly any of the rising men adopt it. But its historical importance has been considerable. For a short time it had immense influence in Germany. Mr. Carroll Everett's "Science of Thought" (Boston: William V. Spencer) is regarded by Hegelians as a good exposition of the fundamental positions of their philosophy. Vague conceptions and complicated reasoning are continually causing Mr. Everett to fall into fallacies; and this is the universal fault of Hegelians. The consequence is that their conclusions are entirely uncertain; and the interesting and profound suggestions with which their philosophy abounds only serve to make the bad influences of their loose reasoning upon half-educated minds all the greater. Ueberweg's treatise ("System of Logic and History of Logical Doctrines," London: Longmans) is an excellent specimen of a modern German logic. The view defended is that the construction of the mind corresponds with the order of nature, so that metaphysical conceptions have a double character, first, as true of things as they really exist; and, second, as merely formal principles of thought. It is a carefully written and scholarly book. The style is clear and precise, more precise than American readers enjoy, but real students do not wish a writer to beat about the bush to avoid an expression merely because it is a little too formal for the taste of literary people. The translator, we regret to say, betrays an ignorance of two things rather essential to his task, logic and the German language. On page 402, we read this extraordinary sentence: "An infinite straight line can proceed but from a figure bounded on all sides in the same plane on two sides only by means of intersecting the boundaries." This will bear a second reading. What Ueberweg says is: "Eine unbegrenzte gerade Linie kann aus einer allseitig begrenzten Figur in derselben Ebene auf beiden Seiten nur mittelst Durchschneidung der Grenzen heraustreten." This is perfectly clear. A straight line lying within an enclosed figure in the same plane cannot be extended indefinitely in either direction without cutting the boundary of that figure. The translator says, "Dr. Ueberweg has himself revised the sheets; and, as he knows English well, this translation may be held to give his opinions as he wishes them expressed in our language." There must be a misrepresentation here.

1873

17 (10 July 1873) 28-29

LAZELLE'S "ONE LAW IN NATURE"

One Law in Nature: A New Corpuscular Theory, comprehending Unity of Force, Identity of Matter, and its Multiple Atom Constitution, applied to the Physical Affections or Modes of Energy.

By Capt. H. M. Lazelle, U.S. Army. New York: D. Van Nostrand.

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

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

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

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

The second citation shall be from page 19:

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

These two opinions, that gravitation acts without a medium, and yet that it takes time to act, do not harmonize. But observe the reasoning: Gravitation cannot act instantaneously because successive positions must be occupied in successive times! But what if these positions are not successive? Cannot there be attraction at different points at once? Physicists are perfectly ready to examine general theories of the forces of nature, notwithstanding the fact that there is not a single instance of such a theory (imagined, and not derived by induction) which has finally taken a place among established truths. For example, the undulatory theory of light is proved up to a certain point, namely, that light consists of some sort of vibration transverse to its direction of propagation. This is a

result of induction. But no attempts to go further and imagine of what sort this vibration is, though the greatest mathematicians have made them, have met with such success as to be admitted to a place among established truths. Yet physicists always look upon such attempts to represent the mechanism of natural forces with favor; but they demand that they shall be developed with mathematical precision, and be shown to express known laws with mathematical accuracy. This Captain Lazelle has not done.

All physicists believe that everything in the outward world may be expressed in terms of mass, of space, and of time. The redness of a rose as it exists in the mind which sees it, is what it appears to be; but as it exists in the rose itself, it is only the fact that the particles vibrate in a certain time. This time may be expressed as a number. And in a similar way, no doubt, every property of any body might, if we only knew how to do it, be expressed numerically in terms of the pound, the yard, and the second. Of these physical constants (or numbers expressing properties) almost all are either peculiar to some particular thing (such as the dimensions of the earth) or to some kind of substance (such as the atomic weight of hydrogen). In the whole range of physics, we can expect to find no others and know of no others, except only two: first, the amount that one gramme attracts another gramme placed at a distance of a metre, which is 0.00000000000006 metre cubes per gram-(second)^2, and the velocity of light, which is 300000000 metres per second.

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

These are the two forces whose constants are the absolute modules of gravitation and the velocity of light. But our whole conception of the universe, and therefore our whole experience, are opposed to there being another general relation, for such a one could only exist by establishing absolute values of our units. Now, it is not to be believed that general considerations in regard to the nature of things could ever lead us to assign a particular numerical value to the measure of any particular thing, such as our standard measure. We have, therefore, reason to believe that while we doubtless are ignorant of the precise form of the fundamental principles of nature, we at least are not mistaken as to their number.

1878

27 (1 August 1878) 74

Popular Astronomy.

By Simon Newcomb, LL D., Professor U.S. Naval Observatory. (New York: Harper & Bros.)

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS 1513 (draft).

Simon Newcomb (1835-1909) received his B.S. from Harvard in 1858, and assumed the position of professor of mathematics with the U.S. Navy. His first station was the Naval Observatory in Washington, D.C. He became the senior professor of mathematics in the Navy in 1877, and was appointed superintendent of the "American Ephemeris and Nautical Almanac." From 1884 until 1893 Newcomb was professor of mathematics at The Johns Hopkins University. He was not only a mathematician, but also an astronomer of international reputation, having been associated with several American observatories. While at Johns Hopkins, Newcomb was the editor of the *American Journal of Mathematics*. He was author of numerous books on astronomy and mathematics, member of the National Academy of Science (vice-president, 1883-1889), president of the American Academy for the Advancement of Science, 1877-1878, and president of the American Society for Psychical Research.

—The public naturally like to hear what a man who has recently distinguished himself has to tell them about his specialty; and astronomers will be glad to have a collection of Professor Newcomb's highly competent opinions in regard to various questions of astronomy. This book will not, however, fascinate the general reader. The style in which it is written suggests that it may have been first composed for a school text-book, and afterwards worked over for popular reading. In Part I. an attempt is made to teach the first elements of astronomy in their historical development; a very good idea, well worthy of a fuller working out. Part II. is entitled "Practical Astronomy," not certainly because it teaches anything practically, but because it supplies information concerning telescopes and the work which is done with them. Part III. describes the solar system, and Part IV. the stellar universe.

1879

28 (3 April 1879) 234-235

READ'S THEORY OF LOGIC

The Theory of Logic: an Essay.

By Carveth Read. London, 1878.

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

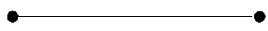
Carveth Read (1848-1931) was an English philosopher and psychologist. He held the Grote professorship of Philosophy in the University of London from 1903 until 1911, and was lecturer on comparative psychology at University College, London, from 1911 until 1921. Read was emeritus professor of philosophy and comparative psychology at the University of London from 1921 until his death.

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

There is certainly this to be said in favor of the last opinion, namely, that the question of the validity of any kind of reasoning is the question how frequently a conclusion of a certain sort will be true when premises of a certain sort are true; and this is a question of fact, of how things are, not of how we think. But, granted that the principles of logic are facts, how do they differ from other facts? For facts, in this view, should separate themselves into two classes, those of which logic itself takes cognizance and those which, if needed, have to be set up in the premises. It is just as if we were to insist that the principles of law were facts; in that case we should have to distinguish between the facts which the court would lay down and those which must be brought out in the testimony. What, then, are the facts which logic permits us to dispense with stating in our premises? Clearly those which may always be taken for granted; namely, those which we cannot consistently doubt, if reasoning is to go on at all: for example, all that is implied in the existence of doubt and of belief, and of the passage from one to the other, of truth and of falsehood, of reality, etc. Mr. Read, however, recognizes no such distinction between logical principles and other facts. For him logic simply embraces the most general laws of nature. For instance, he recognizes as a logical principle the law of the conservation of energy, which is even yet hardly set beyond all doubt. If he excludes the laws of geometry, as being "quantitative," it is by an ill-founded distinction. If he does not mention the law of gravitation nor the existence of a luminiferous ether as logical principles, it must probably be because he thinks them less general truths than the laws of motion.

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

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



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

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

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

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

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

Hardly a model of synthetic orderliness.

But what does the author do with the great body of relations? What pigeonholes has he for them in his scheme of arrangement? Take, for instance, the relation of striker to struck. A man's striking another constitutes certainly no resem-

blance between them. But neither is it an unlikeness, for a man may strike himself, and since he is then a striker only so far as he is struck, and *vice versa*, it is impossible to say that striker and struck are unlike. In short, the relation is neither a likeness nor an unlikeness, for the reason that both these latter are relations between objects similarly related to one another, while the relation of striker to struck, like most relations, is between dissimilarly related objects.

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

29 (16 October 1879) 260

ROOD'S CHROMATICS

Modern Chromatics. With Applications to Art and Industry.

By Ogden N. Rood, Professor of Physics in Columbia College. With 130 original illustrations. New York: D. Appleton & Co. 1879.

CSP, identification: Haskell, *Index to* The Nation (the last two paragraphs are by Russell Sturgis, a contributor specializing in topics on art). See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*.

Ogden Nicholas Rood (1831-1902) entered Yale in 1848, but transferred to Princeton where he was graduated in 1852. He held the position of professor of physics and chemistry at Troy University from 1858 until 1863, and was professor of physics at Columbia University until his death. His *Modern Chromatics* gained immediate acceptance as the most authoritative text on that subject, and was translated into French, German, and Italian. Rood, known as the "Father of American Experimental Physics," was an extensive contributor to the *American Journal of Science*, and was highly regarded among the scientific community. He was a member of the National Academy of Science, the American Association for the Advancement of Science, and the Century Club of New York.

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

are conceptions of the understanding. So that the question presses, What is there, then, in color which is not relative, what difference which is indescribable, and in what way does the pure sense-element enter into its composition?

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

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

ness. The most striking example of this effect is afforded by a mixture of red and green, which gives a strong yellow effect, although the sum of the two sensations is nearly white.

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

As to the question whether scientific investigation is an aid to artistic production or to artistic judgment, the author seems to assume that it may be. In the preface it is asserted that while knowledge of the laws of color "will not enable people to become artists" it may yet help in artistic work, and still more in the appreciation and criticism of artistic work. Now, whether this is so or not there is no chance to discuss in these columns, but a chapter of Professor Rood's book might well have been devoted to the examination of that question, and we regret to find instead of such examination the whole argument of the last two or three chapters resting upon the assumption of what, we think, ought to have been proved. Should the decorative artist regard or disregard Chevreul's 'Laws of Contrast,' Hay's 'Laws of Harmonious Coloring,' and other such tables and treatises? Our author, we think, would say aye to that question, but nearly all artists who are concerned with color would say no; and the more they know of these theories the less, we think, do designers in color respect them. "Red lead with blue-green gives a strong but disagreeable combination; . . . vermilion with blue gives an excellent combination; . . . vermilion with green gives an inferior combination; . . . sea-green with blue gives bad combinations." There are four pages of such statements, arranged in a tabular form and credited to Chevreul (in whose book there are a plenty more) and to Brücke, and tending to no result, for the qualifying terms "good, . . . bad, . . . strong, . . . excellent, . . . weak" at once overset any claim to scientific accuracy, and no color-designer would try more than once to make practical use of such statements. Our author seems, indeed, to be aware that it is not a scientific method he is following here, for he avows his disagreement with one statement of M. Chevreul, both statement and contradiction being given as mere matters of opinion.

The last chapter is devoted to the use of color in painting and decoration; and in this the evident knowledge and right feeling of the author are made useless by the false system adopted—the system of arguing from assumed principles to results, instead of comparing results together with the view of establishing principles. Many of the assertions as to the difference between "painting," as in pictures representing nature, and decoration; as to the difference between transparent color, as in stained glass, and opaque color seen by reflected light; as to the proper

aim and limits of decoration; and as to the proper order of artistic study, will wholly fail to command the adhesion or even the respectful consideration of students of art. And this seems to result wholly from the unfortunate assumption spoken of above—the assumption that the scientific method can be carried beyond the discovery of fact to the laying down of positive laws for practice. "The aims of painting and [of] decorative art are quite divergent" (p. 306). No, but convergent; for, starting from different points, as our author truly says, they reach one and the same result. The objects of the painter of pictures and that of the decorative painter are different; but with different aims they reach the same result, and in all the best work there is in the world there is no saying whether the "painter" or the decorator has been at work.

29 (25 December 1879) 440

NOTES

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

—The current number of the *American Journal of Mathematics*, which is published under the auspices of the Johns Hopkins University, contains an account of a fundamentally new phenomenon in electricity, not explicable by anything hitherto known. The definition of the new action is not yet certainly made out; but it appears to be that if we say that the direction of a galvanic current is from the negative to the positive pole, then a magnet tends to deflect the current within the conductor in the same direction in which it tends to turn the conductor itself. This fact will be a complete surprise to physicists, and its importance to the theory of electricity can hardly be overestimated. The discoverer is Mr. E. H. Hall, assistant in the Laboratory of Professor Rowland, to whose encouragement and assistance the discovery was in a large measure due. It may justly be said that no discovery equally fundamental has been made within the last fifty years. Discoveries so novel have usually been in some degree the result of accident; but in this case elaborate and very delicate experiments were undertaken to ascertain whether or not any such phenomenon could be observed. The new force is exceedingly feeble, so that we cannot predict any practical applications for it.

—The same number of the *Journal* contains several other important papers, including three by the celebrated algebraist Sylvester. All of these afford salient examples "of the importance of the part played by the *faculty of observation* in the discovery of pure mathematical laws." There has been, perhaps, no other great mathematician in whose works this is so continually illustrated as in those of Professor Sylvester. An example of a mathematical proposition known to be true many years before any one succeeded in producing a demonstration of it, is the familiar fact that on any possible map, however complicated, the different countries may be distinguished from those which adjoin them by painting them in only *four* different colors. This has been known for a long time, but the first

proof of it is given in the present number of the *Journal* by Mr. A. B. Kempe, well known for his investigations into linkage. The number also contains an explanation of the "curved ball" of the base-ball players, and a method for representing a space of four dimensions.

1881

32 (31 March 1881) 227

Studies in Deductive Logic.

By W. Stanley Jevons, LL.D. (London and New York: Macmillan & Co. 1880.)

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

William Stanley Jevons (1835-1882) was a leading English economist and logician. He was professor of logic, political economy, and philosophy at Owens College from 1866 until 1879. Jevons was the author of several books on logic and economy, and was also interested in political and social reform.

—Some forty years ago the two mathematicians, De Morgan and Boole, commenced a reform of formal logic. Their researches were continued by a number of other excellent thinkers (Mr. Jevons among them) in different countries, and the work is now so far advanced that the new logic is beginning to take its place in the curriculum of the universities, while many persons have imagined that some almost magical power of drawing conclusions from premises was to be looked for, and that logic would prove as fertile in new discoveries as mathematics. Concerning such hopes Professor Sylvester says: "It seems to me absurd to suppose that there exists in the science of pure logic anything which bears a resemblance to the infinitely developable and interminable heuristic processes of mathematical science." "To such a remark," replies the author of the book under notice, in his preface, "this volume is perhaps the best possible answer." A more exaggerated pretension never was made. The book is a convenient manual of exercises in elementary logic, tinctured with the author's peculiar views, of which there will be different opinions, but, at any rate, sufficiently sound to be useful in the class-room. But if Professor Jevons were to penetrate only a little ways into the heuretic world of the mathematicians—an excursion quite worth the while of a logician—were to learn what discoveries are there made every month, and what sort of a stamp a proposition must bear to be considered, in that field, as really new, it is to be hoped that he would feel something different from self-satisfaction at recollecting that he had set up anything in this little volume as worthy to be compared with the triumphs of a Sylvester. Logic, inductive and deductive, is an important discipline, probably more important than the higher mathematics, just as the multiplication-table is more important than the calculus; but very, very few are the new problems which have ever been solved by the regular application of any system of logic. That part of logic which can best compete with mathematics in the discovery of new truths is the complicated theory of relative terms. But even there the comparison would be very unequal between what is only a branch of mathematics and the whole body of mathematics together. The solution of problems used to be considered as the glory and touchstone of the mathematician; in our time, the aim is rather at the discovery of methods, and we might perhaps look to the logician to produce a *method* of discovering methods. But the main advantages which we have to expect from logical

studies are rather, first, clear disentanglements of reasoning which is felt to be cogent without our precisely knowing wherein the elenchus lies—such, for instance, as the reasoning of elementary geometry; and, second, broad and philosophical *aperçus* covering several sciences, by which we are made to see how the methods used in one science may be made to apply to another. Such are really the chief advantages of the new systems of formal logic, much more than any facilities they afford for drawing difficult conclusions; and it is evident that if logic is to make any useful progress in the future, we must set out with some more or less accurate notion of what sort of advantages we are to seek for.

1884

39 (18 December 1884) 521

THE RECIPROCITY TREATY WITH SPAIN

TO THE EDITOR OF THE NATION:

SIR: The one-sided character of the proposed "reciprocity" treaty with Spain may be judged from the following estimate. I use round numbers:

Sugar consumed in the United States	1,000,000 tons
Sugar produced in Cuba and Porto Rico	700,000 "
The present duty on the latter amount	$30,000,000
Value of total imports into Cuba	$50,000,000

Since the products of the islands would not suffice for our consumption, the growers there could compel us to pay about the same as other markets offered us —that is, as much as we now pay to both the grower and the United States Custom-house; all the present duty—say, $30,000,000—would be their additional profit, while even if we should sell to Cuba all that she now buys (a manifest impossibility), and make the extraordinary commercial profit of 10 per cent., we should receive but $5,000,000. In other words, we are asked to pay the Cubans $30,000,000 for the privilege of making not over $5,000,000 out of them.

Really, Mr. Editor, is Mr. Foster a Yankee? Did he ever learn to *kalkerlate*? —Yours, etc., T. E. C.

BALTIMORE, December 11, 1884.

TO THE EDITOR OF THE NATION:

SIR: You seem to hold that the ratification of the Spanish treaty would not for a number of years affect the price of sugar "to the consumer," in this country; and that during the gradual decline of importations from non-Spanish ports, the price would be fully maintained. I find this position so difficult to understand, that I beg for some further elucidation of it.

1. Would not the Spanish ports immediately begin sending us more sugar, full 20 per cent. more the first year? Would they not import sugar to send us?

2. If the Spanish ports should send us more, would not one of two things necessarily happen, namely, either that the price would fall, or that the non-Spanish ports would send less?

3. But if the importation from non-Spanish ports were to be diminished by the effect of the treaty (as you seem to admit it would be), would not the sugar withdrawn be the product of those lands which among all those now raising sugar for this country are the worst fitted for this purpose? Would not the result be that the worst of the land then producing sugar for us would be better than the worst of the land now doing so? And would not this state of things, by the operation of competition, work a fall in the price? C. S. PEIRCE.

WASHINGTON, December 15.

[It seems to us a very simple and easily understood proposition that all sellers of sugar in the New York market will ask and obtain the same price for the same grade of sugar, treaty or no treaty. The planter in Manila will receive the same rate per pound as the planter in Cuba. The Manila planter, however, must pay two cents per pound duty before he can reach the market at all, while the Cuban planter need not pay. Now, if Cuba and Porto Rico could at once supply us with all the sugar we consume and something more, then the law of competition among Cuban and Porto Rican planters would force down the price, and the American consumers would get the benefit. But so long as those islands produce something less than the whole amount, a portion of our supply must come from other parts of the world and enter the market loaded with the duty. As there cannot be two prices for the same article at the same place, the market price of sugar in New York under these conditions will be the cost of production in Manila, plus transportation, etc., plus duty. This price the Cuban planter will obtain equally with the planters of Manila, Jamaica, Brazil, and every other country, and of course the American consumer will pay it because the importer must be reimbursed for all his expenses. The situation of the Cuban planter under the operation of the treaty will be precisely the same as that of the Louisiana planter under the tariff. If Louisiana could supply the entire American demand and something more, the law of competition would force down the price more or less, and the consumer would get the benefit.

It has been stated that Cuba and Porto Rico are capable of producing all the sugar consumed in this country. It is possible that if all the land in those islands adapted to sugar-growing were utilized for that purpose, the product might be equal to our present demand. But our demand is not a fixed amount. It grows from year to year. The demand for hardly anything grows more rapidly. It is by no means certain that the annual producing capacity of Cuba and Porto Rico, whose areas are limited, would ever overtake our annual consumption, and if it should not, there would still be an importation of duty-paying sugar, which would, by virtue of the economic law already stated, be the sign and evidence that American consumers were deriving no benefit from the treaty. Since the treaty provides for the introduction free of duty only of sugar *grown* in Cuba and Porto Rico, it would be impossible for them to import sugar to send to us. It was charged at one time that Manila sugar had been imported into Honolulu to be reëxported to San Francisco under the treaty with the Hawaiian Islands, but the charge was not sustained upon investigation. Cuba would undoubtedly import sugar for her own consumption, and send us the corresponding amount of her own growth. This would add to her exporting capacity by whatever amount her present population now use, which is not probably equal to one year's increase of our consumption.

The third question propounded by Mr. Peirce would be relevant if we were the only country buying sugar from non-Spanish ports. The sugar which we now take from them would be diverted to England and other importing countries to whatever extent Cuba increased her supplies to us (our consumption remaining the same), or to whatever extent she increased her proportionate supply. Therefore the difference between best lands and worst lands would not necessarily enter into the problem at all.—ED. NATION.]

1885

40 (1 January 1885) 12

THE SPANISH TREATY ONCE MORE

TO THE EDITOR OF THE NATION:

SIR: I have to express my thanks for your clear explanation of your view that the ratification of the reciprocity treaty with Spain would not affect the price of sugar in this country so long as we continued to import any sugar at all from non-Spanish ports. Cuba, you say, would send us more, but the non-Spanish ports would send just as much less, that trade being diverted to England, etc., to replace the falling off in Cuban sugar there.

But I now object that a great volume of trade will not spontaneously divert itself from one market to another, without any motive. Such an event can only be due either to a fall of price in the first market or to a rise in the second. The sugar which is now sent here is sent because, in the existing state of prices, the owner has found it more advantageous to send here than elsewhere; and here it will continue to come, unless prices change sufficiently to overcome the excess of advantage. If, therefore, the price of sugar were not to fall here on the ratification of the treaty, in England it would have to go up. But an advance in price implies diminished sales—diminished production—somebody forced out of the sugar-growing business. Yet nobody could be forced out of that business if the price had nowhere fallen. How can you escape this dilemma?

You say that the price here would be kept up by the duties that would have to be paid on some of the imported sugar (*i.e.*, by the cost of getting it to market), and that when this sugar, thus sent at a disadvantage, ceased to come, then and only then would the price fall. The principle of this seems to me quite sound—only too sound for your conclusion. For the non-Spanish sugar which we now import comes from various countries very differently situated. Upon some of it there is a considerable profit, while some barely pays the cost of production; upon a part of it there is considerably more profit than if it were sent to England, while for a part it is almost a matter of indifference to which market it is sent. If now the treaty should cause less of this non-Spanish sugar to be sent to this country, that which would be diverted would clearly be that which there is now scarce any inducement to send here. It would follow, I think, according to your own principle, that the price here, being no longer kept up by that very unadvantageously sent sugar, must fall when that should cease to come. C. S. PEIRCE.

WASHINGTON, December 22, 1884.

[We "escape this dilemma" by the use of infinitesimals. One-thirty-second of a cent per pound or even less would be a sufficient reduction in price to secure the American market to the Cuban planter for all the sugar he could produce. It would give him all the advantage he needs. One-thirty-second of a cent per pound would, therefore, be the maximum gain to the American consumer from the treaty, until (if ever) the Cuban supply could overtake and exceed the American demand. Mr. Peirce's second paragraph, he will permit us to say, carries us into the region of the differential calculus beyond our depth.—ED. NATION.]

41 (3 September 1885) 203

The Common Sense of the Exact Sciences.

By the late William Kingdon Clifford. New York: Appletons. [International Scientific Series]

Attributed to Peirce by Fisch in *First Supplement* (internal evidence: the reference to F. E. Abbot's concept of space). Also, Peirce was personally acquainted with W. K. Clifford. This piece is unassigned in Haskell's *Index to* The Nation, vol. 1.

William Kingdon Clifford (1845-1879) was an English mathematician and philosopher. He was appointed professor of applied mathematics at University College, London, in 1870, and while there, was elected to the membership of the Metaphysical Society and the London Mathematical Society. During his brief lifetime, he published but one book and various papers based on his college lectures. His work has since been reconstructed and edited, perhaps the most popular item being this edition by Karl Pearson.

IT was in 1875, when Clifford was in fairly good health, that he dictated the whole of three chapters and part of another for a projected book to be entitled 'The First Principles of the Mathematical Sciences Explained to the Non-Mathematical.' Three years later, shortly before his death, he expressed the wish that the book should be published only after very careful revision, and that the title should be changed. It has certainly not received the sort of revision that Clifford desired; for as published it abounds in errors, and contains several quite anti-Cliffordian views. For instance, he says that if a point on the surface of a sphere is brought into contact with a point on the flat face of a cube, "we cannot move the sphere ever so little without separating these points." This is erroneous, because we can spin the surface about the point of contact; but although the passage has passed under the hands of two successive mathematical editors, neither has seen, what the course of reasoning shows, that Clifford in dictating said "move" when he meant *roll*. He wanted to show that all surfaces would fit together at any points where they are not broken by edges or corners, much as a ball may fit into a cup, only that the fitting is confined to a single point. Now surfaces that fit together may or may not be capable of being slipped or spun one on the other, but they cannot be rolled one on the other. A rolling motion, therefore, was the only one which had to be considered. Again, he defines a surface as the boundary between two portions of space which it separates absolutely. Now, without speaking of spirals, which obviously do not separate space into two parts, the most familiar of all surfaces, the plane, does not do so (according to the conception of the modern geometrician). Two planes will separate space, and one of these may be the plane at infinity; but a single plane does not. For if a point (say the focal point of a lens) be carried off with sufficient acceleration from one side of a plane, it will come back on the other side. Every surface may, it is true, form a part of the boundary between two regions of space. But even so modified, the definition is hardly satisfactory; for the calculus requires us to suppose that a solid body may approach indefinitely near to being a surface, which it certainly could not do were the two objects essentially disparate in their nature. Clifford here says:

"The surface of a thing is something that we constantly observe. We see it and feel it, and it is a mere common-sense observation to say that this surface is com-

mon to the thing itself and to the space surrounding it." "The important thing to notice is that we are not here talking of ideas or imaginary conceptions, but only making common-sense observations about matters of every-day experience."

But, as the editor, "K. P.," remarks, "we are compelled to consider the surface of the geometer as an idea or imaginary conception, drawn from the apparent (not real) boundaries of physical objects." The truth is, that the geometrical conception of space itself is a fiction. The geometer thinks of space as an individual thing or (as Mr. F. E. Abbot expresses it) a receptacle of things having an existence as something individual. If this were so, absolute position in space (independent of other bodies) and absolute velocity would have a meaning; but, in fact, they appear to have none. What is true is, that rigid bodies in their displacements are subject to certain laws which are the principles of geometry; and we have an instinctive acquaintance with these positional laws, which makes it easy for us to imagine the fictitious receptacle in which these laws are embodied. Thus, space only exists under the form of general laws of position; there is really nothing individual about it. And easy as is the geometer's conception, it is by no means born in us. The natural man knows of space only as a synonym for "air." Kant is responsible for the perpetuation of the erroneous conception of space which Leibnitz had escaped. It is impossible to have clear ideas concerning the non-Euclidean geometry, space of n dimensions, and such matters, without a proper understanding of this.

The main fault of the whole plan of the book is, that while it gives no adequate explanation of many mathematical conceptions interesting to a large body of non-mathematical minds—such as the square root of the negative, multiple algebra, space of n dimensions, the mathematical conception of the Absolute, non-Euclidean space, invariants, Riemann's surfaces, etc., conceptions perfectly susceptible of clear and interesting explanation, without too severely taxing the powers of the non-mathematical—it does suppose a reader whose interest in the logical *enchaînement* of mathematics is exceptionally great. Nine persons out of ten will read the chapter on number and exclaim, "This is nothing but what we learned at school," thus missing the whole argument, which will fly over their heads unperceived. The book has something of Clifford's style and traces of his power, but only faint ones. It will be of some service, but not very much. The parts added by "K. P.," one chapter and a half, bear comparison with those written by Clifford; it is a pity that the revision of the latter has not been more minute and accurate.

41 (19 November 1885) 431

The Religion of Philosophy; or, The Unification of Knowledge: A comparison of the chief philosophical and religious systems of the world, made with a view to reducing the categories of thought, or the most general terms of existence, to a single principle, thereby establishing a true conception of God.

By Raymond S. Perrin. G. P. Putnam's Sons. 1885.

CSP, identification: MS 1370. See also: Fisch, *First Supplement*. This note is unassigned in Haskell's *Index to* The Nation, vol. 1.

Six pages would have been ample to set forth the doctrine here diluted to six hundred. Motion is the only existence; time and space merely its phases. Time is identical with force; space with matter. God is the universal principle of motion. In place of arguing these propositions, the author tags them incongruously to sketches of the history of philosophy—sketches nil as arguments, and as history rambling, feeble, and ill-proportioned. Some healthy sentiments about morality and religion are expressed in an easy and pleasing style, but the philosophical conceptions seem to be nebulous, and the method of presenting them unsuccessful.

1886

42 (11 February 1886) 135-136

DR. F. E. ABBOT'S PHILOSOPHY

Organic Scientific Philosophy: Scientific Theism.
By Francis Ellingwood Abbot, Ph.D. Boston: Little, Brown & Co. 1885.

Attributed to Peirce by Fisch in *First Supplement* (Abbot wrote in his diary that Peirce was the author). This review is unassigned in Haskell's *Index to* The Nation, vol. 1.

DR. ABBOT is one of the many thinkers who believe that science is destined to produce a theism, and he belongs also to the smaller number who think that it is already possible to say what that doctrine shall be. Considerably more than half of his 'Scientific Theism' is taken up with the proof that the world is intelligible; but this lengthy and metaphysical argumentation will convince nobody for whom very simple considerations would not have sufficed. How is it that one who believes he has the message of a new religion to announce to humanity should choose so roundabout a way of setting it forth? The following is one of the author's own summaries of his line of argument:

"1. Because the universe is in some measure actually known in human science, it must be in itself both absolutely self-existent and infinitely intelligible: that is, it must be a noumenon because it is a phenomenon.

"2. Because it is infinitely intelligible, it must be likewise infinitely intelligent.

"3. Because it is at the same time both infinitely intelligible and infinitely intelligent, it must be an infinite subject-object or self-conscious intellect.

"4. Because it is an infinitely intelligible object, it must possess throughout an immanent relational constitution.

"5. Because it possesses an infinitely intelligible relational constitution, it must be an absolutely perfect system.

"6. Because it is an absolutely perfect system, it cannot be an infinite machine, but must be an infinite organism.

"7. Because it is an infinite organism, its life principle must be an infinite immanent Power, acting everywhere and always by organic means for organic ends, and subordinating every event to its own infinite life: in other words, it must be infinite Will directed by infinite Wisdom.

"8. Because it is an infinite organism, its exient organic end disappears as such, but reappears as infinite Love of itself and infinite Love of the finite.

"9. Because it is an infinite organism, its immanent organic end appears as the eternal realization of the ideal, and therefore as infinite Holiness.

"10. Because, as an infinite organism, it thus manifests infinite Wisdom, Power, and Goodness, or thought, feeling, and will in their infinite fulness, and because these three constitute the essential manifestations of personality, it must be conceived as Infinite Person, Absolute Spirit, Creative Source, and Eternal Home of the derivative finite personalities which depend upon it, but are no less real than itself."

If this last conclusion really follows from the original premise, why need the proof have been so long? It is not like a geometrical demonstration, where there is a complicated diagram, every part of which has to be separately considered. In this case the premise is as simple a fact as can be—that something is known; the conclusion that the universe is an infinite person is also not very complex, and the intricacy of the argument to connect them affords ground for a suspicion that there is a fallacy somewhere. It would be a flattery of metaphysics to say that its history gives any warrant for holding that no more than one deduction in ten as plausible as the above turns out to be fallacious; and therefore the probability that there is no fallacy in the whole of the above chain of ten consequences is only 9-10ths to the tenth power, which is about 1-3. In advance of the verdict of posterity, then, the odds are two to one against Dr. Abbot's argument being sound. The subtlety of Nature, as Bacon says, far exceeds that of the human mind, and has a way of eluding our *must-bes.* To look no further than Dr. Abbot's first consequence, may it not be that nature is sufficiently intelligible to account for the degree of success that natural science has met with, without being necessarily *infinitely* intelligible?

The religion of the book seems to be only an appendage to a system of metaphysics. Whether true or false, this system is certainly valuable as presenting Objectivism, or the doctrine of an existence over against thought, in its extremest form. Its most striking philosophical characteristic is an energetic dualism. It makes the fundamental doctrines of philosophy consist in distinctions, crystalline, sharp, and unyielding; and the oppositions of things to which these distinctions refer go down to the bottom of being. The appearance and the thing are sundered by an impassable gulf, and the element of concrete outward reaction in sense and volition is much more emphasized than in other philosophical theories. The same spirit affects the author's whole style of thought and writing, which is clear and hard, and impels him to destroy every opposing tendency of thought "root and branch," instead of imitating other recent revolutionizers of philosophy in wishing to show that the error need only to receive complete development in order to be turned to the truth. Everything like uniting the members of his main distinctions by insensible gradations, by a deeper underlying unity, or by any mediating cause, except the Divine Mind which creates the relations but not the related things, is foreign to his idea.

Dr. Abbot holds that things, as they are known to physical science, possess absolute existence in themselves, not relative to or dependent upon thought of any kind. He holds that the relations of these things are hard facts, equally independent of all thought. There seems, however, to be some vagueness in his theory of relations, for on page 28 he seems to say that relations are something over and above the related things—"things and relations constitute two great distinct orders of objective reality"; while on page 63 we are told that "the affirmation of the objectivity of the relation [must not be misconceived] as an affirmation that the relation is an entity apart from the things it relates." He holds that relations inhere in groups; but whether the existence of these groups consists in the existence of the relations, or the existence of the relations consists in that of the

groups, or whether groups form a third order of reality distinct alike from things and relations, he does not inform us. And it will be one of his difficulties that his system, from the nature of it, at once opens a multitude of questions of this sort, the consideration of which cannot be shirked. The author is so remarkably loath to admit mediation that he will not admit there is any such thing as a symbolical conception (p. 139):

"The universal notion, or concept proper, is a pure thought-system of relations, reproducing only the objective system of relations of resemblance among many individuals—never the image or mental picture of one individual."

The doctrine seems to be that the relations are reproduced, without being embodied in any diagram, as "concepts of relations, dropping out of consideration the things related." The knowledge of relations depends upon a special "perceptive use of the understanding." This view, although it is not adequately set forth, is the centre of all that is original in the book, and is sure to excite a fruitful discussion of the question of the mode of our discernment of relations. Of all the sciences—at least of those whose reality no one disputes—mathematics is the one which deals with relations in the abstractest form; and it never deals with them except as embodied in a diagram or construction, geometrical or algebraical. The mathematical study of a construction consists in experimenting with it; after a number of such experiments, their separate results suddenly become united in one rule, and our immediate consciousness of this rule is our discernment of the relation. It is a strong secondary sensation, like the sense of beauty. To call it a perception may perhaps be understood as implying that to discern each special relation requires a special faculty, or determination of our nature. But it should not be overlooked that we come to it by a process analogous to induction.

The one great argument which Doctor Abbot uses to support his "noumenism," as he calls it, is that the existence of natural science supposes it. But the physicist always talks and thinks of phenomena or appearances, and makes not the slightest pretension to have anywhere got down to the noumena, bottom facts, or ultimate subjects of appearances. He discovers, for instance, that air is viscous, and viscosity is a non-conservative force. It is a reality; but yet, according to the physicist, only a phenomenal reality. Matter in itself is not viscous; but this phenomenon is due to the air being composed of countless molecules moving very rapidly in nearly rectilinear paths. These molecules themselves are not necessarily the bottom subjects; they may be mere systems of atoms, which in turn may be merely phenomena due to the vortex-motions of an underlying fluid. This fluid may come to be studied in time, and physicists will be quite prepared to learn that it again is only phenomenal. The physicist certainly holds that he reaches real facts, which no more depend upon anybody's thought of them for their existence than the coach in the fable depended on the fly for its motion. For example, he holds this to be true of the laws of the mixture of colors. These laws are realities, which remain what they are whatever our opinions about them may be. But to say this, is not to say that the colors themselves are anything

more than appearances. Further, although science must hold the facts it discovers to be independent of the opinion of any person or persons, it by no means follows that it need insist on their being independent of the final upshot of sufficient investigation, nor that it need hold them to be independent of the creative thought of the Deity. As yet, science does not decide either for or against any of the current systems of philosophy. Some are undoubtedly more in harmony with its spirit than others; but we can hardly reckon among the former a theory so averse to the conceptions of the differential calculus, and so prone to hard and discrete distinctions, as the one we have noticed. It is, however, a strongly characterized and scholarly piece of work, doing honor to American thought; and it is much to be desired that the world should see the system developed in its entirety.

1889

48 (13 June 1889) 488

THE CENTURY DICTIONARY

TO THE EDITOR OF THE NATION:

SIR: Your recent review of the 'Century Dictionary' ought to be supplemented by some remarks upon its definitions of terms in physical science, while there is still time to make corrections. The definitions in question are, in many cases, insufficient, inaccurate, and confused in a degree which is really remarkable. Take, for example, the description of Ptolemy's 'Almagest,' "a book or collection of problems in astronomy and geometry, . . . so named by the Arabs because it was reckoned the greatest work on the subjects." Far from being a collection of problems, I doubt if there is a single problem in geometry or astronomy in the entire work. In no sense of the word is it a book of geometry, nor could it ever have been considered as such. While thus giving an erroneous description, what the work really is—a system of astronomy based upon the doctrine that the earth remains immovable in the centre of the heavens—is entirely omitted. In a rapid glance through a portion of the published pages (A-Appet), I have noticed a number of other cases of insufficient, erroneous, or misleading definitions or statements. The definition of *albedo* is confused and misleading. That of *eccentric anomaly* is entirely wrong. *Absorption lines* are described as occurring just under the conditions when they are impossible. *Law of action and reaction* is accurate, with the exception of a sentence which is so far wrong that I suspect it to have been interpolated after the original article left the writer's hands. *Apochromatic* is insufficiently defined, and is illustrated by a quotation as unintelligible as could readily be found. *Alidade* and *achromatic lens* contain misstatements less remarkable for their seriousness than for their existence.

So many defects in a single subject and in so small a fraction of the book would seem to indicate that the details of the work are not such as we should expect from the attention and care with which the editor and publishers have devised and executed their part of the plan. It ought to be added that, so far as I have noticed, the definitions in mathematics and mathematical physics are not subject to this criticism.

S. NEWCOMB.

WASHINGTON, June 8.

48 (20 June 1889) 504-505

THE CENTURY DICTIONARY

TO THE EDITOR OF THE NATION:

SIR: The faults which Prof. Newcomb finds with my definitions in the Century Dictionary are, I trust, at all events, confined to the earlier pages, where I was unable to see proofs of a part of what I wrote. I ask leave to illustrate my method of preparing definitions, in the instances of the five in my department to which he objects. I take these up in their alphabetical order.

(1.) *Action.* The first step towards defining such a phrase as the "law of action and reaction" is to find who originally introduced it, and where. In this case the author is Newton, the book the 'Principia.' I next inquire whether and where there has been any subsequent discussion of the meaning. This carries me to Thomson and Tait's 'Natural Philosophy.' Finally, I collect the common meaning of the phrase from a series of English writers of different periods. Prof. Newcomb says my definition contains an erroneous sentence. I presume he alludes to that in which I give Newton's definition of "action." Though Thomson and Tait laud Newton's remarks, these certainly confound two distinct phenomena, and we may regret his definition of "action," which does not apply to ordinary cases under the law. I ought to have added something to that effect. But Newton does give that definition, and gives no other, and he lived in an age when men were expected to adhere to their definitions; and I was bound to record his statement. I supplement this in the next sentence by giving the law as it ought to be and is generally understood. There is no error, but only the omission of an explanatory sentence, probably as appearing disrespectful to the "summus Newtonus."

(2.) *Albedo.* This word, introduced by Lambert in 1760, and defined by him, and distinguished into species by Zöllner in 1865, does not belong strictly to my department. I suppose I wrote on the galley-proof: "the proportion of the light falling on a surface irregularly reflected from it," and that the proof-reader, finding this a bad sentence, inserted *and* before "irregularly," where *that is* would have answered better. The slight confusion resulting is corrected by the rest of the definition. I may remark that *albedo* has nothing to do with the light regularly reflected, which is to be reckoned as if absorbed; and, moreover, a body may have *albedo* although slightly self-luminous, as Saturn has been supposed to be. The *albedo* is, therefore, not exactly "the proportion of incident light reflected by a non-luminous body," as Prof. Newcomb defines it.

(3.) *Alidade.* Arabic terms of astronomy have been in nearly every instance hunted up in Arabian authors, generally in old Latin translations. They have been sought preferentially in translations of Ptolemy, so as to connect them with the Greek. They have also been looked up in Lane's or other Arabic Lexicons; and finally they have been traced through various writers from Chaucer to Newcomb. There is some dispute regarding the proper meaning of the word *alidade* in Arabic. In English, it is generally applied to an arm of an optical instrument, traversing a circle, and attached, as such arms commonly are, to a telescope, or carrying sights. (The restriction by some writers to a vertical circle cannot be justified.) It is, however, occasionally extended (as by Newcomb) to all arms of circles, whether carrying sights or not; and as this use is borne out by Arabian dictionaries, we cannot call it wrong. On the other hand, the word is very frequently applied, both in Arabic (see Devic, 'Glossaire') and in English, to a straight-edge unattached to a circle and bearing sights or a telescope. Both these meanings are given in the Dictionary. The first definition fully accords with that given by Newcomb himself, and the second is even more undoubtedly correct.

(4.) *Almagest.* Supposing the editors would delete this proper name, I wrote no description, and that in the text is continued from the Imperial Dictionary. It is

substantially that of Hutton. I took into consideration the alteration of it in the plates, but, after turning over the Almagest itself with this view, decided to retain it. Prof. Newcomb makes two objections to the description—first, that it contains no account of the Ptolemaic system, but that would have been ill-placed here; and, second, that the work contains no problems in geometry and astronomy, as stated, which seems hypercritical when we call to mind the treatise on trigonometry in the first book, and when we reflect that the astronomical memoirs of which the work consists are properly enough called problems. The reason given for the name, though not objected to by Prof. Newcomb, is slightly incorrect.

(5.) *Anomaly.* This definition, perhaps the first I wrote in astronomy, I certainly cannot defend. Besides containing a blunder remarked by Prof. Newcomb, the whole is awkwardly drawn up, the applicability of the name "anomaly" is not explained, nor the mode of reckoning it used by Kepler and his followers before Gauss. I hope I may be able in some way to replace the article by another prepared according to my usual method, being based on an examination (1) of Ptolemy, (2) of Kepler (who defines the eccentric anomaly, a term due to him, very clearly as "arcus circuli eccentrici, in consequentia numeratus interceptusque inter lineam apsidum et inter perpendicularem illi per corpus planatæ"), (3) of Gauss, and (4) of a series of English writers. C. S. PEIRCE.

MILFORD, PA., June 14, 1889.

48 (27 June 1889) 524

THE CENTURY DICTIONARY

The Garrison-Peirce correspondence contains two items of special importance for this issue. MS L 159.2 is a draft of a letter to the editor of *The Nation*, written by Peirce on 14 June 1889, in which he answers some of the charges made by Newcomb. MS L 159.3 is a draft of a later reply, written to *The Nation* by Peirce on 28 June 1889, in which he again responds to Newcomb's criticism. Rebuttal in *The Nation* was given up in favor of a personal letter to Newcomb from Peirce, written 2 July 1889. For more information see Carolyn Eisele, "The Charles S. Peirce-Simon Newcomb Correspondence," *Proceedings of the American Philosophical Society*, vol. 101, 1957, pp. 409-433.

TO THE EDITOR OF THE NATION:

SIR: I am surprised to learn, from Mr. Peirce's very frank letter in your issue of the 20th inst., that some of the definitions of the 'Century Dictionary' which I criticised were his. The contrast which I mentioned between the definitions in mathematics and mathematical physics and those in astronomy and experimental physics I supposed to mark the line between his work and that of some less skilful hand. Still more surprising is it to see him call my strictures on the description of the 'Almagest' of Ptolemy as a "book or collection of problems" "hypercritical." Would he defend a lexicographer who should define the 'Mécanique Céleste' as a collection of mathematical and astronomical problems by Laplace? Yet the description would be fully as correct as that in question.

In the case of the word *alidade*, my objection was directed to the statement that it is an attachment of every instrument for measuring angles. Are the stone

piers on which the meridian circles of our great observatories are supported ever called alidades?

The sentence under *Law of action and reaction* which I supposed to be an interpolation is, as Mr. Peirce correctly infers, this: "By *action* is here meant, according to Newton, a quantity measured by the force multiplied into the velocity of the point of application." I think he is entirely mistaken in supposing that Newton gives this definition of the word as used in his statement of the law. I can find no such definition in the 'Principia.'

Since my strictures upon some of the definitions on the 'Century Dictionary' appeared in your issue of the 13th inst., I have hastily glanced through the remainder of the letter A, and noticed the following faulty definitions. The word *approximation* is defined as if it were identical with what is known as the method of successive approximations. The definition of *diurnal arc* is meaningless: "the arc described by the heavenly bodies in consequence of the diurnal rotation of the earth." Of course there is no definite arc thus described, but only an endless repetition of one and the same circle. The term is actually applied to that portion of the sun's apparent daily path which is above the horizon. The same term is, I believe, applied to the apparent paths of the stars above the horizon. *Nocturnal arc* is new to me, but I think its definition also incorrect. *Argus*, the constellation, is omitted, though *Aries* and *Aquarius* are included. S. NEWCOMB.

49 (15 August 1889) 136-137

Deductive Logic.

By St. George Stock, M.A. Longmans Green & Co. Pp. 356.

Attributed to Peirce by Fisch in *First Supplement* (internal evidence: reference to O. H. Mitchell and the *Studies in Logic*, which Peirce edited). This review is unassigned in Haskell's *Index to* The Nation, vol. 1.

One of the author's friends who looked over this book in manuscript advised him not to publish it because it was too like all other Logics; another advised him to cut out a considerable amount of new matter. We cannot help being of the opinion that both of these friends were persons of a great deal of wisdom. In spite of the fact that the latter advice was followed, a good part of the new matter which is retained is, as we shall presently show, erroneous, and the old matter is, to say the least, not better set forth than in several other text-books which we could name.

This is not saying that it is not, at many points, fresh and admirably expressed and fully mastered by good sense. It would be impossible for a man who has been studying and teaching logic at Oxford for seventeen years to write a thoroughly bad book on the subject. It is merely saying that the teacher who should decide to adopt this book in his class-room instead of Bain, for instance, would be doing his pupils an injury. The trouble which the student usually has with his book on Logic is that it seems to him too much like a mixture of dry bones and sawdust. The best exposition of the subject is one which forces him, at every step, to see that there is an intimate connection between its formal rules and the trains of

thought which actually go on in his own mind. Mill is still the only book for "the gentleman and the scholar" to read; but, for the young person who must be put quickly through the drill established by the schoolmen, and who must at the same time see that it has a case bearing upon the present perplexities of the scientific man and the practical thinker, hardly anything is so good as Bain. Bain, it is true, is open to plenty of objections of another kind; and there is no subject in which there is more urgent need of a new book which shall embody the recent improvements in the science, and which shall at the same time exhibit a kindly consideration for the weaknesses of immature minds.

Mr. Stock, as far as appears from his book, is wholly unacquainted with Symbolic Logic. That is a subject which throws so much light on logical theory that a brief treatment of it ought to be introduced into every text-book; but even if that is not done, no one who writes a book should be content to be ignorant of it. The conventions which Symbolic Logic finds absolutely essential are a source of very great simplicity and consistency in ordinary Logic. Mr. Stock does not mention Venn among the writers who have helped him, and he can hardly have read his persuasive plea for the thorough-going introduction of De Morgan's idea of a limited universe, and of the convention that particular propositions must imply the existence of terms, and universal must not. With this convention, it is true that we must "accept the awkward corollary" of the collapse of the time-honored jingle about opposition; but worse things than that have been lived through. If it has been shown that black swans are not found in Africa, and that they are not found anywhere else, what follows in real life is that there are no black swans; but what the old-fashioned logician wishes us to believe is that one or other of the two statements must be false. It is evident that the former is the more reasonable conclusion.

Mr. Stock calls the statement "If *a* is *b*, *c* is *d*" a complex proposition. It should be called a compound proposition, that is, a proposition about propositions, or, better still, a sequence. The term complex proposition is needed for such as have subjects or predicates that are to be broken up in the course of the reasoning, as when we infer from the statement, "Citizen-students are always revolutionists," the other statement, "All students are revolutionists, or else they are not citizens." The three things which logic considers would then be the concept, the judgment and the sequence, the last being defined to be the statement that one proposition *follows* from another or from several others, either logically (that is, as inference), or materially (that is, as matter of fact).*

Mr. Stock's introduction, on the whole, is good, though a more psychological account of the concept might have been given; and good, also, is his treatment of extension and intension. But he has a curious idea of what constitutes induction. The concluding from "All the metals which we have examined are fusible" to "All metals are fusible," he gives as an example of what induction is *not*, and then he argues that it is a mistake to talk of inductive reasoning as though it were a species distinct from deductive. The above kind of reasoning he stigmatizes as a

*The distinction between the logical and the material sequence is very much the same as that between the verbal and the real proposition.

"vague instinct," but he forgets that before the days of Aristotle the strictest syllogistic reasoning was vague instinct in everybody's mind, that it is so now in the minds of all but a very few, and that it is so even in their minds in all but a very few hours of their existence. Another "curiosity of literature" Mr. Stock furnishes when he argues, under fallacies, that it is wrong to ask your opponent to grant the point under dispute, because it is violating "the first of the general rules of syllogism, inasmuch as a conclusion is derived from a single premise, to wit, itself."

But the most original part of the book is the treatment of immediate inference as applied to compound propositions, and this, unfortunately, is almost wholly erroneous. In the first place, the treatment is totally inadequate on account of the fact that it applies only to singular propositions. The denial of "No kings are tyrants" is "Some kings are tyrants," not "All kings are tyrants." "If all men are gentle, all women are brave" is the same thing as "If any women are not brave, some men are not gentle," but it is far from being the same thing as "If no women are brave, no men are gentle." But even for singular propositions, in which "The sun shines" and "The sun does not shine," for instance, contradict each other, Mr. Stock is still chock-full of error. His mistakes are due to two causes—to his ignorance of the fact that particular propositions necessarily imply the existence, real or logical, of their terms, and to his ignorance of the fact, admirably set forth by the late Prof. O. H. Mitchell, in the 'Studies in Logic,' that propositions in two dimensions are necessarily six and not four in number. The reason for this latter fact is, that "All rivers are sometimes dry" may mean either that there are times when every river is dry, or that every river is dry at one time or another; and that reasoning cannot proceed with safety until it is known which of these two things is meant. We shall not take time to set forth the effects of these two fundamental errors. It is sufficient to point out that no one but a hardened logician would suppose the statement, "Either operators must be careful, or telegrams will sometimes not be correct," to be the same thing as "Either telegrams are correct, or operators are sometimes not careful"; nor would he suppose that in order to deny the statement, "Either men fight, or tyrants reign," we say "Either men fight, or tyrants do sometimes not reign." It gives one a distinct feeling of dizziness, if not of nausea, to be told that these two statements are the denials of each other. To refute him who says, "Either corruption ceased, or the country went to the dogs," it would be necessary to establish *both* that corruption did not cease *and* that the country did not go to the dogs. It happens that statements in *either or* and in *if* are abbreviated forms for *universal* sequences, and that it is impossible to express with those words the particular sequences which are necessary for denying them. All this is as plain as daylight to any one who has been trained in Symbolic Logic, as well as to any one who has not studied Logic at all.

If this author showed greater strength than he does in plain questions of Logic, more interest would attach to the fact, which appears from an advertisement in the end of the book, that he attributes "importance to spiritualism, and gives a degree of credit to its phenomena." There is an admirable collection of examples.

1890

50 (27 February 1890) 184

The Science of Metrology; or Natural Weights and Measures. A Challenge to the Metric System.

By the Hon. E. Noel, Captain Rifle Brigade. London: Edward Stanford. 1889.

CSP, identification: MS 1365. See also: Burks, *Bibliography*. This piece is unassigned in Haskell's *Index to* The Nation, vol. 1.

The metric system is now supposed to be taught in the arithmetic course in every school. If it were well taught—say, if a quarter of an hour twice a week for half a school year were intelligently devoted to it—the pupils would for ever after be more familiar with millimetres, centimetres, metres, and kilometres, with grammes and kilogrammes, with ares and hectares, and with litres, than they are ever likely to be with the English units. Who, except an occasional grocer, can guess at a pound within two ounces; or how many, besides engineers and carpenters, can distinguish seven-eights of an inch from an inch at sight? Yet these are things easily taught. But schools will gradually get better conducted, and foreign intercourse seems destined before very long to receive an almost sudden augmentation; so that the metric system will pretty certainly become more and more familiar, and there may be expected to be some practical movement towards its use in trade. It is quite within the bounds of possibility that, even in a country with as little governmental initiative as ours, fashion may lead to the partial super seding of the old weights and measures, just as the avoirdupois pound superseded the Troy and merchants' pounds, as ells and nails have given place to yards and inches, as lasts and stones, firlots, kilderkins, long tons, great hundreds, and innumerable other units have disappeared within this century. If the litre, the half-kilo, and the metre were only not all severally greater than the quart, the pound, and the yard, there might be shops to-day where the keepers would affect to be unacquainted with English weights and measures.

There is little real difficulty in changing units of weight and bulk, were there any positive motive for it, for the things they weigh and measure are mostly used up within a twelvemonth. But with linear and square measure it is otherwise. The whole country having been measured and parcelled in quarter sections, acres, and house-lots, it would be most inconvenient to change the numerical measures of the pieces. Then we have to consider the immense treasures of machinery with which the country is filled, every piece of which is liable to break or wear out, and must be replaced by another of the same gauge almost to a thousandth of an inch. Every measure in all this apparatus, every diameter of a roll or wheel, every bearing, every screw-thread, is some multiple or aliquot part of an English inch, and this must hold that inch with us, at least until the Socialists, in the course of another century or two, shall, perhaps, have given us a strong-handed government.

We can thus make a reasonable prognosis of our metrological destinies. The metric system must make considerable advances, but it cannot entirely supplant

the old units. These things being so, to "challenge" the metric system is like challenging the rising tide. Nothing more futile can well be proposed, unless it be a change in the length of the inch. Nevertheless, there is a goodly company of writers to keep the Hon. Capt. Noel in countenance in conjoining these two sapient projects. None of these gentlemen supports the constructive parts of the other's propositions; but they are unanimous against the metric system and the existing inch.

Mr. Noel's system is nearly as complicated and hard to learn as our present one, with which it would be fearfully confused, owing to its retaining the old names of measures while altering their ratios. Thus we should have to learn that 2½ feet would make a yard, 4 miles a league, 5 feet a fathom, 625 acres a square mile, 1.953125 cubic ells a cubic yard, 216 cubic inches a gallon, 24 ounces a pound, etc. But it is not intended that this complication shall last for ever, for this lesson, once digested, is to be followed by a clean sweeping away of the decimal numeration and the substitution of duo-decimals. Mr. Noel enumerates sixty-eight advantages of his proposal, among them the following: "Mile, one-quarter hour's walk, better than kilometre"; "cubic foot worthier base than cubic dicemetre"; "old London mile restored." The scheme is not without merit, and might have been useful to Edward I. Even at this day it must at least have afforded some agreeable occupation to its ingenious and noble author, not to speak of the arithmetical practice.

50 (27 March 1890) 265

Epitome of the Synthetic Philosophy.

By F. Howard Collins. With a preface by Herbert Spencer. D. Appleton & Co. 1889.

CSP, identification: MS 1365. See also: Burks, *Bibliography*. This note is unassigned in Haskell's *Index to* The Nation, vol. 1.

A more admirably executed second-hand synopsis of a system of philosophy never was. Considered simply as an index to Spencer's systematic works, this 'Epitome' is invaluable; and to persons who read and reread those thick volumes, not because they believe in them, but only because they want to know what it is that so many others believe, and to whom the writings of the dreariest scholastic doctor are less heartbreakingly tedious, this one volume of 500 pages in place of a library of 5,000 pages is like balm of Gilead. Would it only embraced an introduction boiling the whole thing down to 50 pages! It is printed uniformly with Spencer's works, upon agreeable paper with clear type, and published by the same eminent firm which, by the dissemination of those writings, has contributed so much to the culture and thought of our people.

50 (19 June 1890) 492-493

RIBOT'S PSYCHOLOGY OF ATTENTION

The Psychology of Attention.

By Th. Ribot. Authorized translation. Chicago: The Open Court Publishing Company. 1890. 8vo, pp. 121.

CSP, identification: MS 1365; Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles.*

Every educated man wants to know something of the new psychology. Those who have still to make acquaintance with it may well begin with Ribot's little book on 'Attention,' which all who have made progress in the new science will certainly wish to read. It is the *chef d'œuvre* of one of the best of those students who have at length erected psychology into a science.

Ribot regards the doctrine of attention as "the counterpart, the necessary complement, of the theory of association." He means that attention is related to suggestion as inhibition to muscular contraction. Physiologists, however, would scarcely rank *inhibitibility* with contractility as an elementary property of protoplasm. Besides, though suggestion by association may be likened to muscular action, how can the analogy be extended to the process of association itself, or the welding together of feelings? This welding seems to be the only law of mental action; and upon it suggestion and inhibition of suggestion alike depend. Attention is said by Ribot to modify reverie's train of thought by inhibiting certain suggestions, and thereby diverting their energy to suggestions not inhibited. This makes the positive element of attention quite secondary. At the same time, we are told that the sole incitement to attention is interest. That is to say, a preconceived desire prepares us to seize promptly any occasion for satisfying it. A child's cry, drowned in clatter of talk for others' ears, attracts the mother's attention because she is in some state of preparation for it. Ribot, however, does not remark that to say the mind acts in a prepared way is simply to say it acts from a formed association, such action not being inhibitory. If interest be the sole incitement to attention, it is that the energy spent upon the interesting suggestion leaves none for others, rather than that a positive inhibition of the latter throws waste energy into the former. This only happens when attention is controlled for a conscious purpose. If, in the beginning of his inquiry, Ribot had discarded the unscientific word "attention," and with it his feeble antithesis of association and attention, the truth would have shone out that the main phenomenon is emotional association, aided in certain cases by acts of inhibition.

The most interesting and valuable parts of the book are those devoted to corporeal concomitants of attention. Evidence is that in this act parts of the brain receive increase of blood. This must be due to stimulation of the vaso-motor nerves, belonging to the sympathetic system, under the influence of the desire in the interest of which attention is excited. Moreover, in intense attention the breath is held, and in every case respiration is slackened. There are, besides, certain muscular actions: in external attention, the eyebrows and the skin of the forehead over them are drawn up, the eyes opened wide and directed to the

object, the jaw more or less dropped, and the whole body held immobile in an attitude as if approaching the object. In internal attention, the brow is contracted, the eyebrow lowered, the lid at least partially closed, the jaw clenched, the lips pursed up, the body usually immobile, preferentially in a sitting posture with the whole arms close to the trunk. There are, however, often motions, as walking up and down. These muscular states are indispensable conditions of attention. "It is impossible to reflect while running at full speed or climbing a steep ascent." "A child, seven years old," not able to breathe through its nose, owing to a tumor, "had succeeded in learning, during a whole year, only the first three letters of the alphabet. Having been operated upon for its adenoid tumor, the same child in a single week learned the entire alphabet."

According to Ribot, these muscular actions are not *aids* to attention, but constitute attention. The notion that we think with our muscles is very attractive to the whole new school. Ask why, and you are told, because "every act of volition, whether impulsive or prohibitory, acts only upon muscles and through muscles; any other conception is vague, incomprehensible, and chimerical." This little burst of emphasis signifies defective evidence. When positive evidence is at hand, it is calmly put in; when prejudices have to be addressed, warmth is in order. The truth is, all these physiological psychologists are "monists." For theory of connection of soul and body, they have struck a happy compromise between materialism and spiritualism, in holding that mind and matter are simply two aspects of the same thing. If the balance were really preserved between the opposing tendencies, the result would be a doctrine in harmony with philosophic pessimism, but not easily reconciled with observed facts. But is the balance held even by the psychophysicists? They say, for example, that unorganized matter feels, if at all, very little. But when we expect them to balance this by cases in which mind is barely, if at all, connected with matter, they insist, on the contrary, that the attributes of matter do not admit of augmentation and remission, and that soul only exists as an aspect of that which otherwise appears as corporeal. What is this but making mind to be a special determination of that universal substance which is generally known to us as matter? And to make mind a specialization of matter would seem, metaphysical phrases apart, to be materialism. In our day, the charge of being materialist will scare nobody; and all the facts of life show dependence of soul upon body. Yet common sense will never admit that feeling can result from any mechanical contrivance; and sound logic refuses to accept the makeshift hypothesis that consciousness is an "ultimate" property of matter in general or of any chemical substance.

No philosophy will endure which does not freely allow to every reason, every fact, its full force. But this school is for ever exaggerating the resemblances of psychical and physical phenomena, for ever extenuating their differences. Ribot, for example, often speaks of the "mechanism of association," and even attempts to apply to it the physical distinction of potential and kinetic energy. But looking at the matter without prepossession, or with that of a student of mechanics, the analogy between the process of association and any mechanical motion does not appear to be very close. Both are operations governed by law, it

is true. But the law of mechanics is absolute, prescribing (after two positions are given) the precise point of space where each particle shall be at each instant of time; while the force of association is essentially a gentle one (two ideas that have occurred together having a gentle tendency to suggest one another), and if it were made absolute, ideas would at once be rigidly bound together, and the whole phenomena of learning, or generalization, which is the essence of association, would be put to death.

Again, alike in the physical and the psychical world, we find trains of causation. In the latter, it is the past alone which directly and involuntarily influences the present by association; the future we only divine; and all our efforts are to make our present actions conform to our idea of that future. In the physical world, on the contrary, regard being had to the law of the conservation of energy, which denies any primordial force dependent on velocity, the past and the future are in relations to the present precisely similar to one another—a fact which appears from the circumstance that, in the equations of motion, the sign of the flow of time may be reversed, provided the signs of the velocities are reversed, the forces being unchanged, and still the formulæ will remain intact. We will not say that these distinctions between mental and mechanical actions are facts large enough to blot out their slight resemblances, for these latter should neither be overlooked nor disregarded; but the distinctions will certainly be prominent in a well-proportioned view of the subjects. Undoubtedly, there are physical phenomena in which gentle forces seem to act, and others which seem to violate the principle of energy; but these appearances are due to a principle different from a law of motion, namely, to the action of probability. The type of such phenomena is the viscosity of a gas; and the regularity of this, closely approximate but not strictly exact, is due to the countless trillions of molecules which are flying about in all directions with almost every rate of speed. That there is analogy between spreading of motion through a gas by viscosity and association of ideas need not be denied.

In regard to the doctrine that volition consists in, or is an aspect of, muscular contraction or inhibition, it is to be considered that considerable time elapses during the passage of the motor impulse down the nerve. During this interval we seem to be aware of a striving, like that of nightmare. At any rate, something has taken place in which the muscle had no part. The muscle might even be amputated before the impulse reached it. But if a motor impulse can thus be communicated to a nerve fibre to be transmitted over it, how can we be sure that this latter may not abut against a nerve cell instead of against a muscle cell?

Ribot's terminology sometimes seems open to criticism. Of the two forms of attention, that which is governed by the course of outward perceptions and that which is controlled from within by definite purposes, he terms the former *spontaneous*, the latter *voluntary*. Now, suppose a man in a sudden fit of anger blackguards another, can it be said that his speech was *involuntary* simply because it was not controlled? And if he wished to excuse himself on the ground of sudden provocation, would he say that his language was purely *spontaneous*? It would seem better to call every action which is subject to inward control *volun-*

tary, whether actually controlled or not, and to apply the term *spontaneous* only to those acts which are *not* reflexes from external stimuli.

The translation is sufficiently good, and the *Open Court* is doing useful work in publishing such books.

51 (3 July 1890) 16

Pure Logic, and Other Minor Works.

By W. Stanley Jevons. Edited by Robert Adamson and Harriet A. Jevons. Macmillan & Co. 1890.

CSP, identification: MS 1365. See also: Burks, *Bibliography*. This review is unassigned in Haskell's *Index to* The Nation, vol. 1.

Though called Minor, these are scientifically Jevons's most important writings. As when they first appeared, they impress us by their clearness of thought, but not with any great power. The first piece, "Pure Logic," followed by four years De Morgan's "Syllabus of Logic," a dynamically luminous and perfect presentation of an idea. In comparison with that, Jevons's work seemed, and still seems, feeble enough. Its leading idea amounts to saying that existence can be asserted indirectly by denying the existence of something else. But among errors thick as autumn leaves in Vallambrosa, the tract contains a valuable suggestion, a certain modification of Boole's use of the symbol + in logic. This idea, directly suggested by De Morgan's work, soon presented itself independently to half-a-dozen writers. But Jevons was first in the field, and the idea has come to stay. Mr. Venn is alone in his dissent.

The substance of the second piece in this volume, the "Substitution of Similars," is in its title. Cicero had a wart on his nose; so Burke would be expected to have something like it. This is Mill's inference from particulars to particulars. As a matter of psychology, it is true the one statement suggests the other, but logical connection between them is wholly wanting. The substitution of similars might well be taken as the grand formula of bad reasoning.

Both these tracts warmly advocate the quantification of the predicate—that it is preferable in formal logic to take A = B as the fundamental form of proposition rather than "If A, then B," or "A belongs among the Bs." The question is not so important as Jevons thought it to be; but we give his three arguments with refutations. First, he says the copula of identity is logically simpler than the copula of inclusion. Not so, for the statement that "man = rational animal" is equivalent to a compound of two propositions with the copula of inclusion, namely, "If anything is a man, it is a rational animal," and "If anything is a rational animal, it is a man." True, Jevons replies that these propositions can be written with a copula of identity, A AB. But A and B are not symmetrically situated here. They are not simply joined by a sign of equality. Second, Jevons says that logic takes a more unitary development with the proposition of identity than with that of inclusion. He thinks his doctrines of not quantified logic and the substitution of similars call for this copula, but this is quite an error. And then an inference supposes that if the premises are true, the conclusion is true. The

relation of premises to conclusion is thus just that of the terms of the proposition of inclusion. Thus the illative "ergo" is really a copula of inclusion. Why have any other? Third, Jevons holds the proposition of identity to be the more natural. But, psychologically, propositions spring from association. The subject suggests the predicate. Now the difficulty of saying the words of any familiar thing backwards shows that the suggesting and suggested cannot immediately change places.

The third piece in the volume describes Jevons's logical machine, in every respect inferior to that of Prof. Allan Marquand, and adequate only to inferences of childish simplicity. The higher kinds of reasoning concerning relative terms cannot (as far as we can yet see) be performed mechanically.

The fourth paper advocates the treatment of logic by means of arithmetic—without previous logical analysis of the conception of number, which would call for the logic of relatives. To exhibit the power of his method, Jevons shows that it draws at once such a difficult conclusion as this: "For every man in the house, there is a person who is aged; some of the men are not aged. It follows, that some of the persons in the house are not men." Unfortunately, this is an exhibition not of the power of the method, but of its imbecility, since the reasoning is not good. For if we substitute for "person," even number, for "man," whole number; for "aged," double of an integer, we get this wonderful reasoning: "Every whole number has its double; some whole numbers are not doubles of integers. Hence, some even numbers are not whole numbers."

The remainder of the book is taken up with Jevons's articles against Mill, which were interrupted by his death. The first relates to Mill's theory of mathematical reasoning, which in its main features is correct. The only defect which Jevons brings out is, that no satisfactory mode of proving the approximate truth of the geometrical axioms is indicated. But this is a question of physical, not of mathematical, reasoning. The second criticism, relating to resemblance, seems due to Jevons's not seizing the distinction between a definite attribute, which is a resemblance between its subjects, and Resemblance in general, as a relation between attributes. The third paper concerns Mill's theory of induction. That theory may be stated as follows: When we remark that a good many things of a certain kind have a certain character, and that no such things are found to want it, we find ourselves disposed to believe that all the things of that kind have that character. Though we are unable, at first, to defend this inference, we are none the less under the dominion of the tendency so to infer. Later, we come to the conclusion that certain orders of qualities (such as location) are very variable even in things which otherwise are closely similar, others (as color) are generally common to narrow classes, others again (as growth) to very wide classes. There are, in short, many uniformities in nature; and we come to believe that there is a general and strict uniformity. By making use of these considerations according to four certain methods, we are able to distinguish some inductions as greatly preferable to others. Now, if it be really true that there is a strict uniformity in nature, the fact that inductive inference leads to the truth receives a complete explanation. We believe in our inferences, because we are irresistibly led to do so; and this *theory* shows why they come out true so often. Such is Mill's doctrine. It misses the essential and dwells on secondary features of scientific inference; but it is an

intelligible doctrine, not open to the charge of paltering inconsistency which Mr. Jevons brings against it.

No doubt there is a good deal of truth in Jevons's criticism of Mill, who was a sagacious but not a very close thinker, and whose style, very perspicuous for him who reads rapidly, is almost impenetrably obscure to him who inquires more narrowly into its meaning. But Mill's examination of Hamilton has a logical penetration and force which we look for in vain in Jevons's articles on Mill.

51 (7 August 1890) 118-119

Fundamental Problems: The Method of Philosophy as a Systematic Arrangement of Knowledge.

By Dr. Paul Carus. Chicago: The *Open Court* Publishing Company.

CSP. identification: MS 1365. See also: Burks, *Bibliography*. This review is unassigned in Haskell's *Index to* The Nation, vol. 1.

Paul Carus (1852-1919) was an American author, philosopher, and editor. He was born and educated in Germany, having taken his Ph.D. at Tübingen in 1876. In 1888, Carus assumed the editorship of both *The Open Court* and *The Monist*, which he held until his death. He was author of more than fifty books on philosophy, orientalism, and literature.

A book of newspaper articles on metaphysics, extracted from Chicago's weekly journal of philosophy, the *Open Court*, seems to a New Yorker something singular. But, granted that there is a public with aspirations to understand fundamental problems, the way in which Dr. Carus treats them is not without skill. The questions touched upon are all those which a young person should have turned over in his mind before beginning the serious study of philosophy. The views adopted are, as nearly as possible, the average opinions of thoughtful men to-day —good, ripe doctrines, some of them possibly a little *passées*, but of the fashionable complexion. They are stated with uncompromising vigor; the argumentation does not transcend the capacity of him who runs; and if there be here and there an inconsistency, it only renders the book more suggestive, and adapts it all the better to the need of the public.

The philosophy it advocates is superscientific. "There is no chaos, and never has been a chaos," exclaims the author, although of this no scientific evidence is possible. The doctrine of "the rigidity of natural laws . . . is a κτῆμα ἐς ἀεί." Such expressions are natural to Chicago journalists, yet, emphatic as this is, we soon find the κτῆμα ἐς ἀεί is nothing but a regulative principle, or "plan for a system." When we afterwards read that, "in our opinion, atoms possess spontaneity, or self-motion," we wonder how, if this is anything more than an empty phrase, it comports with rigid regularity of motion.

Like a stanch Lockian, Dr. Carus declares that "the facts of nature are specie, and our abstract thoughts are bills which serve to economize the process of exchange of thought." Yet these bills form so sound a currency that "the highest laws of nature and the formal laws of thought are identical." Nay, "the doctrine of the conservation of matter and energy, although discovered with the assistance of experience, can be proved in its full scope by the pure reason alone." When

abstract reason performs such a feat as that, is it only economizing the interchange of thought? There is no tincture of Locke here.

Mathematics is highly commended as a "reliable and well established" science. Riemann's stupendous memoir on the hypotheses of geometry is a "meritorious essay." Newton is "a distinguished scientist." At the same time, the views of modern geometers are correctly rendered: "Space is not a non-entity, but a real property of things."

The profession of the *Open Court* is to make an "effort to conciliate religion with science." Is this wise? Is it not an endeavor to reach a foredetermined conclusion? And is not *that* an anti-scientific, anti-philosophical aim? Does not such a struggle imply a defect of intellectual integrity and tend to undermine the whole moral health? Surely, religion is apt to be compromised by attempts at conciliation. Tell the Czar of all the Russias you will conciliate autocracy with individualism; but do not insult religion by offering to conciliate it with any other impulse or development of human nature whatever. Religion, to be true to itself, should demand the unconditional surrender of free-thinking. Science, true to itself, cannot listen to such a demand for an instant. There may be some possible reconciliation between the religious impulse and the scientific impulse; and no fault can be found with a man for believing himself to be in possession of the solution of the difficulty (except that his reasoning may be inconclusive), or for having faith that such a solution will in time be discovered. But to go about to search out that solution, thereby dragging religion before the tribunal of free thought, and committing philosophy to finding a given proposition true—is this a wise or necessary proceeding? Why should not religion and science seek each a self-development in its own interest, and then if, as they approach completion, they are found to come more and more into accord, will not that be a more satisfactory result than forcibly bending them together now in a way which can only disfigure both? For the present, a religion which believes in itself should not mind what science says; and science is long past caring one fig for the thunder of the theologians.

However, these objections apply mainly to the *Open Court's* profession, scarcely at all to its practice; for a journal cannot be said to wrench philosophy into a forced assent to religion which pronounces that "it is undeniable that immaterial realities cannot exist," and that "the appearance of the phenomena of sensation will be found to depend upon a special form in which the molecules of protoplasma combine and disintegrate," and that "the activity called life is a special kind of energy" (a doctrine whose attractiveness is inversely as one's knowledge of dynamics).

Dr. Carus writes an English style several degrees less unpleasant than that of many of our young compatriots who have imbibed the German taste by some years' or months' residence in Berlin or Heidelberg. And as to consistency, whatever may be its importance in a systematic work, in a series of brief articles designed chiefly to stimulate thought, strictly carried out, it would be no virtue, but rather a fault. On the whole, the *Open Court* is marked by sound and enlightened ideas, and the fact that it can by any means find support does honor to Chicago.

51 (28 August 1890) 177

The Theory of Determinants in the Historical Order of its Development. Part I. Determinants in General: Leibnitz (1693) to Cayley (1841).

By Thomas Muir, M.A., LL.D., F.R.S.E. Macmillan & Co. 1890.

CSP, identification: MS 1365. See also: Burks, *Bibliography*. This notice is unassigned in Haskell's *Index to* The Nation, vol. 1.

The only history of much interest is that of the human mind. Tales of great achievements are interesting, but belong to biography (which still remains in a prescientific stage) and do not make history, because they tell little of the general development of man and his creations. The history of mathematics, although it relates only to a narrow department of the soul's activity, has some particularly attractive features. In the first place, the different steps are perfectly definite; neither writer nor reader need be in the least uncertain as to what are the things that have to be set forth and explained. Then, the record is, as compared with that of practical matters, nearly perfect. Some writings of the ancients are lost, some early matters of arithmetic and geometry lie hidden in the mists of time, but almost everything of any consequence to the modern development is in print. Besides, this history is a chronicle of uninterrupted success, a steady succession of triumphs of intelligence over primitive stupidity, little marred by passionate or brutal opposition.

Dr. Muir, already well known by many investigations into determinants and continued fractions, and by a charming little 'Introduction to Determinants,' has thoroughly studied the history of this subject, and has arranged his account of it with remarkable clearness. Each writer's results are stated in his own language, followed by a luminous commentary. An ingenious table shows the history of forty-four theorems, and at the same time serves as an index to the first half of this volume, which, it is to be presumed, is one-half of the first part, and not more than one-fourth of the whole work.

Perhaps Dr. Muir attaches a little too much importance to theorems, as contradistinguished from methods and ideas. Thus, he speaks rather unfavorably of Bezout's work (1779), although it contains the idea of polar multiplication; but because this is not made a theorem, Dr. Muir hardly notices it. The first paper analyzed in the book is by Leibnitz, and contains the umbral notation, which is the quintessential idea of the theories of determinants as well as that of matrices, to which the theory of determinants is but an appendage.

We have already mentioned that the last number of the *American Journal of Mathematics* contains an admirable memoir upon matrices by Dr. Henry Taber of Clark University.

51 (18 September 1890) 234

Elements of Logic as a Science of Propositions.

By E. E. Constance Jones, Lecturer in Moral Sciences, Girton College. Edinburgh: T. & T. Clark. 1890. Pp. 208.

Attributed to Peirce by Fisch in *First Supplement* (internal evidence). This review is unassigned in Haskell's *Index to* The Nation, vol. 1.

Emily Elizabeth Constance Jones (1848-1922) was a British logician. She was vice-mistress (1896-1903) and later mistress (1903-1916) of Girton College, Cambridge, and also resident lecturer in moral sciences from 1884 until 1903. Miss Jones was governor of the University College of Wales at Aberystwyth, member of the Aristotelian Society, and of the Society for Psychical Research. Among her other publications are *A Primer of Ethics* (1909) and *A New Law of Thought and its Logical Bearings* (1911).

Prof. Schroeder, in the preface to his important work on 'Die Algebra der Logik,' the first volume of which has recently appeared, says that the chief advance which has been made in late years in exact Logic is due to the labors of the American, Charles S. Peirce, and his school. The inmost secret of this advance, the luminous guiding principle to which it is due, is the fact that attention is concentrated upon thought-relations, and not upon the *words* in which they may happen to be expressed. The meaning of this may be made clear by an example. The older logicians said that in every proposition the copula is *is* (or *are*), and that it can be nothing else. The newer school looks upon this series of affirmations—

All men are mortal,
Every man is mortal,
Any man is mortal,
Being a man implies being mortal,
If any one is a man, he is mortal,

That one is a man implies that he is mortal—as indicative one and all of the same state of things, as expressive one and all of the same kind of relation, and hence as properly subject one and all to exactly the same formal treatment. In other words, it is concerned, to use again the language of Prof. Schroeder, with the *canon* of logical thought, and not with an analysis of the psychological processes of actual thinking. The above unification alone, for instance, makes it possible to do away with the distinction between categorical and hypothetical propositions, and also with the distinction between the application and the signification (or extent and intent) of words; in any proposition the terms may be taken in either sense at pleasure without necessitating the slightest change in the *formal* method of procedure.

The last four of the above affirmations do not contain any very strong implication that there are any such things as men; hence, for the sake of unity, it is desirable to assume that the statement "All *a* is *b*" may still be taken as true when it is not known whether there are any *a*'s or not. When it is said that there may not be any *a*'s, it is not meant that the term *a* is logically inconceivable, but that it is perhaps not contained in an (understood) limited field of thought (what De Morgan has called the universe of discourse). How large the field of thought is at any moment may be gathered from the application which we attribute to our

negative terms; it, in denying that a thing is a virtue, we intend to call it a vice, then our universe is moral qualities; if it may perhaps be an intuition, then our universe is probably all mental qualities; if we take into account the possibility of its being a tadpole or a musical note, then our universe probably is the whole real world.

The connected questions of the existence of terms and of a limited universe are hence intimately connected with a marked simplification of logical procedure, and are therefore of more importance than it would seem at first sight. Recent English writers on logic are in the habit of discussing them from a narrower point of view; and in the handsome volume which Miss Constance Jones has just given to the logical world she does not rise above this narrow point of view. She says, for instance, on the question of existence: It seems to me, in making the assertion, "All albinos have pink eyes," not only that one would not be naturally conscious of a doubt as to there being any albinos living at the present moment, but also that the presence of the doubt in the mind is not even apparent on reflection. This sentence betrays a twofold misapprehension of the position of her opponents on the part of Miss Jones. In the first place, it does not follow, from saying that universal propositions do not, by their form, *necessarily* imply the existence of the subject, that one must be in actual doubt of its existence in every particular case. In the second place, Miss Jones forgets that her opponents have a ready means of expressing the fact when it is known that the subject exists—they have merely to say that it exists. Their position is simply this: They ask that when they say, *e.g.*, "Who breaks, pays; and there are some who break," they shall not be considered to have said over again in the second part of the sentence what they had already said once in the first; and they ask this for the weighty reason, among others, that it enables them to assimilate the treatment of compound propositions to that of simple ones.

Miss Jones has very acute reasoning powers, a great deal of boldness and originality, and untiring patience in tracking out minute distinctions in terms and in propositions. It is a pity that she has not taken a less mechanical, a larger and more common-sense, view of a number of debatable questions. She makes, for instance, too much of the distinction between adjectives and nouns. All names are abstractions. The difference between adjectives and nouns, as far as logic is concerned, is simply that adjectives are more abstract than nouns, and that on account of their having hardly any attributes predicable of them, they have little occasion to stand as subjects of propositions. Miss Jones is in error in saying that Mill distinguishes between attributes and subjects of attributes. Mill says plainly that Logic, at least, has no concern to postulate any substratum for attributes to be attached to; that, for Logic, attributes are not only all we know, but all we need to know. It is true that language is not sufficiently elastic to enable him always to speak strictly in the terms of this theory; but when he uses the word *thing*, he means nothing different from a congeries of attributes. Substance-names are constantly being coined out of adjectives when demand arises; as in "The outs were in ill-humor," "Blue and green are cold colors."

Nor does Miss Jones make out a good case against Mill's view of the nature of induction. The difficulties which she feels have been well set forth and met by Venn in his recent book on 'Empirical Logic.' They are difficulties of a kind not altogether dissimilar to that of the old Greek quibble—that a thing cannot move where it is, and cannot move where it is not, and hence that it cannot move at all.

Although Miss Jones seems to us not to have made her case good in a great many of the questions which she discusses, her book is nevertheless a noteworthy contribution to Deductive Logic.

51 (25 September 1890) 254-255

Locke.

By Alexander Campbell Fraser. [Philosophical Classics for English Readers.] Edinburgh: Wm. Blackwood & Sons; Philadelphia: J. B. Lippincott Co. 1890.

CSP, identification: MS 1365; Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*.

Alexander Campbell Fraser (1819-1914) was an English philosopher and clergyman. He was educated at Edinburgh University, and was ordained to the Free Church ministry in 1844. From 1846 until 1856, he served as professor of logic and metaphysics in Edinburgh Free Church theological college, and from 1856 until 1896 held the same position at Edinburgh University. He was the Gifford Lecturer for the 1894-1896 term. He has been characterized as a stimulating teacher, whose philosophical standpoint was theism based on moral faith.

Mr. Galton's researches have set us to asking of every distinguished personality, what were the traits of his family; although in respect, not to Mr. Galton's eminent persons, but to the truly great—those men who, in their various directions of action, thought, and feeling, make such an impression of power that we cannot name from all history more than three hundred such—in respect to these men it has not been shown that talented families are more likely than dull families to produce them. The gifts of fortune, however, are of importance even to these. It is not true that they rise above other men as a man above a race of intelligent dogs. In the judgment of Palissy the potter (and what better witness could be asked?), the majority of geniuses are crushed under adverse circumstances. John Locke, whose biography by Berkeleyan Professor Fraser is at our hand, came of a family of small gentry, his mother being a tradesman's daughter. The family had shown good, but no distinguished ability, and no remarkable vitality. The philosopher, John, the eldest child of his parents, was born (1632) two years after their marriage; there was one other child five years later. John Locke himself never contemplated marriage.

He resembled not in the least a genius of the regulation pattern—a great beast, incapable of self-control, self-indulgent, not paying his debts, subject to hallucinations, half-mad, absent minded. He did not even, like the popular hero, attribute all that distinguished him to his mother's influence. He called her "pious and affectionate," but rarely mentioned her. On the other hand, he often spoke of his father with strong love, with respect for his character, and with admira-

tion for his "parts." That father gave him all his instruction up to the age of fourteen years; and since he alone of Locke's teachers escaped the bitter maledictions of his later life for their pedantry and "verbal learning," the father it doubtless was who first taught our philosopher to think for himself.

"I no sooner perceived myself in the world," says Locke, "but I found myself in a storm." When he was ten years old, the Civil War broke out, and the house was near Bristol, one of the centres of operations. His father at first joined the Parliamentary army, but returned within two years. Such events made food for reflection and doubtless suggested toleration.

At fourteen he was put to Westminster school, under stern Dr. Busby, whose pedantry he detested; at twenty sent to peripatetic Oxford, and was still thoroughly discontented. He had not been a precocious boy, and was quite unconscious of superior power. At first he only read romances, and probably never studied very hard. He was awakened by the books of Descartes, whose system he did not embrace, but whose lucidity encouraged him to believe himself not a fool. "This same John Locke," says Anthony à Wood, "was a man of turbulent spirit, clamorous and discontented; while the rest of our club took notes deferentially from the mouth of the master, the said Locke scorned to do so, but was ever prating and troublesome." But this is the distortion of hatred, such as that which later prompted the lie that caused Charles II. to order Locke's expulsion from his studentship. The envious tribe said to infest colleges must take everlasting comfort in the reflection that efforts like theirs expelled John Locke from Oxford, and almost stifled the 'Essay concerning Human Understanding.'

Two years before the Restoration, he took his master's degree, and was afterwards appointed to that life studentship, to lectureships in Greek and rhetoric, and to a censorship in moral philosophy. At a later date, he took the degree of Bachelor in Medicine. His father and brother died in 1661, leaving him about half enough to live upon. In 1666, being thirty-four years old, he made the acquaintance of Lord Ashley, afterwards Earl of Shaftesbury, grandfather of the author of the 'Characteristics.' This nobleman took up Locke and formed him into a man of business, a man of the world, and a politician, fit to become, as he did become, the philosophical champion of the Glorious Revolution.

Locke falsifies the maxim that he who has done nothing great at twenty-seven years of age never will. His first publication (barring a few early verses) at double that age consisted of two anonymous articles in an encyclopædia. He never learned to write a good style. His great 'Essay' appeared three years later, May, 1689, though he had been at work upon it for nearly twenty years. He only lived fifteen years more, during which he was much engaged in public business, so that the time of his active authorship was brief.

Locke's was a frail and diminutive figure, with sloping shoulders, a gracefully set head, a forehead appearing low because cut off below by strong eyebrows rising to an angle over a nose long, pointed, and high-ridged. His eyes were prominent, his mouth well-formed, his chin strong. He must have resembled a little the late E. H. Palmer. His health was always delicate; he was a great sufferer from asthma.

That great observer, Sydenham, many years before Locke became famous, wrote of him as "a man whom, in the acuteness of his judgment and in the simplicity—that is, the excellence—of his manners, I confidently declare to have amongst the men of our own time few equals and no superiors." That Locke's *manners* should have made so powerful an impression upon Sydenham bespeaks magnetism if not greatness. A fascinating companion, gay, witty, observant, shrewd, thoroughly in earnest in his convictions, he added to his good fellowship the air of meaning to get himself all the happiness out of life he could, and to impart it to those about him. He maintained he had the sanction of Scripture in living for enjoyment, and the great pleasures he pursued were, he tells us, these five: health, reputation, knowledge, the luxury of doing good to others, and the hope of heaven. Few men have had so many warm friends; and to these friends he was devoted with a passion strong as a lover's.

At the same time he was no mean diplomatist, knew well enough how to play upon weaknesses, and no one more that he possessed the art of turning men inside out. Many little maxims on this head are scattered through his writings. He himself was impenetrable. "I believe there is not in the world," said one who had tried a lance with him, "such a master of taciturnity and passion." He confesses himself to be choleric, though soon appeased; but, in fact, self-control is the characteristic mark of his thoroughly well-regulated life. His personal economy was strict. He was methodical in business to a fault. His prudence was carried to the point of excessive caution. He was moderate in everything, and probability was his guiding star. He was deeply religious; but it was public spirit, the benevolent wish to improve the condition of his country and the world, which was the mainspring of his life and inspired all he wrote.

Hence, the vast influence which Locke's philosophy exerted upon the development of Europe for more than a century. If it be said that in truth no such force was exerted, but that Locke only happened to be the mouthpiece of the ideas which were destined to govern the world, can there after all be anything greater than so to anticipate the vital thought of the coming age as to be mistaken for its master? Locke's grand word was substantially this: "Men must think for themselves, and genuine thought is an act of perception. Men must see out of their own eyes, and it will not do to smother individual thought—the only thought there really is—beneath the weight of general propositions, laid down as innate and infallible, but really only traditional—oppressive and unwholesome heritages from a barbarous and stupid past." When we think of the manner in which the Cartesians, Spinoza, and the others had been squeezing out the quintessence of blindness from "First Principles," and consider to what that method was capable of lending itself, in religion and in politics, we cannot fail to acknowledge a superior element of truth in the practicality of Locke's thought, which on the whole should place him nearly upon a level with Descartes.

Prof. Fraser's is the fourth life of Locke drawn more or less from unprinted sources. It cannot be said to be a sympathetic account of him. The biographer seems to see no charm in his hero, and is perpetually speaking of his want of imagination; which only means he was not given to unpractical dreaming. The

account of Locke's writings is, however, unusually good; and the insufferable sophistry of T. H. Green is well disposed of in a paragraph. Prof. Fraser pleads for a new edition of Locke's works, and it is very true that this great man, whose utterances still have their lessons for the world, with wholesome influences for all plastic minds, should be studied in a complete, correct, and critical edition.

51 (23 October 1890) 326

NOTES

Attributed to Peirce by Fisch in his *Third Supplement* (internal evidence). This note is unassigned in Haskell's *Index to* The Nation, vol. 1.

—Many minds nowadays are turning towards high philosophy with expectations such as wide-awake men have not indulged during fifty years of Hamiltonianism, Millism, and Spencerianism; so that the establishment of a new philosophical quarterly which may prove a focus for all the agitation of thought that struggles to-day to illuminate the deepest problems with light from modern science, is an event worthy of particular notice. The first number of the *Monist* (*Open Court* Publishing Company) opens with good promise, in articles by two Americans, one Englishman, three Germans, two Frenchmen. Mr. A. Binet, student of infusorial psychology, treats of the alleged physical immortality of some of these organisms. In the opening paper, Dr. Romanes defends against Wallace his segregation supplement to the Darwinian theory, *i.e.*, that the divergence of forms is aided by varieties becoming incapable of crossing, as, for instance, by blossoming at different seasons. Prof. Cope, who, if he sometimes abandons the English language for the jargon of biology, is always distinguished by a clear style, ever at his command in impersonal matters, gives an analysis of marriage, not particularly original, and introduces a slight apology for his former recommendation of temporary unions. Prof. Ernst Mach has an "anti-metaphysical" article characteristic of the class of ingenious psychologists, if not perhaps quite accurate thinkers, to which he belongs. Mr. Max Dessoir recounts exceedingly interesting things about magic mirrors considered as hypnotizing apparatus. Mr. W. M. Salter and M. Lucien Arreat tell us something of the psychology of Höffding and of Fouillée. Among the book-notices, a certain salad of Hegel and mathematics excites our curiosity and provokes an appetite for more of this sort. The writer makes much ado to state Dr. F. E. Abbot's metaphysics, certainly as easily intelligible a theory as ever was.

—It remains to explain the name *Monist*. Dr. Carus, the putative editor, says: "The philosophy of the future will be a philosophy of facts, it will be *positivism*; and in so far as a unitary systematization of facts is the aim and ideal of all science, it will be *Monism*." But this is no definition of monism at all; in fact, the last clause conveys no idea. The search for a unitary conception of the world, or for a unitary systematization of science, would be a good definition of *philosophy*; and, with this good old word at hand, we want no other. To use the word *monism* in this sense would be in flagrant violation at once of usage and of the accepted

principles of philosophical terminology. But this is not what is meant. Monism, as Dr. Carus himself explains it in his 'Fundamental Problems,' p. 256, is a metaphysical theory opposed to dualism or the theory of two kinds of substance—mind and matter—and also conceiving itself to be different both from idealism and materialism. But idealism and materialism are almost identical: the only difference is that idealism regards the psychical mode of activity as the fundamental and universal one, of which the physical mode is a specialization; while materialism regards the laws of physics as at the bottom of everything, and feeling as limited to special organizations. The metaphysicians who call themselves Monists are usually materialists *sans le savoir.* The true meaning attaching to the title of the magazine may be read in these words of the editor:

"We are driven to the conclusion that the world of feelings forms an inseparable whole together with a special combination of certain facts of the objective world, namely, our body. It originates with this combination, and disappears as soon as that combination breaks to pieces. . . . Subjectivity must be conceived as the product of a coöperation of certain elements which are present in the objective world. . . . Motions are not transformed into feelings, but certain motions, . . . when coöperating in a special form, are accompanied with feelings."

51 (30 October 1890) 349

Our Dictionaries, and Other English-Language Topics.
By R. O. Williams. Henry Holt & Co. 1890.

CSP, identification: MS 1365. See also: Burks, *Bibliography.* This notice is unassigned in Haskell's *Index to* The Nation, vol. 1.

This little book is mainly taken up with notes upon the use of a few words. The hasty dictum of Dr. E. A. Freeman, that the non-ecclesiastical use of *metropolis* is "slang," is easily and amply refuted. Mr. Williams well says that "for more than two hundred years the secular meaning has been the prominent one," and the only reason for not extending the statement is that Elizabethan secular writers were not fond of the Greek forms. They often alluded to London as the "mother towne" of England.

The account of "our dictionaries" could not well be flimsier; but a discriminating guide to books of reference, useful as it would be, can hardly be looked for from American publishers. "The examples collected by Johnson," says Mr. Williams, "have formed the main stock of the citations used by subsequent dictionary-makers." This, of course, does not apply to Richardson, to say nothing of Murray. The 'Century Dictionary' has as many quotations as Johnson and Richardson together. It is no wonder that the fraction of the population which has not been engaged in the production of this world of words, has included every person capable of supervising the quotations in a really masterly way; for there was no possibility of competing with Murray and his 1,300 readers. Still, most of the 'Century' citations are judicious and unexceptionable; and if the treatment of them is less severely scientific, it is more agreeable than that of the Philological Society's vast collection.

In the first ten pages of part xvi. of the 'Century' (the latest to hand), we count 260 quotations, fewer than in the earlier parts of the work, which seems to be overrunning its limits. Quotations under *pilfer* from Dryden and Bacon, under *pilgrim* from Grew's 'Anatomy of Plants,' and under *pilotage* from Raleigh, have been taken from Johnson, apparently without verification, and quotations under *pillery* from Daniel and under *pimping* from Crabbe have been similarly drawn from the 'Imperial.' An abridged quotation and wrong definition, under *pinax*, come from Webster. We may state here that a few references appear to be either erroneous, misleading, or insufficient. Under *pile*, in the electrical sense, it might have been well to quote from Volta's own description, which was originally published in English.

1891

52 (12 February 1891) 139

NOTES

CSP, identification: MS L 159.4. See also Fisch, *First Supplement.* Fisch suggested that only a part of this might be attributable to Peirce. This is unassigned in Haskell's *Index to* The Nation, vol. 1.

—Mr. George Shea has printed a pamphlet with the title, 'Some Facts and Probabilities relating to the History of Johannes Scotus, surnamed Duns, and concerning the genuineness of the Spagnoletto Portrait belonging to the General Theological Seminary of the United States' (Cambridge: Riverside Press). Three other portraits of Duns Scotus, he says, are known, one at Windsor, one in the Bodleian, and one at Merton, and these are all admitted to be copies. The New York picture came from the shop of Mr. John Chaundy in Oxford; Mr. Chaundy had it from a gentleman who "understood that it had been brought into England from the south of France," and this gentleman's family believed it to be the original Spagnoletto. This, it must be confessed, is a somewhat indefinite pedigree. Mr. Shea adds that "the painting is recognized by connoisseurs as a genuine Ribera." Here is the gist of the question. The genuineness of the portrait can be decided on only by experts. We cannot rest on the opinion of unknown "connoisseurs"; if some acknowledged Spagnoletto authority should examine the picture, his decision would carry weight, but for the present, it will be generally felt, opinion must be reserved. The figure of Scotus, as represented in the photograph, is striking, and it will be pleasant if it should prove to be an original Ribera. The sketch of the great schoolman's life in the pamphlet is not carefully done. The author says, for example (p. 17): "So rapid was his advance that in his first year at the University [of Paris] he was appointed Regent of its Theological School." But the title "regent" belonged to any Master of Arts who chose to teach; and though there was a theological "Faculty," and the Sorbonne was in existence in 1304 (when Duns went to Paris), it is doubtful whether there was "a Theological School," for colleges had already been established, and in all of them theological instruction was given. The statement (p. 15) that "upon a vacancy occurring by the removal to Paris of his master, William Varron (A.D. 1301), Scotus was appointed to the chair of Philosophy," has too modern a tone. There was then, properly speaking, no "chair of philosophy" at Merton College; any master might lecture on any or all of the subjects of the curriculum (in which the philosophy of the time was, of course, prominent), and had to trust to his ability to attract pupils. A similar looseness of expression occurs in Mr. W. J. Townsend's 'Great Schoolmen of the Middle Ages.' Why so much space should be given to Erigena, who had nothing to do with Duns, is not clear. Mr. Shea has, however, done well to call attention to the portrait, and it is to be hoped that the authorities of the Union Theological Seminary will submit it to a competent expert who may enlighten us on the question of its genuineness.

52 (19 February 1891) 160

NOTES

There is a strong chance that the editorial reply at 54 (11 February 1892) 110 is by Peirce. If that is the case, then this note on Cajori is probably by Peirce, since this book is mentioned in the later editorial comment. See also: Fisch's new supplement. This note is unassigned in Haskell's *Index to* The Nation, vol. 1.

Florian Cajori (1859-1930) was graduated from the University of Wisconsin in 1883, and from 1884 to 1885 studied mathematics and physics at The Johns Hopkins University. From 1898 until 1918, he held a chair in mathematics at Colorado College, and from 1903 to 1918 also was dean of the department of engineering at that school. From 1918 until 1929, he taught at the University of California where he held the post of professor of the history of mathematics, the first of its kind in America. He authored over 200 journal articles and a dozen textbooks. He was a member of the American Association for the Advancement of Science (of which he held the presidency, 1917-1918) and the American Mathematical Society.

—The Bureau of Education's Circular of Information, No. 3, 1890, is a bulky pamphlet on 'The Teaching and History of Mathematics in the United States,' by Prof. Florian Cajori of Colorado College. Three-quarters of the 400 pages are given to the history proper, full in facts and decidedly anecdotical, but sadly wanting an index. Some of the stories are rather personal. The following relates to Prof. J. J. Sylvester, who is referred to as "Silly":

"His manner of lecturing was highly rhetorical and elocutionary. When about to enunciate an important or remarkable statement, he would draw himself up till he stood on the very tips of his toes, and in deep tones thunder out his sentences. He preached at us at such times; and not infrequently he wound up by quoting a few lines of poetry to impress on us the importance of what he had been declaring. I remember distinctly an incident that occurred when he was at work on his Universal Algebra. He had jumped to a conclusion which he was unable to prove by logical deduction. He stated this fact to us in the lecture, and then went on: 'GENTLEMEN' [here he raised himself on his toes], 'I am *certain* that my conclusion is correct. I will WAGER a hundred pounds to *one*; yes, I will WAGER my *life* on it.' The capitals indicate when he rose on his toes, and the italics when he rocked back on to his heels. In such bursts as these he always held his hands tightly clenched and close to his side, while his elbows stuck out in the plane of his body, so that his bended arm made an angle of about 140°."

Following this historical matter are twenty-three questions concerning methods of teaching and the like answered in the briefest manner by professors of 168 colleges, with other decisions by teachers in normal schools and others by principals of high schools. All this part of the book is diffuse and ill-edited. Little or no discrimination has been exercised in selecting the institutions; and from many of the most important there are no replies. There are none from Harvard, Yale, Princeton, the University of Pennsylvania, Ann Arbor, Cornell, Clark University, or the University of Wisconsin, all of which seats of learning should have been visited.

—The arrangement of the answers is such as to cover a great deal of paper while affording the reader no facilities; the whole thought, apparently, having

been to save trouble to the compiler. As a fair sample of the value of these decisions, we may summarize those which sprawl over the half of three pages in response to the question, "Do scientific or classical students show the greater aptitude for mathematics?" The answers are:

Decidedly, the scientific, from	41	colleges.
Decidedly, the classical, "	28	"
Apparently, the scientific, "	7	"
Apparently, the classical, "	9	"
Sensibly equal, "	13	"
Doubtful or nearly equal, "	14	"

The more expanded statements could easily be put into half-a-dozen lines more. These replies prove nothing, unless proof be needed that most college professors know little of the aptitudes of their students. The last forty pages of the book are occupied with historical essays, germane to the subject, though of no great value. An appendix gives a useful bibliography of American treatises on the calculus, thirty-three in number.

52 (26 February 1891) 178

A CARICATURE

TO THE EDITOR OF THE NATION:

SIR: As one of Sylvester's pupils, I wish to express my regret that the *Nation* should have reproduced a passage so ill-calculated to give a correct impression either of his personality or of his influence, as that which was quoted in a note in your current issue. The intention of the writer may have been good enough, but no reader would gather, from what he says, that Sylvester's bursts of "rhetoric" were merely the overflow of that burning enthusiasm for his science which animated him constantly, which inspired his pupils (at least for the time) with something of the same ardor, and which enabled him, when past the age of seventy, to kindle a remarkable mathematical revival at Oxford upon his return to England. It is to be regretted that if any personal sketch was to be presented to readers who have not known Sylvester, it should have been one showing such bad taste, and preceded by the use of a silly nickname which, I believe—and for the credit of Johns Hopkins students' sense and breeding I trust that I am right—was never in use among the students at Baltimore. X.

FEBRUARY 22, 1891.

52 (12 March 1891) 217-218

THE TEACHING OF MATHEMATICS

"F. H. L." is identified by Haskell (*Index to* The Nation, vol. 1, p. 201) as being F. H. Loud. The editorial reply is attributed to Peirce by Fisch and Haskell in *Additions to Cohen's Bibliography*. If the review of Cajori's book—see 52 (19 February 1891) 160—was written by Peirce, then it is probable that the editorial remark following Cajori's letter is also by Peirce. This piece is unassigned in Haskell's *Index to* The Nation, vol. 1.

TO THE EDITOR OF THE NATION:

SIR: A quotation made in your issue of the 12th ult., in the course of a review of Prof. Florian Cajori's 'Teaching and History of Mathematics in the United States,' has, I see, called forth the objection of a correspondent that the passage gives an unfair impression of one of the most eminent of living mathematicians. Permit me to add that it seems to me equally misleading as a specimen of the contents of the book. The words quoted are not those of Prof. Cajori himself, and they occur in the course of a survey of Sylvester's work the whole spirit of which is the exact reverse of disrespect.

The history begins with the colonial period, and, while perhaps "anecdotical," certainly not tedious, in style, it gives evidence of much pains taken to secure accuracy. To all of this historical work—the main subject of the volume—the reviewer devotes but three lines, except as he treats the author's account of the last fifteen years, and this chiefly by making the above-mentioned strange selection. F. H. L.

CLARK UNIVERSITY, March 2, 1891.

TO THE EDITOR OF THE NATION:

SIR: Will you kindly allow a little space for a few remarks on the somewhat unjust criticism which appeared in the *Nation* of the 19th inst. on my work entitled 'The Teaching and History of Mathematics in the United States'? The reviewer places undue confidence in his own opinions when he asserts that the replies given by 168 teachers of mathematics in our leading colleges "prove nothing, unless proof be needed that most college professors know little of the aptitudes of their students." The reviewer finds fault because no replies to questions concerning methods of teaching were secured from Harvard, Yale, Princeton, the University of Pennsylvania, Ann Arbor, Cornell, Clark University, and the University of Wisconsin. Is it possible that he failed to see that the mathematical teaching at all these institutions but two was described at length in another place? From most of the eight institutions just named I had received letters with detailed accounts of their work in mathematics before the 1,000 letters with the printed questions above referred to were sent out. For that reason, most likely, these institutions did not consider it necessary to send in information a second time. The obtuseness of the reviewer is brilliantly displayed when he expects reports from Clark University at a time when it had not yet opened its doors to students.

Respectfully yours, FLORIAN CAJORI.

COLORADO COLLEGE, February 27, 1891.

[Complaints about book notices, when not made with a view to the advertisement, are mostly based on the idea that such a notice is mainly written in order to do justice to the author's merit. In fact, its purpose is to give the public such information about a book as it desires, and particularly to show in what way the book may be useful. While we would not deliberately do an author injustice, we cannot go into the question of "pains taken," except in those rare cases where the public desires to hear about that. When so distinguished an astronomer as

Sears C. Walker is called "Mr. C. Walker," when other names are misspelled, dates are erroneous, and the information generally defective, great pains may have been taken, but not pains enough. We repeated the nickname and anecdote concerning a great living algebrist, as being well calculated to convey to readers of the *Nation* a hint as to the degree of delicacy of Prof. Cajori's discrimination. "F. H. L." thinks these things "misleading as specimens" of the work; but in truth there is much which were better withheld while the subjects are living, such as: "Professor ____ was appointed . . . to supplement Professor ____'s shortcomings," "____ is a far more amiable and congenial person to meet than Professor ____," and the like, the names of the living persons being given. The excuse put forth by "F. H. L." that these things were communicated to Prof. Cajori in private letters, is an explanation that fails to explain.

Another correspondent, "X." (*Nation*, No. 1339), blames us for repeating the story. But in what age of the world, pray, are we living? It was already in print, it was quite true, and, after all, is merely a tale of a bit of eccentricity such as theoretical mathematicians and thinkers generally have been proverbial for since antiquity, and such as may be told of nearly every man living who has made important contributions to pure mathematics. There was a phase of American development (not yet, unfortunately, altogether past) when to say that a person was different from others was an accusation, to call him eccentric simply shocking. Whenever such a charge was made, those of the party's friends who were conscious of superior powers of mendacity, naturally hastened to repel the odious libel, and to assure the public of the maligned gentleman's eminent mediocrity. No wonder that in such an atmosphere mathematical studies have not flourished.

Prof. Cajori must not represent us as pooh-poohing the opinion of 168 teachers. No doubt, were judicious questions asked, their replies would be wellnigh conclusive. We merely said that replies pretty equally divided between "yes" and "no" proved nothing; adding only that, the question being as to the relative aptitudes of two classes of students for mathematics, answers very positive and yet irreconcilably conflicting do go to prove that most of the answerers know little of those aptitudes. From hardly any of the best schools of mathematics were replies to the questions received at all, nor is it true that there is anything in the book equivalent to such replies. The publication is 'Circular of Information, No. 3, 1890,' and therefore one naturally expects to find the opinions concerning methods of teaching held by the instructors at Clark University under the head of "The Mathematical Teaching of the Present Time." But there is nothing of the sort there concerning most of the chief seats of mathematical learning in this country. There are only some generalities under the title, "Influx of French Mathematics," which is surely a thing of the past. The detailed information concerning methods of teaching relates, with some exceptions, exclusively to second-rate institutions.—ED. NATION.]

53 (2 July 1891) 15

JAMES'S PSYCHOLOGY.—I

The Principles of Psychology.

By William James, Professor of Psychology in Harvard University. [American Science Series, Advanced Course.] Henry Holt & Co. 1890. 2 vols., 8vo, pp xii + 704.

CSP, identification: MS 1365; Haskell, *Index to* The Nation. See also: Burks, *Bibliography: List of Articles.* For biographical information on James see Ralph Barton Perry, *The Thought and Character of William James*, 2 vols. Boston: Little, Brown and Company, 1935.

Upon this vast work no definitive judgment can be passed for a long time; yet it is probably safe to say that it is the most important contribution that has been made to the subject for many years. Certainly it is one of the most weighty productions of American thought. The directness and sharpness with which we shall state some objections to it must be understood as a tribute of respect.

Beginning with the most external and insignificant characters, we cannot much admire it as a piece of bookmaking; for it misses the unity of an essay, and almost that of a connected series of essays, while not attaining the completeness of a thorough treatise. It is a large assortment of somewhat heterogeneous articles loosely tied up in one bag, with tendencies towards sprawling.

With an extraordinarily racy and forcible style, Prof. James is continually wresting words and phrases of exact import to unauthorized and unsuitable uses. He indulges himself with idiosyncrasies of diction and tricks of language such as usually spring up in households of great talent. To illustrate what we mean, we will open one of the volumes at random, and we come upon this: "A statement *ad hominem* meant as part of a reduction to the absurd." Now a *reductio ad absurdum* is a species of demonstration, and as such can contain no *argumentum ad hominem*, which is merely something a man is obliged by his personal interests to admit. On the next page, we read: "This dynamic (we had almost written dynamitic) way of representing knowledge." On the next page: "They talk as if, with this miraculous tying or 'relating,' the Ego's duties were done." It is the same with the technical terms of psychology. Speaking of certain theories, our author says they "carry us back to times when the soul as vehicle of consciousness was not discriminated, as it now is, from the vital principle presiding over the formation of the body." How can anybody write so who knows the technical meaning of *vehicle*? On the same page occurs this phrase, 'If unextended, it is absurd to speak of its having space relations at all," which sounds like a general attack on the geometry of points.

Prof. James's thought is highly original, or at least novel; but it is originality of the destructive kind. To prove that we do not know what it has been generally supposed that we did know, that given premises do not justify the conclusions which all other thinkers hold they do justify, is his peculiar function. For this reason the book should have been preceded by an introduction discussing the strange positions in logic upon which all its arguments turn. Even when new theories are proposed, they are based on similar negative or sceptical considera-

tions, and the one thing upon which Prof. James seems to pin his faith is the general incomprehensibility of things. He clings as passionately to that as the old lady of the anecdote did to her total depravity. Of course, he is materialistic to the core—that is to say, in a methodical sense, but not religiously, since he does not deny a separable soul nor a future life; for materialism is that form of philosophy which may safely be relied upon to leave the universe as incomprehensible as it finds it. It is possible that Prof. James would protest against this characterization of his cast of mind. Brought up under the guidance of an eloquent apostle of a form of Swedenborgianism, which is materialism driven deep and clinched on the inside, and educated to the materialistic profession, it can only be by great natural breadth of mind that he can know what materialism is, by having experienced some thoughts that are not materialistic. He inclines towards Cartesian dualism, which is of the true strain of the incomprehensibles and modern materialism's own mother. There is no form of idealism with which he will condescend to argue. Even evolutionism, which has idealistic affinities, seems to be held for suspect. It is his *métier* to subject to severe investigation any doctrine whatever which smells of intelligibility.

The keynote of this is struck in the preface, in these words:

"I have kept close to the point of view of natural science throughout the book. Every natural science assumes certain data uncritically, and declines to challenge the elements between which its own 'laws' obtain, and from which its deductions are carried on. Psychology, the science of finite individual minds, assumes as its data (1) *thoughts and feelings*, and (2) a *physical world* in time and space with which they coexist and which (3) *they know*. Of course these data themselves are discussable; but the discussion of them (as of other elements) is called metaphysics, and falls outside the province of this book. This book, assuming that thoughts and feelings exist, and are the vehicles of knowledge, thereupon contends that Psychology, when she has ascertained the empirical correlation of the various sorts of thought and feeling with definite conditions of the brain, can go no farther—can go no farther, that is, as a natural science. If she goes farther, she becomes metaphysical. All attempts to *explain* our phenomenally given thoughts as products of deeper-lying entities (whether the latter be named 'Soul,' 'Transcendental Ego,' 'Ideas,' or 'Elementary Units of Consciousness') are metaphysical. This book consequently rejects both the associationist and the spiritualist theories; and in this strictly positivistic point of view consists the only feature of it for which I feel tempted to claim originality."

This is certainly well put—considered as prestigiation. But when we remember that a natural science is not a person, and consequently does not "decline" to do anything, the argument evaporates. It is only the students of the science who can "decline," and they are not banded together to repress any species of inquiry. Each investigator does what in him lies; and declines to do a thousand things most pertinent to the subject. To call a branch of an inquiry "metaphysical" is merely a mode of objurgation, which signifies nothing but the author's personal distaste for that part of his subject. It does not in the least prove that considerations of that sort can throw no light on the questions he has to consider.

Indeed, we suspect it might be difficult to show in any way that any two branches of knowledge should be allowed to throw no light on one another. Far less can calling one question scientific and another metaphysical warrant Prof. James in "consequently *rejecting*" certain conclusions, against which he has nothing better to object. Nor is it in the least true that physicists confine themselves to such a "strictly positivistic point of view." Students of heat are not deterred by the impossibility of directly observing molecules from considering and accepting the kinetical theory; students of light do not brand speculations on the luminiferous ether as metaphysical; and the substantiality of matter itself is called in question in the vortex theory, which is nevertheless considered as perfectly germane to physics. All these are "attempts to explain phenomenally given elements as products of deeper-lying entities." In fact, this phrase describes, as well as loose language can, the general character of scientific hypotheses.

Remark, too, that it is not merely nor chiefly the "soul" and the "transcendental ego," for which incomprehensibles he has some tenderness, that Prof. James proposes to banish from psychology, but especially *ideas* which their adherents maintain are direct data of consciousness. In short, not only does he propose, by the simple expedient of declaring certain inquiries extra-psychological, to reverse the conclusions of the science upon many important points, but also by the same negative means to decide upon the character of its data. Indeed, when we come to examine the book, we find it is precisely this which is the main use the author makes of his new principle. The notion that the natural sciences accept their data *uncritically* we hold to be a serious mistake. It is true, scientific men do not subject their observations to the kind of criticism practised by the high-flying philosophers, because they do not believe that method of criticism sound. If they really believed in idealism, they would bring it to bear upon physics as much as possible. But in fact they find it a wordy doctrine, not susceptible of any scientific applications. When, however, a physicist has to investigate, say, such a subject as the scintillation of the stars, the first thing he does is to subject the phenomena to rigid criticism to find whether these phenomena are objective or subjective, whether they are in the light itself, or arise in the eye, or in original principles of mental action, or in idiosyncrasies of the imagination, etc. The principle of the uncritical acceptance of data, to which Prof. James clings, practically amounts to a claim to a new kind of liberty of thought, which would make a complete rupture with accepted methods of psychology and of science in general. The truth of this is seen in the chief application that has been made of the new method, in the author's theory of space-perception. And into the enterprise of thus revolutionizing scientific method he enters with a light heart, without any exhaustive scrutiny of his new logic in its generality, relying only on the resources of the moment. He distinctly discourages a separate study of the method. "No rules can be laid down in advance. Comparative observations, to be definite, must usually be made to test some preëxisting hypothesis; and the only thing then is to use as much sagacity as you possess, and to be as candid as you can."

53 (9 July 1891) 32-33

JAMES'S PSYCHOLOGY.—II

The Principles of Psychology.

By William James, Professor of Psychology in Harvard University. [American Science Series, Advanced Course.] Henry Holt & Co. 1890. 2 vols., 8vo, pp. xii + 689, and vi + 704.

We have no space for any analysis of the contents of this work, nor is that necessary, for everybody interested in the subject must and will read the book. It discusses most of the topics of psychology in an extremely unequal way, but always interesting and always entertaining. We will endeavor to give a fair specimen of the author's critical method (for the work is essentially a criticism and exposition of critical principles), with a running commentary, to aid a judgment. For this purpose we will select a short section entitled "Is Perception Unconscious Inference?" Perception in its most characteristic features is, of course, a matter of association in a wide sense of that term. If two spots of light are thrown upon the wall of a dark room so as to be adjacent, and one of these is made red while the other remains white, the white one will appear greenish by contrast. If they are viewed through a narrow tube, and this is moved so that the red spot goes out of view, still the white one will continue to look green. But if the red light, now unseen, be extinguished and we then remove the tube from the eye, so as to take a new look, as it were, the apparent greenness will suddenly vanish. This is an example of a thousand phenomena which have led several German psychologists to declare that the process of perception is one of reasoning in a generalized sense of that term.

It is possible some of the earlier writers held it to be reasoning, strictly speaking. But most have called it "unconscious inference," and unconscious inference differs essentially from inference in the narrow sense, all our control over which depends upon this, that it involves a conscious, though it may be an indistinct, reference to a genus of arguments. These German writers must also not be understood as meaning that the perceptive process is any more inferential than are the rest of the processes which the English have so long explained by association—a theory which until quite recently played little part in German psychology. The German writers alluded to explain an ordinary suggestion productive of belief, or any cognition tantamount to belief, as inference conscious or unconscious, as a matter of course. As German writers are generally weak in their formal logic, they would be apt to formulate the inference wrongly; but the correct formulation is as follows:

A well-recognized kind of object, M, has for its ordinary predicates P_1, P_2, P_3, etc., indistinctly recognized.

The suggesting object, S, has these same predicates, P_1, P_2, P_3, etc.

Hence, S is of the kind M.

This is hypothetic inference in form. The first premise is not actually thought, though it is in the mind habitually. This, of itself, would not make the inference unconscious. But it is so because it is not recognized as an inference;

the conclusion is accepted without our knowing how. In perception, the conclusion has the peculiarity of not being abstractly thought, but actually seen, so that it is not exactly a judgment, though it is tantamount to one. The advantage of this method of explaining the process is conceived to be this: To explain any process not understood is simply to show that it is a special case of a wider description of process which is more intelligible. Now nothing is so intelligible as the reasoning process. This is shown by the fact that all explanation assimilates the process to be explained to reasoning. Hence, the logical method of explaining the process of association is looked upon as the most perfect explanation possible. It certainly does not exclude the materialistic English explanation by a property of the nerves. The monist school, to which the modern psychologists mostly belong, conceives the intellectual process of inference and the process of mechanical causation to be only the inside and outside views of the same process. But the idealistic tendency, which tinctures almost all German thought not very recent, would be to regard the logical explanation as the more perfect, under the assumption that the materialistic explanation requires itself ultimately to be explained in terms of the reasoning process. But Prof. James is naturally averse to the logical explanation. Let us see, then, how he argues the point. His first remark is as follows:

"If every time a present sign suggests an absent reality to our mind, we make an inference; and if every time we make an inference, we reason, then perception is indubitably reasoning."

Of course, every psychological suggestion is regarded as of the general nature of inference, but only in a far more general sense than that in which perception is so called. This should be well known to Prof. James, and he would have dealt more satisfactorily with his readers if he had not kept it back. Namely, perception attains a virtual judgment, it subsumes something under a class, and not only so, but virtually attaches to the proposition the seal of assent—two strong resemblances to inference which are wanting in ordinary suggestions. However, Prof. James admits that the process *is* inference in a broad sense. What, then, has he to object to the theory under consideration?

"Only one sees no room in it for any unconscious part. Both associates, the present sign and the contiguous things which it suggests, are above board, and no intermediary ideas are required."

Here are two errors. In the first place, "unconscious inference" does not, either with other logicians or with the advocates of the theory in question, mean an inference in which any proposition or term of the argument is unconscious, any more than "conscious inference" implies that both premises are conscious. But unconscious inference means inference in which the reasoner is not conscious of making an inference. He may be conscious of the premise, but he is not conscious that his acceptance of the conclusion is inferential. He does not make that side-thought which enters into all inference strictly so called: "and so it would be in every analogous case (or in most cases)." There is no doubt, therefore, that ordinary suggestion, regarded as inference, is of the unconscious variety. But

Prof. James further forgets his logic in hinting, what he soon expresses more clearly, that such an inference is to be regarded as a mere "immediate inference," because it has no middle term. We might suppose he had never heard of the *modus ponens*, the form of which, A and B being any proposition, is

If A, then B;
But A:
Hence, B.

Those who think a light is thrown upon the ordinary process of suggestion by assimilating it to reasoning, assimilate it to the *modus ponens*. The proposition "If A, then B," is represented by the association itself, which is not present to consciousness, but exists in the mind in the form of a habit, as all beliefs and general propositions do. The second premise A is the suggesting idea, the conclusion B is the suggested idea.

Already quite off the track, our author now plunges into the jungle in this fashion:

"Most of those who have upheld the thesis in question have, however, made a more complex supposition. What they have meant is that perception is a *mediate* inference, and that the middle term is unconscious. When the sensation which I have called 'this' is felt, they think that some process like the following runs through the mind:

'This' is M;
but M is A;
therefore 'this' is A."

Those who have upheld the thesis are not in dispute among themselves, as represented. They make no supposition throughout not admitted by all the world. To represent any process of inference now as a *modus ponens*, now as a syllogism with a middle term, is not necessarily taking antagonistic views. As for the syllogism given, it is the weakest mode of supporting the thesis, far more open to attack than the form first given above. But Prof. James makes no headway, even against this. He says:

"Now there seem no good grounds for supposing this additional wheelwork in the mind. The classification of '*this*' as M is itself an act of perception, and should, if all perception were inference, require a still earlier syllogism for its performance, and so backwards *ad infinitum*."

Not one of the authors whom we have consulted makes the M entirely unconscious; but Prof. James says they do. If so, when he insists that "this is M" is an act of perception, he must mean some ultra-Leibnitzian *unconscious* perception! Has he ever found the German authors maintaining that that kind of perception is inferential? If not, where is his *regressus ad infinitum*? What those authors do say is that M, and with it the two premises, are thrown into the background and shade of consciousness; that "this is M" is a perception, sometimes in the strict sense, sometimes only in that sense in which perception embraces every sensation. They do not hold sensation to be inferential, and consequently do not suppose a *regressus ad infinitum*. But even if they did, there would be no *reductio*

ad absurdum, since it is well known to mathematicians that any finite interval contains an infinite number of finite intervals; so that supposing there is no finite limit to the shortness of time required for an intellectual process, an infinite number of them, each occupying a finite time, may be crowded into any time, however short.

The Professor concludes:

"So far, then, from perception being a species of reasoning, properly so called, both it and reasoning are coördinate varieties of that deeper sort of process known psychologically as the association of ideas, and—"

We break the sentence, which goes on to something else, in order to remark that "a species of reasoning properly so called" must be a slip of the pen. For otherwise there would be an *ignoratio elenchi*; nobody ever having claimed that perception is inference in the strict sense of conscious inference. Instead of "a species of reasoning properly so called," we must read "reasoning in a generalized sense." Remembering also that Prof. James began by insisting on extending the controversy to association in general, we may put association in place of perception, and thus the conclusion will be, "so far from association being reasoning in a generalized sense, reasoning is a special kind of association." Who does not see that to say that perception and reasoning are coördinate varieties of association, is to say something in entire harmony with the thesis which Prof. James is endeavoring to combat? To resume:

"—physiologically as the law of habit in the brain. To call perception unconscious reasoning is thus either a useless metaphor or a positively misleading confusion between two different things."

Here the section ends, and in these last words, for the first time in the whole discussion, the real question at issue is at length touched, and it is dismissed with an *ipse dixit*. There is no room for doubt that perception and, more generally, associative suggestion, may truthfully be considered as inference in a generalized sense; the only question is whether there is any use in so considering them. Had Prof. James succeeded in establishing his *regressus ad infinitum*, he would have refuted himself effectually, since it would then have been shown that an important consequence, not otherwise known, had been drawn from the theory. As it is, he says nothing pertinent either pro or con. But a little before, when an unconscious predication was called perception, was this perception "properly so called"? And if not, was calling it by that name a "useless metaphor," or was it a "positively misleading confusion between two different things"?

53 (13 August 1891) 129

Vorlesungen über die Algebra der Logik.

Von Dr. Ernst Schröder. Leipzig: Teubner. 1890. Vol. 1, Pp. 717.

This review of Schröder's first volume is unassigned in Haskell's *Index to* The Nation, vol. 1. This leaves open the possibility that it is a review by Peirce, based on certain internal signs such as the reference to Peirce's work and that of O. H. Mitchell.

Ernst Schröder (1841-1902) was a German mathematician and logician. As a young man, Schröder studied physics and chemistry with such famous men as Bunsen, Kirchhoff, and Hesse. From 1870 until 1874, he held the post of professor of mathematics and natural sciences at the Pro- und Realgymnasium at Baden-Baden. For the two years following 1874, he taught mathematics at the Technische Hochschule at Darmstadt, from which he moved in 1876 to the Technische Hochschule at Karlsruhe, his final academic post.

The Algebra of Logic has here received an admirable setting forth at the hands of Dr. Schröder. The book is doubtless too large and too diffuse, but it is chiefly intended for a German audience (the subject has been hitherto neglected in that country), and Germans are not frightened away by voluminous reading. The doctrine is almost uniformly sound, and, what is of chief consequence, the arguments in favor of admitting the subject among the branches of human learning are well calculated to convey conviction. The arguments which have been advanced on the other side have sometimes been of a very curious nature. For instance, Mr. Bradley, in his 'Principles of Logic,' scouts it because it does nothing for reasoning that is not syllogistic—for example, for such reasoning as this: A is north of B, E = C, therefore A is north of C. In the first place, it is not true that reasoning of this kind is not included in an Algebra of Logic. The formal definition of the primary copula is simply that it is transitive—that is, that it is subject to the single condition that when A stands in a certain relation to B, and B stands in that same relation (or a limiting case of it) to C, then A stands in that same relation to C. Any relation whatever which fulfills this condition is already included in the Algebra of the primary copula—the copula, that is, which represents, in the first instance, the words "all . . . are . . ."

But in the second place, even if the Algebra of Logic covered syllogism only, no one could doubt its value who had tried to perform without it the extremely complicated pieces of syllogistic reasoning which it can work out by purely mechanical processes. The fact that not many instances of reasoning of this sort can be got from real life shows nothing. As Dr. Schröder points out, it is not strange that this kind of reasoning was seldom attempted at a time when it was almost impossible of accomplishment. Mr. McColl has already made a useful application of the theory to the determination of the new limits of several integrals upon a change in the order of integration.

Dr. Schröder makes constant acknowledgment, in very graceful terms, to the work of Mr. Charles S. Peirce and his school. He rightly considers that Boole's contributions to the subject possess, at present, only an historical interest. He seems to us to attribute rather less value than is due to the method of Mr. O. H. Mitchell as described in the 'Studies in Logic by Members of the Johns Hopkins University.'

Dr. Schröder's book is the only one, in any language, in which the subject can be properly approached by one who takes it up for the first time. We learn that a Spanish logician has undertaken a translation of it. For an English-speaking public, a somewhat different presentation of the subject would be preferable.

53 (8 October 1891) 283

Essays, Scientific, Political and Speculative.

By Herbert Spencer. Library Edition, containing seven essays not before republished, and various other additions. 3 vols., 8vo, pp. 478, 466, 516. With an alphabetical index. D. Appleton & Co. 1891.

CSP, identification: MS 1365. See also: Burks, *Bibliography*. This review is unassigned in Haskell's *Index to* The Nation, vol. 1.

Herbert Spencer (1820-1903) was an English socioligist and popularizer of the terms "evolution" and "survival of the fittest." He attempted in his writings to apply Darwinian theories to social development, but succeeded only in becoming one of the most controversial figures of his time.

The theory of ethics which has latterly been taking shape under the hands of Stephen, Spencer, and others, is from a practical point of view, one of the most important boons that philosophy has ever imparted to the world, since it supplies a worthy motive to conservative morals at a time when all is confused and endangered by the storm of new thought, the disintegration of creeds, and the failure of all evidences of an exalted future life.

The little of new which is contributed to the ethical theory in the present edition of Mr. Spencer's essays is contained in the essays on the "Ethics of Kant" and on "Absolute Political Ethics." It was hardly to be expected that the additions would go to enhance Mr. Spencer's well-built-up reputation. The popularity of his doctrine has probably passed its meridian. In one of the new essays, he quotes with admiration Huxley's fine saying. "Science commits suicide when it adopts a creed." That is just the principle of death lurking in Spencer's philosophy. It is a creed in that it is erected upon axioms founded only on the inconceivability of their contradictory opposites, and regarded as absolutely indubitable. One of the seven essays mentioned on the title-page refers to the discussion concerning the a-priori origin of axioms. Few psychologists, if any, would now dispute the instinctive origin of the ideas from which the three laws of motion have become evolved under the influence of experience and reflection. But it is a widely different thing to say that these laws are without doubt exactly true. For such a belief there cannot be the slightest warrant. In the same way, it may be true that all scientific reasoning postulates something which men seek to formulate as the general uniformity of nature; but it by no means follows that reasoning cannot discover that this postulate is not exactly true. That would be like insisting that because astronomy rests on observations, therefore the astronomer cannot deduce from these observations their probable error. Science or philosophy cannot itself commit suicide; but a method of inquiry which provides no means for the rectification of its first principles, has mixed and swallowed its own poison and has to expect an inevitable doom. What explains the success of modern science is that it has pursued a method which corrects its own premises and conclusions. It reminds us of certain methods of arithmetical computation where mistakes of ciphering have no effect but what disappears as the process goes on. In like manner philosophical inquiry, which necessarily begins in ignorance, must not pursue a method by which the error of its first assumptions is allowed to retain its full effect to the end, or else it will come to naught.

The most interesting of the new essays is that "On the Factors of Organic Evolution," in which the author urges almost irresistibly the indirect evidence of the transmission of acquired characters. As in the question of spontaneous generation, the direct evidence is feeble, if not quite wanting. But the force of general facts and indirect considerations would appear, at least to onlookers of the controversy, as sufficient to remove all doubt. Spencer well says that many of the modern evolutionists are more Darwinian than Darwin ever was; yet in part the reverse is true. The intellectual motive which has prompted evolutionary speculation in biology is the desire to discover the laws which determine the succession of generations. This involves in some sense a "postulate" that the phenomena are subject to law; but to jump to the assumption made by neo-Darwinians that the form of each individual is a mathematical resultant of the forms of its ancestors, is not to be more Darwinian than Darwin, but, on the contrary, it is seriously to maim his theory.

Spencer cites the old dogma that Nature abhors a vacuum as an example of a merely verbal explanation. A reader of Boyle's attack upon the maxim, made while it was a living belief, would hardly so judge it, since Nature was conceived as a sort of living being mediating between the Creator and the universe. Yet, as Nature's abhorrence of a vacuum remained somewhat unreasonable, Spencer is right in saying that the theory gave little help towards understanding the facts. But what, then, shall we say of a theory which proposes to explain all growth and its inexhaustible manifold of results by the law of the conservation of energy—that is, by a mere uniformity in the motion or matter, a mere general description of certain phenomena? To suppose an intelligence, provided only we can see its acts intelligently, is to suppose that which is intelligible *par excellence*. But to suppose that blind matter is subject to a primordial law, with nothing but an Unknowable beyond, would seem to leave everything as incomprehensible as well could be, and so fail completely to fulfil the function of a hypothesis.

Besides, the law of *vis viva* is plainly violated in the phenomena of growth, since this is not a reversible process. To explain such actions—of which viscosity and friction are examples—physicists resort to the consideration of the chance encounters between trillions of molecules, and it is an admirable scientific feature of the Darwinian hypothesis that, in order to account for a similar irreversible operation, that of growth, it equally resorts to the doctrine of chances in its fortuitous variations. The attempt of some of Darwin's followers to drop this feature of the theory is unscientific. It is also destructive of the theory, for if any laws of heredity are followed with mathematical exactitude, it becomes at once evident that the species of animals and plants cannot have arisen in anything like the manner in which Darwin supposed them to arise.

Another interesting part of this essay is where the author draws attention to the strong evidence of an enormous direct effect upon animal and vegetable forms due to the circumambient element. Such considerations strengthen Mr. Clarence King's suggestion that transmutations of species have chiefly been caused by geological changes of almost cataclysmic magnitude and suddenness, affecting the chemical constitution of the atmosphere and ocean.

In the essay, or prepared "interview," on "The Americans," Spencer holds, it will be remembered, that we carry the gospel of work too far.

53 (15 October 1891) 302

Geodesy.

By J. Howard Gore, Professor of Mathematics in Columbian University. [The Riverside Science Series.] Boston: Houghton, Mifflin & Co. 1891.

CSP. identification: MS 1365. See also: Burks, *Bibliography*. This note is unassigned in Haskell's *Index to* The Nation, vol. 1.

James Howard Gore (1856-1939) was a noted authority on geodetics and mathematics. He served as commissioner-general to the international expositions at Antwerp, Amsterdam, and Brussels. He was president of the Philosophical Society of Washington, and secretary of the American Meteorological Society. He was the author of three books on geodesy and a series of mathematics text books.

Of Prof. Gore's competence to treat of ancient geodesy, it is sufficient to say that he makes Sanskrit the scientific language of Chaldæa. But he is well informed in regard to the modern history of higher geodesy, and writes his own language with unusual grace and ease. A less promising subject for popularization than that which he has chosen could not be conceived; but in a space equal to ninety pages of *Harper's Magazine* he has contrived to sketch its history in a manner which will carry along any reader with a taste for questions of precision. He does scant justice to our Coast and Geodetic Survey, and to the manner in which it has been supported by our Congress. No man of sense or of conscience in the position of Bache, Peirce, Patterson, or Hilgard, could have asked the Government to measure an arc of the meridian from Canada to the Gulf. As much as it was right to ask was asked for and accorded; and the works of these geodesists will, when completed, constitute a great contribution to our knowledge of the figure of the earth. It is a problem which was steadily pursued by them, as it is by the present head of the Survey.

53 (22 October 1891) 313-314

THE LAW OF "VIS VIVA"

The reply to Hoskins' letter is surely by Peirce, since the review of Spencer was by Peirce. See also: Fisch, *First Supplement*. This reply is unassigned in Haskell's *Index to* The Nation, vol. 1.

Leander Miller Hoskins (1860-1937) was graduated from the University of Wisconsin in 1883, where he continued as assistant professor of mechanics and mathematics. In 1892, he began teaching applied mathematics at Stanford and held this chair until he retired with the title Professor Emeritus in 1925.

TO THE EDITOR OF THE NATION:

SIR: In your review of Herbert Spencer's 'Essays: Scientific, Political, and Speculative,' occurs the following sentence:

"Besides, the law of *vis viva* is plainly violated in the phenomena of growth, since this is not a reversible process."

The words "law of *vis viva*" seem from the context to be used as synonymous with "law of the conservation of energy." Does your reviewer really mean to assert that in the phenomena of growth we are presented with a *plain* violation of

the law of the conservation of energy? Such an assertion would be so astonishing that I cannot refrain from asking for further explanation. L. M. HOSKINS.

MADISON, WIS., October 12, 1891.

[It ought not to be necessary to remind a professor of mechanics in a reputable university that the law of *vis viva* was familiar to mathematicians for much more than a century before the law of the conservation of energy was heard of. The one is a principle of molar mechanics, the other of general physics. The kinetical theory of matter, which is intimately associated with, but is not involved in, the law of the conservation of energy, supposes that when the motions of molecules are taken account of, the law of *vis viva* is not violated in the action of viscosity, etc., where, considered as relating to *molar* motions, it is violated. As we referred to this, there is little excuse for saying that our context seems to confuse the two propositions. But since our correspondent is astonished at our saying that growth is an irreversible process, and therefore plainly violates the law of *vis viva*, and since, as professor of mechanics, he is familiar with the theorem that every action under a conservative system of forces is reversible, it appears that he would say that growth (including reproduction and the evolution of new species) is a reversible process in the sense in which the actions of viscosity, etc., are not reversible.

We said nothing about the law of the conservation of energy, which is the grandest discovery of science. Still, as a scientific generalization, it can only be a probable approximate statement, open to future possible correction. In its application to the ordinary transformations of forces, it has been pretty exactly verified. But as to what takes place within organized bodies, the positive evidence is unsatisfactory, and, in connection with the question of the will, we cannot feel sure the principle holds good without assuming a partisan position which would be unwise and unscientific. In an age when the axioms of geometry are put in doubt, it would not be astonishing to hear any physical principle challenged; but we repeat that our remark looked only to explaining the irreversibility of growth, in the same way in which inorganic irreversible processes are explained, by the application of probabilities and high numbers.—ED. NATION.]

53 (12 November 1891) 372

ABBOT AGAINST ROYCE

TO THE EDITOR OF THE NATION:

SIR: Dr. Francis Ellingwood Abbot makes substantially the following charges against Prof. Josiah Royce:

(1.) That Prof. Royce libelled Dr. Abbot, and that maliciously.

(2.) That Prof. Royce used unfair means to stifle Dr. Abbot's reply.

I propose to consider impartially what the verdict of students of philosophy ought to be regarding these public accusations against one of the most eminent of their number.

The charge of libel has two specifications, viz:

(1.) That Prof. Royce warned the general public against Dr. Abbot as a blatant and ignorant pretender in philosophy.

(2.) That Prof. Royce accused Dr. Abbot of plagiarizing Hegel at second hand.

From the point of view of propriety of conduct in a student of philosophy, the only adequate excuse for the first of these acts would be that the fact proclaimed was so unmistakable that there could be no two opinions about it on the part of men qualified by mature study to pass judgment on the merits of philosophical writers. In case the act were not so justified, the offence would be enormously aggravated if it were dictated by malice. The first question, then, is: Did Prof. Royce, as a matter of fact, so warn the public against Dr. Abbot? He certainly did, unequivocally and with full consciousness of what he was about; that is the unmistakable import of his whole article in the *International Journal of Ethics* for October, 1890. The next question is whether it is so plainly true that Dr. Abbot is a blatant and ignorant pretender in philosophy that it is impossible competent men should think otherwise? So far is that from being the case that philosophers of the highest standing, such men as Kirchheiss in Germany, Renouvier in France, and Seth in England, have drawn attention to the remarkable merit of his work. I am not personally intimate with Dr. Abbot, and am far from being a partisan of his doctrines, but as an humble student of philosophy, endeavoring to form my estimations with the eye of truth, I recognize in him a profound student and a highly original philosopher, some of whose results are substantive additions to the treasury of thought; and I believe that the prevalent opinion among competent men would be that Prof. Royce's warning is an unwarranted aspersion. Next, what excuse was there for such conduct, what motive prompted it? Prof. Royce and Dr. Abbot have their rival ways out of agnosticism. Both start from the same premises to come in the main (at least, so Royce says) to the same conclusion. Shall we say, then, that a passer-by cannot loiter near Dr. Abbot's shop, attracted by the placard, "THE WAY AND THE TRUTH," without Prof. Royce's rushing out and shouting from across the street that he can offer the same article at a lower figure? No; for how far a spirit of rivalry may have influenced him no man can know, Prof. Royce least of all.

Passing to the second specification, we ask: Did Prof. Royce accuse Dr. Abbot of plagiarizing Hegel? No; he only accused him of giving a maimed version of Hegel's theory of universals, naïvely supposing it to be a product of his own brain. That was no libel in the sense now considered. But, says Dr. Abbot, I have stated so clearly the antithesis between Hegel's view and mine that Prof. Royce cannot be sincere in saying they are identical. No matter; the more absurd the accusation, the less injurious; the less the truth, the less the libel. On this count Dr. Abbot is entirely in the wrong.

Passing to the second charge, we ask whether Prof. Royce used unfair means to stifle Dr. Abbot's reply? The ex-parte evidence indicates that he did contrive that Abbot's reply should be first postponed (as postponed it was over two numbers of the quarterly), and at last, as the third quarter was drawing to a close, should be excluded; in which performances Dr. Adler, the editor-in-chief, does

not appear as very strong in the practical department of ethics. Afterwards Prof. Royce, through a lawyer, threatened Dr. Abbot with legal proceedings if he published his proposed reply at all.

All this would be abominable to the last degree in the case of a philosophical discussion. But then it must not be forgotten that the contention had never had that character. Prof. Royce's article was written with the avowed purpose, clearly and openly conveyed, though not by direct declaration, of ruining Dr. Abbot's reputation; and what little discussion there was was merely to subserve that purpose, not to ascertain or prove any truth of philosophy. Thus, it was a brutal, life-and-death fight from the first. Prof. Royce clearly perceived this, for he ends the article *by saying that he shows no mercy and asks none*! That's ethics. And his subsequent proceedings make it, in my judgment, as plain as such a thing can be, that his cruel purpose never left his heart. Dr. Abbot, on the other hand, stood like a baited bull, bewildered at such seemingly motiveless hostilities.

It is quite impossible not to suppose that Prof. Royce conceived it was his duty thus to destroy Dr. Abbot's reputation, and with that the happiness of his life. A critic's stern and sacred duty, and all that! Besides, it must be remembered that he is a student of ethics; and it is not to be imagined that a person can study ethics all his life long without acquiring conceptions of right and wrong that the rest of the world cannot understand. C. S. PEIRCE.

53 (12 November 1891) 375

NOTES

This note is surely by Peirce, inasmuch as it is a continuation of the "*vis viva*" dispute that began with his review of Spencer. This is unassigned in Haskell's *Index to* The Nation, vol. 1.

—Prof. Hoskins sends us a rejoinder on *vis viva* too long and irrelevant to print, nor is the discussion, by its nature, exactly suited to our columns. Instead of showing how he could maintain that growth is not an irreversible process in the sense in which the action of viscosity is irreversible, he holds that an irreversible process does not violate the law of *vis viva*. But an irreversible process is such that if the final velocities have their signs reversed, the equations of motion will not be satisfied by the movement of all the particles back over their previous paths with the same (reversed) velocities. Now the equations will be so satisfied unless the forces are changed by this reversal of the velocities—that is, unless they depend on the velocities. Further, if the accelerations depend on the velocities, it is easily shown that the *vis viva* cannot always be the same in the same configuration, and thus the equation of *vis viva* is violated. Therefore growth, so far as it is an irreversible process, violates this principle. It is true that the kinetical theory explains not only irreversible processes (for which it was needed), but also reversible ones (which is supererogatory). But our correspondent is surely mistaken in saying that a similar apparent violation of the law of *vis viva* admits of any acceptable explanation not based on probabilities. Friction, viscosity, diffusion, conduction, in all states of matter must be so explained.

53 (19 November 1891) 389-390

ABBOT AGAINST ROYCE

TO THE EDITOR OF THE NATION:

SIR: Mr. Peirce's letter on this subject in your last week's issue unfortunately brings it before the larger public; and, since Mr. Peirce professes to be a neutral judge, it may leave on your readers an impression unfair to Prof. Royce if nothing more gets said. May I take a little of your space to record my opinion of the merits of the case?

First, the facts, Professor Royce, one of the editors of the *International Journal of Ethics*, wrote, in its first number, a review, seventeen pages long, of Dr. Abbot's 'Way Out of Agnosticism.' This review was altogether technical in character, but hostile in content, impugning both the value and the originality of Dr. Abbot's philosophy. Reviews of philosophical books in technical journals are apt to be destructive—that is what philosophers expect of each other; and in this review there was nothing unusually intolerable, as reviews go, till the page before the last, in which (set in some sentences of a rhetoric characteristic of Prof. Royce) the following passage occurred:

"But Dr. Abbot's way is not careful, is not novel, and, when thus set forth to the people as new and bold and American, it is likely to do precisely as much harm to careful inquiry as it gets influence over immature or imperfectly trained minds. I venture, therefore, to speak plainly, by way of a professional warning to the liberal-minded public concerning Dr. Abbot's philosophical pretensions. And my warning takes the form of saying that if people are to think in this confused way, unconsciously borrowing from a great speculator like Hegel and then depriving the borrowed conception of the peculiar subtlety of statement that made it useful in its place—and if we readers are for our part to accept such scholasticism as is found in Dr. Abbot's concluding sections as at all resembling philosophy—then it were far better for the world that no reflective thinking whatever should be done. If we can't improve on what God has already put into the mouths of the babes and sucklings, let us at all events make some other use of our wisdom and prudence than in setting forth the 'American theory' of what has been in large part hidden from us."

This passage is Dr. Abbot's chief ground of complaint. It contains the expression "professional warning," which certainly has a conceited sound. Dr. Abbot assumes that by "professional" Prof. R. meant *professorial*, and that he claimed the authority of Harvard University for the warning conveyed. This is the basis of his application to the President and Fellows of Harvard to punish in some way their employee.

That an author should feel sore at being so handled by a critic is inevitable. That he should wish to reply is natural. Dr. Abbot replied. Mr. Peirce says that the editors first postponed, then excluded this reply, and finally threatened legal proceedings if it were published apart. A falser impression of the facts cannot be imagined than this statement gives. The editors were liberal as few editors are. An editor's first duty, if controversy must be, is to restrict it to one number so

that it may not disgust the readers by trailing its slow length along. Dr. Royce and his colleagues, accordingly, in accepting Dr. Abbot's reply (although it was some thirty pages long and bitterly personal), insisted that a rejoinder from Prof. R. should appear after it *in the same number.* Dr. Abbot agreed to the rejoinder, but stoutly protested that it should not appear *in that number.* On condition, however, that the rejoinder should have appended to it a retort from him which should close the controversy, Dr. Abbot agreed that one number might contain both his own and his reviewer's words. These negotiations and the documents they demanded could not be finished in time for the then pending number of the review, which consequently appeared without the controversy in it. Mr. Abbot charges the editors with wilful delay; one as familiar as Mr. Peirce with the conditions of getting a "number" out might easily imagine less far-fetched reasons.

The July number was then in order, and the editors, who had not yet got Abbot's retort, now claimed that it should "not exceed Royce's rejoinder in length," that it should "not raise new issues," and that, since the twenty-eight-page reply was full of personal aspersions, these last words from Abbot "should not assault Royce's personal character, and should be parliamentary in form, and free from personally abusive language." To this proposal Dr. Abbot's reply was, to quote the words of his memorial to the President and Fellows, "a short and dry rejection *in toto.*"

Then came rumors of a lawsuit and a pamphlet on the part of Dr. Abbot. Is it wonderful that Dr. Royce should now consult a lawyer as to how the growing tide of unpleasantness might best be minimized? The lawyer warned Dr. Abbot that to publish a pamphlet might make him legally liable, this being of course an ordinary routine precaution against future legal trouble of any sort. Mr. Peirce, following Dr. Abbot's *ex-parte* statement, treats it as part of a plan to "stifle" the latter's reply. Now Dr. Abbot (though in general correct in his record of the facts) has omitted the important fact that in the very letter in which the lawyer conveyed the warning as to liability, he also made an offer to Dr. Abbot from Prof. Royce to print his long reply in the next *Journal,* with no editorial comment in that number, provided Dr. A. would prune it of degrading personalities, leaving the argument untouched. The *quid pro quo* seems fair enough; yet the sacrifice demanded was intolerable to Dr. Abbot, and he published his memorial to the Harvard Corporation instead.

A more grotesque accusation of unfair editorial treatment than that made by Dr. Abbot and echoed by Mr. Peirce was consequently never made.

Now as to Mr. Peirce's talk about Prof. Royce's "cruel purpose" of "ruining Dr. Abbot's reputation." When did a critic ever deny the value of a book *without* the purpose of ruining the author's reputation—his reputation, namely, for competency in that field? That Prof. Royce had any animosity to Dr. Abbot's reputation in *other* relations of life is too silly a charge even for denial. And what Mr. Peirce means by the affair being a "brutal life-and-death combat from the first," I confess is too dark a thing for me to understand. Had *I* written a book with such ambitious aims as Dr. Abbot's, I should expect my differently-thinking compeers to handle me without gloves, and should despise them if I suspected

that the fear of wounding my feelings stayed their hand. Were Prof. Royce's review one of *my* book, I should probably be considerably stirred-up by his low opinion of me, and should feel the genial latitude of his style, when expressing the same, to be peculiarly exasperating. At the same time I should recognize the inevitableness of such differences of understanding, and should feel that I had no avowable *grievance*, since, unlike those critics who dismiss a volume of poems or a novel with a sneer for which no grounds are given, Prof. Royce had given his own reasons for all that he had said. My only remedy would lie in beating down my critic's philosophy and strengthening my own. Mr. Abbot's remedy of heaping personal outrages upon Prof. Royce and his motives, admits of no excuse but a pathological one. It is truly deplorable that the quarrel should spread beyond the academic world. But since Mr. Peirce has served it up for your readers in what they also may imagine to be an "impartial" statement, it seems but fair that one with a less *ex-parte* knowledge of the facts should also be heard. WILLIAM JAMES.

HARVARD UNIVERSITY, November 15, 1891.

53 (26 November 1891) 408

THE SUPPRESSION OF DR. ABBOT'S REPLY

TO THE EDITOR OF THE NATION:

SIR: Since Mr. Peirce has thought fit to bring this subject before your readers, and to comment on Prof. Royce's conduct, as charged by Dr. Abbot, in stifling Dr. Abbot's reply by a threat of legal proceedings, I feel compelled to ask you to publish the evidence on that point in full.

Dr. Abbot bases his charge upon a letter written by me, as Prof. Royce's counsel. In a pamphlet addressed to the governing boards of Harvard College (but widely circulated and put on public sale), Dr. Abbot characterizes that letter as an attempt, on Prof. Royce's part, "to gag the man he had injured," and formally sums up his accusation by asserting that Prof. Royce "has sought, with incredible cowardice and meanness, to deprive me of all opportunity of being heard in self-defence."

I now give the letter (of which Dr. Abbot publishes only the few lines of formal protest), and also Dr. Abbot's reply. I should premise that I knew nothing of the controversy until Prof. Royce sought my advice in consequence of threats of a law-suit from Dr. Abbot. At that time Dr. Abbot's reply had been set up in type by the *Journal of International Ethics* with the expectation of publishing that as it stood, together with a rejoinder by Prof. Royce, and a final retort which Dr. Abbot was to write, all in the July number. This plan had broken off, as stated by Dr. Abbot in his pamphlet, because Dr. Abbot could not agree with Dr. Adler as to the tone in which he should write his final reply; Dr. Adler requiring a parliamentary tone, while Dr. Abbot demanded a freedom which he called "the freedom of the courts." It is Dr. Abbot's main reply, already in type, which is referred to in my letter. Dr. Adler and Prof. Royce are both editors of the *Journal.*

BOSTON, June 9, 1891.

Dr. Francis E. Abbot, Cambridge, Mass.:

MY DEAR DR. ABBOT: Your article entitled "Dr. Royce's Professional Warning" has been submitted to me as a part of the case upon which my professional advice is sought, and I must call your attention to some passages in it which I trust you will think it well, upon deliberation, to revise.

I will say at the outset that, considering the severity of Dr. Royce's article, I think, for my own part, that you are justified in replying with spirit, and that you should perhaps be allowed more warmth than the ordinary discussion of such subjects calls for. Of that Dr. Royce, I know, would not complain, but in the heat of your reply you have in some places used language which I think you will hardly wish, upon cooler judgment, to allow to remain to lower the tone of your argument.

Conceding, for the moment, that you are right in thinking that Dr. Royce has transgressed the limits of courteous controversy, I must say that your article, in some places, goes far beyond anything that he has said.

On Dr. Royce's behalf, I must warn you that he protests against the publication, or any circulation of it, in its present shape, and must point out to you that it may, if circulated, entail a serious legal responsibility.

In it you charge Dr. Royce with being guilty of "a slanderous attack" and of "libel," and with having called you an "impostor"; you seek to belittle and injure him in his profession and business as a teacher in Harvard College; you imply that he is guilty of wilful misrepresentation; you seek to bring him to contempt by a degrading comparison; you charge him with untruth, with having made a wanton and injurious attack upon your personal reputation, having abused his academical position, compromised the dignity of Harvard College, degraded the office of professor, publicly traduced and libelled a fellow-citizen; and finally you pronounce him professionally incompetent.

Such language, even though used in controverting an irritating review of your book, so far exceeds the proper limits that in my judgment you cannot indulge in it without danger of legal liability.

Permit me, too, as a cool spectator of the controversy, to say that this language greatly weakens and lowers a very forcible argument, and must have the effect of distracting attention from the points you wish to make, and stamping the whole discussion as a strangely undignified attack for such a combatant. And aside from the effect of such an article upon yourself, let me call to your attention the scandal which is brought upon Harvard College by such a public wrangle between two of her instructors.

I have not read carefully the whole of Dr. Royce's article, but I have read the parts which must be most offensive to you; and while I do not defend, in all respects, the tone of the review, I think that you have greatly exaggerated and misinterpreted it. As I said to you on Sunday evening, Dr. Royce has disclaimed, in the strongest way, any intention to wound you, or to reflect in any way upon your personal character; and after this, is it not a perversion to insist upon putting the worst and most personal construction on all that he says, omitting the qualifications which go far to soften his hostile expressions?

As I remember his article, he nowhere calls you an impostor, as you repeatedly charge; and in speaking of you as "sinning against the demands of literary property rights," you omit the word "unaware," which wholly changes the sense.

That the *Journal of Ethics* should publish the article as it now stands is not to be thought of. It could not do so with self-respect. The editors are, however, very willing to publish the body of your reply as you have written it, if you will leave out those passages which are merely personal.

I send with this a copy of your article, with the objectionable passages marked. You will, I think, admit that your argument is untouched, and that enough of anger and indignation are left to save the paper from any appearance of tameness. If these passages are omitted, or so changed as to be free from objection, the *Journal* will publish it in the July number, and without any other comment than a statement that a reply is reserved for the October issue.

I trust that you will adopt my suggestions and make the changes, which I believe will strengthen the article in the minds of those whom you most wish to persuade. You will not overlook the great advantage it will be to you to have your reply appear in the same journal which originally published the review, and I trust that you will be willing, for that reason if no other, to conform to the very obvious requirements which the *Journal* must impose.

I hope you believe me when I say that I should not advise the *Journal* to refuse the article in its present shape, as I do, unless I were fully persuaded that you are offered the fullest opportunity of reply which fair play can demand.

Very sincerely yours,
J. B. WARNER.

P.S.—Please let me know your decision as soon as possible, as the *Journal* must be made up. Will you kindly return my copy of your article? J. B. W.

LARCH STREET, CAMBRIDGE, Mass.,
June 9, 1891.

J. B. Warner, Esq., Exchange Building, Boston:

MY DEAR MR. WARNER: I beg leave to acknowledge receipt of your obliging letter of this date, with thanks, and to return at once the enclosed printed paper, as you request.

With great personal regard. I remain

Very sincerely yours,
FRANCIS E. ABBOT.

Dr. Abbot declined to make any change in his reply and it has never been published. JOSEPH B. WARNER.

BOSTON, November 20, 1891.

53 (26 November 1891) 415

Pictorial Astronomy for General Readers.

By George F. Chambers, F.R.A.S. Macmillan & Co. 1891. 16mo, pp. 267.

CSP, identification: MS 1365. See also: Burks, *Bibliography*. This notice is unassigned in Haskell's *Index to* The Nation, vol. 1.

There is no lack of popular books about astronomy by those who look upon the subject from the inside, as, Herschel, Secchi, Newcomb, Langley, Young, Lockyer, Ball. Mr. Chambers is none of these. He is not a scientific observer of the stars, nor has he an ordinary astronomer's acquaintance with celestial mechanics. He is a well-known compiler of astronomical books, useful in their way, but marked by incompleteness and a want of discrimination. The present little treatise will serve the purpose of a person who wants some light reading with pictures touching most of those important topics of astronomy that call for no mental exertion, about right in most of its statements, and not seriously unjust in many of its appreciations. To show how simple everything is here made, we annotate a short passage taken almost at random. The numerals in parentheses refer to our remarks below:

"In calculating the different positions of Mars (1), and comparing his own observations (2) with those of Tycho Brahe, Kepler was astonished at finding numerous apparent irregularities (3) in Mars's orbit, and still more in its distance from the earth (4). He soon saw (5) that the orbit could not be circular, and eventually recognized that it must be (6) an ellipse, with the sun occupying one of the two foci. . . . The path of a planet once traced, the next thing (7) to determine was what regulated the irregularities observed in its course. Kepler, having remarked (8) that the velocity of a planet (9) seemed to be greatest when it was nearest to the sun, and least when it was most remote from the sun, proceeded to suggest that an imaginary line joining the centre of a planet and the centre of the sun would pass over equal areas in equal times. . . . He sought to discover if any relation subsisted between the diameters of the orbits and the times occupied by the planets in traversing them. After twenty-seven years (10) of laborious research (11), he found out that a relationship did subsist, and thus was able to assert his third law."

(1.) Kepler did not set out by calculating places of Mars from its elements, but on the contrary by endeavoring to deduce from the observations the eccentricity of the orbit.

(2.) At the time referred to, Kepler is not known to have observed Mars, and only a very few of his observations were used by him in the investigation of the motions of that planet.

(3.) What incited Kepler to his great work was not finding irregularities, but a belief that by a method of calculation different from that in use (based on apparent instead of mean oppositions) known seeming irregularities could be made to disappear.

(4.) The distance from the earth could not be a subject of observation, and consequently irregularities in this distance could not be detected. The only thing

in the work with which we can connect this belongs to a later time, after a great part of the work had been done and a corrected theory of the earth's motion had been made.

(5.) For "soon" read: after five years of diligent research.

(6.) This "must be" conveys no hint of the mode in which the opposite errors of two hypotheses directed Kepler's suspicions to the ellipse as the form of the orbit.

(7.) Mr. Chambers writes as if Kepler first ascertained the form of the orbit and then introduced the principle of areas. But it was the other way. He had assumed this principle long before he dreamed of the orbit not being circular. Indeed, without some such assumption he would not have had sufficient data to determine the shape of the path, since the distance of Mars could not be determined except by an intricate procedure seldom applicable. Indeed, except for movements in latitude too slight to prove much, all that is observed is variable movements in longitude.

(8.) This remark was of course one of the earliest generalizations concerning planetary motion.

(9.) A *superior* planet is meant.

(10.) The discovery was made 1618, May 8. Twenty-seven years before, Kepler had not taken up the pursuit of astronomy.

(11.) Although he puzzled long over the figures before he happened to light on the true relation, there was nothing to be called systematic research, nothing comparable for an instant with the work upon Mars.

In short, the author correctly states Kepler's laws; but as to how he came by them (further than that two were from studies of the motions of Mars) he seems to have not the slightest idea. To show that the passage is not exceptional, as this comes from p. 10, we will see what we can find on the tenth page from the end. We find this:

"His [Ptolemy's] great work was the celebrated Μεγάλη σύνταξις, better known by its Arabian designation of The Almagest. This work contains, amongst other things, a review of the labors of Hipparchus; a description of the heavens, including the Milky Way; a catalogue of stars; sundry arguments against the motion of the earth, and notes on the length of the year."

Even the title is wrongly given, and the description of the contents is as if one should explain that the Bible is a work containing among other things a discussion of the age of Moses, a description of Solomon's temple, a list of commandments, sundry exhortations against sloth, and the memoirs of Paul of Tarsus.

53 (3 December 1891) 426

MR. WARNER'S "EVIDENCE IN FULL" COMPLETED

Francis Ellingwood Abbot (1836-1903) was an American philosopher and active religious reformer. He was the founder of the Free Religious Association, editor of *The Index*, and Colonel Bob Ingersoll's running-mate on the Liberal League's presidential ticket of 1880. He was graduated A.B. from Harvard in 1859, along with Peirce, and spent one year at

the Harvard divinity school. In 1863 he was graduated from the Meadville Theological Seminary, and was ordained in the First Unitarian Society of Christians at Dover, New Hampshire, in 1864. When the National Unitarian Conference of 1865 adopted a constitution that referred to its members as "disciples of the Lord Jesus Christ," Abbot found that he could no longer accept the creed of that church, and so set out to organize the Free Religious Association. *The Index*, which was the literary branch of the Association, served Abbot as a forum for his philosophical and theological views. His experience with the Association led Abbot to form the National Liberal League, which became important as the strongest opponent of a drive to secure an amendment to the Constitution citing "God as the source of all authority and power in civil government." In 1881, Abbot received his A.M. and Ph.D. from Harvard in Philosophy. After this, he sought academic positions with Cornell and Harvard, but despite strong recommendations, all attempts failed. He did, however, win a position as temporary replacement for Josiah Royce at Harvard in 1889, during the latter's leave of duty. He authored three books: *Scientific Theism* (1885), *The Way Out of Agnosticism* (1890), and *The Syllogistic Philosophy* (1906), published posthumously.

As Peirce pointed out in his letter of 12 November, the argument between Abbot and Royce arose over Royce's scalding review of Abbot's *The Way Out of Agnosticism*, which appeared in the first number of the first issue of the *International Journal of Ethics*. Abbot's book was a compilation of lectures he had delivered at Harvard in 1889 while taking Royce's place during the latter's leave of absence. Ironically, Royce had recommended Abbot for this position. But upon his return, Royce was outraged when word reached him of certain statements Abbot was alleged to have made concerning Royce's teachings. This can partially explain the vehemence of Royce's review.

Several years prior, however, Royce had already shown his distaste for Abbot's work in a review for *Science* of Abbot's *Scientific Theism*. This is the same work that Peirce reviewed in *The Nation*, and was in its third printing in a German translation. Despite such signs of approval, Royce attacked even Abbot's use of capitals and italics, and characterized the book as indicative of "Dr. Abbot's not uncommon, but highly amusing state of mind." (*Science*, 7:335-338)

Aside from the philosophic merits of Abbot's books, there was a certain measure of pride at stake. Although a classmate of Peirce at Harvard in 1859, Abbot was 45 years old before he took his Ph.D. (1881). And so he was forced to compete for an academic position with men many years his junior. Royce, however, was young, bright, successful, and enjoyed the influential backing of William James, who was responsible for Royce's first position at Harvard. He was already making a name for himself while Abbot was still looking for a permanent job.

Abbot's radical religious views had caused him to be a maverick in the academic world, where success still depended heavily upon religious orthodoxy. Had *The Way Out of Agnosticism* only proved itself to be valuable, it might have become Abbot's "way out of obscurity." But even after the attention drawn to Abbot's cause by Peirce's letter in *The Nation*, Abbot slipped back into the shadows and never attained the prominence he thought was due him.

Joseph Bangs Warner (1848-1923) was an American lawyer. He was graduated A.B. from Harvard in 1869, A.M. in 1872, and LL.B. in 1873. He began his practice in Boston in 1873 with the firm Warner, Warner, and Stackpole. He served as trustee for Radcliffe College and Simmons College, and, together with O. W. Holmes, coedited James Kent's *Commentaries on American Law*.

To The Editor Of The Nation:

Sir: In your last week's issue, Mr. J. B. Warner professes to give the "evidence in full" respecting Prof. Royce's suppression of my reply to his (the latter's) avowed "attack." The long letter he publishes as "evidence" on this point is evidence of nothing but the lawyer's attempt to put forward his own baseless assumptions in his client's behalf as if they were assured facts. The adroit assump-

tion in this case is, that the "language" of my suppressed reply was improper, and justified exclusion of the reply from the *Journal of Ethics.* This assumption I deny with vigor; and, what is more telling than any denial of mine, Dr. Adler and Dr. Royce, as editors of the *Journal,* denied it themselves, when at first they both accepted the reply for publication, had it put in type, and sent me proofs both of the reply and of Dr. Royce's rejoinder to it. The subsequent rejection of my reply, under Mr. Warner's advice, cannot undo the effect of their previous sanction of it as *perfectly fit for publication.*

But the "evidence in full" on this point cannot be given without showing, by actual quotation, what really was the "language" to which Mr. Warner so unreasonably objected. I have no right to ask you to devote much space to such quotation; but, relying on your well-known fairness, I must ask leave to cite, as a fair specimen of the "language" objected to, the opening of the suppressed reply. The passages here italicised were marked by Dr. Royce himself as the grounds upon which he and his lawyer based their threat of prosecution and their suppression of the reply itself. It will be perfectly clear to any fair-minded man that they were aiming to force me either to concede that Dr. Royce's original article was a legitimate criticism, or else to lose all opportunity of being heard in self-defence.

That his article was a libel, and not a fair criticism at all, has been proved in my pamphlet beyond all possibility of a successful reply; and the reader, bearing this in mind, will judge for himself whether the "language" as such, or whether the effort to defend myself against the libel, was the real ground of Mr. Warner's threatening letter. The following passage from the suppressed reply is a fair sample of its "language" throughout:

"The mere fact that, in the *International Journal of Ethics* for last October, there appeared a hostile review of my book entitled 'The Way Out of Agnosticism,' by Dr. Josiah Royce, assistant professor of philosophy in Harvard College, would not induce me to break my uniform custom of silence in such cases, were it not that Dr. Royce *oversteps the limits of legitimate literary criticism, throws out personal accusations of a slanderous nature, and resorts to empty and undignified offical denunciation* in order to flank indirectly a philosophical position which he has not ventured openly to assail. *His mode of attack is a marked case of 'reversion' to controversial methods which, common enough some centuries ago, are happily going out of use to-day. Dr. Royce presumes to accuse me, falsely and injuriously,* of 'frequently making, of late, extravagant pretensions as to the originality and profundity of [my] still unpublished system of philosophy,' *and of 'sinning* against the most obvious demands on literary property rights'; and he even goes so far as to issue a solemn 'professional warning,' formally addressed to 'the liberal-minded public,' against myself as a philosophical thinker and author. Such tactics as these *are unknown among reputable literary men. They are justified by no higher ethical principle than that which dictated the old pettifogger's advice to the young one: 'If you have no case, abuse the counsel on the other side.'*

"This paper, therefore, is written *as a reply, not to a critique, but to a libel.* If I notice below what Dr. Royce puts forward as 'criticisms,' it is not because they deserve to be noticed as such, but solely because they are made to serve as the ostensible warrant and support of *his libellous 'professional warning.' And the only reason why I make my defence in these columns is that believing the 'liberal minded public' to be a just judge, I have greater confidence in the court of reason than I have in the courts of law.*

"When civil-service reformers plead the urgent necessity of political reform, they are irrelevantly charged by the adherents of the spoils system with being 'hypocrites and pharisees.' Precisely so, when I plead the urgent necessity of philosophical reform, *I am irrelevantly charged by Dr. Royce, in effect, with being a false pretender, a plagiarist, and an impostor.* The charge is just as true in one case as in the other. But, be the charge true or untrue, the attention of keen and candid minds is not to be diverted by this perfectly transparent device from the main point of reform. In both cases, interests more important than any personal reputation are at stake; and loyalty to interests more important than my own reputation requires me *now to expose Dr. Royce's endeavor to divert attention by irrelevant, useless, and utterly unprovoked vituperation from the main point of philosophical reform.*"

Will any fair man say that the "language" here used is other than temperate, dignified, and parliamentary? I protest against Mr. Warner's attempt to misrepresent the character of my "language," as improper in any degree. A libelled citizen has a right to defend himself against the libel; and, when Dr. Royce blew his bugle-blast of defiance, "We must show no mercy, as we ask none," he deprived himself of all excuse, in the eyes of men who prize the good old English principle of fair play, for seeking refuge behind a menace of prosecution. And here I must express my surprise at Mr. Warner's statement that "Prof. Royce sought my advice in consequence of threats of a law-suit from Dr. Abbot." I never threatened Dr. Royce with a law-suit at all. FRANCIS E. ABBOT.

CAMBRIDGE, NOVEMBER 28, 1891.

[We cannot print any more letters respecting this controversy.—ED. NATION.]

53 (17 December 1891) 474

An Introduction to Spherical and Practical Astronomy.

By Dascom Greene, Professor of Mathematics and Astronomy in the Rensselaer Polytechnic Institute. Boston: Ginn & Co. 1891. Pp. viii, 150.

CSP, identification: MS 1365. See also: Burks, *Bibliography*; MS 1371a (draft). This piece is unassigned in Haskell's *Index to* The Nation, vol. 1.

Dascom Greene (1825-1900) was graduated in 1853 from Rensselaer Polytechnic Institute, where he was appointed assistant professor of mathematics and practical astronomy. He wrote on both astronomy and mathematics, and was a member of the American Association for the Advancement of Science.

In this small and convenient volume is contained nearly everything of astronomy that an engineer will need. Unfortunately, the few omissions, such as precession, aberration, parallax, and refraction (the last not quite excluded), with the few excessive abridgments, are of radical importance. By adding two more sheets to the book, so as to include sufficiently these subjects, and amending it in some other respects, it may in a future edition be rendered a work of exceptional merit. Professor Greene, in his preface, by way of excuse for the omissions, says: "It [the book] claims to be no more than an introduction to the subject, and aims to present its first principles in an elementary and practical form for the use of beginners." But, in the first place, a student has no time to go through such a subject as astronomy twice; and it is not practical teaching to omit any practical topic in such a branch, to be taken up at a later period. In the second place, the class of students resorting to Troy, though they be beginners, must not be put off with any inferior presentation of a science. Indeed, logic and completeness are of even more importance in elementary than in advanced treatises. A book such as this might easily have been, which should touch upon every necessary matter with logical severity, giving all that is needed and excluding all that is superfluous, would serve as an intellectual tonic for the young man, and operate in some degree as a corrective to the dissipating and demulcent influences of other modern textbooks. Besides, in any subject, but above all in mathematics, it is a great advantage to keep the treatise which has been deeply conned, but which has been partly forgotten in after years, at one's elbow as a book of reference, and for that reason elementary works should be as nearly tabular in form as the nature of their subjects will permit, uniting the utmost brevity with completeness in certain systematic limits.

Prof. Greene's descriptions of instruments and the ways of using them are pretty good, but not full enough for practical needs. Thus, it is said that the adjustment of the principal focus of a transit instrument "may be verified by moving the eye so as to detect any parallax existing between the star and the wire." This does not sufficiently emphasize to the beginner the indispensableness of this operation; nor are the precautions to be observed in performing it pointed out. Nor is anything said about ascertaining the fixity of the parts of the telescope, the collimation of the objective, etc. Nor is the reduction of time observations set forth and illustrated in a practically adequate manner.

The mathematical deductions throughout the book are given with commendable clearness and brevity; but the treatment of the foundations of the method of least squares is decidedly antiquated. The principle of the arithmetical mean is assumed as self-evident. Such reasoning will not go down in our day, and to teach boys to be satisfied with it is a grievous wrong. It may, no doubt, be said that a book intended for boys who seek instruction solely as a means of livelihood should not notice mere speculative doubts, and should sedulously avoid opening temptations to purely intellectual engrossments, the joys of which their situation in life must forbid; and there is some truth in this. But there can be no advantage to anybody, in our swiftly progressive age, in being unable to distinguish bad reasoning from good. Good logic is the most fundamental thing which any kind of student can possibly be set to acquire.

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54 (21 January 1892) 54-55

THE COMTIST CALENDAR

The New Calendar of Great Men: Biographies of the 558 Worthies of all ages and countries in the Positivist Calendar of August Comte.

Edited by Frederic Harrison. Macmillan. 1892.

CSP, identification: MS 1365; Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS 1373 (draft).

Frederic Harrison (1831-1923) was an English lawyer and positivist philosopher. He was professor of jurisprudence, constitutional and international law for the Council of Legal Education from 1877 until 1889. Harrison was a prolific writer on historical and literary subjects.

That the contemplation of the lives and characters of great men is a salutary and invigorating spiritual exercise has always been admitted and often proved. But it is so only on condition that the heroes are apprehended in all their living reality and passion; and, unfortunately, biography is infested with pious frauds. Washed-out accounts of Washington and Franklin have done incalculable injury to American characters. Such portraits had their origin in that deep faith in mendacity, as the only thing to be trusted to excite a desire to be good and to keep society straight—a sacred duty, too, to the dead—which was pervading and powerful in this country up to thirty years ago; and, thus engendered from the spirit of lies, how could these biogaphies bear living seeds of anything but hypocrisy and sordidness? Auguste Comte's calendar is a more systematic and resolute endeavor to belie the facts, not of this or that man's existence, but of biography in the wholesale. A list of Worthies which excludes Napoleon while admitting Harûn-ar-Rashîd, as if there were a single littleness of the former which was not still littler in the latter, or a single spark of greatness in the latter which was not a fiery blaze in the former, is plainly animated by some ulterior purpose; and a portraiture of man's development, as this calendar professes to be, that is arranged in thirteen equal divisions, each of these subdivided into four equal parts, and each of these again into seven others, of which six are equal and the seventh superior in every case, is no transcript from nature, but a fanciful invention.

Among Comte's contemptible traits, none more marks his smallness than this calendar—not the drawing of it up, for that might pass for a rational pastime, but the failure to inspire his disciples with manhood enough to cast it aside. Auguste Comte was as utterly wanting in admiration and sympathy for great men as he was for his neighbors. He thought of them, not as concrete souls, but only as factors in the advancement of the human race, abstractly considered. What are his thirteen greatest men? Mere figureheads: Moses, Homer, Aristotle, Archimedes, Cæsar, Charlemange, Dante, Gutenberg, Shakspere, Descartes, Frederick the Great, and Bichat. Gutenberg! What did Comte know or care about Gutenberg as a man? But he is not thinking of the men themselves. Where is Jesus? Not among these 13; nor among the 52 of the second rank; nor yet among the 312 of the third rank; nor even among the 181 of the fourth rank—

not there at all! Buddha, Confucius, and Mahomet—the gentle Mahomet!—are in the second rank; Zoroaster, Manco Capac, Menu, Sesostris in the third. Jesus is omitted because he does not represent the sentiments which it is Comte's purpose to inculcate. Most persons, asked to name a baker's dozen of the greatest names of all time, would choose twelve of them from among Alexander, Buddha, Cæsar, Charlemagne, Dante, Homer, Jesus, Mahomet, Michelangelo, Moses, Napoleon, Newton, Pythagoras, Shakspere, and Socrates; but only half of Comte's list is drawn from these. Those great leaders of armies and rulers of peoples, Attila, Belisarius, Charles XII., Clive, Cortez, Queen Elizabeth, Genghis Khan, Julian, Marlborough, Napoleon, Omar I., Peter the Great, Pitt, Timour, Turenne, Wallace, Wellington, William the Conqueror, are not recognized as great men at all. Godfrey de Bouillon goes in as a stanch Catholic. Comte held everything like Protestantism in detestation; we search his list in vain for the names of Calvin, Huss, Knox, Luther, Savonarola, Swedenborg, Wesley, Wiclif. These he excluded as mere destroyers, thus betraying a very false notion of the process of mental development in which destruction plays an essential part—there was no other good in Comte himself. He took little stock in doctors, and did not know John Hunter, Jenner, Sydenham, nor Vesalius.

Some great names in science, too, are omitted, as Gilbert, Herschel, Rumford; and perhaps not a single scholar is named as such. Yet mediocrities like Vaucanson and Montgolfier appear in the second rank! And this in the year of our Lord 1849, with the mechanical theory of heat in the air! Of course, Sadi-Carnot, Gaulois, Abel, are not to be dreamt of. Queer notions of philosophy find places for the stupid Albertus Magnus, the superficial John of Salisbury, the crazy Raymond Lully, the empty Ramus, the emotionalist Bonaventura, the unsound Hobbes, the insignificant Cusa, Campanella, Vauvenargues, Duclos, and others equally destitute of all claim to greatness, while such important thinkers as Epicurus, Abelard, Duns Scotus, Ockham, Berkeley, Rousseau, Bentham, James Mill, pass unnoticed. Comte was too small a mathematician to appreciate Cauchy, Fresnel, Gauss, Laplace, or even Ricardo. Fermat, who as a reasoner cannot possibly be placed lower than second in the whole history of mind, for he invented a form of inference absolutely novel, and, besides, discovered the mode of reasoning of the differential calculus, all but its notation—this man is admitted to the list, but only in the lowest rank, as second to a second-rate mathematician, John Wallis.

But this is only an example of how little the utilitarian world cares for reasoners. Reasoners are of no use, for the most part, except to posterity—and unborn posterity rouses but a mild interest. The calls of a bread-and-butter profession prevented Fermat from accomplishing much. He could do little but jot down on margins of books and on stray leaves surprising propositions about numbers without demonstrations, destined to puzzle his ablest successors for over two hundred years. One of his theorems is still under investigation. Nevertheless, Comte, a professed mathematician, wearily assigns to Fermat bare standing-room among his assemblage of Worthies. So it was with Jacob Steiner, another most wonderful reasoner, with little question the greatest of all geometers, though not in

Comte's list. Extreme poverty quite prevented the publication of the greater part of his discoveries, which have thus been lost; and those that were published had to be published without demonstrations. Thus they remained a dead wall for geometry during thirty-six years. It was the same, in a measure, with the remarkable reasoner, Thomas Young, though he practised a lucrative profession. Kepler's great work, beyond comparison the most marvellous piece of ampliative reasoning ever executed, as well as the most momentous in its consequences, was rendered possible only by his wife's riches and the bounty of the Emperor. His contemporaries, no doubt, held Kepler in high esteem, but they could not dream that his performance was destined to lead to a Newton's, and thence, through the development of modern physics, was to revolutionize the daily life of every civilized being. It was only a sinecure professorship, which another had voluntarily resigned to him, that enabled Newton to do his work (the wealthy Halley paying the printer); and surely this one result by itself fully justifies and compensates all the expenditures that have ever been made in England to establish foundations. Rowan Hamilton, the inventor of quaternions, likewise held a professorship without definite duties. The annual dues of the Royal Society prevented him from the advantage of membership; Newton had been excused from their payment.

Aristotle himself, the prince of thinkers, would scarcely, we believe, be heard of to-day, it if had not been for Alexander. Even as it was, his great works excited so little attention that if a single copy had not been exhumed from the cellar where it had been rotting, forgotten, for nigh two centuries, the whole current of human thought would have been different, and we should perhaps be this day in semi-barbarism. The same thing would equally have resulted if it had not happened that the greatest man of thought of all time was beloved by the greatest man of action. It needed an Alexander to appreciate an Aristotle: ordinary men have not imagination enough to be interested in posterity.

If Newton had not done his work, it would have got done piecemeal, with a delay of, say, fifty years in the establishment of the law of gravitation. If Kepler had not done his more difficult work, it would have had to wait for the further development of mathematics and of philosophy, which would themselves have been greatly retarded, so that civilization would probably have been put back almost two centuries. We should now be living in something like the age of Queen Anne. If Aristotle had not done his work, the result would have been too vastly altered for our comprehension.

Palissy the Potter, a man of great penetration, and the best possible judge of such a matter, gave it as his opinion that a large majority of the world's powerful thinkers are either crushed by circumstances or forced into the pursuit of wealth, and so lost for the world's uses. Who can imagine how we should be situated now if these men had had the encouragement that the public interest required? No doubt, human misery would have been greatly abated, for poor and for rich, and human life much prolonged. But we have to suffer for our forefathers' improvidence. There is no civilized country where a great work of reasoning is less feasible than in ours. We have most superb observatories and laboratories, it is

true, but what would a Kepler, with his bad sight and awkward hand,* be doing in such an establishment? Perhaps among our sixty millions there may just now live such a mind; certainly, nobody is on the lookout for him. If he does exist, one wonders what he is doing. Reading examination papers?

The notices in this volume are perfunctory, dead-and-alive things; considering the men, not as they lived and breathed and burned, but as they appear in relation to the cult of Comte's *Grand Être.* There are but 644 pages for 558 biographies, so they could hardly but be jejune and dull. Yet the reader might at least have been referred to the best sources of further information. In fact, he is referred to second and third-rate authorities. Loose and incorrect statements are so abundant that we do not think it necessary to give instances of them.

54 (4 February 1892) 97

The Principles of Chemistry.

By D. Mendeleef. Longmans, Green & Co.

This review is attributed to Peirce by Fisch in *First Supplement*, but in a letter to Peirce dated 21 January 1892 (MS L 159.12), Garrison apologized to Peirce for not giving him the opportunity to review Mendeleef. This letter indicates that someone other than Peirce also contributed articles on chemistry to *The Nation*; in fact, Garrison refers to him as Peirce's rival (probably Oliver Wolcott Gibbs). This notice is unassigned in Haskell's *Index to* The Nation, vol. 1.

Dmitri Ivanovich Mendeleev (1834-1907) was the most famous chemist in the world in the nineteenth century. After finishing college in 1855, he worked with Bunsen; he independently developed the concept of critical temperature, for which Andrews usually gets credit. He wrote the *Principles of Chemistry* between 1868 and 1870, and published his work on the periodic table in 1869. He traveled to many countries on behalf of his government, and in 1882 was given special honors by the Royal Society.

The work before us will be heartily welcomed by English and American chemists. It gives the mature views of a man of remarkable originality and talent, who has enriched chemistry with one of the finest and most fruitful discoveries ever made in that science—the so-called "periodic law." This law constitutes the keynote of the work. It is constantly referred to, and is brought in to explain, illustrate, and suggest. The work differs very materially from other books on the branch of science of which it treats. The main text embraces general descriptions only, while very ample details are given in footnotes at the bottoms of the pages, in much finer print. These contain a vast deal of varied information. The little-known work of the Russian chemists receives full justice, the physics of chemistry being the field in which the Slavonic mind seems to labor with especial pleasure. The author's own views differ in many respects from those now generally received, and will certainly not always find favor, though the ideas of an original and independent thinker must command a certain respect. Details are not always given correctly. It is not easy to keep up with a science which advances with such extraordinary rapidity—witness the three gigantic annual volumes of

*"Ad observationes vero sum hebeti visu, ad mechanica inepta manu, ad negotia domestica et politica curiosa et cholerica natura, ad continue sedendum (præsertim ultra justum et statum tempus epularum) infirmo corpore, etiam cum valetudo constat."

the German Chemical Society. Yet the author's industry has been noteworthy, and the omissions and inaccurate statements will probably be corrected in another edition.

The book is eminently readable. It is written in an agreeable, almost colloquial, style. The translation from the Russian is fairly good, so far at least as our own language is concerned, with an occasional quaintness which is not unpleasing. The work is in no sense a textbook, but, as the most original and suggestive treatise on inorganic chemistry which we possess, it is well worthy of the student's attention, and must be regarded as a very important addition to chemical literature.

54 (11 February 1892) 110

SCIENCE IN AMERICA

CSP, identification: the evidence here is internal, but quite convincing. The review of "The Comtist Calendar" was written by Peirce, as was the review of Lombroso's "Man of Genius," which is mentioned as forthcoming. See also: Fisch, *First Supplement*. The editorial reply is unassigned in Haskell's *Index to* The Nation, vol. 1.

TO THE EDITOR OF THE NATION:

SIR: In the able review of 'The Comtist Calendar' in the *Nation* of January 21 occurs the following:

"There is no civilized country where a great work of reasoning is less feasible than in ours. We have the most superb observatories and laboratories, it is true, but what would a Kepler, with his bad sight and awkward hand, be doing in such an establishment? Perhaps among our sixty millions there may just now live one such mind; certainly nobody is on the lookout for him. If he does exist, what is he doing? Reading examination papers?"

No, indeed, it seems quite unlikely that this should be his occupation. If it were, we should soon hear of him. He would find time enough to work out some great thing that we should be able to admire, and soon he would rise to positions of high honor and comparatively high pay. He is much more likely, as a true son of his time and his country, to be wasting his time in making money. In this pursuit, no doubt, he is successful, his great talent having taken that direction. In Germany it is the army that absorbs the best talent of the country, and with us it is the chase after riches that causes many a brilliant talent to be lost to the world.

The trouble is that our colleges do not, in general, attract great talents. But it is not the universal craving for wealth alone that produces this result. Much of the blame no doubt falls to other causes, and these I prefer to state in the words of the Rev. James T. Bixby (*Unitarian Review*, August, 1888):

"On the other hand, the man among us who, outside the college circle, does original and able work, may hope in vain for a college appointment. He is not in the line of promotion, and the solid phalanxes are unwilling to admit a new-comer to interfere with the rules of seniority. And even if he has gained admission to the

charmed circles, he must square his instruction with the demands of his superiors, or he is likely to be unceremoniously shown the door, as Prof. Felix Adler (by current rumor) was at Cornell University after his lectures on Buddhism, or as Prof. Alex. Winchell was at Vanderbilt University because of his belief in Pre-Adamites and Evolution."

To return to our Comtist Calendar review. Fermat and Jacob Steiner are mentioned among those who are barely assigned a place by Comte in his Calendar of great men, though Comte was himself a mathematician. Now, I would ask, how many are there, even among the readers of the *Nation*, who know anything about Jacob Steiner, or what he did? And this leads me to the subject I wish to urge namely, that it seems high time we should begin the study of modern geometry. So far as I am aware, not a single book upon the subject has been published in America, nor is the study of modern geometry pursued in any of our institutions by purely geometric methods. Yet modern geometry is by far more interesting, more systematic, more fruitful of results than the Euclidian geometry. Steiner, in the preface to his 'Systematische Entwickelung,' says: "There exists a small number of simple fundamental relations, from which the remaining great number of theorems can be derived, and without difficulty. Thus, by the acquisition of the few fundamental relations, one becomes master of the whole subject." And in the book itself, with wonderful ease, he derives almost everything from *one* fundamental relation, namely, the anharmonic ratio. The principle of duality there shows itself in its wonderful fecundity. Any one who will take the trouble of studying Chauvenet's "Introduction to Modern Geometry," appended to his 'Elements,' will understand the possibilities of the method, and, furthermore, will be prepared for the study of the larger works, say, Chasles's. It would be easy to compile a good text-book from easily accessible sources. Thus, the 'Traité de Géométrie,' by Rouché and De Comberousse, alone, in its various appendices, contains all or nearly all the material necessary, and collections of problems exist in goodly number.

Permit me to plead for one more neglected mathematical study, namely, the functions of complex variables (functions of $x + \sqrt{-1}\, y$). When we find that the realm of number is not of one dimension, so as to be representable upon a single infinite straight line, but that it has *two* dimensions, and requires a whole infinite plane to represent it, our horizon is at once infinitely widened. When next we find that by taking $z = f(x + iy)$ as the general form of the equation of a plane curve, and that we encounter no longer the puzzling difficulty of having the dependent variable become "imaginary" and meaningless, while undoubtedly it continues to vary, we see the usefulness of the method. And, finally, when we follow Rieman to his *mehrfach zusammenhängende Flächen*, we cannot help admiring the beauty of this modern analysis.

Shall we continue to plead that this is a new country, and that as yet we have no time for the sciences, except so far as they serve some technical purpose in the useful arts? The Australians seem to disdain such a plea, and we hear of vigorous scientific work done at Melbourne.—Respectfully, WERNER A. STILLE.

ST. LOUIS, JANUARY 25, 1892.

[These things are worth saying; albeit the first question, upon which our correspondent so lightly expresses himself, involves points of high debate among those who of late years have studied the problem of genius. We shall return to some of these in a notice of Lombroso's 'Man of Genius.' Meantime, we remark that Kepler, with all his advantages, did but one great work, and that that was by no means the sort of thing a college tutor "would find time enough" to toss off.

As to the teaching of the two important branches of mathematics to which Mr. Stille refers, we derive the following information, relating chiefly to the year 1888-9, from Prof. Cajori's document on the 'Teaching of Mathematics in the United States.' The Theory of Functions was the subject, at the Johns Hopkins University, of several courses; at Cornell of a two-years' elective course, with sessions thrice a week one year, twice a week another; at Harvard of an advanced course; at Princeton of a University course; at Madison of a "special advanced elective" (possibly not taken). Projective (modern synthetic) Geometry was the subject of a course at the Johns Hopkins; at Cornell was *required* for some students, elective for others; at Ann Arbor was studied (and *really* studied, as we happen to know) from Reye's admirable treatise; and at the Universities of Texas (where Cremona's charming book was used), Virginia, and South Carolina formed the subject of post-graduate lectures or examinations. This, though a poor showing, yet makes a beginning.—ED. NATION.]

54 (11 February 1892) 116

THE NON-EUCLIDEAN GEOMETRY

Geometrical Researches on the Theory of Parallels.

By Nicholaus Lobatchewsky. Translated from the original by George Bruce Halsted, A.M., Ph.D., ex-fellow of Princeton College and Johns Hopkins University, Professor of Mathematics in the University of Texas. Austin. 1891.

CSP, identification: MS 1365; Haskell, *Index to* The Nation. See also: Burks, *Bibliography; List of Articles.*

Nikolai Ivanovich Lobachevski (1793-1856) was a Russian mathematician. He demonstrated his genius in mathematics at the University of Kazan, where ultimately he became president. He published his geometry in 1829, after holding it back for several years. Philosophically, the development of non-Euclidean geometry shattered the notion of self-evident truth in its most secure stronghold, mathematics.

George Bruce Halsted (1853-1922) was a mathematician of considerable fame. He took his Ph.D. from Johns Hopkins in 1879 (where he was the first student of J. J. Sylvester), and in 1884 moved to the University of Texas, where he assumed the post of professor of mathematics. While at the University of Texas, he published 25 books on mathematics, and authored numerous articles for journals such as *The Monist*, *The Educational Review*, and *The Popular Science Monthly*.

Lobachevski's little book, 'Geometrische Untersuchungen,' marks an epoch in the history of thought, that of the overthrow of the axioms of geometry. The philosophical consequences of this are undoubtedly momentous, and there are thinkers who hold that it must lead to a new conception of nature, less mechani-

cal than that which has guided the steps of science since Newton's discovery. The book has been published many years—in fact, the essence of it was set forth before 1830; so long does it take a pure idea to make its way, unbacked by any interest more aggressive than the love of truth. In this case, the idea is lucid, easy, and convincing. Nobody with enough mathematical capacity to be able to understand the first book of geometry need fear the least difficulty in mastering Lobachevski's tract; and really it is high time that every thinking man and woman should know what is in it.

In the pre-Lobachevskian days, elementary geometry was universally regarded as the very exemplar of conclusive reasoning carried to great lengths. It had been the ideal of speculative thinkers in all ages. Metaphysics, indeed, as an historical fact, has been nothing but an attempt to copy, in thinking about substances, the geometer's reasoning about shapes. This is shown by the declarations of Plato and others, by the spatial origin of many metaphysical conceptions and of the terms appropriated to them, such as *abstract*, *form*, *analogy*, etc., and by the love of donning the outer clothing of geometry, even when no fit for philosophy. For instance, one of the remarkable features of geometry is the small number of premises from which galaxies of theorems result; and accordingly it has been the effort of almost all metaphysicians to reduce their first principles to the fewest possible, even if they had to crowd disparate thoughts into one formula. It did not seem to occur to them that since a list of first principles is a work of analysis, it would not be a small number of elementary propositions so much as a large number that would bespeak its thoroughness. Admiration for the elements of geometry was not, however, confined to metaphysicians. Euclid's treatise was acknowledged by all kinds of minds to be all but absolutely perfect in its reasoning, and the very type of what science should aim at as to form and matter.

In the empyrean of geometry there was but one little speck—the theory of parallels. Euclid had had a difficulty in proving the sum of the angles of a triangle to be not less than two right angles. His treatment of the subject betrays a very profound study of it; for instead of slipping over the difficulty unaware, as forty-nine out of fifty mathematicians would have done, instead of even bringing the necessary assumption to a persuasive shape, he takes as his fifth postulate (or 11th axiom, in incorrect editions) a proposition that begs the question in the frankest manner—namely, if two straight lines in a plane are met by a third making the sum of the inner angles on one side of this third less than two right angles, then these two lines will meet on that side if sufficiently produced. Innumerable attempts were made to demonstrate this; but, at length, the efforts of Legendre and others made it pretty clear that this proposition could be deduced only from some other nearly equivalent. The least unsatisfactory assumption ever proposed was that of Playfair, that if of three unlimited straight lines lying in one plane two intersect, the third must cross one or both. It was at this point that Lobachevski cut the knot by supposing Euclid's postulate untrue, and showing that the result was a perfectly consistent system of geometry which may, for all we can yet observe, be the system of nature. All this time, Euclid's proof (elements, Bk. I., props. 16 and 17) of what substantially amounts to the proposition,

that the sum of the three angles of a triangle are *not greater* than two right angles, was regarded as perfect. It was not till 1854 that Riemann first discovered that, though accepted for two thousand years as conclusive (and it stands to-day unchanged in almost all the text-books), this pretended proof is really quite fallacious. It is plain that it is so, because it uses no premises not as true in the case of spherical as in that of plane triangles; and yet the conclusion drawn from those premises is known to be false of spherical triangles.

The truth is, that elementary geometry, instead of being the perfection of human reasoning, is riddled with fallacies, and is thoroughly unmathematical in its method of development. It has in some measure confused all mathematics, by leaving unnoticed most of the really fundamental propositions, while raising to an undue rank certain others almost arbitrarily selected. It leads young men into bad logical ways; and it causes pupil and teacher to think that whoever has difficulty with this sophisticated logic is wanting in aptitude for the apprehension of mathematics. The study of geometry ought to begin with the theory of perspective. Let a man be supposed to stand on an unbroken sandy plain. Let him fix a needle upon a post, and set up a plate of glass in a steady position, and draw a perspective picture upon the glass by placing his eye so as to bring the needle point over each point in the sand to be represented and marking it on the glass in the same line of sight. The horizon is where the lines of sight just skim the surface of the rounded earth. These lines of sight form a cone, and their perspective representation will be the section of this cone by the plane of the glass. But for simplicity let it be supposed that the earth is flat and indefinitely extended, so that the *plain* is also a *plane*, and an unbounded one. Then every straight line in the sand will have a straight line for its picture, for all the lines of sight from the needle-point to points in that straight line will lie in one plane; and this plane will cut the plane of the glass in a straight line.

Lobachevski and Riemann cast no manner of doubt upon the geometry of perspective, so far as this is confined to questions of incidence and coincidence. But when it comes to the measurement of distances and angles, their objections begin. According to the Euclidean notions, the infinitely distant parts of an unbounded plane would be represented in perspective by a straight horizon or vanishing line. But Lobachevski says we cannot be sure that this line would be straight, that may be it would be a hyperbola like the perspective of the terrestrial horizon; and, in fact, the straight line being only a special case of the hyperbola, it is proper to say that such is its form. Riemann, however, points out that we cannot even be sure there would be any such line at all, for we cannot be sure that space has any infinitely distant parts, since it may be that if we were to move off in any direction in a straight line, we might find that, after traversing a sufficient distance, we had got around to our starting-point again.

Prof. Halsted's translation (which, while our notice has been waiting, has reached, we are glad to see, a fourth edition) is excellent; his useful bibliography of non-Euclidean geometry was already well known. We could only wish there were a more copious appendix. The work of Lobachevski, though simple and convincing, is not what would now be considered a scientific presentation of the sub-

ject, and is open to a good deal of criticism. A new synthetic exposition is much needed, and might well accompany a collection of the contributions of Lobachevski, Bolzai, Riemann, Cayley, Klein, and Clifford.

54 (18 February 1892) 131

GEOMETRY NOT MATHEMATICS

Maxime Bôcher (1867-1918) was graduated A.B. from Harvard in 1888, and Ph.D. from the University of Göttingen in 1891. He began as an instructor of mathematics at Harvard in 1891, and in 1904 was given full professorship. He was a member of the American Mathematical Society (president, 1913-1914) and of the National Academy of Science.

TO THE EDITOR OF THE NATION:

SIR: A dissatisfaction with the method in which geometry is at present taught finds frequent expression, a case in point being the review of Mr. Halsted's translation of Lobachevsky in the last number of the *Nation*, and, by a peculiar coincidence, in the communication by Mr. Stille on "Science in America," in the same number. In the one case, the author desires to have geometry attacked by the beginner from the point of view of perspective; in the other, criticism is not aimed at the elementary presentation of the subject, but a spirit of discontent is visible which expresses itself in the demand that more students should go beyond the dull elements and breathe the purer air of the "modern geometry" of Jacob Steiner.

Does not the difficulty lie deeper than is suggested by either of these two writers? There seems to be a fundamental misconception of the nature of geometry in the minds not merely of the general public, but also of almost every teacher of the subject. "Mathematics is the science which draws necessary conclusions," so wrote the late Prof. Benjamin Peirce in 1870, and no clearer and more accurate definition could be imagined; and yet it has produced but little effect as yet on students of geometry. Geometry is the simplest of the natural sciences to which, owing to its simplicity, mathematics (*i. e.*, the methods of formal logic, either with or without the assistance of symbols) has been applied with such wonderful success, and which of late years has so richly repaid its debts to mathematics in the hands of Riemann, Clebsch, and others still living. We should laugh at the idea of teaching the mathematical theory of electricity before the student knows in a qualitative way what electricity is, either by a description of the phenomena or, better still, by actual laboratory contact with them. Why should not the same rule apply to geometry? Even then logic need not be entirely discarded, but let it assist the learner, not clog his progress. It is not a perfectly safe guide, as history tells us, even in the hands of a master, and the learner is almost as safe in trusting to his "common sense" as to his own unaided deductions.

The training of the logical faculty is, moreover, of far less vital importance than the development of the mind which results from this first real draught from the cup of science, which strengthens a healthy imagination, and should even yield an

aesthetic enjoyment. If we could lead the student first to see the truth of a proposition, and then, perhaps much later, to prove it, we might hope in time to have mathematicians in America. For every mathematical discovery is made in this way, let the mathematician conceal his footprints as he will; it must come as an intuition, and the man to whom it has thus come is its discoverer, even though he never succeed in finding a proof.

Of course there will still remain innumerable points of detail to consider. The wonderful geometric developments of the last hundred years should not be completely ignored; "projective" rather than metrical properties of figures might be brought to the front; but this will all regulate itself when once it is really understood that geometry is not mathematics, but is a physical science to which mathematics may be applied. MAXIME BÔCHER.

CAMBRIDGE, February 13, 1892.

54 (25 February 1892) 151-153

THE MAN OF GENIUS

The Man of Genius.

By Cesare Lombroso, Professor of Legal Medicine in the University of Turin. [The Contemporary Science Series.] Charles Scribner's Sons. 1891.

CSP, identification: MS 1365; Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MSS 1374 and 1374(s) (drafts).

Prof. Lombroso comes to us with a proposition not absolutely new, but which he makes claim now to prove for the first time. It is that genius is a mental disease, allied to epileptiform mania and in a lesser degree to the dementia of cranks, or mattoids, as he calls them; so that, far from being a mental perfection, it is a degenerate and diseased condition. The inevitable corollary must be, though Prof. Lombroso does not draw it, that the whole of civilization is due to insanity. If so, it is a disease like pearls, fat livers, and ambergris, which we had better try to propagate, in others. But our Napoleons, our Pythagorases, our Newtons, and our Dantes must no longer run at large, but be confined in Genius Asylums as fast as they betray themselves.

To prove his proposition, Prof. Lombroso proceeds inductively. In order, therefore, to judge of his work, we will examine the first induction he offers with some care. This first generalization is that geniuses are, on the average, of smaller stature than ordinary men. Here is his reasoning:

"Famous for short stature as well as for genius were: Horace (*lepidissimum homunculum dicebat Augustus* [Lombroso fails to note that this implies that Augustus was himself large]), Philopœmen, Narses, Alexander (*Magnus Alexander corpore parpus erat*), Aristotle, Plato, Epicurus, Chrysippus, Laertes, Archimedes, Diogenes, Attila, Epictetus (who was accustomed to say, 'Who am I? A little man'). Among moderns one may name Erasmus, Socinus, Linnæus, Lipsius, Gibbon, Spinoza, Haüy, Montaigne, Mézeray, Lalande, Gray, John Hunter (5 ft., 2 in.), Mozart, Beethoven, Goldsmith, Hogarth, Thomas Moore,

Thomas Campbell, Wilberforce, Heine, Meissonier, Charles Lamb, Beccaria, Maria Edgeworth, Balzac, De Quincy, William Blake (who was scarcely five feet in height), Browning, Ibsen, George Eliot, Thiers, Mrs. Browning, Louis Blanc, Mendelssohn, Swinburne, Van Does (called the Drum, because he was not any taller than a drum), Peter van Laer (called the Puppet), Lulli, Pomponazzi, Baldini, were very short; so, also, were Nicholas Piccinino, the philosopher Dati, and Baldo, who replied to the sarcasm of Bartholo, 'Minuit præsentia fama,' with the words 'Augebit cetera virtus'; and again Marsilio Ficino, of whom it was said, 'Vix ad lumbos viri stabat.' Albertus Magnus was of such small size that the Pope, having allowed him to kiss his foot, commanded him to stand up, under the impression that he was still kneeling. When the coffin of St. Francis Xavier was opened at Goa in 1890, the body was found to be only four and a half feet in length.

"Among great men of tall stature I know only Volta, Goethe, Petrarch, Schiller, D'Azeglio, Helmholtz, Foscolo, Charlemagne, Bismarck, Moltke, Monti, Mirabeau, Dumas père, Schopenhauer, Lamartine, Voltaire, Peter the Great, Washington, Dr. Johnson, Stein, Arago, Flaubert, Carlyle, Turgeneff, Tennyson, Whitman."

Now we remark, at once, that the thirty names in the latter list are nearly all great names; while to collect the sixty in the former list, the anthor has been compelled to descend to Narses, Chrysippus, Laertes, Mézeray, Lalande, Thomas Campbell, De Quincey, William Blake, Does, Laer, Pomponazzi, Baldini, Piccinino, Dati, and Baldo! Nor are the statements always accurate. As for Epictetus, his expression of submission to God has nothing to do with his stature, concerning which there seems to be no information. Ancient references to his person merely allude to the story of his master breaking his leg. It is quite unlikely that Plato was diminutive, because his beauty was such that he was believed to be the son of Apollo. The statements about Epicurus and Diogenes are very doubtful; and that about Archimedes far from certain. Attila was short, like all Huns, but not shorter than the average. Balzac, instead of being small, was colossal; Spinoza and Hunter were about of medium height, notwithstanding the measurement given of the latter; George Eliot and Linnæus were somewhat above the average; and Erasmus, though not tall, was not noticeably short. Let us be glad that Signor Lombroso's credit for fairness is saved by one mistake on the other side, Schopenhauer being under the middle height.

Making these corrections and disregarding the insignificant names, the two lists are not far from equal. Taking, however, a list of great men* that was drawn up some years ago, without the slightest thought of their stature, and which therefore may be supposed to afford a fair sample, we have looked up the heights of as many of them as we readily could, with the following result:

Short Men.—Alexander, Archimedes (?), Aristotle, Francis Bacon, Beethoven (5 ft., 6 in.), A. Comte, Descartes, Epicurus (??), Erasmus, Faraday, Frederick the Great, Garrick, Jacob Grimm, Harvey, Warren Hastings, Horace, Howard, Kant, Thomas à Kempis, Kepler, Locke, Louis XIV., Mendelssohn,

*That is to say, of those who make a certain impression upon us in advance of any critical examination.

Montesquieu, Mozart, Napoleon, Schopenhauer, Wagner, St. Francis Xavier—29.

Middle-sized Men.—Attila, Burns (5 ft., 10 in.), Calvin, Camoens, Cromwell (5 ft., 10 in.), Dante, Jeanne Darc, George Eliot, John Hunter, Lagrange, Linnaeus, Machiavelli, Mahomet, Clerk Maxwell, James Mill, Milton, Rachel, Adam Smith, Spinoza—15.

Tall Men.—Alcibiades, Aquinas, Balzac, Bismarck, Boyle, Cæsar, Carlyle, Champollion, Charlemagne, Clive, Columbus, Constantine, Darwin, Dürer, Dumas père, Queen Elizabeth, Emerson, Fielding (over 6 ft.), Gilbert, Goethe, Hawthorne, Helmholtz, Alexander von Humboldt, Lavoisier, Leonardo da Vinci, Lessing, Abraham Lincoln, J. S. Mill, Mirabeau, Molière, Moltke, Peter the Great, Petrarch, Rumford, Schiller, Shelley (5 ft. 11 in.), Mrs. Siddons, Tennyson, Titian, Voltaire, Washington, Daniel Webster, Wellington, William the Silent—44.

This is an honest induction, from a list of instances drawn up without reference to the character for which the sample was to be examined, and seems to show that great men are a little above the average height.

It may perhaps be suspected that the above quotation does not do justice to the general run of Prof. Lombroso's reasonings; but, in point of fact, the induction examined is one of the best in the book, being quite exceptional as showing some effort, however feeble, to be fair. His ordinary method is to take up each symptom of insanity, and to search high and low for instances which may look as if some men of genius have had that symptom. Such reasoning would be rejected without hesitation were there not such a deluge of cases as must give us pause. In considering their value as premises, the first question to be asked is how many men there are in universal biography whom Prof. Lombroso would call geniuses. That his standard is pretty low, his first list in the above extract suffices to show. He never puts himself to the trouble of making the reader or himself understand what he means by "genius." He delights in repeating that by *genius* he does not mean *talent*, and finds fault with Galton for confusing these qualities. But the truth is, Galton is far too sound a reasoner to potter over the meaning of two popular words, mere accidents of language. Such categories can be of no use in reasoning until they have passed through the fire of a scientific revision such as Prof. Lombroso seems little to dream of. He covers his confusion of thought by the commonplace that "genius is original, talent not." Of course, maniacs are original enough, if the quality of the product is nothing. But to look at his instances, he does not seem to stickle for originality very much. Among his geniuses we find Mrs. Southey, whose nearest approach to brilliancy was going crazy; Nathaniel Lee, absurdest of dramatists; Bishop Dupanloup; the poet Thomson; Buhl, whoever he may be; Sir Everard Home; Ann Lee, the Shakeress; Lord Palmerston; Florence Nightingale; George Washington, a truly great man, but hardly, one would suppose, within Lombroso's category of genius; Prof. Asaph Hall, a remarkably sane mind; Talleyrand; Mrs. Stowe; William Pitt; Richard Steele. Addison and Pope are mentioned as men of genius, and in one place even as "normal" men of genius; yet when their traits do not seem to

fit the theory, they are set down as men of talent, merely. In short, the author ranks almost anybody as a genius whom it happens to be for the moment convenient to reckon as such.

There is a well-known book called Phillips's 'Index of Biographical Reference,' said to contain over 100,000 names. We have set down in a list the first name on every twenty-fifth page; and we find among these names, thirty-nine in number, no less than seven that impress us as fully as distinguished as some of Lombroso's instances of genius. Namely, these seven are: Biela, for whom a comet is named; Sir James R. G. Graham, a well-known statesman; Naumann Köprili Pasha, the last of his celebrated family; Gen. Longstreet; Alexis Piron, the French satirist; Robert Semple, the early Scottish poet; and Evelina Stading, the contemporary Swedish painter. In that proportion there should be no less than $100{,}000 \times 7 + 39$, or 18,000 "persons of genius," in Lombroso's sense of the term, named in Phillip's Index. But, notwithstanding the diligent researches of the learned Italian, it may well be supposed that five-sixths of these (or whatever Italian names might replace some of them) could have symptoms of insanity without his being likely to know of it; so that we will suppose he is drawing his cases from only 3,000 geniuses.

The question next arises how much insanity he finds. There are, perhaps, a thousand cases of symptoms of insanity in the book; but they are, for the most part, of the slightest nature—to show how slight, we here give the first case on every tenth page for the first hundred pages:

(1.) Volta had the largest brain known (p. 10). The next largest was that of an idiot.

(2.) Dante wrote:

> "Son un che, quando
> Amor mi spira, noto, ed a quel modo
> Che detta dentro vo significando."
>
> —(p. 20.)

"I am one that, when love inspires, note, and in what manner he dictates within, proceed to express." This is supposed to indicate something like epilepsy.

(3.) Boileau could not hear any one praised, not even his shoemaker, without annoyance (p. 30).

(4.) Ann Lee saw Christ coming to her (p. 40). This is supposed to be a hallucination of *genius*.

(5.) Tolstoi confesses that philosophical skepticism at one time brought him to a condition approaching insanity (p. 50).

(6.) Petrarch's love-misery was a mere pretext for writing poetry (p. 60). This is supposed to be an example of that insensibility which is said to be a common trait of genius and insanity.

(7.) There was insanity in Baudelaire's family (p. 70).

(8.) Swift had a softening of the brain (p. 80). This came on ten years after he wrote 'Gulliver'; and the subsequent disease is supposed to be an evidence of derangement at the time the great work was writing.

(9.) The story-teller Hoffmann was a drunkard (p. 90).

Now, we may fairly assume that for each case of real insanity there would be at least ten cases of symptoms like the average of the above; for these would not occur all ten in one person. If so, Lombroso's thousand instances imply only about a hundred cases of insanity; and 100 cases of insanity *at some time in the lives* of 3,000 persons of intensely active brains, and for the most part in uncomfortable circumstances, is not extraordinary. Certainly, it by no means compels us to suppose that the whole body of them were more or less crazy their whole lives long. On the whole, therefore, the main argument of the book proves nothing and renders nothing probable. At most, it creates a problematic state of mind, and makes us wish to see the subject treated with a stricter attention to the logical conditions of valid induction.

But Prof. Lombroso presents another and much stronger argument. Namely, he shows that an unbroken series of cases exists, ranging from those where there is undoubted genius through imperceptible gradations to cases of undoubted mania, in which last the patient performs intellectual feats of which he would be utterly incapable in his normal state. Thus, he may write poetry, speak a foreign language, or play a game of chess, being unable to do the same thing in his ordinary health. A patient said to his physician, who thought him convalescent, "I am not quite cured; I am still too clever for that. In my natural condition I am stupid; wait, and I shall become so again" (p. 168). In these cases there were *other* symptoms of mania. But as for this disease of genius, if it consists *solely* in the brain functioning more perfectly than when it is well, why, what a very peculiar disease this mental disease must be! There would certainly be no difficulty in finding an unbroken series of cases passing by imperceptible gradations from cases of the working of undoubted genius to cases of the working of plain common sense. Accordingly, if the first series proves that genius is insanity, the first and second together prove that good sense is insanity.

But, after all, there is a puzzle about the matter not easily resolved; and those who are themselves visited with genius have always been ready to admit there is something like a malady about it. No doubt, our ordinary sense of behaving rationally is in the main, though not entirely, an illusion. The right hand, for instance, is connected with a certain part of the brain, and that is joined by commissures to other parts connected again with the eye, ear, tongue, etc.; and it is the structure of the commissures, medial and lateral, between different parts of the brain which determines how we shall act under given stimuli. It is true that, no matter how, we can control our actions to a certain extent; at a short notice, only slightly, but if time for preparation be allowed, a great deal. We can force ourselves to take habits, certain commissures becoming partially atrophied, while others are brought into activity under exercise. But in the main we behave as it is our nature to, like wild animals; and, as it happens that our nature is adapted to our circumstances, we take occasion to compliment ourselves upon our rationality. If the brain becomes diseased, the connection between certain parts get broken, and we begin to act in new ways. As we acted right in the main before, to act differently is to act ill. Yet it may happen, in special cases, that the breaking down of certain commissures may cause certain special actions to be done better

than before; because the wave of nerve action is restricted to certain channels and its dissipation prevented. Indeed, it is probable that an excess of medial commissures, or those between the two halves of the brain, causes stupidity, deliberation becoming impossible when the thinking vessel leaks so fast. If so, we can see how disease of the brain may cause an improvement in the general intelligence.

Now, the brain of a genius, say of a great mathematician, a Gauss or a Dirichlet (of which two brains Prof. Lombroso gives drawings), is seen at a glance to be quite unlike that of a common man. It may be larger; it is certain to be far more complicated and implicated. These foldings imply that the parts are more disconnected. Its connections of parts being different, such a brain must act differently from common brains; and consequently it will in general be less adapted to the ordinary purposes of life. It is not disease, but greater development; yet the unfortunate man whose shoulders have to carry it, becomes the victim of his own higher organization. Of course, there will be special things for which such a highly complicated brain will be specially adapted; and in being exercised continually on those things, as it naturally will be, it will grow more adapted to them. Such actions will not be insane; they will be like the operations of common sense, only more perfect. In doing such work, such a brain will take steps for the advancement of mankind of which ordinary heads would be quite incapable. The world will reap the benefit of it, and the unfortunate individual will have to pay for it. But, circumstances being generally unfavorable, the energies of such a brain are largely spent in vainly trying to make it do things for which it is entirely unadapted, though other brains do them with ease. The result is, that first derangement, then disease ensues, and we get the phenomenon of aberrations of genius.

54 (3 March 1892) 168-169

THE PERIODIC LAW

Attributed to Peirce by Fisch in *First Supplement* (no evidence cited). Possibly the reason for assigning this comment to Peirce lay in a belief that he had been the reviewer of Mendeleef's *Principles of Chemistry* at 54 (4 February 1892) 97. But as we indicated there, Garrison's letter to Peirce (MS L 159.12) appears to contradict such a view. Therefore, chances that this comment is by Peirce appear to be decreased. The reply is unassigned in Haskell's *Index to* The Nation, vol. 1.

TO THE EDITOR OF THE NATION:

SIR: Without detracting an iota from the fame of Prof. Mendeléef, who is one of the greatest chemists of this or any age, I may venture to correct the statement made in the *Nation* of February 4 (review of 'The Principles of Chemistry'), that this great Russian scientist was the discoverer of "the Periodic Law." In 1882, it is true, the Royal Society of London did award the Davy medal to Prof. Mendeléef, conjointly with Lothar Meyer, "for their discovery of the periodic relations of the atomic weights." This action was taken in ignorance of the fact that in 1864, a number of years before Mendeléef and Meyer had begun their investi-

gations, Prof. John A. R. Newlands of England had pointed out the law of octaves and the periodicity of functions. For some strange reason the discovery attracted little notice at the time, but after the Royal Society had honored his rivals, Prof. Newlands claimed recognition as the original discoverer. The fact having been conclusively established, the Royal Society in 1887 awarded the Davy medal for that year to Prof. Newlands "for his discovery of the Periodic Law of the Chemical Elements," adding, "Although in the somewhat less complete form in which the law was enunciated by him, it did not at the time attract the attention of chemists, still, in so far as the work of the two foreign chemists above mentioned was anticipated, the priority belongs to Mr. Newlands" (*vide* Proceedings of the Royal Society of London, 1887-'88, vol. 43, p. 195).

Even Newlands was almost anticipated by Beguyer de Chancourtois, who was catching a gleam of the great truth enunciated by Newlands, and elaborated by Mendeléef, when he brought out his treatise in 1863, entitled, 'Vis Tellurique, classement naturel des corps simples ou radicaux, obtenu au moyen d'un systéme de classification hélicoïdal et numérique.'—Yours truly, C. DE K.

NEW YORK, February 5, 1892.

[If our correspondent will read carefully what the Council of the Royal Society say about Newlands, he will see that they do not commit themselves very far. In truth, the step taken by him was not a difficult one. The principal precursor of Mendeléef was, as it seems to us, that penetrating intellect, Josiah P. Cooke, who first proved that all the elements were arranged in natural series. For though his classification was not free from uncertainties, which have since been removed, nor from such mistakes as the association of borax and silicon according to the ideas of that day, yet his memoir could leave no real doubt of the general serial arrangement; and this was a great advance upon what Dumas and others had done. After the new atomic weights came in, it was inevitable that every well-informed and ingenious chemist, in speculating upon the relations of the properties and atomic weights of the elements, should be led to write the different series in a certain succession, somewhat as follows:

Li	Na	K	—	Rb	Ag	Cs	
7	23	39		85	101	132	
—	Mg	Ca	Zn	Sr	Cd	Ba	Pb
	24	40	65	87	112	137	206
C	Si	Ti	—	Zr	Su		
12	28	48		90	118		
N	P	V	As	Nb	Sb	—	Bi
14	31	51	75	94	122		210
O	S	Cr	Se	Mo	Te	W	
16	32	52	78	96	128	184	
Fl	Cl	—	Br	—	I		
19	35^1_2		80		127		

It would also be remarked that the atomicities of these series seemed to be i, ii, iv, iii, ii, i. No doubt, many a chemist in those days drew up a table more or less

like this, but refrained from publishing it, feeling that a great discovery was imminent. An obscure American chemist actually assigned this as a reason for not attaching his name to such a table. Yet this was all, if not more than all, that Newlands did; and his papers, in a very widely circulated journal, made no sensation.

Of all those who were puzzling over the relations of the elements, Mendeléef alone had the sagacity to discern the true scheme of relationship; and this he was aided in doing by his more profound study of the relations between their general properties. His wonderfully vivid conception of the scheme, as well as his clear perception of its evidence, is shown by the formal and audacious descriptions he gave of the properties of several elements then undiscovered, but required to fill blank spaces, and by the subsequent triumphant verification of his predictions, especially of what seemed the most wild and improbable of all—that relating to gallium. Very few inductions in the whole history of science are worthy of being compared with this as efforts of reason. The work of Lothar Meyer also has great value as supplementary to that of Mendeléef. In all branches of physics, inductions concerning periodic laws are most difficult to establish; and the pursuit of indications of periodicity leads all but the most wary of minds to some wild-goose chase. Yet how quickly and completely the periodic law of chemistry was put out of doubt!

While we are upon this subject, we may suggest that if there are atomicules that are Boscovichian points, two or more of these might attract one another according to such a law that they would approach one another in spirals without ever becoming separated; and in this way it might be supposed that the atoms of most of the existing chemical elements have been built up from a few different kinds, and that it is in this way that the relations between the atomic weights have arisen.—ED. NATION.]

54 (3 March 1892) 169

SCIENCE IN AMERICA

Probably by CSP. This is a continuation of the comments that began with Stille's letter at 54 (11 February 1892) 110. Therefore, it is very likely that Peirce wrote this reply. See also: Fisch, *First Supplement.* The reply is unassigned in Haskell's *Index to* The Nation, vol. 1.

TO THE EDITOR OF THE NATION:

SIR: The fine aphorism of Calgacus, "Omne ignotum pro magnifico est," has its obverse in the fact that "Omne proximum pro ignobili est." One would hardly suspect, from reading the interesting and excellent letter of Mr. Stille in your issue of the 11th inst., that the doctrines of the complex variable, of conform depiction—*zusammenhangend und in den kleinsten Teilen ähnlich*—of multiply compendent multifoliate Riemannian surfaces, with their *Windungs- und Verzweigungspunkte, lacets fondamentaux, et id omne genus*, were taught carefully and studied zealously almost within ear-shot of the writer. Yet such is the case. The enclosed conspectus shows that courses in such subjects not only are offered—there is little art in that—but are actually pursued at the University of the State of Missouri. Next year the class will meet not three times, as now, but

five times per week, throughout the year, and follow in the fresh tracks of Hermite, Halphen, and Weierstrass. But even Freshman students here are familiarized with $x+iy$ and imaginary exponentals, while for the whole body of geometric discipline the researches of Bolyai, Lobatchevsky, and Riemann are regulative.—Respectfully, WILLIAM BENJAMIN SMITH.

COLUMBIA, MO., February 19, 1892.

[We are pleased to learn that mathematics is so deeply studied in Missouri. The lectures of Hermite form a good introduction to the theory, and the work of Halphen to the practical side, of the doctrine of functions. As we are unacquainted with any treatise of Weierstrass capable of being used for a text-book, the study of this author (concurrently, perhaps, with that of the orations of Calgacus) cannot fail to impress us very much. Nothing is said of projective geometry, which ought to be a compulsory study where there are any compulsory studies; but we cannot expect a relatively small university to cover every branch of mathematics, nor would such an ambitious attempt be wise. We are glad the ideas of Riemann (including doubtless those of Cayley and Klein) are regulative, and that Lobatchevsky is at hand for students who wish to approach the non-Euclidian geometry by the elementary method. Bolyai is a make-weight.—ED. NATION.]

54 (10 March 1892) 190-191

IS INDUCTION AN INFERENCE?

CSP, identification: internal evidence here is very good. Furthermore, this reply continues a series of letters and replies arising out of Peirce's review of Halsted's translation of Lobachevski at 54 (11 February 1892) 116. See also: Fisch, *First Supplement*. The editorial comments are unassigned in Haskell's *Index to* The Nation, vol. 1.

TO THE EDITOR OF THE NATION:

SIR: In the communication of Maxime Bôcher in the *Nation* of February 18, referring to the unsatisfactory condition of geometrical studies in our schools, he says:

"If we could lead the student first to see the truth of a proposition, and then, perhaps much later, to prove it, we might hope in time to have mathematicians in America. For every mathematical discovery is made in this way; let the mathematician conceal his footprints as he will; it must come as an intuition, and the man to whom it has thus come is its discoverer, even though he never succeed in finding a proof."

Is not this statement of general application? Is ever the boundary of knowledge advanced in any other way, whether in mathematical or physical science? Reasoning serves merely to verify and confirm the intuitions of genius by applying general principles to concrete cases; but *inference*, in any proper sense of the word, cannot *advance* knowledge—it cannot grasp more than is contained in the premise.

I know the common doctrine represents this as the province of induction. But induction is not reasoning; it is intuition, or happy guessing, if you like. Take any so-called inductive syllogism, and substitute for the major premise what is tacitly assumed, and it is converted at once into a strictly deductive argument. By no possible reasoning, or *inference* proper, could Newton have attained to the law of gravity. It was a happy guess, an inspiration of genius. It was based on wide knowledge, it is true; but it was not a necessary consequence of that knowledge. Assuming the law to be true, reasoning applied it, and the conclusions were found to agree with experience and observation. But the conception of the law was an intuition; it was not a *conclusion* involved in any known premises.

Take Whately's old school-book illustration of an inductive syllogism: "The ox, sheep, goat, deer, bison, etc., are a sample of the class 'horned animal,' or represent the class; the ox, sheep, etc., are ruminants, therefore all horned animals are ruminants." But what do you mean by "the ox, sheep, etc., are a sample, or represent the class 'horned animal' "? Evidently you mean that whatever is true of them is true of all horned animals. Unless this is true, your conclusion is worthless. But if you substitute this, which is tacitly assumed, the argument is deductive, not inductive. The same is true in every case of so-called inductive argument. The real induction—the advance in our bounds—is contained in the assumption that what is true in the cases we know will be found true in all cases having a certain other similarity to these. But this is not an inference, it is not reasoning, it is intuition. J. McL. S.

DAYTON, O.

[It is plain that in no case of genuine induction is *everything* that is true of the sample true of the whole class; so, according to our correspondent, all inductions must be worthless. But he supports this position by nothing, nor does he notice a single one of the objections to it which have been urged from the days of Philodemus to our own, and are found in common American books, such as the 'Studies in Logic, by Members of Johns Hopkins University.' The main distinction between induction and statistical deduction (the only kind of deduction which bears much resemblance to induction) is that the prediction made by the deductive form of inference is applicable in many cases, and, while it may be false in any one, it will probably and approximately be true in the long run; but the inductive conclusion, on the other hand, may be false—only, if so, the further pursuit of the same method will in the long run probably and approximately *correct* it. The distinction is between getting confirmed in the long run and being corrected in the long run.

Our correspondent's proposition that induction is not inference will meet with less favor than it might do, owing to his use of the unfortunate word *inference*, which most people particularly appropriate to the designation of uncertain presumption, amounting to little more than conjecture. But in its philosophical sense inference is defined in the 'Century Dictionary' as "the formation of a belief or opinion, not as directly observed, but as constrained by observations made of other matters or by beliefs already adopted." For instance, we wish, let us

suppose, to know whether among negroes male births are more numerous than female births or not. This general proposition cannot be directly observed. We turn, then, to the compendium of the tenth census, and find a considerable excess of female over male births there recorded among negroes in this country for one year. This brings us to the belief that the same phenomenon would generally occur among large populations of negroes, and our proceeding is *inference* according to the received definition, as certainly as it is induction.

But the 'Century Dictionary' adds this remark: "The act of inference consists psychologically in constructing in the imagination a sort of diagram or skeleton image of the essentials of the state of things represented in the premises, in which, by mental manipulation and contemplation, relations which had not been noticed in constructing it are discovered." This recognizes an intuitive or perceptive element as an important part of reasoning itself—a doctrine which results from the study of the logic of relatives, where the perceptive element comes into great prominence. Proof believed to be conclusive has been offered of the truth of this view, which has been accepted by many philosophers. "J. McL. S." seems to offer no rational objection to it. He says, indeed, that Newton's discovery was "a happy guess, an inspiration of genius"—that is, it came directly from on high, or from the action of chance, and was not based upon any knowledge already in Newton's mind, or dependent from any luminous conception which he had carefully worked out. But the truth is, our correspondent seems to have taken his notion of reasoning from the 'Elements of Euclid,' which was written before logic was much understood, and from texts-books of logic inspired by theological doctors. If, as he says, "reasoning serves merely to verify [something] by applying general principles to particular cases," there is next to no reasoning in mathematics; for the *nodus* of a mathematical demonstration does not consist in the application of a general principle.

The original passage quoted has some truth in it. The mathematician usually sees a thing dimly before he sees it clearly. But between the processes of coming to see a mathematical truth as probable, and coming to see it as evident, there is no radical difference. It is all reasoning, and, as such, it is an act of perception—or, rather, of experiment, followed by observation.—ED. NATION.]

54 (17 March 1892) 211-212

EXPERIMENTAL PSYCHOLOGY

TO THE EDITOR OF THE NATION:

SIR: Your review of Prof. Lombroso's book, 'The Man of Genius,' in the *Nation* of February 25, shows, I think, that his method of inquiry is of very little value. His first induction (in reference to the connection between genius and stature) more especially seems open to various objections, and though much labor must have been bestowed upon the subject, how many great names are omitted, and how many obscure names are brought in seemingly because some account of stature was accessible! Your statement of the case at once brought to mind Lotze, who was a remarkably small man, and who, in his lectures on psychology,

would mention as a notable fact that excellence in mental powers, also culture and refinement, were usually accompanied by smallness of stature. His listeners smiled, perhaps, and the small men in the audience felt "big" for the time being; but no further signs of mirth were perceptible, the Professor being much honored.

Of all the subjects of study concerning our own nature there is none that we would pursue more eagerly, if some sort of success seemed attainable, than the nature of the human mind, its forces, aptitudes, capabilities, and the conditions favoring their development. But it must be confessed that all this study is still in its infancy, and that only lately some attempts are being made at an experimental psychology. We were wont to treat psychology about as Seneca treats physics in his 'Naturales Quaestiones,' namely, by philosophizing about it. And this method, indeed, has its advantages, for the less we know of a thing, the easier it is to philosophize about it; the generalities flow more easily when not hampered by awkward facts, and these fine generalities are just indefinite enough to allow each reader to accommodate his own notions to the author's learned-sounding phrases.

The most valuable power of a scientific mind is the power to draw correct inferences from given premises, and scientific geniuses are superior to other minds chiefly in this logical power. Let us here make a useful distinction. He who can see a remote and difficult inference is *tiefsinnig* (deep); whereas he who perceives all the premises that must be taken into account as bearing upon the conclusion in question, is *scharfsinnig* (acute). Now it is true that no measurements of judgment exist, and it seems tacitly understood that they cannot be made. Nevertheless, judgment is that faculty which more readily than any other lends itself to grading, if not to numerical measurement. The problems often given in the text-books of logic suggest something of the kind. It must be possible to arrange sets of questions that can serve to test the judgment of people; and I may add that I have gradually collected a number of such questions which I have employed with this result, that by repeated trials their relative difficulty was pretty well ascertained and the questions could be arranged accordingly in a rising scale. If any one should here interpose the question, But whom do you find willing thus to have his measure taken at the risk of being put down as a dunce in the first or second degree? the answer is that young people in the classroom commonly enjoy the sport very much. Of course they are not told about the grading of the questions.

One of the earliest questions I used arose out of the following incident, of which I have personal knowledge. In a small town in Illinois a boy was bitten by a dog and it was feared that the dog was mad and would soon show signs of hydrophobia. A lively discussion arose. It was said by some that the dog must be killed, for if the dog got the hydrophobia, so would the boy. "Useless," said others; "the killing of the dog could do the boy no good after he has been bitten." "Of course it would," retorted the others, "for it is quite sure that if the dog gets mad, so will the boy." Simple as this thing appears, the village was actually divided into two contending parties, so that finally a deputation of two men was sent to this city to inquire of a renowned surgeon whether or not the dog should be killed. "You had better kill the dog," was the doctor's reply. Now,

how great was the percentage of the people in the village unable to arrive at the correct conclusion? Suppose it was 30 per cent., how should this question be graded upon a scale of 10? By a scale of 10 I mean this: imagine all possible questions arranged according to their difficulty into ten degrees, No. 1 being so easy that any adult not an idiot will solve it, and No. 10 so difficult that only the highest intellect will solve it.

Now, before presenting further examples, I must premise an important consideration in the choice of questions. They should be so chosen that the subject-matter is practically equally well known to all concerned, as it was in the above dog incident, the question not hinging upon the nature of the poision or any scientific matter. I will now select the following example: I have two spiral springs similar to those used upon the trucks of railway-cars. I place the two springs upon the floor side by side, and, putting weights upon them, find that each spring is pressed all the way down by a weight of 100 pounds. Now I place the one spring on the top of the other and again apply weights. The question is what weight will press *both* springs all the way down. Simple as the solution may seem to many, the question is not answered correctly by more than 40 per cent., and is analyzed correctly by less than 20 per cent., according to a number of trials made.

Let me present another example. We have a steam-boiler filled with cold water. The boiler is tightly closed, but has a pipe fitted in, which is also filled with water communicating freely with the water in the boiler. This pipe at the other end is connected with a force-pump. We work the pump and produce a certain pressure in the boiler. Now, while the pressure lasts, we close a valve between the boiler and the pump. What now about the pressure in the boiler? Is it still there or not? This question is answered by a considerably smaller number than the one about the two springs. But upon substituting a rubber bag for the iron boiler, it becomes at once very much easier.

Here is a neat little question: A life-insurance agent from one of the larger towns of Illinois caught many people of my acquaintance in one of the smaller towns by the following simple talk: Our company, said he, is a mutual company and it has about ten times as many members as any other company. The consequence is, that when a member dies and an assessment has to be made to pay the loss, there falls to each of the surviving members a much smaller sum to be paid than would be the case in a smaller company.

These few examples will suffice to show the method of procedure. Now it would be desirable to bring together more material than can be collected by one person. If several persons who will take an interest in the matter would join, there might be made a valuable collection.

One remark as to those persons who are most apt to solve these logical questions. It is not the scholars commonly called "quick," nor is it the slow, nor those who excel in arithmetic or algebra. But a rule that seldom fails is this, that a scholar who is good in geometry will prove good also in all sorts of logical questions, so that observation agrees with what we should expect according to Plato.

Respectfully, WERNER A. STILLE.

ST. LOUIS, March 2, 1892.

54 (17 March 1892) 212

NOTES

Attributed to Peirce by Fisch in *First Supplement*. Peirce and Halsted were in correspondence (see MS L 181). On 15 January 1892, Halsted wrote that he would be sending his translation of Bolyai in a few days. This note is unassigned in Haskell's *Index to* The Nation, vol. 1.

Prof. George Bruce Halsted, whose valuable translation of Lobatchevsky's 'Non-Euclidian Geometry' we noticed the other day, has now published a translation of Bolyai's celebrated work on the same subject. The translator says in his introduction: "This strange Hungarian flower was saved for the world, after more than thirty-five years of oblivion, by the rare erudition of Prof. Richard Baltzer. In the second edition of his 'Elemente der Mathematik,' in 1867, Dr. Baltzer called attention to this remaking of geometry, and the name Bolyai was at last given its place in the history of science. Before that, the father, Wolfgang Bolyai, seems to have been the only person who really appreciated the work of his son John Bolyai." We are told that "Bolyai, when in garrison with cavalry officers, was challenged by thirteen of them at once. He accepted all, stipulating only that between each two successive duels he might play a bit on his violin. He was victor thirteen times." He left a manuscript of a thousand pages which has never been examined by a competent mathematician. The work now translated will be seen by most of those who are specially interested in the subject, for the first time. It is historically of the deepest interest, but, as an introduction to the subject, is inferior to the work of Lobatchevsky. Prof. Halsted's publication confers, however, an even greater boon upon mathematicians than his other translation.

54 (17 March 1892) 214

NOTES

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

—Prof. William James has produced an abridgment of his important treatise upon Psychology, with some additional matter for the use of beginners (Henry Holt & Co.). His "natural science" method, which consists of ignoring all general doubt, is carried even further in the briefer than in the fuller work. An epilogue is appended for the further defence of this method, but to no avail. Students of molecular physics presume, for reasons that seem good to them, that certain things are absolutely true of the universe in every part, such as the tridimensionality of space, its infinity, the law of action and reaction, the principle of energy, and the like. These universal truths, as they are held to be, have a basis in experience, but are extended so far beyond the domain of observation as to be fairly termed metaphysical. In many branches of physics it is easy to show that they are near enough true for practical purposes; but in molecular discussions the question of the truth of such things has to be sifted to the bottom, on pain

of leaving a grave doubt over the whole subject. In psychology the same thing is far more true. The list of metaphysical positions is longer, and they are far more dubious; so much so that students of psychology have hitherto considered metaphysical discussions as unavoidable. Such discussions have by no means been omitted by physicists, although the present unsatisfactory state of molecular theory is in part owing to the small aptitude of laboratory men for the kind of thinking requisite for the solution of such problems. Prof. James's method practically comes to keeping the most general questions out of the focus of distinct apprehension and thus entrapping himself, or at least the reader, into confident but dangerous and unexamined assumptions. To have pointed these out all along would not have made the book much longer nor seriously harder for the student.

54 (24 March 1892) 237

A Treatise on the Geometry of the Circle, and some Extensions to Conic Sections by the method of Reciprocation. With numerous examples.

By William J. McClelland, M.A. Macmillan. 1891.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS 1375 (draft).

Of the smaller and outlying branches of mathematics which have received attention of late years, none is more interesting than that of the remarkable lines, points, and circles of a triangle, which really forms the principal subject of this little treatise; and it is a subject in which any person skilled in elementary geometry and trigonometry can take pleasure. Mathematicians will welcome a convenient compendium of the theorems connected with Brocard's, Taylor's, and a dozen other circles, as to which they are now all expected to be *au fait*.

The methods of the book are rather promiscuous, the nomenclature is not always good (as when the well-known theorem of Matthew Stewart is called "Euler's theorem," a designation applied already to half-a-dozen different propositions), the enunciations are sometimes not clear, and the writer throughout has consulted his own comfort rather than the reader's. But, notwithstanding such relatively unimportant faults, the book is acceptable.

54 (12 May 1892) 358

NOTES

CSP, identification: MS 1365. See also: Burks, *Bibliography*; MS 1375a(s). This note is unassigned in Haskell's *Index to* The Nation, vol. 1.

Every mathematician who proposes to carry his studies beyond the college curriculum will be glad to learn that Prof. F. N. Cole has published a translation of Netto's well-known 'Theory of Substitutions,' which can be obtained by enclosing three dollars to the Register Publishing Company, Ann Arbor, Mich. If not a great classic, it is a standard treatise; and the student who is not thoroughly familiar with its contents (which can nowhere else be so conveniently

mastered) will find himself put to serious inconvenience in reading many a recent German memoir. The present edition has been extensively revised and altered by the author, so that it is better than the German original—not to speak of the comfort of reading mathematics in our clear and concise language.

We may also call attention to the second edition of Joseph Edwards' 'Elementary Treatise on the Differential Calculus' (Macmillan), a sensible and useful treatise, including everything necessary, and excluding subtilties not called for. The examples are specially copious and well chosen, and in general the treatise has the best qualities of the English text-books, without their worst defects. Still, we cannot help thinking that the examination of such Continental treatises as those of Schlömilch and of Jordan would have led to some improvements. However, the student will find the presentation easy, interesting, and tolerably full.

54 (12 May 1892) 366

Mathematical Recreations, and Problems of Past and Present Times.

By W. W. Rouse Ball. Macmillan. 1892.

CSP, identification: MS 1365; Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*.

Mr. Ball, whose sketch of the history of mathematics has been noticed in these columns, now selects a subject in which a flimsy treatment is excusable and almost expected; and as his book is decidedly entertaining, perhaps no fault ought to be found with it. On page 33 he gives an amusing example of a fallacy in geometry. The reasoning is of precisely the same nature as that of Euclid i. 16, and of several other theorems which are found in most of the elementary treatises. The only difference is that those propositions are true, or very nearly so, while this amounts to saying that any one line has the same length as any other. If the reasoning were thrown into the form of a *reductio ad absurdum*, it would appear somewhat more deceptive.

The expression "mathematical recreation" has acquired a pretty definite meaning. It signifies a puzzle amusing to any person of average capacity, and involving a mathematical question that does not readily yield to well-known methods. Such, for example, are Solitaire, the Knight's Tour, the arrangement of dominoes so that each number occurs in squares of four. A tolerably complete and accurate account of all such problems hitherto proposed is a desideratum; but the want is hardly filled by a work in which the author ingenuously says of one of his chief topics, "I know nothing of recent Continental works on the subject." This remark, extended to other subjects and other works, would very well describe the method of Mr. Ball's exposition.

Having disposed of mathematical recreations, in 149 pages, Mr. Ball ekes out his volume with five chapters upon miscellaneous subjects, treated in the same scrappy manner, to which copious and obvious references in footnotes impart an air of erudition. There is a chapter upon astrology in which the 'Tetrabiblon' is said to be "ascribed" to Ptolemy. This is not accurate, since the 'Tetrabiblon' *purports* to be by Ptolemy, and is addressed to his brother Syrus, like genuine

productions of his pen. There is a chapter upon hyper-space, in which perhaps as good a notion of a fourth dimension is given as could be acquired without serious study; but the sketch of the non-Euclidian geometry is not good, and the results of the "Continental" Klein and Riemann are misstated. There is a chapter upon time and its measurement in which we are informed that the earliest pendulum clock was made by Harris of Covent Garden, some "Continental work" being overlooked. The book concludes with a brief account of some theories, mostly British, of the constitution of matter.

54 (2 June 1892) 417

Moral Teachings of Science.

By Arabella B. Buckley. D. Appleton & Co. 1892.

CSP, identification: MS 1365; Haskell, *Index to* The Nation. See also: Burks, *Bibliography: List of Articles.*

Another subject so important, vast, and difficult it would be hard to name—a subject which not every philosopher of the first rank would be competent adequately to treat. Not mere clear insight into one aspect of philosophy is sufficient; a full appreciation of what belongs to the spirit of all the different leading schools of thought is required. To say that the subject is far beyond the powers of the authoress is no disparagement. Nor has she attempted any thorough or philosophical discussion. It is not science which has dictated her teachings, but traditional ideas, for which she ingeniously finds considerable countenance in facts of natural history. But these facts are somewhat isolated and sporadic; they are not the leading facts of any current scientific theory. That they play so little part in science perhaps indicates a defect in scientific theories.

Two widely different things might be understood by the "moral teachings of science." In the first place, the prosecution of scientific research necessarily requires and strengthens certain moral qualities, quite independently of what the results of that research may be, and the moral teachings involved must undeniably be good so far as they go, although they may be one-sided, fortifying only a part of the moral nature, and leaving another part neglected. The first of these teachings is perfect fairness and moral indifference as to the outcome of any inquiry. Suppose, for instance, the inquiry be as to the correct reading of a text of Scripture, "Thou shalt not steal," or "Thou shalt steal." (We purposely select an impossible case, in order to free the example from perplexities.) There is a conclusive argument to be drawn from the moral nature of man that the former and not the latter must be the correct reading. Nevertheless, in estimating the force of the purely historical evidence—in order to be scientific, in order to be logical—we must for the time being remove, if we can, all such prepossessions from our mind, and look upon the two commandments with an indifferent eye; not rejecting any considerations, but putting them aside for the time being. Many great scientists go to church, and are there very unlike what they are in their laboratories. At one time they are studying one aspect of truth, at another time another. To regard either aspect fairly and honestly, the other must for the time

be excluded. If they conflict, the presumption, the faith of the scientific man is, that it is because the last word has not been said, on one side or on the other; at any rate, it must at least be hoped that there is an ultimate resting place which will be satisfactory from both points of view.

Perfect candor in recognizing facts and their bearings, without trying to explain away real difficulties so as to make out a decided conclusion, is the very first point of scientific morals. To get at the facts of observation, uncolored by any theory or doctrine, moral, political, or physical, is what the scientific man must strive for. It does not please him at all to have his observations agree too well with one another. It makes him suspect that something is wrong. An obstinate discrepancy is his delight, because it shows that he is on the road to learning something he does not yet know. It was a little discrepancy in the place of the planet Mars, of a fourth of the breadth of the moon, that forced Kepler, who would not blink it, to the discovery of his first two laws, and so made the discovery of Newton possible, and opened the way for all modern science. Nothing, it is true, is more common than to find admirable scientific men strangely incapable of seeing the force of certain kinds of evidence; as many medical men long were blind to the evidences of the germ-theory of acute constitutional diseases. Perhaps they are even better scientific men for that, within a limited field; but in a broader field it is a fatal defect. Let lawyers have their rules for excluding certain kinds of testimony if they will, but science must exclude nothing, not even the fancies and traditions of men. On the other hand, science must not confound different orders of premises. It must let instincts and superstitions have their say, unchecked, and listen to them; and then it must let scientific observation have its say, equally unchecked. Science will erect a theory which shall do full justice to both orders of facts, if it can. But whether it can or not, it will collect new facts in all departments to see whether they confirm an existing theory or suggest a new one.

But Mrs. Buckley Fisher rightly understands by the "moral teachings of science" something different from such teachings of scientific logic. She means the moral and spiritual beliefs and tendencies which are in harmony with the discoveries and theories of science. Now, to hold that *these* moral teachings of science are necessarily sound and wholesome is an utterly unscientific belief, because it is not borne out by facts, but is merely an airy optimism. Science is incomplete; it is *essentially* incomplete, for what we mean by science is the sum of human activity at any epoch in the path of discovery; and were everything once found out, this activity must cease. True science never pronounces an ultimatum. Philosophical writers are always doing so. Men like Spencer lay down the principle of the conservation of energy as an ultimate, primordial principle of the universe; but a pure scientist is puzzled to know what can be meant by such a truth. For him the conservation of energy is a principle which he may safely assume in all reasoning about large and unorganized masses, and with little hesitation for molecules and atoms, and which is certainly applicable, to a great extent at least, in regard to living matter. But what absolute universality means, or whether it means anything at all, he does not know nor greatly care. Science is dealing only with what is likely to come into the field of experience in a moder-

ate time. It has nothing whatever to say about eternal verities, and its moral teachings are necessarily defective if such verities have anything to do with morals. But science really makes no pretence to teaching spiritual things; and what are rightly enough called its moral teachings—that is to say, the views of spiritual questions which have a general resemblance to the discoveries science has made up to date—are doctrines for which science does not vouch in the least.

Nobody who analyzes these teachings understandingly and without bias can well deny that, so far, they have been in the main distinctly anti-Christian. The first general feature of nature which attracted the attention of modern scientists was the prevalence of mechanical law; and Robert Boyle, one of the most devout of Christians, formulated the mechanical philosophy according to which the universe works like a machine. Make this proposition absolute and universal, and it jars utterly with the creed of Christendom, for it leaves no room for final causes. Boyle himself made room for them by limitations of the proposition. For instance, he held that in the beginning there was no such mechanism, until God had made his plans; and that, that done, the whole character of his action was changed. There is a fearful want of philosophical unity about such a conception. The natural "teaching" of science, though far from being a scientific conclusion, is that all appearance of final causation is illusory. Christ taught that God loves his children, and that not in an entirely inscrutable way, but humanly, so that there is intercourse between each man and God, and prayers are sent up and answered. Yet the "teachings" of science reduce God to the condition of a limited monarch, acting under laws which leave no room for personal favors. This view has penetrated so far that when Tyndall proposed a prayer test, the attitude of the clergy was less courageous by far than that of Elijah towards the Tyndalls of his day.

54 (23 June 1892) 472-473

The Origin of Metallic Currency and Weight Standards.

By William Ridgeway, Professor of Greek in Queen's College, Cork. Cambridge (Eng.): University Press; New York: Macmillan. 1892.

CSP, identification: MS 1365. See also: Burks, *Bibliography*. This review is unassigned in Haskell's *Index to* The Nation, vol. 1.

Sir William Ridgeway (1853-1926) was an English classical scholar. In 1883, Ridgeway was appointed professor of Greek at University College, Cork. He held the Disney professorship of archaeology at Cambridge from 1892 until 1926, and was the Brereton reader in classics there from 1907 until 1926. While at Cambridge, he opposed granting degrees to women and abolition of compulsory Greek study. He was knighted in 1919, and remained throughout his life a controversial figure.

Compound arithmetic can certainly make itself very disagreeable. From the urchin writhing in the agonies of a long sum in long measure, up to Belshazzar, watching the hand write upon the wall those distressful words, "Pounds, pounds, ounces, drams," that suggested there was an account to settle with God, mortals have doubtless undergone more misery, first and last, from this branch of mathe-

matics than from any other. On the other hand, to accompany a learned and ingenious essayist in his explorations of ancient metrology, to cut the rope that ties us to the here and now, to mount the heights of speculation, borne up by a beautiful and globular theory, to cleave the thin air of ancient texts, and trust to our guide to get us back to *terra firma*, this is a most delightful and entertaining pastime. Alas! we have blown our last parting kiss to the theorists of our boyhood, Boeckh, Queipo, Hultsch, and the rest. They have sailed away for ever, and we shall never see their like upon earth again, with those two beautiful propositions of theirs, first, that, in the ancient systems generally, the units of weight, length, and capacity were connected in much the same scientific way as the gramme, the metre, and the litre are connected; and, second, that in the ancient world pretty much all the weights and measures of all climes and ages were in simple commensurable relations to one another. We know that, before the adoption of the metric system, different towns of Europe used at least 400 different pounds, and probably twice as many. The units of capacity and of length were quite as numerous; and there was no rational connection between them. In short, the language of quantity was as various as the dialects of speech. But the accepted doctrine until lately was that the Babylonian (or, as some said, the Egyptian) system was strictly scientific; and that all the peoples of antiquity followed that, or, at least, used only standards commensurable with those of that system, or, at most, slightly modified from it. These propositions rested upon the testimony of ancient authors, supplemented by divers ingenious arithmetical computations by which certain relations between certain quantities were made to appear. If anybody objected, as many a man of logical sense did, that such calculations proved nothing but the idle industry of their inventors, and that the documents were almost all of extremely late date, and probably expressed merely convenient approximations, like "A pint's a pound the whole world round," the answer was that we were not at liberty to reject the only evidence in our possession. Yet some enduring work was accomplished by the old metrologists; namely, they weighed and measured, besides coins, perhaps a hundred ancient standards and a smaller number of other monuments.

Within a few years Mr. W. M. Flinders Petrie has determined the values of many hundred additional ancient standards and has measured thousands of monuments. What is far more important, he has contrived methods by which scientific logic can be brought to bear with all its force upon questions of ancient metrology. His conclusions will be found summarized in the article "Weights and Measures" in the 'Encyclopædia Britannica,' last edition. Having determined no less than 516 weight-standards unearthed by him in the Greek-Egyptian town of Naucratis, he has embodied the results in a curve whose abscissas measure the quantities of the weights, while its ordinates are proportional to the numbers of specimens of the different quantitative values. This curve shows certain maxima; and upon these maxima it is precisely that Petrie bases his reasoning. We know from many careful experimental researches that when men try to reproduce many times any quantity, the values they do produce will cluster about the truth, or about the truth affected by a constant error. The curve of these values will show a maximum at that point. Now, the Naucratis makers of weights were

undoubtedly trying to reproduce some standards, legal or illegal. Consequently, each well-marked maximum of the curve represents the value of a standard they were trying to reproduce. This logic is irrefragable. Prof. Ridgeway endeavors to break it down by the remark that many of the weights may have been fraudulent, and *that* as well by being too heavy as by being too light. Granted; but this in no wise weakens Mr. Petrie's reasoning from maxima, which Prof. Ridgeway does not seem fully to apprehend. In order that these falsifications should produce maxima in the curve, it would be requisite that the counterfeiters should aim at quantitatively definite falsifications, and the definitely false standards so produced and put into use would be, *ipso facto*, new units. In short, a clearly marked maximum must represent a distinct unit, a distinct aim, explain its origin as you may. The general upshot of Mr. Petrie's inquiry into the weights of Naucratis is that many more units were in use in that town than could have been found in Nuremberg, or in Venice, or in any great mediæval mart. So the theory that the ancients did not have the same variety of standards that has marked the modern world down to our generation is exploded at one blast; and, that gone, the ridiculous idea that the units of mass, length, and capacity were scientifically adjusted evaporates by its intrinsic volatility. It is only commerce, extensive, pervasive, and voluminous, that can bring about a unification of units, and nobody can maintain that there was as much commerce when Gibraltar was at the end of the world as there was after men had circumnavigated the globe.

Prof. Ridgeway's theory is that before the use of metals there was a universal unit of barter throughout Europe, Asia, and Africa, to wit, the cow. Of all the metals gold first became known, and the balance was invented in order to weigh gold. Consequently, the earliest unit of weight was the gold unit; and this was fixed at the amount of gold exchangeable for a cow. Strangely enough, the cow had the same gold value in all countries and in all ages, namely, 130 to 135 grains Troy ($5 is 129 grains, 900 fine). In order to reproduce this unit, there were rules for building it up from the seeds of different kinds of grains. The author does not mention that similar rules were given by Arabian metrologists for forming the dirhem and mithcal. All the leading systems of weights of antiquity are passed in review and explained on these principles. The Roman *as* is made to have been a bar of copper from which fractions were broken off as required.

Though the author finds much fault with the "school of Boeckh," especially for supposing that units of different kinds were originally connected in the French style, yet he himself really belongs to the old school, because he attaches more importance to documents than to monuments. Now, experience has shown that this method cannot lead to any fixed conclusions. Many places in this essay are marked by the arbitrariness and the one-eyedness which belong to the old books. But judging it as a performance of the old school, in which complete truth and finality are not to be expected, but only valuable suggestions mixed with fancies, it must be acknowledged that this is a strong work. It bristles with interesting facts many of which have never before been used by metrologists. The evidences are marshalled with consummate skill, and we cannot doubt that several of the doctrines here put forth will take a permanent place among the principles of ancient metrology.

55 (7 July 1892) 15

The Grammar of Science.

By Karl Pearson, M.A., Sir Thomas Gresham's Professor of Geometry. [The Contemporary Science Series.] Imported by Charles Scribner's Sons. 1892.

CSP, identification: MS 1365. See also: Burks, *Bibliography*. This is unassigned in Haskell's *Index to* The Nation, vol. 1.

Karl Pearson (1857-1936) was an English scientist and philosopher of science. He had been acquainted with James Clerk Maxwell at Cambridge, and in 1880 became a fellow at King's College. In 1884, he was appointed to the chair in applied mathematics and mechanics at University College, London, which he retained until 1911. In 1896, he was elected a Fellow of the Royal Society. He retired from academic life in 1933, and died 3 years later.

The title of this book hardly prepares the reader for its real nature. It is an attempt to elucidate, in an original train of thought, what amounts, generically speaking, to Kantian nominalism, and to show its applicability to contemporary scientific problems. Although the metaphysical doctrine from which it proceeds is all but exploded, and rests upon an inaccurate psychology and an uncritical logic, in our opinion, yet it must be conceded that the book is one of considerable power, and contains matter for salutary reflection for anybody who cares to think deeply.

"The object of the present work," says the author, "is to insist that science is in reality a classification and analysis of the contents of the mind." This suggests that investigation consists in first collecting one's facts, and then locking one's laboratory door and retiring to one's study to work out one's theories; whereas, in truth, it involves experimentation alternately with things and with the diagrams of things. The realist will hold that this alternation is helpful, because the reason within us and the reason in nature are essentially at one; while the conceptualist will wish to separate his facts and theories as much as possible. He holds that any uniformity or law of nature is, as Prof. Pearson says, a mere "product of the perceptive faculty." Newton's great work was "not so much the discovery as the *creation* of the law of gravitation"; and the force of gravity, because it is a concept, not a percept, has no reality. "The mind of man," he tells us, "in the process of classifying phenomena and formulating natural law, introduces the element of reason into nature; and the logic man finds in the universe is but the reflection of his own reasoning faculty." This is (as we think) very false; but it is the definite position, broadly taken, of a vigorous thinker.

It is hardly necessary to say that the nodus of the whole argument lies in an attempt to show that "the reality of a thing depends upon the possibility of its occurring as a group of immediate sense-impressions." But the author hardly seems aware that this statement will be regarded by most psychologists as involving an analysis of consciousness now quite out of date. In the first place, it is not possible, as here implied, for the same sense-impression to occur twice. It is an individual event which happens once only. When a sensation had today is said to be identical with one had yesterday, what is true is, that two sensations are recognized to be alike; and this likeness resides not in those sensations, nor in any others, but in the irresistibleness of an act of generalization. Thus, generality

is essentially involved in that whereon the reality of a thing is said to depend; and that consideration is fatal to nominalism. Besides, there is no such thing as an "immediate" sense-impression: the only things immediately given are total states of feeling, of which sense-impressions are mere elements; and to say that they are elements is a metaphorical expression, meaning, not that they are in the immediate feeling in its immediateness, but that the act of reflective judgment is irresistible which perceives them there. Here, as before, therefore, a product of analytic thought is detected as essential to that whereon the reality of a thing depends; and, as before, nominalism is refuted. Moreover, in both these cases, and in all others, that which is most essential to reality is the irresistibleness of something; and this sense of resistance is a direct presentation of externality—what Hamilton called an immediate perception. Let the subjectivism out of which nominalism springs be modified by the recognition not merely of immediate feeling, but also of this sense of reaction, and further of the generalizing movement, and it will become a harmless doctrine enough—a mere aspect of realism.

In his application of his nominalism to problems of science, Prof. Pearson has adhered to the spirit of the 'Kritik der reinen Vernunft' with surprising fidelity. He has said things which Kant did not say, but which are so completely in his line of thought that we almost seem to be reading the old master himself. Many of his observations are interesting; others seem quite untenable. Thus, he adheres to Laplace's doctrine of indirect probabilities in its least acceptable form, relying here upon Mr. F. Y. Edgeworth's cobwebs. In a still weaker fashion, he allows himself to be confused by such a writer as Dr. Ernst Mach, in regard to the relativity of motion. The conclusion to which his nominalism leads him is that motion is wholly relative. If this were proved, the truth of Euclid's postulate concerning parallels would be an easy corollary; but, unfortunately, as far as rotation is concerned, the proposition is in flat conflict with the accepted laws of mechanics, as Foucault's pendulum-experiment will remind us.

55 (14 July 1892) 35

The Province of Expression: A search for principles underlying adequate methods of developing Dramatic and Oratoric Delivery.

By S. S. Curry, Ph.D., Dean, School of Expression; Instructor of Elocution, Harvard College, etc. Boston: School of Expression. 1891.

CSP, identification: MS 1365. See also: Burks, *Bibliography*; MS 1376 (draft). This notice is unassigned in Haskell's *Index to* The Nation, vol. 1.

Samuel Silas Curry (1847-1921) was graduated from Grant University in 1872. About the age of twenty-six, Curry completely lost his voice, and while under treatment for the recovery of it, he took elocution lessons from fifty or more of the best known authorities. From 1891 on, he served as instructor of elocution at such schools as Yale Divinity School, Harvard University, Harvard Divinity School, and Newton Theological Institution.

The name Elocution, which, even with our own early writers, was nearly equivalent to eloquence, having been subsequently transferred to the subsidiary art of delivery, is at last degraded by Dr. Curry to designate an offensive display of technique without soul or real art. This leaves him no better word than "expres-

sion" by which to designate the art usually termed elocution. In this essay, which it is certainly not too much to call thoughtful and refined, although it might be found disappointing to a reader who were to expect the profound philosophy to which the adepts of this art nowadays make pretensions, four different schools of delivery are recognized as traditional—the imitative, the mechanical (that of Rush), the impulsive, and the speculative (that of Delsarte). These are all more or less criticised, although not always with entirely convincing arguments. Especially Rush's method is condemned, partly on the ground that it is mainly based upon observations, not of a natural and universal style of expression, but only of a conventional and peculiar style—an allegation far too lightly supported—and partly on the ground that the mechanical application of its rules is found to produce results very odious. But there is a confusion here between the question of the truth and utility of the rules and of the value of a stupid, unrefined, and tasteless application of them. Elocution, in this, may be compared to the art of writing, the usual rules for which are universally acknowledged as sound, so that they cannot be violated with impunity. But let them be applied with never so much technical skill, yet in a soulless, perfunctory, and undiscriminating manner, and the result will be called an academical or rhetorical style. Precisely the same effect is too often produced by elocutionists of the school of Rush. But in the one case, as in the other, it is not the rules that are at fault, but the inartistic use of them.

Nor is it fair to expect that an elocutionist should be a great artist or orator; Mr. Mackaye himself, to whom Dr. Curry seems to assign a preëminent position among teachers, is not that. The truth is, that the attitude of mind in studying principles is so opposed to its attitude in applying them, that excessive devotion to the theory of any art is somewhat unfavorable to its practice. It is so in some measure even in that principal art to which rhetoric and elocution—if not, indeed, all other arts—are subsidiary, namely, the art of thinking and of feeling aright. And the more an art is of a subsidiary character, the more theory and practice are, or seem to be, at war.

When we inquire what Dr. Curry would propose in place of the four rejected methods, we do not find a very definite reply in this volume. He insists strongly upon training, but that is a matter of course; the whole question is what the method of the training shall be. He thinks it highly important to say that the art of delivery shall be made chiefly a manifestative, and only in small degree a representative, art. But this again seems to be too far back among first principles to escape vagueness. We are promised, however, nine succeeding volumes by Dr. Curry upon his art, so that we cannot expect that the first should contain anything more than generalities.

55 (11 August 1892) 114-115

Dynamics of Rotation: An Elementary Introduction to Rigid Dynamics.
By A. M. Worthington, M.A., Headmaster and Professor of Physics at the Royal Naval Engineering College, Davenport. Longmans, Green & Co. 1892.

CSP, identification: MS 1365. See also: Burks, *Bibliography*; MS 1377 (draft). This notice is unassigned in Haskell's *Index to* The Nation, vol. 1.

This little volume of only 155 pages will be very useful to those persons who have a slight knowledge of the most elementary principles of mechanics, giving them in a dozen sittings, or not many more, all that is necessary to render their previous knowledge applicable to practical problems. It is written, too, in no perfunctory way; but the author has seriously addressed himself to the problem in practical psychology of how to bring a mind uncultivated in mechanical conceptions into a state comparable with that of a good mechanical engineer, and has produced a successfully working solution of it. Having judiciously divided his subject, so as to separate the difficulties which the learner must encounter, he begins, under each branch, by pointing out either familiar phenomena or else simple experiments to be performed; and by reflection upon these—by comparing and analyzing them under the guidance of the author—the student is gradually brought to a conception mathematically clear, which is at the same time firmly attached to well-recognized facts of observation. The result, we are confident, will be found to be that, considering the small amount of mathematics this little book supposes or teaches, the mastery it imparts to the student will be very satisfactory. As a good instance of the author's care, the mathematician should look over the explanation he gives of the gyroscope. We do not know where we have seen another, equally elementary, that has been so clear and detailed as this.

The merits of the work are such that we must forgive a few little slips of logic. Some persons might object that it does not cover the whole ground, in that, not only in the broad realm of rigid dynamics, but even in the narrower province of the dynamics of rotation, it does not teach all with which every man of good ordinary education ought to be well acquainted. But we are inclined to think that the author has exercised sound judgment in restricting his subject as he has done.

55 (25 August 1892) 152

The Philosophy of Spinoza.
[Series of Modern Philosophers. Edited by E. Hershey Sneath, Ph.D.] By George Stuart Fullerton. New York: Henry Holt & Co. 1892.

CSP, identification: MS 1365. See also: Burks, *Bibliography*. This is unassigned in Haskell's *Index to* The Nation, vol. 1.

The idea of this series of books is to present the substance of the leading systems of modern philosophy in selections from the original works. Its object is to facilitate the study of the history of philosophy in colleges, as well as to meet the wants of clergymen and others who desire to make a pretty thorough, but yet not a professional, study of philosophy. Whether the plan, although it is skilfully executed

in this volume, attains its ends as nearly as careful and extended but free expositions would do, may well be doubted. A student who does not want Locke's 'Essay Concerning Human Understanding' (which he can easily procure for about the price of the abridgment) on his shelves, nor Hamilton's 'Reid,' nor Kant's 'Critique of the Pure Reason,' is a person who will not understand selections from these works, and whose wants would be better served by such expositions as modern scholars to whom the several works are specially sympathetic would be able to set forth.

Of no writer is this nearly so true as Spinoza. Nobody but a ripe philosopher, profoundly versed in the history of thought, is fit to read Spinoza's 'Ethics.' A collegian will be sure to miss the essence of it, and any amateur metaphysician whose ideas have not been matured on a special side by deep reading in theology will be almost sure to fall into the same error. The reason is that Spinoza did not understand himself—that may be said of nine out of ten great thinkers, but above all of Spinoza—and consequently was a miserable expositor of his own ideas. That which will chiefly attract the attention of any inexperienced reader of the 'Ethics' is its argumentation and its pseudo-mathematical form; and if he is well versed in modern logic, these can only excite his scorn. If he is not so versed, the kernel of the book will remain still more completely shut away from his apprehension.

Another fault of this series is that not sufficient attention is given to the biographies of the philosophers. Light may be thrown upon any doctrine from the life and personality of its author; but this is particularly true of Spinoza's. Imagine a not very little but rather short Jew, somewhat shabby, but scrupulously neat and almost prim, too formal, though in forms of his own, walking with short steps, talking in short-clipped syllables, of colossal conceit. His morality is stern, not to say narrow. He so values his self-respect that he not only will not accept a pension from his own people, on condition of living like a respectable member of the synagogue, but he will not accept a pension from Louis XIV. without conditions; he refuses his consent to a fortune being left to him, and when the will of the proposed giver enjoins his heir to take care of Spinoza, the latter reduces the yearly payment from 500 florins, which the heir himself had proposed, to 300. In fact, Spinoza carries his love of independence and detestation of being under obligations so far that he will not accept any employment proper to an educated man, but practises a handicraft. He lives his life among artisans and the lower middle class, and meets no other persons except his own devoted admirers. To a man of genius, such a life would have been utterly unendurable; but Spinoza, however extraordinary his ideas, was a sluggish mind. He passed his days in a narrow circle of ideas, concerning which he was continually inventing quillets or catches, more or less puzzling, which he took for arguments. The great ideas of pantheism could not have come to him in that way; but how they did come he does not tell us. He thought that a matter of no consequence, and would have been unable to give any accurate account of it. Nobody has yet elucidated the real nature of pantheistic thought, nor shown its relation to matters of experience. The account usually given, like Spinoza's own, remains on the surface. But we cannot go

further into such a subject here than just to remark that the service he performed was to render certain conceptions, as that of the Absolute, more sharp and clear than they had before been, but not to prove any truth.

Prof. Fullerton repeats doubts concerning Spinoza's love affair which appear to us perfectly gratuitous. He did not practically leave Amsterdam when the lady was twelve years old, but when she was seventeen; and the whole history seems to be from every point of view exceedingly probable.

55 (8 September 1892) 190-191

Dreams of the Dead.

By Edward Stanton. Boston: Lee & Shepard. 1892.

CSP, identification: MS 1365. See also: Burks, *Bibliography*: MS L 159.14. This notice is unassigned in Haskell's *Index to* The Nation, vol. 1.

There is a stage of scientific inquiry, ineluctable as the calentures of youth, whose work is pure play of fancy. The wonderful molecular theory which has served our age as master-key to the arcana of nature, would never have come into our possession if Democritus and Epicurus had not first dreamed it. Copernicus had never dared his leap, the audacity of which we cannot easily appreciate, if he had not had Pythagorean fancies of a central fire to egg on his mind. Concerning a future life and the nature of spirit, we know about as much to-day as Democritus and Epicurus did about the cosmos. We are most of us hoping now that our descendants, at least, may some day find out in this world something positive about the other. Meantime, speculation must mew its plumage for a new flight; for it is surprising how feeble all the attempts of the Dantes and the Aquinases have been —the Aquinases vainly trusting to the flappers of ratiocination to raise them from the earth; the Dantes hampered by preconceived downdragging baggage; and both too much occupied with ideas of Hell to wing their way freely in a spiritual ether. Swedenborg might have helped us, if he had not been so positive and peremptory. Dogma weighs down; it is unsubstantial suggestions and light interrogations that are wanted to bear the mind aloft.

The author of 'Dreams of the Dead' makes no effort to persuade his reader; he insists upon nothing—he just sets forth his reveries, with an unaffected power that makes itself felt. Were the book a product of art, it would bespeak an imagination not less than extraordinary; but be it the production of a one-book man, the brooding of many years, and it is none the less valuable to the public. The author quotes on his title-page those lines of young *Hamlet*, "For in that sleepe of death, what dreames may come," etc. He has raised the thought that the dead dream, that the disintegrating brain has its flickering consciousness, and he has clothed this idea with so vivid a form that it refuses to be exorcised or shaken off. Had he argued it scientifically, as there was every temptation to do (for, after all, what solid facts are there against it?), he would have failed to impart to it such a clutch upon the imagination as he has effected by a simplicity and unpretentiousness very seductive. What an awful variation upon the pur-

gatorial conception it is, one must read the book to know. In fact, it is too dreadful for human belief. The attractive and elevating features of the conception (and such are by no means wanting) are the ones the book mainly dwells upon; but surely no subsequent paradise could indemnify the soul for such fearful bondage to the flesh. For that reason, not many readers will be inclined to accept the theory as true. Besides, Calvinism is in ebbing favor, nowadays—some persons will call it an unsavory ebb. Now this book exhibits curious vestiges of the Calvinistic, or rather the old theological philosophy, though these are softened down till barely perceptible. Mr. Stanton does not, for instance, hold, with St. Augustine, that the honor and glory of God demand that the great majority of mankind should be predestined to everlasting fires; but he does tell with theologic glee of the misery of two old hidebound Puritans prolonged throughout two centuries. He has faith in the radical reprehensibleness of certain created spirits, which to some minds has always seemed vile blasphemy. Above all, he cannot free his conception of the other world completely from that of retributive justice, and practically postpones the Beatitudes to the comfort of another life.

Considered merely as a tale, 'Dreams of the Dead' is a story that, once read, will never be forgotten; and the lessons it impresses are not unprofitable. Any reader of it who might have been inclined to repine at the thought of mortality, will be safely cured of that complaint by the perusal, and be glad enough to adopt, if he can, the opinion of old *Prospero*, "Our revels here are ended," etc.

55 (6 October 1892) 260

THE BOSTON PUBLIC LIBRARY

The letter by Samuel A. B. Abbott, to which Peirce refers, was printed in *The Nation* [55 (22 September 1892) 220-221]. Abbott's letter was intended as a rebuttal to an editorial note in *The Nation* of 55 (18 August 1892) 127, in which it was stated that the Boston Public Library had changed its policy of permitting nonresident scholars to borrow books. Criticism similar to that advanced by Peirce was also raised by Forest Morgan in a letter to the editor of *The Nation* [55 (29 September 1892) 241].

TO THE EDITOR OF THE NATION:

SIR: The hopes of one student were mightily raised when Mr. Samuel A. B. Abbott averred that there was "*not a particle* of foundation" for the statement that the Boston Public Library "no longer grants to persons actually engaged in authorship the privilege of drawing books, though non-residents." Certainly I knew there were several particles of foundation, at least, for the statement, but I inferred that the Trustees were not aware of such facts, and were determined they should not exist. I therefore ventured to address the President of the Board, saying this, and asking, for the reason that I am writing a course of lectures for the Lowell Institute, on the History of Science, that I be allowed to borrow Gilbert's treatise, 'De Magnete.' I offered, at the same time, if desired, to deposit $50 as security for the book, which usually fetches about $35 in the market. My letter was returned to me by Mr. Abbott unanswered. I wonder how the kingdoms of this world appear when viewed from that awful pinnacle, the Presidency

of the Board of Trustees of the Public Library of the City of Boston. What funny little creatures ordinary men must seem! Such a situation would be quite enough to render many a poor gentleman so dizzy that he would not know whether he was telling the truth or not. C. S. PEIRCE.

55 (27 October 1892) 324-325

Distinction and the Criticism of Belief.

By Alfred Sidgwick. Longmans. 1891.

CSP, identification: MS 1365. See also: Burks, *Bibliography*; MSS L 159.18, L 159.19; MS 1378 (draft). This review is unassigned in Haskell's *Index to* The Nation, vol. 1.

Mr. Sidgwick enjoys a certain reputation, he carries an air of distinction and mundanity in his style, and he professes to discuss questions of logic in a fresh and enlightened way; so that we open his books in high expectation. But we lay them down with a sigh. All that has been accomplished in this department of thought since the days when it was possible for a Hegel to publish such attempts at analysis as Hegel's were, might as well have remained unrecorded as far as Mr. Sidgwick's teachings are concerned. Now, that a man can do fine work in logic without being well read in its literature, several eminent instances render more than evident. But the requisite to such fruitfulness is an extraordinarily vigorous mind, that brings forth genuine flowers of thought, bright, delicate, and redolent of suggestion, and not mere fabrications of tissuepaper, needing wires stuck through them to hold them in shape.

The author opens by explaining that the subject of his studies is Ambiguity. This promises well, for there is nothing thinkers of his quality need more to study. But we soon find ourselves wondering whether he knows what the word ambiguity means. He can hardly be unaware there is such a fault, but he appears to have little dread of it. The real topic of his book is not that, but vagueness. Ambiguity is a confusion between ideas quite distinct, such as the *unlimited* and the *immeasureable*; and though 'Distinction' does not *treat* of this, it richly illustrates it. Vagueness is an indeterminacy in the limits of the application of an idea, as to how many grains of sand are required to make a *heap*, and the like. It is not necessarily a fault of reasoning; in its lower degrees it is but an unavoidable and harmless imperfection of thought. The problem Mr. Sidgwick sets himself is to note the precautions needful that vagueness may not lead into positive error; and a problem of elementary simplicity it is. Yet 280 pages might suffice to muddle it, and this volume has 279. An efficient aid in treating such a subject, so as to satisfy the skimmer of books that he has gone over matter which would have been worth reading—and this class numbers important critics—is a vocabulary well chosen to render the meanings of dubious propositions questionable, and to dress up familiar ideas in queer disguises.

Mr. Sidgwick informs us that "distinction as such—distinction at all—is the separation of kinds; and the notion of separate kinds is unavoidably opposed to the notion of differences which are merely of degree." The first half of this state-

ment is, of course, true, if the writer chooses to take the word "distinction" in the sense which makes it so. In the received language of logic the separation of kinds is called *division*, and *distinction* is restricted to a separation of significations; in metaphysics, *distinction* is any kind of otherness. But the second part of the statement, that a "distinction" cannot be merely quantitative, is a fair specimen of Mr. Sidgwick's logic. Is there any "distinction" between the color of scarlet iodide of mercury and that of Paris green? If not, we fear the new meaning of "distinction" is not a very useful one. The two colors are defined by the following equations:

$$Scarlet = .78\ R + 0.10\ G - 0.05\ B.$$

$$Emerald = -0.03\ R + 0.91\ G - 0.12\ B,$$

where R, G, and B denote a standard red, green, and blue respectively. It is seen that the colors differ only by the magnitudes of certain coefficients. There seems to be some conflict here.

Is Mr. Sidgwick quite sure of his position? Here is his argument, with which he is plainly very well satisfied: "In order to put any meaning into the name 'difference in kind,' we must have some alternative contrasted with it, and that alternative is 'difference in degree.' " What shall we say of this reasoning? It is highly philosophical, no doubt; but a favorite division with Mr. Sidgwick is that of thought into philosophy and good sense.

He tells us that wherever there is continuity, every distinction must be vague and hazy in its outlines. If he means that a surface cannot be part scarlet and part emerald, with a sharp boundary between them, he is making a large draft upon the confiding trust of the reader. But on p. 72 Mr. Sidgwick lets drop a remark about continuity (and a long annotation shows it to be no inadvertency) which disqualifies him from teaching the properties of continuity, by showing him ignorant of one of the fundamental discriminations established by modern discussions, and no longer in intelligent dispute. The remark in question implies that infinite divisibility—that is, the presence, in a row of points, of intermediate points between every two points—excludes the existence of finite gaps in the row. But put this to the test. From the whole series of rational fractions remove 1/2 and 2/3 and all fractions intermediate between these in value. This makes a gap in the series; yet it remains as true of the series so mutilated as it was of the unmutilated series, that if any two fractions which belong to it are given, a fraction of intermediate value can be found belonging to it.

Mr. Sidgwick says that if nature is continuous, it *certainly* follows that "the laws of thought" (the quotation-marks are his) are false in every case, as applied to actual things. By the laws of thought he means the principles of identity, contradiction, and excluded middle, which he says are "usually" so called. If he would look into the last fifty treatises on logic in German, English, and French, he would find, we think, that these principles are not now usually called the laws of thought. Any deeper acquaintance with the actual state of logical analysis would show that such a designation is the mark of an obsolescent and degenerate school of logicians. But let us see what his reasons are for saying these principles are falsified by continuity. In the case of the principle of iden-

tity, the reason is that "any actual A has been non-A and will be non-A again; it has therefore some non-A in it." But suppose we grant this (though its *therefore* is absurd), it does not touch the principle of identity, which simply says, "A is A"—*i.e.*, every term can be predicated of itself—and makes no reference to the relation between A and non-A. For the principle of contradiction his reason is, that "any actual A may deserve to be called non-A." For the principle of excluded middle his reason is, that "between the actual A's and non-A's there is always a middle region, or borderland." Besides being the baldest possible *petitiones principii*, these reasons overlook the paradox which really does give to continuity an appearance of inconsistency. If a surface be painted part red and part green, it is true that points on the boundary-line are equally green and red, and thus for them it seems that either the principle of contradiction or that of excluded middle must be violated in form. But this is not true of points in general, nor of any *region*, as Mr. Sidgwick's reasons imply. The violation of consistency is merely apparent, as any sound brain will feel. Every portion of the surface is either red or green, those which cross the boundary being partly red and partly green. But a point is not a portion of a surface; and the true characters of the points with reference to the colors are three: namely, they are either (1) wholly surrounded by red portions, or (2) wholly surrounded by green portions, or (3) partly surrounded by red and partly by green portions. Literally, nothing but a surface is colored; to call a point colored is a figure of speech, and this figure of speech it is which alone gives the appearance of a violation of the principle of contradiction.

But enough of this. The spectacle of Mr. Alfred Sidgwick grappling with the problem of continuity is like an infant slapping the face of the Great Sphynx: it is so ridiculous as to become positively touching. He is more in his element with such questions as these: "Is snow a thing, or is it only an accidental state of matter? And is water, for that matter, anything more than an imperfectly stable condition of its two component gases?" He reaches his largest proportions in our eyes when we find him criticising with success the reasoning in those gigantic efforts of intellect, the debates in the British House of Commons, such as the following:

"Lord R. Churchill—He says it is well known in war that movements which are offensive in their nature are sometimes defensive in their essence.

"Mr. Gladstone—Offensive in their *form*.

"Lord R. Chruchill—What does that come to—that the attack of Gen. Graham was offensive in its form but not in its nature? Three thousand men or more were slaughtered, as a matter of form by movements which were not offensive in their nature!"

Until our "G. B." has his way, it may be feared we shall not hear debating like that in the House of Representatives. In this country we have not time for such reasonings, nor for the other argumentations which Mr. Sidgwick is occupied with refuting, nor for the closely similar ones with which he would replace them.

55 (10 November 1892) 359-360

Logarithmic and Other Mathematical Tables.

By William J. Hussey, Professor of Astronomy in the Leland Standford Junior University, etc. Ann Arbor: Register Publishing Co. 1892.

CSP, identification: MS 1365. See also: Burks, *Bibliography*; MSS 1379, 1513 (drafts). This review is unassigned in Haskell's *Index to* The Nation, vol. 1.

William J. Hussey (1862-1926) was a successful American mathematician and astronomer. He was graduated B.S. from the University of Michigan in 1889, and remained at that school for three years as instructor of mathematics and astronomy. In 1892, Hussey joined the Leland Stanford University as assistant professor of astronomy, and was promoted to full professorship the following year. For the term 1891-92, he was acting director of the Detroit Observatory, and from 1896 until 1905 served as astronomer at the Lick Observatory. From 1911 until 1917, Hussey held the post of professor of astronomy at the University of LaPlata, Argentina, and was director of the Observatorio Nacional de LaPlata. He had been honored by the French Academy and the Royal Academy. He was a member of the American Astronomical Society and the American Mathematical Society.

For the semi-occasional user of logarithms, collections like Köhler's are best. But a person who is destined to use up several books of tables by the wearing-down of the paper under his fingers—which commonly happens to expert mathematicians—will prefer to be provided with four-place, five-place, six-place, and seven-place tables, since the expenditure of time in working with these is in the ratios of 1:2:3:4, respectively. Can the tables before us be recommended as being about as good as others? They are printed upon paper fairly opaque and quite free from sheen, substantial but rather cottony to the touch and too white. A small page is a recognized advantage in tables of logarithms. These pages are taller than those of any five-place tables we know except Hoüel's. The ink is not quite so black as we could wish, and some pages are a little gray. Very many figures look as if printed from worn types. The fourth figure of log. 4092 comes from a wrong font. The alignment leaves much to be desired. The type is of the old pattern, which in our judgment is preferable to the Huttonian character (the pattern now common in ordinary printing, invented, it is guessed, by Dr. Charles Hutton in 1783), but inferior to the Egyptian, which are all of one height but without hair-lines.

We may examine the arrangement of the tables of logarithms of numbers. Each tenth value of the argument is printed in Huttonian type. This gives it sufficient prominence; the large black round-numbers of Babbage are unnecessary. The table is arranged in a Newtonian block, which we deem more convenient than the columnar form, especially since it brings twice as many numbers on each page. The table everywhere opens to exactly 1,000 logarithms, not counting those on the last line, which are a sort of catch, or rehearsal of the first line of the next page. This is a point of great superiority over Bowditch, Schlömlich, etc. The numbers in each tenth line are placed between horizontal rules, while the intervening nine lines are divided by leads into three sets of three. This is the plan of the highly approved tables of Bremiker; yet we prefer, with Schrön and others, the division by leads into sets of five. The first two figures of the five-figure logarithms are given only in the first column at the top of the page and where

they change. Bremiker thus separates only one figure, while Bowditch gives all five in every column. The ten columns of the block are all separated by vertical rules, that after the fifth being extra heavy. This is the customary way, but we are fully persuaded that all these vertical lines are productive of error in following the horizontal lines with the eye. We consider the tables of Schlömlich, Oppolser, J. M. Peirce, and others, which omit all but the line after the fifth column, as much the more comfortable.

The indication of a change of the figure in the last place of unrepeated decimals is by an asterisk prefixed to every logarithm affected. This is decidedly the best method. The proportional parts are exact to the sixth place. The practice of thus printing the proportional parts arose in consequence of Babbage, in his seven-place table, printing a dot under every terminal figure which had been increased. This he did on the ground that all information which could be given without disadvantage should be given—a good principle for seven-place tables, without a doubt. Only, upon that principle, De Morgan's plan should be adopted of distinguishing the quarters of the last unit by means of the four ordinary punctuation marks, thus making the tables accurate to a fraction of the number entered equal to unity divided by a power of ten. However, Babbage's system was extensively adopted, and consequently it was necessary to give the proportional parts more accurately. Prof. Hussey prints a dash over every increased 5, whether it be terminal or not, and over no other increased numbers. It luckily happens that there is no case in the tables of an increased 5 followed by three zeros, otherwise the system would break down. Now, we think a system illogical, and therefore inelegant, which can only be carried out by virtue of an accident. But what is the use of carrying the proportional parts to six places? Everybody must allow that it would be bad economy of time in computing to write down one's numbers alternately to five and to six places of decimals. Now, what difference does it make that we add the six-place numbers in our heads? A centimetre and a half at the bottom of each page of the table is devoted to giving the values of S and T, and that not unambiguously. This seems decidedly awkward.

There are trigonometrical tables, both logarithmic and natural, tables of addition and subtraction logarithm, etc. At the end of the book are given formulae and constants. The latter are pretty carelessly collected and copied. The velocity of light is made to be 296.944 kilometres per second! Clarke's value of the metre in inches, 39.370432, is given, although its error has been known for many years. First Prof. Rogers and then Gen. Comstock made fairly concordant determinations, very different from Clarke's. In fact, his was merely the result of measuring copies of Bessel's toise in inches, and then deducing the length of 443.296 lines of the toise, this being the number of lines of the toise de Pérou intended to make the metre at the time of the construction of the latter. But recently M. Benoit of the International Bureau has shown that the metre so deduced from Bessel's toise is too long by its 74,000th part. So, correcting Clarke's determination, and combining it, reduced to a weight of 1/5, with the values obtained by Rogers and Comstock, we find:

	Inches
Rogers	39.37027
Comstock	39.36985
Clarke, corr. by Benoit	39.36990
Weighted mean	39.37004

This makes 25.40003 millimetres in an inch. If we remember, then, that 39.37 and 25.4 should each be increased by one-millionth part of itself, we shall have the fact as accurately as it is known. We find this convenient rule used in the Yaryan Company's Tables. Prof. Hussey's book will do for easily contented computers.

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56 (2 February 1893) 90

A Treatise on the Mathematical Theory of Elasticity.

By A. E. H. Love, M.A., Fellow and Lecturer of St. John's College, Cambridge. Volume I. Cambridge, Eng.: University Press; New York: Macmillan. 1892.

CSP, identification: MS 1365. See also: Burks, *Bibliography*; MSS L 159.37, L 159.36. This notice is unassigned in Haskell's *Index to* The Nation, vol. 1.

Augustus Edward Hough Love (1863-1940) was an English mathematician and geophysicist. He was Sedleian professor of natural philosophy at Oxford from 1898 until 1940. Love's main interest was the investigation of the elasticity of solids in relation to problems of the earth's crust. His *Treatise*, reviewed here by Peirce, has become a standard work in the theory of elasticity.

Elaterics, or the science of elasticity, consists of a purely physical investigation (called, especially with reference to its ruder determinations, the doctrine of the strength of materials) and an elaborate mathematical theory designed to bring the fundamental facts to bear upon questions of applied mechanics; and this mathematical part might very well be called stereostatics. This name would hint at its intimate alliance with hydrodynamics. The two theories alike suppose the solution of complicated partial differential equations with boundary conditions; and the equations of many problems in the one are identical in form with those of problems in the other. They are sister studies, too, in their exceeding economical and philosophical importance. Hydrodynamics has to direct hydraulics and ship-designing; stereostatics to govern almost every operation of engineering, from the vastest erections down to the fashioning of a horse-shoe or a snaffle. The two theories are, finally, in one and the same pickle, in that nearly all the questions that are put to them are beyond the power of our mathematics satisfactorily to answer. Although a wealth of thought of all but the finest quality has long been lavished upon them in a geometrically increasing yearly outpour, so that none of the physical sciences shows greater advances than do the departments of mathematics which may be expected to aid elaterology and hydromechanics, still the practical problems we should wish to solve remain unsolved, and in all likelihood will so remain for a long time to come. Mathematicians, when they cannot solve the problem that real facts present—and this is what always happens—substitute for the real problem a simpler one, as near like the former as they can manage, and are guided by the solution of that. This is that method of abstract or analytical thought which Hegel and his countrymen obligingly teach us is mere futility. This is the style of thinking which makes English political economy so ludicrous to the superwise. They never tire of laughing at the two or three men on a desert island by the study of whose conduct political economists propose to regulate the policy of nations. Yet the contrast between such a little community and a modern State is, after all, certainly not so great as the contrast between any real, practical problem in hydrodynamics or stereostatics and the problem that the engineer succeeds in solving. The resemblance

between the actual motion of water in any case and that represented in the pure hydrodynamical solution is so very slight that some study would be required to detect its existence. The contrast between the stresses in a real structure and those in an engineer's diagram are so enormous that for safety he is obliged to allow that they may amount to from five to ten times the latter. If the deriders of abstract thinking would only reflect that theories thus miserably imperfect have nevertheless sufficed to "possibilitate" (as a Spaniard would say) all the great engineering works of our age, they might, in their turn, learn something. Ships and bridges constructed after the directions of concrete historical thought would hardly be likely to prove much cheaper or much safer.

Mr. Love goes so far as to say, "The only logical way would be to use, instead of the elastic equations, others in which set is properly taken into account, and these are, unfortunately, unknown"; but this is exaggerated, for in most cases it is not merely rupture that we desire to avoid, but the passage of the limit of elasticity. But let the reader fancy what the fairy grace of the structures of the future shall be when the theory of stereostatics shall really have been mastered! How gross and stupidly costly ours will appear in comparison, which make the gazer think only of how much money they cost, instead of singing, as those will do, the pæan of triumphant mind. Even to-day, great steps were altogether practicable could a mathematician of real genius be engaged in the task.

Mr. Love's treatise cannot fail to hasten the blessed advent of structural truth. Of late, engineers who have really understood their business have been dependent upon such works as Müller-Breslau's treatise, upon the French edition of that of Clebsch, brought out by the veteran elastician Barré de Saint-Venant, and upon the same eminent author's edition of the 'Leçons de Navier.' But now the whole subject, with the actual state of the most important of the open questions with which it is infested, is lucidly set forth in almost its most modern developments. The reader, for example, has the advantage of Betti's process of integration, though that is hardly twenty years old; but the still more recent methods of Castigliano, Mohr, Fränkel, and others we look for in vain. It must, however, be admitted that some of these are objectionable—one of them decidedly so. The latest things we have noticed in the book are a discussion published by Boussinesque in 1885, and something by Mr. Chree, who read the proofs.

Mr. Love assumes, with Green, that there are 21 independent elastic constants, and does not, with Cauchy, reduce them to 15. This is at present the assumption best supported by observation, even if it be not demonstrated by Voigt's determinations. A very fair account of the whole controversy is given. Mr. Love denies the inference of Sir William Thomson, Lord Kelvin, that because the tidal "effective" rigidity of the earth is intermediate between the rigidities of steel and glass, but nearer the former, therefore the earth's interior cannot be fluid. Certainly, the argument that because the earth does not yield much in a day or a fortnight, it would not yield to a slight force in thousands of years, never did have much force with most minds.

The notation appears to us the most stupid of all the notations, none of them very good, which have ever been proposed for the subject. The first solecism

we meet with is that P, Q, R, S, T, U correspond respectively to *e, f, g, a, b, c.* This is truly British; and this is the general style of the whole.

57 (27 July 1893) 65

NOTES

CSP, identification: MS 1365. See also: Burks, *Bibliography.* This note is unassigned in Haskell's *Index to* The Nation, vol. 1.

Part 2 of vol. xix. of the Annals of the Harvard College Observatory contains two memoirs, one, by Prof. Arthur Searle, upon the Zodiacal Light, and the other, by the Director, Prof. E. C. Pickering, upon the Atmospheric Absorption of Photographing Rays. Photographic plates are affected most by a bluer part of the spectrum than that which most affects the human eye, and this bluer part is much more absorbed by the atmosphere, so that the correction to the brightness of a star on account of its zenith distance is much greater when the photographic method of observation is used than under direct vision. The form and value of the correction are in this paper worked out elegantly and satisfactorily, and the photographic magnitudes of nearly 900 stars between declinations 55° and 65° are incidentally determined. Photographically, the blue stars are shown as brighter and the red stars as fainter than to the eye, the discrepancy often amounting to a whole magnitude. The effect of the atmosphere is to lower the photographic brightness of an average star in the zenith by 0.44 of a magnitude. This effect is diminished with a high barometer, and is slightly increased in warm and moist weather. It is decidedly greater in the autumn than in the spring of the year.

57 (3 August 1893) 88-89

The Meaning and Method of Life: A Search for Religion in Biology.
By George M. Gould, A.M., M.D. G. P. Putnam's Sons. 1893.

CSP, identification: MS 1365. See also: Burks, *Bibliography*; MS I. 159.24; MSS 1382, 1513, 1382(s) (drafts). This review is unassigned in Haskell's *Index to* The Nation, vol. 1.

George Milbry Gould (1848-1922) was an American physician. He was graduated in 1873 from Ohio Wesleyan University, and from 1868 until 1871 studied at the Harvard divinity school. He was awarded his M.D. from Jefferson Medical College, Philadelphia, in 1888. Gould was a widely published writer on medical topics and served as editor of *Medical News* from 1891 until 1895, and of *Medical Journal.* He held the office of President of the American Academy of Medicine from 1893 until 1894.

An accord between scientific and religious thought must come about, when it comes, chiefly by the natural, unforced development of each. We may hopefully strain all our efforts to find out the truth about special questions, but here we have to do with a great historical rearrangement of ideas, in which no single individual can count for much, and in which it is very undesirable that mere individual characters should have any influence. The most that volition can hope to accomplish is to turn the attention of scientific thinkers to those subjects of science, and the attention of religious thinkers to those aspects of religion, the study of which

seems likely to moderate their antagonistic tendencies. It would seem, for example, that through biological studies science may be led to modify the existing mechanical theory of the universe, which is not at all requisite to its progress, but is merely the coloring which scientific thought acquired during the period beginning with Galileo and ending with Helmholtz's great dynamical memoir, when mechanics and allied branches of physics were the chief subjects of thought, and which in the new period that opened with Darwin is already beginning to be corrected. Many biologists are pleading to-day for the admission of genuine spontaneity. On the other hand, it would seem that studies of historical criticism, in an age in which truth can not only no longer be plugged up or stanched, but cannot be prevented from quickly filtering down from the great scholars among the clergy even to the most Philistine among the laity, must surely lead the churches to great retraction in the matter of infallibility. Now, these two things, mechanicalism and infallibilism, are the great obstacles to any common understanding between religious thinking and scientific thinking.

There is such a thing as mechanical infallibilism. Büchner's 'Kraft und Stoff' affords an example of what we mean. Scientific workers do not insist on anything as absolutely certain. There is not a more marked characteristic of the true scientific investigator than his perfect readiness to entertain any question which there is any possibility of settling by experiment. Indeed, "science" is an unfortunate designation for the department of civilized life that it denotes. It implies a body of knowledge. But it is not half so much knowledge that makes the scientific man as inquiry—the effectual wanting to know that involves the acknowledgment one does not know already. In the days of our childhood, before the present jargon came in, people talked of natural *philosophy*; and philosophy, or wanting to know, much better than science, describes the most precious endowment of the physicist or naturalist. But people who have learned the conclusions of the natural philosophers out of books are very proud to be called "scientists"; and a good name it is for them. They do not want to know, for they are cocksure already. We hear them reason every day as if natural selection, as the exclusive agency, not only in the development of animal and vegetable species, but of everything else, were a self-evident truth. The discovery of the conservation of energy may well be considered as the greatest achievement of natural philosophy. Yet, after all, we know nothing about it except what experience teaches us; and the experiential verifications of it, except in a few simple cases, do not attain any extraordinary degree of precision; while in regard to muscular work and brain activity there is little but analogy to lead us to think it so much as a close approximation to the truth. Every physical determination of a continuous quantity has its "probable error"; and the probable error of the equation which expresses the conservation of energy is large in comparison with those which express, for example, the three laws of motion. Nevertheless, we often find the "scientists" treating the law of the conservation of energy, in its extremest applications, the most remote from anything we can measure, as something it would be absurd to doubt. Such an opinion, which on the one hand sets up certain propositions as truth infallible and past all doubt, and which on the other hand

leaves no possibility for motions not produced and completely swayed by blind mechanical force, may properly be termed mechanical infallibilism. It would seem a strange basis for any reconciliation between religion and science, being deeply hostile to the spirit of both. Yet it is upon this basis, in part at least, and by giving the name of God to an abstraction which it is not pretended has any sort of consciousness or exerts any sort of agency, that some of those who are endeavoring to bring about that reconciliation hope to effect it. Others, again, are aiming at a kind of compromise which would hamper science and mutilate religion, without at all furthering the purposes of either.

In strong contrast to all this is the genuine biologist's religion set forth by Dr. Gould in the book before us. To begin with, it is truly a religion, and no sham. Whoever believes anything like it must, no doubt, be filled with the spirit, if not of worship, yet of devotion, hearty, tender, and passionate; and for how many confessions can we say as much? Next, whether we accept the doctrine or not, we cannot but grant that it does truly spring, by methods of thought analogous to those of natural philosophy, out of observations of nature. Insisting upon the absolute distinction between living and lifeless things, Dr. Gould sees in the former an invisible Life, purposeful and intelligent. This is his god. He names him Biologos. He is a regular Aryan nature-god, very wise and clever, but existing in nature, not the creator of matter, and very far from being omnipotent.

"Every expression of Life we know shows process; difficulties unconquerable and difficulties conquerable, mastery by fate or ingenious partial conquering of fate—never a suggestion of omnipotence. The inference is quite clear, that if life were a worker in matter in all the past eternity, it would have been a more successful conqueror of it than is pathetically evident. The most patent aim of life is to win itself a home in worlds of inorganic matter, and to obtain progressive control of purely physical matter and forces. The fact that the success is only partial in our own world, that it has been attended with such difficulty and such expense (suffering, evil, death, reproduction, etc.), and that not more than two or three worlds of our solar system can possibly allow life a home in them, together with the certainty that like conditions exist everywhere else—all this points to the finiteness, if one may so speak, of God, and His struggle with adverse circumstances. But it also gives blessed reasons and incentives for sympathy with Him, and makes duty clear, unravels a thousand mysteries of our being here, makes religion a psychical as well as a biological necessity—indeed, forms the ground of an indissoluble and necessary identity of religion and biology."

Dr. Gould believes in his god without one shade of doubt, and with a fervid joy that would render his book delightful reading even if it were not filled with interesting suggestions gracefully and strikingly expressed. He really makes his doctrine decidedly attractive, at least for some of our moods. Doubtless, everybody has, at some time, envied the condition of our domestic animal pets. A mother's love is passionate, physiological, forced upon her. But a man's love for his dog is at once disinterested and voluntary. Though the dog does not reflect much, he does so enough fully to understand his relation to his master. Great

comfort he takes in his master's love; but his greatest delight is in the reflection that, despite the man's incomparable and incomprehensible intelligence (of which the dog is quite aware), he is yet neither omniscient nor omnipotent, so that he, dog, is, or may be, positively helpful to the man. Now, the Biologos religion makes of a man God's dog.

It is little to say that there must be some truth in Dr. Gould's idea if there is any truth in religion; for every religion worthy the name represents a struggle between the god and some dark and baleful resistance. Faults in the theory are easily found. The first condition to which a hypothesis should conform is that it should be such that from it definite, verifiable predictions can be deduced. To deduce definite consequences from Dr. Gould's theory, it is requisite that the purpose of life should be formulated. Dr. Gould says this purpose is to conquer and govern matter. But is there no ulterior design? Is the barbaric delight in triumph all? The purpose of vitality should be discoverable by considering what growth in general, or the process of vitality, accomplishes. Certainly growth is not mainly an operation upon something outside; it is a development of the organism itself. Whatever be its formula, it is this that describes the great struggle of the universe, and it is this that the greatest myths seek to embody. But there are besides sundry other processes which have to be considered in any full philosophical study of the question.

57 (17 August 1893) 123-124

HALE'S NEW ENGLAND BOYHOOD

A New England Boyhood.

By Edward Everett Hale. Cassell. 1893.

CSP, identification: MS 1365; Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS 1513 (draft). In Hale's book, Benjamin Peirce is mentioned on pp. 175-177 and p. 197.

Edward Everett Hale (1822-1909) was an American clergyman and author. He was the grand-nephew of Captain Nathan Hale, a patriot of the Revolution. He entered Harvard at the age of 13 and was graduated in 1839. As his *New England Boyhood* states, he was a pupil of Benjamin Peirce while at Harvard. Upon graduation, Hale became a teacher at the Boston Latin School, the source of his earliest education. He resided and preached in Worcester, Massachusetts, from 1846 until 1856, at which time he assumed the pulpit at Boston's South Congregational Church. He was the author of *The Man Without A Country* and was coeditor with Edwin D. Mead of *The New England Magazine*.

Dr. Hale was born in 1822. His memoirs begin about 1826. In 1835, at the age of thirteen, he entered Harvard College, and duly graduated in 1839. He mentions only one or two incidents after the last date, and little about Boston after his entrance to college. Thus, the memories are those of a child, affording no insight into the ways and thoughts of men, nor even picturing in detail the external aspect of anything, but taken up with the all-important doings of the boy. The swinging signs before the old Boston auberges (if Dr. Hale won't let us call them inns) did attract his attention—"The Indian Queen" in Bromfield's Lane, "The Bunch of Grapes" in State Street, "The Lamb" in Washington Street; and

he mentions that when the Tremont House was built, it seemed to the boys wonderful that there should be a "tahvern" with no sign before it and no stable behind it. While he was at the Latin School, between 1831 and 1835, the first omnibus appeared on the streets of Boston—a very long affair, drawn by four horses, and blessed with a name, the "Governor Brooks." Dr. Hale says "the first omnibus in the world was put on its work in Paris. It was called 'La Dame Blanche' from the White Lady of Scott's novel of 'The Monastery,' about the year 1821."

The old Latin School of this book stood on School Street (christened after it), opposite the little green just below King's Chapel. The boys began the study of Latin—boys nine years old—by learning the Latin Grammar by heart in English. Such stupidity seems almost incredible, at a time when Hoole's 'Visible World,' a translation of the instruction-book of Comenius, had gone through numberless editions in English. It was quite a different thing at an earlier date to make boys commit the Latin Grammar to memory *in Latin.* That taught them a great deal, just as learning any other simple Latin book by heart would have done. It was a method somewhat similar to those of Comenius and of Robertson. Yet, doubtless, the substitution of the grammar in English for the grammar in Latin was conceived to be a reform. No wonder the language was never conquered. Many of Hale's afternoons were spent sailing toy boats in the neighborhood of Beach Street (so called because it ran down to the beach); he made magic lanterns, dabbled in chemistry and electricity, went to Papanti's dancing-school and to the swimming-school, coasted from the foot of Walnut Street to the head of West Street, flew kites and invaded houses to get on the roofs to recover the twine, played marbles on the malls and baseball on the parade ground, shook props (deepest of crimes), and doubtless battled with Fort Hill boys, though that is more than he will confess to—in short, did everything that boys did fifteen years later. He also went to the gymnasium in Washington Gardens.

Dr. Hale notes many vestiges of Puritanism in those days. We should like to have been informed whether, in a house like the Hales', a great deal of theology was talked, and whether most people passing the evening casually together would be likely to talk about the points of Calvinism. According to Dr. Hale, the settlement of the Massachusetts Bay colony was due to the insistence of the Puritans upon their Thursday morning religious lecture, which was prohibited in England; and he attributes the preëminence of Boston in population to people being attracted by Cotton Mather's preaching—rather narrow causes for such broad effects. Mr. Palfrey, he tells us, at the Brattle Square Church would frequently break off his long morning's sermon with the words, "I shall continue this subject in the afternoon," entirely sure of having the same hearers. He notices the high pews in that edifice; but he does not say whether those along the sides were square, nor whether the deacons faced the congregation. He does not recall much about the drama, although it must have been within the period of his memories that the Tremont Theatre was built only a few doors from his home. Fanny Kemble is mentioned, not J. B. Booth, nor even Fanny Elssler. The subsequent forswearing of the drama by the good people of Boston was a curious movement, which we wish somebody would describe. Of the early secular lectures we learn something here, but that would be a subject for a volume. It was Dr. Webster's

on chemistry that most attracted the boys. Emerson's were nowhere in comparison.

Dr. Hale does not represent Boston as being so pretty as one would think it must have been, with its many fine old gardens, with the superb orchids and exotics in the greenhouses of his classmate Boott's father (where the Revere House now stands), with much of Beacon Hill blooming with roses, with Summer Street an enchanting alley of verdure and peace, with Franklin Street's pretty park and fountain, with Winthrop Place and other delightful nooks. But neither does he say anything of the beauty of Cambridge, which was then so rural. Nor do the imposing buildings which were put up in those years seem to have made any impression—the Quincy Market, the Custom-house, the Court-house, the Merchants' Exchange, and other buildings in State Street, etc.

The most interesting person in the book, by far, is the author's father, Nathan Hale, editor of the Boston *Daily Advertiser*, chief promoter of the early railways, especially the Boston and Worcester Road, and a man of much wisdom about education. Indeed, we may perhaps say that only two other characters are brought clearly before us, Fulham, an old family servant, and Prof. Peirce. The evidence of Nathan Hale's great good sense in the bringing up of his children, and the little glimpse that we catch, when the corner of the curtain is momentarily pushed aside, of the Hale interior, are so attractive that one is annoyed that there is so little of them. But, indeed, this boy was as unobservant of the inside of the house as he was of the outside. All kinds of interesting people must have frequented it, yet we hear nothing of them. There was Edward Everett, a man who had the art of charming boys as well as he did everybody else. Mr. Webster and Judge Story were there. The railway engineers are mentioned in a general way. Those men, Daniel Treadwell and Maj. Whistler, for example, were as strong and striking personalities as ever walked the earth; but for this boy they were mere abstractions—railway engineers, in a general way. As for the anti-slavery movement, or the temperance movement, or Transcendentalism, they did not exist for him.

Of course, it would be unreasonable to complain of this. Yet the history of Boston during those times is so fascinating, and it so clearly appears, as we can see now, that all the culture and all the good in it sprang out of philanthropy, while all the fogyism, and all that caused the city's overthrow in certain departments, came from the love of money, or "intelligent selfishness," that we are naturally a little impatient to see the real Boston so closely approached without ever being touched upon. There is some description of the Broad Street riot of 1837, but the important anti-anti-slavery riot of two years before and the burning of the Ursuline Convent are not so much as mentioned.

There is a pretty good account of a student's life at Harvard College from 1835 to 1839, the Navy Club parade, the exhibitions, the long Commencement exercises under the auspices of the Governor, the commons in University Hall, with the bill of fare for each day of the week, indelibly stamped on the author's brain, etc. The following shows how the custom of dancing on the college green on class day arose:

"Class day seems to have originated as early as the beginning of the century. The class itself chose a favorite speaker as orator, and some one who could write a poem, and had its own exercises of farewell. There grew up side by side with those farewell exercises the custom by which the class treated the rest of the College, and eventually treated every loafer in Cambridge. As I remember the first class days I ever saw, they were the occasions of the worst drunkenness I have ever known. The night before class day some of the Seniors—I do not know but what all—went out to the lower part of the plot, where there was still a grove of trees, and 'consecrated the grove,' as the phrase was, which meant drank all the rum and other spirits that they liked. Then, on the afternoon of class day, around the old elm tree, sometimes called Rebellion Tree and sometimes Liberty Tree, which stood and stands behind Hollis, all the College assembled, and every other male loafer who chose to come where there was a free treat. Pails of punch, made from every spirit known to Cambridge innkeepers, were there for everybody to drink. It was a horrid orgy from end to end, varied, perhaps, by dancing round the tree.

"With such memories of class day, President Quincy, in 1838, sent for my brother and one or two others of the class of that year in whom he had confidence, to ask what could be done to break up such orgies. He knew he could rely on the class for an improvement in the customs. They told him that if he would give them for the day the use of the Brigade Band, which was then the best band we had in Boston, and which they had engaged in the morning, they felt sure that they could change the fête. The conditions, observe, were a lovely July day, the presence in the morning at the chapel, to hear the addresses, of the nicest and prettiest girls of Boston and neighborhood with their mammas, and the chances of keeping them there through the afternoon. Mr. Quincy gladly procured the band, and when the day came it became the birthday of the modern 'class day,' the most charming of fêtes. Word was given to the girls that they must come to spend the day. In the chapel Coolidge delivered a farewell oration. Lowell, alas! was at Concord, not permitted to come to Cambridge to recite his poem; it had to be printed instead. When the ode had been sung, the assembly moved up to that shaded corner between Stoughton and Holworthy, the band people stationed themselves in the entry of Stoughton, between 21 and 24, with the window open, and the dancing on the green, of which there are still traditions, began. The wind-instrument men said afterward that they never played for dancing before, and that their throats were bone dry; and I suppose there was no girl there who had ever before danced to the music of a trombone. When our class came along, in 1839, we had the honor of introducing fiddles. I shall send a copy of this to the charming lady—the belle of her time—with whom I danced in the silk gown in which I had been clad when I delivered the class poem of the year. Does she remember it as well as I do?"

Although we cannot help regretting some omissions, yet, after all, these very omissions demonstrate that the book is made up of genuine living recollections, without resort to documents, and the same is true of some little errors of detail;

thus, when Dr. Hale informs us that he spouted at the Latin School a poem of Tom Moore's containing the line

"If there lingers one spark of their fire, tread it out!"

instead of

"If there lingers one spark of her light, tread it out!"

we see that the whole is a faithful record of the actual state of the author's memory—and a very pleasant memory it is.

57 (24 August 1893) 143

An Elementary Treatise on Pure Geometry, with numerous examples.
By John Wellesley Russell, M.A. Oxford: Clarendon Press; New York: Macmillan. 1893.

An Elementary Treatise on Modern Pure Geometry.
By R. Lachlan, M.A. Macmillan. 1893.

Geometry in the Grammar School: An Essay. Together with illustrative class exercises, and an outline of the work for the last three years of the Grammar School.
By Paul H. Hanus, Assistant Professor of the History and Art of Teaching, Harvard University. Boston: D. C. Heath & Co. 1893.

CSP, identification: MS 1365. See also: Burks, *Bibliography*; MS L 159.28; MS 1513 (draft). This notice is unassigned in Haskell's *Index to* The Nation, vol. I.

Paul Henry Hanus (1855-1941) was graduated B.S. in 1878 from the University of Michigan. He assumed the post of professor of mathematics at the University of Colorado from 1881 until 1886. In 1891, Hanus was called to Harvard by President Eliot to initiate a new program in the teaching of educational procedures. Though scorned by several of the faculty, who saw this as an attempt to transform Harvard into a teachers' normal college, Hanus persevered in his endeavor, and it was through his efforts that finally the Graduate School of Education was established at Harvard. He was the author of several books on education and many journal articles.

The two text-books cited above, one from Oxford, the other from Cambridge, are on a subject lately introduced into the university examination papers. Neither is of great merit. The Oxford book shows somewhat more mathematical and geometrical ability, and contains upwards of 1,500 examples, mostly of real interest. The elementary explanations of the Cambridge book are somewhat superior, and it deals with some interesting topics altogether omitted from the other treatise. It is, doubtless, the more convenient text-book for the teacher, though the less profitable for the earnest student. The great arbitrariness of the arrangement of both books is well shown by comparing them together in this respect. The theorems are pitchforked together upon no principle, and as for the examples, it is really curious to remark under what diverse heads one and the same proposition may be treated. The leading propositions of each book are mere illustrative examples for its rival.

The reason why analytical methods are more easily handled than the synthetical geometry is chiefly that the former arrange the whole subject in a perfectly

definite and unmistakable manner. No wonder a pupil is puzzled to apply a theory consisting of some thirty fragments not connected by any intrinsic bonds. As long as this state of things exists, notwithstanding the infinitely greater elegance of the pure geometry, its great practical use will be to serve as a guide in the reformation of analysis. The older treatises upon modern geometry did not exhibit this loose articulation, for the reason that they dealt chiefly with projective properties, and introduced what little metrics they gave as corollaries to the projective theorems. This could no longer be thought of, yet it suggests the proper way of arranging the subject. No text-book of either synthetical or analytical geometry omits that grand proposition of Cayley, that every metrical fact is a projective fact about a certain fixed quadric, or in plane geometry about the section of this quadric by the plane; nevertheless, writers of text-books put them together as if they did not really believe this. If it be true, surely an eternal fitness requires that the projective geometry of rectilinear diagrams and conics should precede all metrical matter, and that the Euclidean geometry should be taught as a particular case of the non-Euclidean.

Prof. Hanus's want of acquaintance with geometry, beyond what everybody knows, is very apparent. He applies general principles of pedagogy to give a few maxims too vague to be of much positive value, and upon that basis proceeds to pronounce *ex cathedra* upon perhaps the most difficult problem of intellectual education—the question of what, when, and how to teach in the first instruction in geometry. The illustrative exercises exemplify some methods in teaching applicable to many subjects and widely used in our schools. The course laid out could not well be much worse than it is, and is calculated to impart to the scholar ideas of geometry as confused as those of Prof. Hanus himself.

57 (7 September 1893) 178-179

Pioneers of Science.

By Oliver Lodge, F.R.S. Macmillan. 1893.

CSP, identification: MS 1365. See also: Fisch, *First Supplement.* This is unassigned in Haskell's *Index to* The Nation, vol. 1.

Sir Oliver Joseph Lodge (1851-1940) was an English physicist noted for his work with electromagnetic radiation. He took his Ph.D. from the University of London in 1877, and became a professor of physics there in 1881. He was knighted in 1902 in recognition of his work. After World War I, he became a leader in "psychical research," inspired by hopes of reaching his son, who had been killed in battle.

This is a very handsome volume, printed upon the heaviest calendered paper, full of attractive cuts, written in an easy style, dealing with an important and absorbing topic, and the work of an eminent physicist. The puzzle is to conceive how so good a man was ever induced to write a book upon a subject of which he appears to know nothing—the history of astronomy. The first chapter contains a sketch of the life of Copernicus, of which Mr. Lodge says "we know very little." Speaking for himself and his three friends who read his proofs, this seems to be very true. For those who have read Prowe's great biography in three volumes, it is

less true. Even before the appearance of that work, ten years ago, it would hardly have been admissible to say that "in study and meditation his life was passed." We now know that during most of his life he was an active member of Parliament, dealing with the most practical subjects, as well as managing the extensive possessions of the bishopric of Regensburg, and, in fact, almost governing it. When he retired from politics, so necessary was activity to Coppernicus (we follow the orthography approved by Prowe), that he took up the life of a practising physician, and continued in it almost to the very end of his long life. It is true, of course, that he was a man of study and meditation, yet an inaccurate impression is conveyed by the statement that his life was passed in such occupations as if in them alone. How much better it would be if writers—and the recommendation particularly applies to English writers—when they know nothing would say nothing.

Dr. Lodge says: "His father is believed to have been a German." True, this is the belief, and it is founded on an accurate knowledge of the genealogy of the family, which, for a family of merchants, was locally far from obscure. Much is made by Dr. Lodge of the Copernican theory of the precession of the equinoxes; but so far as this theory differed from what was involved in the general statement of Hipparchus and Ptolemy, it was utterly erroneous. There was a certain *intorta corolla*, which was invented to account for falsified observations, reported at a time when authority overweighed the testimony of the senses.

The account of Tycho Brahe is not so bad; but when we come to Keppler, grave inaccuracies reappear. This astronomer is represented as suffering all his life from "bitter poverty." The truth is, his first wife was a rich woman, and he was always fairly well, often very well, paid. But, with an entirely cheerful and contented disposition, he made it his business to grumble, because his pay was always in arrear (as was everybody's), and that was the way to get it. When his wife died, the money, it is true, went to the children; but by that time Keppler was pretty well to do. The account of Keppler's work is certainly not so bad as some recent English statements, drawn purely from their writers' imagination; but it is needlessly confused. The whole book is upon this low level of almost simple ignorance.

When Dr. Lodge comes to the discovery of Neptune, he makes a diagram of the orbits of Neptune and Uranus, and describes straight lines between corresponding places of the two planets. This, he says in the legend, illustrates the direction of the perturbing force. He forgets that a perturbation that remained constant would not be a perturbation, that the only way it can be detected is in changing the elements of the orbit, and that the unperturbed orbit is really as much perturbed as the perturbed orbit. Consequently, the whole question of the discovery of Neptune rests upon considerations which cannot possibly be popularized. There are some things which demand serious study. In these cases, all that can be done for the general reader is to show him clearly that such is the case.

We wish we could counterbalance these strictures by reporting that the book does something to inculcate the sincere spirit of inquiry, or gives any general picture of the life of "scientific pioneers," even though inaccurate in details, or that it communicates information about the labors of these men not better, more

easily, and more securely to be gathered elsewhere. We can say that it is a handsome volume, well suited to a person who wishes to know as much of the history of astronomy as he can gather in an hour or two.

57 (5 October 1893) 248

WAS COPERNICUS A GERMAN?

CSP, identification: MS L 159.33. See also: Fisch, *First Supplement.* The reply is unassigned in Haskell's *Index to* The Nation, vol. 1.

TO THE EDITOR OF THE NATION:

SIR: In your review of Mr. Oliver Lodge's book on the 'Pioneers of Science,' I was quite surprised to read that the father of Copernicus—and Copernicus himself necessarily too—was believed to have been a German. I had always taken it for a well-established fact that Copernicus was a Pole by birth and nationality. Copernicus is merely a Latinized form of the original name Kopernik, which corresponds with it in sound; and Kopernik is not a German, but a Slavonic name. It is not Polish, but Bohemian, and in the light of documentary evidence the family of the Koperniks can really be traced back to Bohemian ancestry. The *zemani* (knights) of Kopernik were Bohemian noblemen whose name appears in the historical records of the fourteenth century, and has been preserved to this day in the name of the Bohemian village Kopernik, their former seat, situated between the cities of Kosmonosy and Bakov in northeastern Bohemia. In the records of the fifteenth and sixteenth centuries the name can no longer be found, but a clue to its disappearance may be found in the archives of the city of Cracow. From the 'Acta Consularia Cracoviensia' (Proceedings of the Cracow City Council), *ad annum* 1396, we learn that A.D. 1396 citizenship was conferred upon Nicolaus Kopernik. The attesting witness, a citizen of Cracow named Dambrova, testifies that Nicolaus Kopernik had come to Cracow from Bohemia. This testimony is highly important. It is corroborated by the Bohemian origin of the name Kopernik, derived from the word *kopr.*

Historically, the naturalization of Nicolaus can easily be explained. The relations of the two Slavonic kingdoms, Bohemia and Poland, and notedly of the two great cities of Prague and Cracow, were friendly, and, as the two languages do not very materially differ, Mr. Nicolaus Kopernik found little difficulty in establishing himself at Cracow and finally obtaining its citizenship. He probably emigrated to Cracow towards the end of the fourteenth century. His family evidently was Bohemian, but his sons and grandsons spoke Polish. One of his grandsons, the eldest, also named Nicolaus (the frequent occurrence of this name in the Kopernik family is not to be overlooked), left Cracow in 1462 and settled at Thorn, where, on the 19th of February, 1464, his son Nicolaus, the great astronomer, was born.

These facts show pretty clearly, I think, that both Copernicus and his father were Poles of Bohemian ancestry, and, therefore, doubly Slavs. I add some minor facts which may throw more light upon this question: The coat-of-arms of the

Bohemian Koperniks bears the figure of a man; so does the escutcheon of Copernicus. Copernicus went to study at a Polish University, Cracow, in preference to that of Leipzig, and when in Italy, at the University of Padua, he registered as a Pole and not as a German.

J. J. KRÁL.

CHICAGO, ILL., September 18, 1893.

[If our correspondent has not heard of Copernicus being called a German, then he has not heard of perhaps the bitterest and most loud-resounding literary dispute of our day. We simply followed the authority of Prowe, whose great biography in three volumes (Berlin, 1883, 1884) occupied many years of its author's life, and was, we fancy, chiefly written in Thorn. Prowe says that until the father Niklas married Barbara Watzelrode, the family was "ein durch und durch deutsches Geschlecht." The greatest living historian of mathematics, Moritz Cantor, in a letter published in the Augsburg *Allgemeine Zeitung* for August 1, 1876, fully sustains this opinion. It has further been defended by Max Curtze, Perlbach, A. Knoetel, and S. Günther. The Italians, who almost lead Europe upon questions of the history of science, seem to favor the German side. All the arguments advanced by our correspondent will be found fully considered in the above works.

Although Greek was not taught in Cracow, writings brought to light in 1873 show that Copernicus had more knowledge in that direction than one would suspect from the 'De Revolutionibus.' He gives a Greek form of his name, Νικολαυς ὁ Κοπφερνικος, showing that he understood the first two syllables to mean *copper.* The astronomer's family, in fact, was a family of coppersmiths, by copper they had made a fortune. Now, the Thorn directory for 1422 shows that Margaret Koppernigk had business connections in the town of Frankenstein in Silesia, and in other ways the family has been traced to that point. Near that town is a hamlet named Köppernick, where there is an old coppermine. This neighborhood is distinctly German, and always has been so, although it is on the very border of Bohemia, and was at one time in the kingdom of Bohemia.

As for the assertion that Copernicus was registered as a Pole at Padua, that was investigated, at the instance of Prince Boncompagni, by Favaro, and found utterly baseless. On the other hand, Carlo Malagola, in his admirable work on Urceo Codro, showed that "Niccolò Kopperlingk di Thorn" had registered as a law student at Bologna in the album of the "Nazione Alemanna." This may not prove much, but it is, at least, not an invention. As for such coat-of-arms as this family of coppersmiths may have used, it can prove nothing at all. They never were ennobled. It may be granted that Copernicus (ἀνὴρ παντὸς λόγου κρειττων, Scaliger called him) was, as a member of the Polish Parliament, a sturdy adversary of the Teutonic knights. But on his father's side, the evidence seems to be that his blood was German.

We take this opportunity to correct an inadvertence not pointed out by our correspondent, by which, in the notice under discussion, we spoke of the bishopric of "Regensburg" in place of Ermeland.—ED. NATION.]

57 (5 October 1893) 251-252

MACH'S SCIENCE OF MECHANICS

The Science of Mechanics: A Critical and Historical Exposition of its Principles. By Dr. Ernst Mach, Professor of Physics in the University of Prague. Translated by Thomas J. McCormack. With 250 illustrations. Chicago: *Open Court* Publishing Company. 1893.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS L 159.22; MS 1513 (draft). In addition to reviewing this important book, Peirce participated intimately in the preparation of the American edition. One finds the following comment on that matter in the translator's preface. "The thanks of the translator are due to Mr. C. S. Peirce, well known for his studies both of analytical mechanics and of the history and logic of physics, for numerous suggestions and notes. Mr. Peirce has read all the proofs and has rewritten § 8 in the chapter on Units and Measures, where the original was inapplicable to this country and slightly out of date."

Ernst Mach (1838-1916), an Austrian physicist, is perhaps best remembered for his experiments on airflow, published in 1887. He took his Ph.D. from the University of Vienna in 1860. He was the founder of Mach's Principle (the name given by Einstein to Mach's thesis), which states that the properties of space have no independent existence, but are dependent on the mass content and distribution within it.

Dr. Ernst Mach's 'Die Mechanik in ihrer Entwickelung historisch-kritisch dargestellt' has for its ostensible purpose elementary instruction in the principles of mechanics. A secondary purpose is to narrate the history of that science. The ulterior design is to illustrate the author's views of the philosophy of science. This is the vital spark of the book; and doubtless this it is which recommends it to those who by "homilies" and "catechism" are engaged in propagating a "religion of science."

Considered as a history of mechanics, the work is admirable. It mentions all the great steps in the development of the science, down, at least, to 1847; it sets forth their nature, and explains them so lucidly that every reader will easily get a general understanding of them. We do not mean to defend all the criticisms upon the reasonings of Archimedes, of Galileo, of Newton, of Lagrange, of Gauss, and of many others, which cannot always meet the assent either of physicists or of logicians. Thus, when Mach objects to the assumption of Archimedes, that two equal weights at the ends of two equal arms of a lever will balance, that it is not evident, because the different colors of the lever-arms might affect the phenomenon, the obvious reply is that Archimedes did not mean that the two weights would balance in spite of everything. He did not mean, for instance, that nobody could push or blow one side down; nor, when we repeat the statement to-day, do we mean to deny that the magnetism of the lever might interfere with the experiment. When a physicist says that a certain phenomenon will happen under certain general conditions, he never means that no circumstances can possibly prevent it. What Archimedes meant was to lay down a proposition in regard to the geometrical relations of lever-arms to which nobody could object. Nor was he writing about the theory of cognition. He said nothing about the origin of the belief. He simply put forth the proposition as one to which, it was safe to assume, every sane man would assent.

Again, Galileo, being not much over twenty years old, investigated the law of falling bodies. It was not until he was past the age of eighty, and with failing powers, that he wrote out his reasonings. One point he made was that if the velocity of a falling body were proportional to the distance it had fallen from a state of rest, then, after it had fallen a unit of distance in a finite time, it would in precisely the same time have fallen double that distance. But the aged Galileo had evidently forgotten how the young Galileo had reached that conclusion, and his attempt to reproduce his former reasoning is impotent. Nevertheless, that conclusion does truly follow from that assumption. Mach flatly denies this, but he is wrong. Galileo's original reasoning was probably somewhat like this: Imagine two bodies, which we may designate as Achilles and the tortoise, to have fallen from the same height, but at such instants that at another and given instant the tortoise has fallen one yard and Achilles two. Then, on the assumption that the velocities are proportional to the spaces fallen from rest, Achilles will be falling twice as fast as the tortoise. Now, Galileo could easily show that this implies that Achilles had at *every* instant been twice as far from the starting point and had been falling twice as fast as the tortoise. Consequently, Achilles must have performed his total fall of two yards in the same time that the tortoise performed his total fall of one yard. But both bodies are supposed to fall by the same law. Hence, this is a law which would make them fall one foot in the same time as two feet. These instances illustrate how important it is that the reader should be upon his guard against Mach's very inaccurate reasoning.

The author declares that it is quite impossible to get a full comprehension of the different mechanical principles without being acquainted with the discussions which originally led to their acceptance. Probably he would extend the same remark to many other sciences. We might name this the embryological principle in pedagogy, since the embryologists inform us that each individual animal has in his growth to pass through a series of transformations which roughly copy those through which his race has passed in the pal ontological development. No doubt, this principle is important in teaching all those subjects in which the conceptions are really difficult, such as metaphysics, logic, ethics, political economy, and several branches of mathematics. Yet it might very easily be carried too far, and it probably has been carried too far in this very treatise. If a student's sole object is to learn mechanics as thoroughly and quickly as possible, there are certainly text-books enough in our own language which would better serve his turn.

For a good many years Germany has in philosophical matters been quite as anglomanian as England and Anglo-Saxon America have been tudescomaniac. Dr. Mach's metaphysics belongs to the good old Lockian sect of sensationalism. The proposition that all our knowledge rests upon and represents experience is nowadays accepted by sensualists and their opponents alike, the latter taking "experience," in its ultimate sense, for whatever has been forced upon our minds, willy-nilly, in the course of our intellectual history. To major force we can only submit, and it is idle to dispute the reality of such things as food, money, beds, shoes, friends, enemies, sunshine, etc. But the anti-sensationalists, or perhaps the most advanced of them, say that, having once surrendered to the power of

nature, and having allowed the futile ego in some measure to dissolve, man at once finds himself in synectic union with the circumambient non-ego, and partakes in its triumph. On the simple condition of obedience to the laws of nature, he can satisfy many of his selfish desires; a further surrender will bring him the higher delight of realizing to some extent his ideas; a still further surrender confers upon him the function of coöperating with nature and the course of things to grow new ideas and institutions. Almost everybody will admit there is truth in this: the question is how fundamental that truth may be. There are those who hold that while the brute compulsiveness of things may be said to constitute their reality, yet the whole fact of reality, with the relation of the ego to the non-ego, is not described until the individual Will is recognized as merging into the environing non-ego, as the individual instant of time merges into its past and future. For these thinkers, the line between fact and figment (which may or may not resemble and represent fact), so far as it can be drawn at all, is to be drawn between the involuntary and the voluntary parts of cognition; so that products of sense-perception—this chair, this table, this inkstand—belong to the realm of unquestionable reality. But they do not fail to remark that the process of compulsion exercised by the non-ego upon the ego is not altogether instantaneous. A part of it is continued through centuries. Nor is this compulsion always definitive. Resolute endeavor, aided by ingenuity and by favorable experiences, will often succeed in throwing off a part of the yoke. As for *immediate* experience, the individual sensation, it is the affair of an instant; it is transformed before it can be recognized; it is known to us as immediate only inferentially.

The sensationalists, and Dr. Mach with them, draw the line between fact and figment otherwise. Individual sensation is for them the only reality; all that results from the elaborative action of the mind is unreal. "Nature," says Mach, "is composed of sensations." A chair or a table is not real. "The *thing*," he tells us, "is only an abstraction." And again: "The world is not composed of things as its elements, but of colors, tones, pressures, . . . in short, what we ordinarily call individual sensations." Thus, all knowledge is based on and is merely representative of individual sensations, and all thought, all intellect, is of value only as subservient to peripheral or visceral sensation.

It was a favorite opinion of the pre-scientific sensationalists—Hobbes, Locke, and others—that abstraction and generalization were mere matters of convenience. Mach pushes this idea so far as to see no value in science except as an economy. "The end of science," he says, "is to *save* experiences, by the reproduction and anticipation of facts in thought." He does not make it quite clear why he should wish to save experiences, unless they are disagreeable, nor how he can save experiences except by slumbering. However, it is not our purpose to make objections, but only to outline Dr. Mach's opinion. It would seem that, all thought, memory, and higher feeling being held by the sensationalists as merely subservient to "individual sensations," if they could only be assured of a series of highly agreeable individual sensations for the rest of their lives, they should be content to forego all thought and all memory, and pass the time in an "Epicurus stye" of individual sensations.

In science, metaphysics may be useful in furnishing a system of pigeon-holes in which all possible facts may be conveniently arranged, but what the scientific inquirer chiefly asks of it is that it should efface itself, as the French say, and not block the road of experimental inquiry. But Dr. Mach's sensationalism appears upon most important points quite at odds with the conclusions of science, the nature of the difference being this, that the scientific men wish to leave questions to be settled by experiment, while Mach wishes to forestall this by deciding them by metaphysics.

For instance, the crowning doctrine of physics is that all the events in the physical universe are motions of matter. Heating and cooling, changes of color, sounds, electrification, all may have their physical qualities; but so far as they are extramental they are nothing but motions of particles in space. Many a metaphysician will offer to show you in advance that it must be so. The physicists at first propounded it as a question, and then went on to put that question to Nature in experiments. By this time they are pretty well satisfied that the answer is affirmative. But still they keep up their eternal teasing of the great mother, to see if the same answer will always be given. Mach, however, decides it is not so; his metaphysics has revealed that to him. He seems to deny the kinetic theory of gases, and regards the whole atomic theory as destined to be overthrown.

Again, Sir Isaac Newton formulated the three laws of motion which stand today in all the text-books. The first, due to Galileo, is that a body left to itself continues for ever to describe equal spaces in equal times on one straight line. The third, Newton's own achievement in great measure, the law of action and reaction, is that one body cannot be drawn back without other bodies on the same line being drawn forward to balance it. Now Newton, with his incomparable clearness of apprehension, saw that the third law implies that spatial displacement is not merely relative, and further that, this being granted, the first law implies that temporal duration is not merely relative. Hence, Newton drew the conclusion that there were such realities as Time and Space, and that they were something more than words expressive of relations between bodies and events. This was a scientific conclusion, based upon sound probable reasoning from established facts. It was fortified by Foucault's pendulum experiment, which showed that the earth has an absolute motion of rotation equal to its motion relative to the fixed stars. Moreover, Gauss and others were led to ask whether it be precisely true that the three angles of a triangle sum up to two right angles, and to say that observation alone can decide this question. Now, the mathematicians demonstrate that if that sum is not precisely two right angles, there is such a thing as an absolute velocity of translation. Whether there be or not is to the minds of scientific men a question for experiment and observation to decide. But Mach will not let it go so. His metaphysics tells him that there is no such thing as absolute space and time, and consequently no such thing as absolute motion. The laws of motion must be revised in such a way that they shall *not* predict that result of Foucault's experiment which they did successfully predict, and the non-Euclidean geometry must be put aside on metaphysical grounds. Is not this making fact bend to theory?

The English of this translation has received the emphatic approval of Prof. Mach himself.

57 (19 October 1893) 293-294

Negative Beneficence and Positive Beneficence. Being Parts V. and VI. of the Principles of Ethics.

By Herbert Spencer. D. Appleton & Co. 1893.

CSP, identification: MS 1365. See also: Fisch, *First Supplement*; MS L 159.34. MS 1371 is a draft of this review, not of Spencer's *Essays*—see 53 (8 October 1891) 283—contrary to what is suggested in Robin's *Catalogue* (p. 155). This review is unassigned in Haskell's *Index to* The Nation, vol. 1.

What magic art can Herbert Spencer practise to render his books so marvellously dull? It is a psychological problem. Dry they are not, nor are dry books more apt to be dull than others. Books that are both there no doubt are, say, in ethics, Whewell's 'Morality'; so there are books that just manage to keep dulness at arm's length by an agile exercise of a virtue the opposite of dryness, such as the 'Tale of a Tub.' But very often method, condensation, and business-like exclusion of all flights, which make up dryness, serve to keep the reader's mind alert. Stephen on 'Evidence,' Kant's 'Critic of the Pure Reason,' Ricardo's 'Political Economy,' Cremona's 'Geometry,' are dry to the last degree; yet, given an interest in the subject, any of them will detain your attention till you are exhausted by the mental labor it demands. On the other hand, such eternal monuments of dulness as the Koran, Volney's 'Ruins,' Tucker's 'Light of Nature,' Wordsworth's 'Excursion,' and, most overpowering of all, Spencer's 'Synthetic Philosophy,' are not a bit dry.

The phenomenon of dulness in Spencer's books is partly explained by his inimitable method of expressing himself—inimitable, at least, by any man of taste. He has disclosed the secret of it in his famous 'Essay on the Philosophy of Style': it lies in the "economy of attention." He artfully induces the reader to relax the muscles of the mind until nothing hinders the last stages of narcosis but the irritation produced by the Spenceresque diction. That, for all this, people read him is a great compliment to him and a great credit to them. The present volume opens thus:

"One division of an earlier work in this series of works—*The Principles of Psychology*—was devoted to showing that all intellectual operations are ultimately decomposable into recognitions of likeness and unlikeness."

And so the author proceeds through five mortal pages of platitudes about discrimination, in a psychological vein both cheap and superficial, and all to what purpose? Why, simply that he himself is intending to draw a distinction, one of the most familiar of distinctions, upon which all this dishwater about discrimination has just as much bearing, and no more, as upon any other distinction that any author might anywhere draw upon any subject. What an accomplished artist in tedium!

But the most stupefying principle in Spencer's writings is, for some readers, not his method of expressing himself; much less is it the essence of his thought, which (we need not say) is almost always striking and impressive. It is that the thought is developed in an old-fashioned way. In 1857 Mr. Spencer was not a remarkably well-read man in philosophy. He has himself admitted his ignorance of Kant. There is much in German idealism having an intimate relation to the philosophy of evolution of which he knew no more than an Italian monk would to-day know of Spencer. Outside his great conception, he was hardly more than abreast of his reader's stage of thinking. But to construct a durable system of philosophy it is necessary to build upon the solid foundation of deep reflection upon all that man has excogitated. During the many years that Spencer has devoted to writing his books, he has read little, especially of the kind that records advancing thought. The list of authorities in the volume before us illustrates this. It contains something like a hundred books. Two-thirds of them relate to savage life, and were very likely epitomized by readers. At any rate, it is for the most part crude and uncritical material. A quarter of the whole are old stand-bys which every educated man is supposed to be familiar with, or at least to know about. Then there are works on ethnology, anthropology, and biology, of a special character; and the small residuum is hardly calculated to give a serious idea of modern thought. The inevitable result has been that Mr. Spencer has fallen behind the times. He treats at wearisome length difficulties that are no difficulties, and consequences that are obvious, while many of the questions, objections, and suggestions that most interest the reader he soon finds have not entered into the author's head.

If Mr. Spencer's shortcomings and blindnesses were such as one could see were natural and almost inevitable to a mind engrossed with the conception of evolution, though they would make him more one-sided, they would not prevent the full presentation of his side of the question. But that is not the case; they in fact either have no relation to evolutionism, or, and that more frequently, actually antagonize it. Take, as the first example at hand, the passage quoted above, which represents every operation of the mind as a recognition of a likeness or the recognition of an unlikeness. According to this, every operation of volition, every operation of going to sleep, and every other mental operation, is but an act of recognizing. The first objection to this is that recognizing is something which takes place in the focus of attention, so that all the operations of the mind would take place in that focus, while all modern psychologists agree that most mental operations are so far into the dark that there is room for doubt whether they are in the field of consciousness at all. Clearly, a theory of the evolution of the mind would be aided by thus conceiving mind to shade off into unconsciousness. The next thing we notice about the opinion quoted is that it implies that all relations can be analyzed into likenesses and unlikenesses, the falsity of which has been recognized by every analyst who has seriously examined the question. Spencer says that sequence is unlikeness in order. Undoubtedly, a sequence is *an* unlikeness, but that is no sign that it is nothing but unlikeness, or nothing but a compound of likenesses and unlikeness. It clearly cannot be so, for when A is like

or unlike B, B has that same relation to A; while when A is followed by B, B is *ipso facto* not followed by A. Spencer is therefore tiresome, with his old-world psychology of likeness and unlikeness; it is particularly unfavorable to clear conceptions of evolution, which demand a recognition of the distinction between temporal relations and the mere acervations of the crudest form of generalization.

In ethics Mr. Spencer adopts the hedonistic theory. Almost no reason has ever been given for this, except that most dangerous of reasons, that we cannot help thinking so. We certainly are under no such necessity, and the theory ought to be regarded with great suspicion for the present, until scientific observation can be brought to bear upon it. At any rate, it is nothing but a disfigurement of evolutionary ethics, which it only weakens.

A system of philosophy ought to consist in the development of an idea, in the tracing of it out into its necessary consequences, and in the comparison of these consequences with experiences. This comparison will show how far the philosophy may be accepted and what modifications of it are required. This view makes of a system of philosophy nothing more nor less than a very general scientific theory, and it follows as a consequence that a system of philosophy, like every other scientific theory, must stand or fall with its power of making successful *predictions.* But Herbert Spencer, instead of trying to show what characters his first principles require the facts of biology, of psychology, and of sociology to possess—what those principles virtually predict—and then proceeding to compare those predictions with the facts, has begun by endeavoring to make out what the character of those facts *is*, and has loosely traced, as he went along, such harmony with his theory as he could. No philosophy can be firmly established in that way. Nevertheless, it is incontestable that shortly after Mr. Spencer began to write, the world began to take up the idea of evolution, and that today nine-tenths of all thinking men carry it just about as far as Spencer does. A man who should have a theory carrying it a little further, although that theory should be of such a nature that it should afford predictions mathematically deduced from it, and capable of being compared with experience, would find no encouragement to develop his theory, or even to state it as far as already developed, or to compare by laborious mathematical calculations the predictions already made with observations already made.

As for the present volume, its contents have little or nothing to do with the theory of evolution. Its discussions of special questions, such as the poor-laws, coming from such a mind as Spencer, must, of course, have their value. But its general principles are little more than prejudices, and that of a pretty old-fogy kind. No doubt, in questions of morals, prejudices are proper subjects of respect, especially when they are such as are shared by all parties and all civilized nations. At the same time they are not unlikely to contain errors which may become important when applied to novel questions. We have always understood that the last parts of the Ethics were to be the crowning glory of the Synthetic Philosophy. Will the world be persuaded that British toryism is the truth with which the universe has so long been in travail?

57 (26 October 1893) 313-314

Personal Recollections of Werner von Siemens.

Translated by W. C. Coupland. D. Appleton & Co. 1893.

CSP, identification: MS 1365. See also: Fisch, *First Supplement*; MS L 159.35. This review is unassigned in Haskell's *Index to* The Nation, vol. 1.

Ernest Werner von Siemens (1816-1892) was a German inventor and industrialist. He designed the first electric locomotive in 1879, which he demonstrated at an exhibition in Berlin. He is most widely known for his simplification of the selenium photoconductive cell, the "electric eye."

Such a number of the Siemens brothers have distinguished themselves in the engineering world, and especially in electricity, that really a guide-book to their respective performances was becoming a public desideratum. This book in some measure fills that want. Siemens Brothers, Siemens & Halske, and other firms of Siemenses, which practically make one concern, are renowned all over the world for executing in the most scientific way possible everything connected with telegraphy, such as operating land lines, laying cables, inventing and manufacturing all sorts of electrical instruments, preparing gutta-percha, making the glass required in the business, mining the copper, and also as inventors and manufacturers of regenerative furnaces and the regenerative Siemens gas-burner. The brother whose name is the most familiar to readers of English books was Sir William Siemens, who worked a great improvement in the quality of English enginery, and first forced the practical Englishman to entertain a sincere respect in practical matters for the scientific physicist. He died ten years ago, receiving the last distinction of a burial in Westminster Abbey. He was the fourth of the brothers. Hans, the second, made the glass. Friedrich, the fifth, inventor of the Siemens burner, devotes himself to regenerative combustion. Charles, the sixth, is probably the greatest business manager among them. Walter, the seventh, was the developer of the wonderfully scientific copper-mine in the Caucasus; while Otto, the eighth, was his successor there. Several others of the name have been connected with the business; but the most interesting man of them all has been the eldest of the brothers, Dr. Werner von Siemens, Member of the Berlin Academy of Sciences, inventor of the dynamo, discoverer of electrostatic charging by means of a battery, author of the Siemens unit of resistance, earliest adherent of Faraday's theory, and founder of the fortunes of the house of Siemens.

The mechanical perfection of the volume is worthy of the author. A sea-green linen binding, leaves of tinted paper so thick that 175 of them make an inch, tastefully cut pica, superb presswork, all proclaim that commercial remuneration has not been the first care. In fact, the work is in part quite frankly of the nature of a *réclame*. Dr. von Siemens had perhaps some share in a moral quality not unknown among his countrymen, a deep compunctious sense that his besetting sin was excessive modesty and self-depreciation, with an earnest resolve to fight it down, if so be by God's grace he might. But there are in truth several reasons why Werner Siemens did not for long receive all the credit to which he was justly entitled. In the first place, he was not a thoroughly educated physicist, and often made slips that show it. We will not recount the deplorable history of the bathym-

eter, but will rather select a less decided example. He says in this book (p. 327) that the problem of a flying-machine "is, for every mind possessing a slight mechanical training, a very simple one." He proceeds to say that inclined planes to assist in supporting the weight are worthless. Now, such argumentation was pardonable in Babinet forty years ago, but it has since that time been plainly shown that it rests on assumptions in regard to the motions of fluids which resemble but very slenderly the facts of nature. To-day, to accuse those who are engaged upon the problem of the flying-machine of ignorance is simply to expose one's own. Now, it is very natural that scientific men, with the enormous volume of new work that they have to examine, should be somewhat slow in finding out the real merit of those who so make themselves ridiculous. In the second place, Dr. Siemens stood, for the greater part of his life, outside the circle of German university professors, and advocated a theory to which they were disinclined. In some branches, such as that of philosophy, to be opposed to the official view in Germany means utter neglect. In physics, it is not so bad; still, even in physics nobody who understands the German can think that it could be unimportant. In the third place, Siemens's profession was one in which enormous profits were to be made—profits depending upon the man's reputation. No wonder, then, that, in that line, competition for the honor of discoveries should be particularly bitter and ungenerous. Add that Siemens himself had shown himself as adroit as anybody in combining scientific research with the pursuit of wealth, and it was hardly to be expected that the friends of his competitors should do him any public honor which, by any means not positively dishonorable, they could wrest from him.

After all, his scientific merit, which is certainly considerable, though hardly to be called great, is everywhere recognized. His highest capacity is not in pure science but in engineering, or, rather, it is of a military kind. He makes a marvellously clear and penetrating judgment quickly, and is ready to stake his fortunes upon it. The first successful deep-sea cable was laid in 1857 from Bona in Algeria to the island of Sardinia. The house of Siemens had furnished only the electrical apparatus; but Werner Siemens was to do the testing of the cable during and after the laying. He had no further responsibility. The cable was an old-fashioned affair weighing at least four pounds per yard. The problem of how to lay such a thing down, without breakage and without waste, at a depth of 1,000 to 1,500 fathoms, was so difficult that the different engineers who were to be upon the vessel found themselves, on the passage from Genoa to Sardinia, of the most widely different opinions about the matter. The man who was responsible, an Englishman, thought the best way was to proceed quickly, and let the cable run out without check, so as to bring no strain upon it. A French engineer, on the other hand, thought that the cable would hang down in a catenary curve, and would necessarily break. This was certainly far from a foolish idea. Siemens did not expect to have anything to do with the mechanical business, but declared that the operation could not be performed as the Englishman proposed, yet that it could be done by putting on a break sufficient to support a weight of cable equal in length to the depth of the water. They started from Bona in the evening, proceeding on the English plan. By dawn they found they had laid a

third part of the cable, though they had covered only a fifth of the distance. They had only just enough left to reach a shallow spot near Sardinia. The contractor then went to Siemens and requested him to lay the remainder of the cable. Many a man would have simply washed his hands of it. Why should he undertake so difficult a task and such enormous responsibility, without preparation, without any surplus of cable, and without adequate machinery? Incredible as it may seem, they did not even have a ship's log-line on board. Here was this untried problem of laying down perhaps a million dollars' worth of cable at the bottom of the deep sea, without losing it if one could help it. Yet Siemens does not seem to have hesitated. He laid the cable; and, although he strained it a little, he laid it successfully. We can well believe him when he says:

"The continuous mental strain, and the consciousness that any error committed may occasion the loss of the whole cable, makes the laying of a deep-sea cable a very anxious, and for a length of time thoroughly exhausting, affair for all concerned, and especially for the leader of the undertaking. Towards the end of the foregoing work, in which I would not allow myself a moment's rest and refreshment, I could only keep myself up by frequently taking strong black coffee, and required several days for recovering my strength."

The full account of Siemens's work will be most interesting to the engineer and to the man of science; but even the reader who may choose to skip all this will find it one of the most charming publications of the year. He will be surprised to find how many exciting adventures Siemens met with. At the very outset of his career he found himself defending a fort at Kiel against the Danes. For this purpose he was obliged to recruit a force, and, having enlisted them, to persuade them to go out of their own territory. It is needless to say that the defence was conducted on scientific principles. Submarine mines, or torpedoes, were used. They so scared the Danes that there was no attack. Another time he was shipwrecked in the Red Sea, and, with a whole steamer full of people, was cast upon a bare rock, where they nearly perished from thirst. Once, when he was laying a cable, a waterspout passed over the vessel. As for such incidents as accidental explosions, imprisonment, duels, being under fire in war, getting nearly frozen to death, complete destitution, peril from sharks and from robbers, danger of being put to death as a wizard, his life seems to have been full of them. There are many spirited descriptions of scenes and of phenomena of sky and sea. The anecdotes about curious personalities and amusing situations are many and good. Unfortunately the English of the translation, seldom excellent, is in many a place painfully awkward, quite ungrammatical, or downright unintelligible. The translator seems to be one of those persons who think they can improve upon accepted English idioms, and reform the language on a German model. There is, no doubt, some analogy between a rude, obscure style and disobliging, surly manners. It is singular how many admirers both find in Prussia. Siemens himself, though his style, when he is off his guard, is often delightful, yet explains, evidently with an approving conscience, that he has taken no pains whatever to write agreeably; and one of his main regrets at leaving the Prussian army was that he found the bluntness of the Prussian military manners so charming. He

then considered whether or not he should become a Prussian Amtmann; but the manners of that class were not sufficiently rasping for his taste. Such tastes are certainly not to be disputed, but we should like to have them expounded. The idea seems to be that whatever is unamiable is sterling and virtuous.

57 (9 November 1893) 350

NOTES

Attributed to Peirce by Fisch in *First Supplement*. Peirce, in a letter to F. C. Russell on 8 September 1894 (see MS L 387), claims authorship of this review, among others. This note is unassigned in Haskell's *Index to* The Nation, vol. 1.

—Beckford's heart would have leaped to embrace the delicious 'Vathek' of which Macmillan & Co. publish 150 numbered copies in America, with its dress of green silk reminding us of the annuals of his time, and its Arabian design stamped in gilt upon its side, such as Beckford's England could only dream of. Its etchings, by Herbert Nye, are steeped in the spirit of the story. The type, imitated, with supposed improvements, from a quarto Elzevir (a type not compressed like that of the pocket Elzevirs), is too modern in its businesslike roundness and with its typewritery short tails to the *b*s and *d*s, *p*s and *q*s. It is printed moist upon a hand-made laid and creamy paper. A book-lover might wish it were in duodecimo instead of a nine-inch octavo. For the "library editions" of books of entertainment—say, a stately Alfred de Musset printed in a way fit for a *recueil* of treaties—are not for those that read their books. This volume, however, is by no means a flagrant offender. The old duodecimo was calculated, when bound, to weigh half a pound; a post octavo, a pound. A pretty tome that weighs no more than a pound and a half, instead of two, like most of its octavo brethren, may still pass for light reading. This can be said for 'Vathek': though written for young people just beginning to disregard the advice of their elders as to what they had best read (*virginibus puerisque*, said Beckford), and though it *was* read by most of us at that susceptible epoch, yet, no matter how old we have grown, so long as memory holds her seat, we never can cease to remember the termination of this tale. The present editor, Dr. Garnett, says, indeed, that everybody must like 'Vathek' who likes its *genre*. That depends upon what its genus is taken to be. If it is to be defined as a romance which seeks to make amends for the sensuality of its earlier parts by a heart-rending and terrible ending—if, in short, 'Vathek' is to be tossed upon the heap where rot 'Les liaisons dangereuses' and such—then the remark is, beyond doubt, true. But if by its *genre* is meant that of the 'Arabian Nights,' we must protest that the greatest charm of those stories, their childlike irreflectiveness, is signally lacking in 'Vathek.'

—Nevertheless, it is an immortal book, and it was written in one sitting—one sitting, of three days and two nights! So said Beckford himself, fifty years later; and why doubt it? Dr. Garnett thinks he disproves this by showing that the author was engaged upon the composition for at least a year altogether. But that proves nothing. Of course, Beckford had been turning it over in his head for months; and, of course, he made corrections and even alterations, later. More-

over, when he had written it, he did not perceive that he had written it. He thought he had only made an amazing good beginning. "My Arabian tales go on prodigiously," says he, April 25, 1781, and a few days later, "The tale of the Caliph Vathec goes on surprisingly." But all the labor of the many months following, down to the end of 1784, went to the production of additions, which his own incomparable good taste rejected *in toto* at last. Recipe for making an immortal book: Write it at one sitting in 3 days and 2 nights; devote 3½ years to improving it, and then publish it as near as possible as it originally was. It was written, by the way, in French, and our English text is nothing but Henley's translation, which was published in advance of the original, in spite of Beckford's prohibition. The French reads far the better.

57 (16 November 1893) 370

CONUNDRUM

CSP, identification: MS 1365. See also: Fisch, *First Supplement.* This is unassigned in Haskell's *Index to* The Nation, vol. 1.

TO THE EDITOR OF THE NATION:

SIR: Part VII. of Murray's Dictionary seems to afford the derivation of the word *conundrum*, though the editor fails to notice it. As he says, there is evidence that it is an Oxford word, and he gives the following from Bedell, 1651:

"These *conimbrums*, whether Reall or Nominall, went down with Erasmus like chopt hay."

There surely can be no doubt what this word is. The reference to realists and nominalists shows that something in the scholastic philosophy is referred to; and "conimbrum" is easily recognized as meaning *argumentum Conimbricum.* The doctors of Coimbra, in their celebrated commentaries published in the sixteenth century, have in all cases a great deal to say of the "multiplex significatio" of one word and another. Indeed, such remarks are their great weapon. They used it for all it was worth, and a little more. Accordingly, a dealer in verbal quibbles might naturally have been called by Oxford students a *Conimbricus*, and his quillet *Conimbricum argumentum.* The original *c*, which this hypothesis requires, is preserved in another old form of the word, "conuncrum." *Conimbrica* was in the sixteenth century the most usual Latin form of the name Coimbra, though Conimbria is also common. Colimbria was obsolete. C. S. P.

57 (23 November 1893) 393-394

RITCHIE'S DARWIN AND HEGEL

Darwin and Hegel, with Other Philosophical Studies.

By David G. Ritchie, M.A., Fellow and Tutor of Jesus College, Oxford. London: Swan Sonnenschein & Co.; New York: Macmillan & Co. 1893.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; *List of Articles*; MS 1383 (draft).

David George Ritchie (1853-1903) was graduated M.A. from Edinburgh University in 1875. He then went to Balliol College, Oxford, where he was graduated B.A. in 1878. Ritchie was professor of logic and metaphysics at St. Andrews from 1894 until 1903. He served as president of the Aristotelian Society from 1898 to 1899.

An undeniable knack for clear analysis of questions has Mr. Ritchie. He shows symptoms, too, of a power of grasping and handling very broad philosophical arguments, without which power it would be useless to attack such a problem as he has set himself. His greatest fault is no doubt plain to himself, and should correct itself with time: it is that he has not thought enough. His own suggestions are not thoroughly worked out; and there are very pertinent questions that do not seem to have occurred to him.

The object of Mr. Ritchie's studies has been to determine how far the conceptions of Hegel can advantageously be applied in Darwinian speculation. But he does not pretend to offer any definite answer to the question, speaking, indeed, of his "philosophic creed" as "but partially formulated." Everybody qualified to form even a rudimentary judgment upon Hegel has long ago recognized in his 'Phenomenology' and 'Logic' rich mines of philosophic thought, whose ore, however, is intimately combined with the gangue of error—some say with more, some with less—from which we hardly know how to separate it. Germany, after following with docility the Hegelian method, was certainly in the best possible situation to judge it by its fruits. The outcome, as all the world knows, was an overpowering disgust; so much so that not the slightest attention is any longer paid in Germany to any of the Hegelian ideas. But by this revolution the Germans unconsciously confess their own weakness in logic—a weakness that has always been evident enough to foreigners. Even without that awful warning, American and English thought could never have been caught in Hegel's too easily detected traps. Not going in so blindly as did German thought, it will be able to derive more good from Hegel's endeavors. However, the world still awaits a satisfactory criticism of Hegel; and towards that Mr. Ritchie helps us little.

The author adopts provisionally the hypothesis of materialism. He speaks of "that materialistic monism which is nowadays the working hypothesis of every scientific explorer in every department." This attitude is certainly very much more moderate than that of the ardent Büchnerites and Haeckelites with whom Germany and the German parts of this country swarm, who inscribe Materialistic Monism upon their banners. Yet Mr. Ritchie unquestionably goes too far in saying that materialism is the working hypothesis of all explorers. For what is a working hypothesis? It is a problematic proposition that touches a question of

fact, and from which can be deduced definite consequences which the inquirer is engaged in testing by comparing them with observations. Now it would be absurd to say that an astronomer, a physicist, or a chemist is engaged in testing the consequences of materialism; for even if materialism be false, nobody doubts that the phenomena with which those men deal are the same as if it were true. Equally ridiculous would it be to say that a geometrician, or an historian, or an economist, or a student of jurisprudence is engaged in testing the consequences of materialism. Indeed, the only inquirers whom the question of materialism at all concerns are a certain class of biologists and a certain class of psychologists. Now there cannot be the slightest question that the initial working hypothesis of these inquirers must be the hypothesis of materialism. *Entia non sunt multiplicanda*, etc.; and their first business must be to see whether they can get along without supposing a second kind of substance and a second order of laws, or not. Therefore, to say that materialism remains the working hypothesis of those whom the question concerns is, after all, merely to say that nothing decisively fatal to that hypothesis has yet been brought to light. Even now, there are eminent biologists who hold that the hypothesis is refuted, and at least half the psychologists are of the same opinion—and this, although the question is whether the facts *can be made to fit* that hypothesis. Were the question simply whether the facts seem on the whole to be favorable or unfavorable to materialism, the vote against it would, of course, be larger.

Wherein do materialistic monism and idealistic monism differ? Only in this, that the former makes the laws of mind a special result of the laws of matter, while the latter makes the laws of matter a special result of the laws of mind. Now, one of the Hegelian ideas that Mr. Ritchie wishes to introduce (though it would be needless to raise the ghost of Hegel merely for this suggestion) is that of teleology—that states of things are to be explained, not by instantaneous conditions, nor by what went before, but by what is to result later. Indeed, Hegel or no Hegel, the materialist is plainly confronted with the following problem: The laws of matter are entirely blind, or non-teleological, only prescribing that in given relative positions the motions of particles shall have given accelerations: now, mind does not act blindly, but pursues purposes; therefore the problem is how teleological or purposed action can be a secondary effect of non-teleological action. This problem, says Mr. Ritchie, Darwinism solves. The tendency to an end, according to generalized Darwinism, or the tendency towards the production of definite forms of phenomena, is due to the combination of two agencies, the first being fortuitous insensible variation, or the gradual diversification of forms, and the second the destruction of forms whose modification shall have carried them over certain limits. This second agency may undoubtedly be supposed to be of the nature of mechanical law; but whether the phenomenon of diversification can be explained by the action of unyielding law is a question which Mr. Ritchie has yet to consider.

That which conferred upon the Darwinian hypothesis its sovereignty over subsequent thought was its power of explaining what seemed so mysterious by conceptions mathematically definite. The conception of fortuitous variation is so

exact that it can be expressed by a mathematical equation. In fact, it is expressed by the formula which expresses the conduction of heat, the action of viscosity, and the diffusion of gases. All these phenomena are explained by physicists as results of Bernouilli's law of high numbers, where the same idea of multitude reappears which is directly involved in the Darwinian hypothesis. The same formula shows itself in the doctrine of chances, in the theory of errors of observation, and in the logic of inductive reasoning. As well as we can make it out, the law of mental association, which is at least strongly analogous to induction, is probably of the same form. All these things seem to be connected. These considerations serve to illustrate, what can be shown in many ways, how the perfect definiteness of the conceptions which enter into a theory contribute to its fruitfulness. One of the worst faults of the Hegelian philosophy is that its conceptions are wanting in this definiteness, and that its consequences are not unmistakable. When Mr. Ritchie undertakes to "Hegelianize natural selection" by the remark that "Heredity and Variation are just particular forms of the categories of Identity and Difference, whose union and interaction produce the actually existing kinds of living beings," he makes us think that Hegelianism needs to be Darwinized much more than Darwinism needs to be Hegelianized.

The first essay in the book is entitled "Origin and Validity." Its purpose is to show that it is one thing to ask how a belief has arisen and another to ask how it is justified. Surely, we are not justified in believing a proposition not yet sufficiently proved. But no doubt that which suggests a proposition is one thing and that which proves it is another; and the formula of generalized Darwinianism would make this to be so. A theory arises by some slight original modification of an idea already in our possession. It is not yet justified, but it is provisionally allowed a place among the possibilities as a "working hypothesis." After that it has to fight its way, and it is by its results that it is destined sooner or later to be condemned or modified. But whether this is a complete and accurate formulation of the universal history of science is a question that it were best to be in no haste to answer.

But no sooner have we made the innocent admission that the question of origin is one thing and the question of validity another, than we find Mr. Ritchie purposes to use our concession as a gate at which Kant's transcendental proof and Hegel's idealism may gain entrance. If we wish to avoid the terrible loss of time from which Germany suffered during the Hegelian period, we shall do well to be very cautious here. A metaphysical philosophy, in the sense of that which is to be definitively accepted in advance of scientific inquiry, is, or should be, a system of pigeon-holes in which facts are to be filed away. Its first merit is to give a place to every possible fact. Whatever could conceivably be settled by experiment, metaphysics should abstain from settling in advance. Mr. Ritchie professes a readiness to admit all that Auguste Comte said in condemnation of what he called "metaphysics." What Comte called "metaphysics" was unverifiable hypothesis—unverifiable, not in the sense of supposing a fact not capable of being directly observed, for many indispensable hypotheses do that, but unverifiable in the sense of leading to no unmistakable consequences capable of

being put to the test of comparison with observation. An a-priori philosophy ought not to pronounce in advance upon the truth of anything which is capable of verification or refutation by subsequent experience. But beyond the realm of verification truth and falsity lose their meanings. Hence the moment a philosopher, upon a-priori or epistemological grounds, enunciates any proposition whatever as true, we are warned to be upon our guard against some jugglery. Where we have no scientifically observed facts to go upon, the prudent thing is to confess our downright ignorance. Even where we have such facts, we are subject to a probable error. From this pregnant fact, if one only takes it to heart, can be developed a whole Darwinianized Hegelism, having fruitful suggestions and indications for the prosecution of science and for the conduct of life.

57 (30 November 1893) 414-415

LELAND'S MEMOIRS

Memoirs.

By Charles Godfrey Leland (Hans Breitmann). D. Appleton & Co. 1893.

CSP, identification: Haskell, *Index to* The Nation. See also: Burks, *Bibliography*; Fisch and Haskell, *Additions to Cohen's Bibliography*; MS L 159. 38; MS 1384 (draft).

Charles Godfrey Leland (1824-1903) was an American author and journalist, who wrote under the pen-name "Hans Breitmann." He was graduated from Princeton in 1846, after which he undertook briefly the study of law. He was an extensive contributor to *Knickerbocker Magazine*, *International Magazine*, *Sartain's*, and *Graham's*.

In the preface to the first instalment of his biography, Mr. Leland resents the imputation of having "expatriated" himself:

"During more than ten years' residence in Europe," he says, "I had one thing steadily in view all the time, at which I worked hard, which was to qualify myself to return to American and there introduce to the public schools of Philadelphia the industrial or minor arts as a branch of education, in which I eventually succeeded, devoting to the work there four years, applying myself so assiduously as to neglect both society and amusements, and not obtaining, nor seeking for, pay or profit thereby in any way, directly or indirectly."

If Americanism were to be acknowledged according to St. Paul's rule of following the man's professions, Leland would have vindicated his in saying: "I hope at some future day that I shall still further prove that, as regards my native country, I have only changed my sky, but not my heart, and labored for American interests as earnestly as ever."

Mr. Leland is known chiefly as a mystic, corrected by science. He is himself a "Sunday-child," having been born on the ninth Sunday after Trinity, 1824. His parents were Episcopalians; but he was brought up in Philadelphia, and his own youthful diction seems to have had a Quaker twang. He was fond of Scriptural words and phrases from his tenderest infancy. The following is an example:

"Now, I was a great reader of Scripture; in fact, I learned a great deal too much of it, believing now that for babes and sucklings about one-third of it

had better be expurgated. The Apocrypha was a favorite work, but above all I loved the Revelations, a work which, I may say by the way, is still a treasure to be investigated as regards the marvellous mixture of neo-Platonic, later Egyptian (or Gnostic), and even Indian Buddhistic ideas therein. Well, I had learned from it a word which St. John applies (to my mind very vulgarly and much too frequently) to the Scarlet Lady of Babylon or Rome. What this word meant I did not know, but this I understood, that it was 'sass' of some kind, as negroes term it, and so one day I applied it experimentally to my nurse. Though the word was not correctly pronounced, for I had never heard it from anybody, its success was immediate, but not agreeable. The passionate Irish woman flew into a great rage and declared that she would 'lave the house.' My mother, called in, investigated the circumstances, and found that I really had no idea whatever of the meaning of what I had said. Peace was restored, but Annie declared that only the divil or the fairies could have inspired such an infant to use such language" (p. 29).

Here is another example: "My mother said that I, having had a difficulty of some kind with certain street-boys, came into the house with my eyes filled with tears, and said: 'I told them that they were evil-minded, but they laughed me to scorn' " (p. 52).

Although always very dull in mathematics, in other departments the boy seems to have been rather bright, and studious quite to excess. For some years the family sat under Dr. Furness's pulpit; and when they returned to the Episcopal Church Charles obtained permission to continue at the Unitarian chapel; but later, while in college at Princeton, he was confirmed in the parental church of his own volition, and seems still to hold to that faith, somewhat abraded, one may suppose, by historical studies. His father, he tells us, looked very much like Thomas Carlyle, and had the same sort of disposition, only much more so. He himself went to school to two celebrated men, Bronson Alcott and Sears C. Walker. It would have done one's heart good to hear the kindly but Gargantuan laugh of the wit-loving astronomer over Charley's speeches.

At the age of fourteen the boy was a tremendous reader in English and French. At fifteen he fell in love with François Villon long before that charming Bohemian had been made fashionable. It must have been in 1838 that his father presented him with 'The Doctor,' which can only mean the first five volumes of the original edition. "This," he says, "I read and reread assiduously for many years, and was guided by it to a vast amount of odd reading." He had already dipped into Henry Cornelius Agrippa and Giambattista Porta—in translation of course—and before he entered Princeton in 1841 he was deeply versed in (Taylor's translations unquestionably of) Porphyry, Proclus, Jamblichus, Hermes Trismegistus, and other writers who we now know, and as the boy then believed, drew from Egyptian lore. Not only that, but the still less easily intelligible 'Sartor Resartus' was read by him in its original numbers, and gone "through forty times ere I left college, of which I 'kept count.' " He had also read "a translation of Kant's 'Critique of Pure Reason,' the first half of it many times." This was Hayward's translation; and it will be observed that he studied the construc-

tive part of it with great determination, but was evidently less impressed with its destructions. Emerson's Essays appeared in the May before he entered college, and he had read them before he went to Princeton. He furthermore says that he had read Strauss's 'Life of Jesus.' If so, it must have been in some French translation; for George Eliot's English version did not appear till June, 1846. He had also dipped into Schelling (in French) and Spinoza's 'Tractatus Theologico-Politicus' (in English). He mentions many other books he had read at this time. All this is marvellous. He need not say that he did not fully comprehend these books, for the deepest historian of philosophy cannot boast of doing that; yet he certainly studied them sufficiently to avoid making any absurd slip about any of them now. We have lain in wait to catch him doing so; but though he shows a little heedlessness, he does not betray false pretensions. More extraordinary still, he enjoyed, and as he now assures us, he understood, Rabelais! If that be so, he must have been an uncanny boy. Many a lad who would be annoyed by the suggestion that he does not fully comprehend 'Don Quixote,' after he grows up is by no means confident that none of its wisdom escapes him. To like Rabelais implies a strong stomach for a boy. Reading always, everything, and with the speed of lightning, Leland passed the college years, and thoroughly unfitted himself for the business of life.

The summer he was graduated he went to Niagara. He says:

"It is usual, especially for those who have no gift of description, to say that Niagara is 'utterly indescribable,' and the Visitors' Book has this opinion repeated by the American Philistine on every page. But that is because those who say so have no proper comprehension of facts stated, no poetic faculty, and no imagination. Of course no mere description, however perfect, would give the same conception of even a pen or a button as would the *sight* thereof; but it is absurd and illogical to speak as if this were *peculiar* to a great thing alone. For my part, I believe that the mere description to a *poet*, or to one who has dwelt by wood and wold and steeped his soul in Nature, of a tremendous cataract a mile in breadth and two hundred feet high, cleft by a wooded island, and rushing onward below in awful rocky rapids with a mightly roar, would, could, or should convey a very good idea of the great sight."

In the same autumn Mr. Leland sailed in a packet for Marseilles, in company with his cousin Samuel Godfrey. From Marseilles he journeyed through Italy and passed the Carnival in Rome. Our eye catches this remark, which is sadly un-American:

"And here I may say, once for all, that one can hardly fail to have a mean opinion of human common-sense in government when we see this system of examining luggage still maintained. For all that any country could *possibly* lose by smuggling in trunks, etc., would be a hundredfold recompensed by the increased amount of travel and money imported, should it be done away with, as has been perfectly and fully proved in France; the announcement a year ago that examination would be null or formal having had at once the effect of greatly increasing travel" (p. 124).

Un-American, too, for the time, was what follows:

"I returned [from Europe] fully impressed with the belief that slavery was, as Charles Sumner said, 'the sum of all crimes' [an unlucky double slip this for John Wesley and 'the sum of all villanies']. In which summation he showed himself indeed a 'Sumner,' as it was called of yore. Which cost me many a bitter hour and much sorrow, for there was hardly a soul whom I knew, except my mother, to whom an abolitionist was not simply the same thing as a disgraceful, discreditable male-factor. . . . It was so peculiar for any man, not a Unitarian or Quaker, to be an *Abolitionist* in Philadelphia from 1848 to 1861, that such exceptions were pointed out as if they had been Chinese—'and d—d bad Chinese at that,' as a friend added to whom I made the remark" (p. 136).

Leland entered himself as a law-student in Heidelberg. Later, he studied philosophy in Munich, though already imbued with the spirit of physical science. In November, 1847, he arrived in Paris, where he entered the secret society which made the revolution of the following February. In January, he wrote home to his brother that there was to be a revolution on February 24, the very day on which it actually occurred. From Paris he went to London, and in the autumn came home, making the passage from Portsmouth to New York in thirty-five days. He now entered a law office; but, his father's resources becoming more limited, he began writing for publication in 1849.

"So time rolled on for three years. I passed my examination and took an office in Third Street, with a sign proclaiming that I was attorney-at-law and *Avokat.* During six months I had two clients and made exactly three pounds. [But he probably dealt with dollars in those days.] Then, the house being wanted, I left and gave up law. This was a very disheartening time for me. I had a great many friends who could easily have put collecting and other business in my hands, but none of them did it I felt this very keenly."

He next became editor of the *Illustrated American News*, owned by P. T. Barnum, whom he found remarkably free from guile. The circulation sometimes reached 150,000, yet Leland wrote the whole thing. The salary was so infinitesimal that he ultimately gave up, and became writing editor of the Philadelphia *Evening Bulletin.*

"All my long-suppressed ardent Abolition spirit now found vent, and for a time I was allowed to write as I pleased. A Richmond editor paid me the compliment of saying that the articles in the *Bulletin* were the bitterest published in the North."

But he was soon checked by the proprietor and left the newspaper. He now became editor of *Graham's Magazine*, raised the circulation from 0 to 17,000, and received a salary of $50 a month. It was at this time that the Hans Breitmann ballads began to appear. Mr. Leland does not state precisely at what date he became editor of *Vanity Fair*, which had been running for some time; on the breaking out of the war this comic journal expired. Leland presently became editor of the *Knickerbocker Magazine*, and later of the *Continental Magazine*—

an organ, he says, of the Cabinet. This was published in Boston, so that he went to live there in December, 1861. He was private in a volunteer company raised at the time of Lee's invasion of Pennsylvania; but they were not quite in the battle of Gettysburg. In 1866 he became managing editor of Forney's *Press*, and fought Andrew Johnson with all his might. After Grant was elected, Forney, who conceived that the result had been in great measure due to him, no doubt expected a place in the Cabinet; but he was not the only person who was at that time disagreeably surprised. Leland, most likely, expected a place abroad, though he does not quite confess it. At any rate, there is no doubt that he was now suffering from nervous exhaustion. His father's affairs had been prospering, and he found himself in a position to take a rest. As soon as he stopped work his system gave way, and for some years he was a downright invalid. At that point the volume before us breaks off.

57 (7 December 1893) 431

NOTES

"F. E. M." sends us the following correction:

"In your review of 'Memoirs,' by Charles Godfrey Leland, in No. 1483, vol. 57, you say, in speaking of the comic journal *Vanity Fair*, that 'on the breaking out of the war this comic journal expired.' I think you will find that the last number of *Vanity Fair* (volume 7) was issued in July, 1863. The volume was not completed, but only a few numbers issued. Vol. i, No. 1, of this entertaining and witty journal was issued December 31, 1859, and during its comparatively short career it was edited by Frank Wood, Charles Godfrey Leland (some time in 1861). 'Artemus Ward,' and Charles Dawson Shanley, successively."

57 (7 December 1893) 436-437

L'Ennemi des Lois.

Par Maurice Barrès. Paris. 1893.

CSP, identification: MS L 159. 26. See also: Fisch, *First Supplement*. This notice is unassigned in Haskell's *Index to* The Nation, vol. 1.

Another book by the new light, Maurice Barrès. From behind the stalking-horse of a story, slender, nebulous, unreal, unnatural, unpleasant, and Parisian, intended to be Wilhelm Meister-y in flavor, the author aims to make converts to his social theories, by vague reflections upon laws, upon young ladies of the new intellectual type, upon Russian princesses highly emancipated, upon the French reformers St. Simon and Fourier, upon the German reformers Lassalle and Karl Marx, upon Louis II. of Bavaria, upon dogs as companions and as educators of children, upon vivisection, and upon feeling versus thinking. The book produces an artistic impression, but could not well be feebler. One wonders by what courses the author, after he had once resolved to put forward his ideas, could have managed so to reduce his powers of persuasion below the average of think-

ing men, or, if there be any people whom he influences, what excessively peculiar persons they must be. The doctrine may be classed as sentimental individualism. The proposition is that the time has come to throw off laws—not merely those which are enforced by the tribunals, but every means by which the ideas of the public are made in any respect to dominate the conduct or ideas of the private individual. Especially, all that is traditional should be broken up. The use of language is not disapproved; still, it is traditional, and those only should teach who cannot speak—that is, the "hairy," and particularly dogs. As all thinking rests more or less upon common notions, individual feeling is much to be preferred to thinking. The conduct of people is to be made social through their sensibilities, but is not to be governed by the public will. The last words of the book are:

"For these persons other egos exist just as much as their own, so that the conditions of others' happiness are at one with the conditions of their own: they crush not the flowers they love to breathe. That they should suffer would lessen their own joy. Their refined sensibility suppresses every immorality."

All this is set forth in exaggerated and shocking language. Yet as the author never says he would carry out the principle to its furthest possible limit, nor ever says how far he proposes to carry it, except that he means to carry it further than we now do, there is really nothing new in the substance of the book, nor anything (except an extravagant tone) but what multitudes of sober-minded people would be quite disposed to admit. The truth is, that the essence of what he wishes to inculcate is nothing but a tone, and that tone he contrives to render as disagreeable and as repugnant to good sense and to good taste as the Enemy of Laws' worst enemy could desire. Excessively one-sided works, vigorously executed, have many times done much good by stimulating reflection. They can never do much harm, because they convince nobody; they can at most cause those who already entertain the same opinions to speak out. But the present volume can hardly have any effect at all, beyond that of amusing a few people who may like to listen to the praises of Louis II., to analyses of modern libertinism, and to twenty-page dissertations upon St. Simon. The admirers of M. Barrès think him startlingly brilliant and original; his detractors treat him as little better than a vulgar *poseur.* The truth is, he has nothing definitely new to say, but often succeeds in imparting to old ideas a pretty well-emphasized tenor of expression. Here is a specimen or two.

"I say things abruptly, as I feel them. Besides, I agree with everybody who feels anything. No matter for the formulas by which we express our emotion, the important thing is to be warmed by life. If that lady interests herself in what does not interest me, what right or propriety would there be in substituting my feeling for hers?"

"Many of the ideas of St. Simon have filtered into modern Europe: but they have not been improved in their renewal. The industrial system of which he dreamed is just our moneyed society against which the reformers of to-day revolt."

"Clara Pichon-Picard, wonderfully intelligent, saw less accurately into life than the frivolous Marina, who decided all questions under the guidance of a sensuality which is precisely the sense of life."

"They slept, and met in dreams. There was an orchard surrounded by a high hedge, and they walked in procession towards the rising sun, amid children and animals, under the direction of Hairy the Second [a dog], their monitor. All busied themselves with burning their material humors in the open air. To expel the useless, to keep the essential, there lies the whole secret."

"Kennel, stable, hen-house, fish-pond clustered about a peaceful house, a copy of the Museum laboratory [a laboratory of vivisection], but a reversed copy. Here the problem is, not to destroy humble beings for the joy or material benefit of augmenting the sum of knowledge! Here, in an atmosphere purified of all dead ideas, are formed young persons who breathe nothing but what is living, and who develop that new sensibility which the new aspect of the world requires. Yes, here in the open air is a laboratory of sensibility."

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