

CHARLES SANDERS PEIRCE: CONTRIBUTIONS TO THE NATION

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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.

Mediaeval 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 *hæcceity*; 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 *coryphæi* 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, Crookes, 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 nebulae, 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 nebulae 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 nebulae 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 nebulae. 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 nebulae. None of the nebulae 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 repository 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 owing 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 conceive 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 "Vorlesungen über die Menschen- und Thierseelen."

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

We said last week that the best book for instruction in logic in colleges was Fowler's "Deductive Logic." We added that a young man who has been through it under a teacher of power will have had his mind enlightened and strengthened, and will be the better prepared for life. In point of fact, we did not intend to apply these expressions to Fowler's "Deductive Logic," but to his *Deductive and Inductive Logics* taken as one work. The mistake enables us to express, in a more emphatic way, our opinion of the almost utter worthlessness of deductive logic in education, except as an introduction to the logic of science. In former ages, logic was a pretty good representation of the methods of thought of the greatest minds. The systematic exposition of the art of thinking naturally lagged behind the practice, and men always reasoned better than if they had strictly followed the rules of their logic. Still, the discrepancy was not very great. The logic of Petrus Hispanus (which was written about 1270) exhibits well the character of thought of his time, as that of 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. Jeyons, 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.

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.

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 1908 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.

KETNER AND COOK—CHARLES SANDERS PEIRCE

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: $A \rightarrow B$.

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

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

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

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

Hardly a model of synthetic orderliness.

But what does the author do with the great body of relations? What pigeon-holes has he for them in his scheme of arrangement? Take, for instance, the relation of striker to struck. A man's striking another constitutes certainly no 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*; Ejsch 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 tri-dimensional space, invites the mathematician to the exercise of his powers. And then there is the psychological phenomenon of a multitude of sensations as unaltered by the operation of the intellect, and as near to the first impression of sense, as any perception which it is in our power to extricate from the complexus of consciousness—these sensations given, too, in endless variety, and yet their whole diversity resulting only from a triple variation of quantity of such a sort that all of them are brought into intelligible relationship with each other, although it is perfectly certain that quantity and relation cannot be objects of sensation, but

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

In view of these different kinds of interest which the scientific 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, tinged 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 heuristic 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

64 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 reexported 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 planetæ"), (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.

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-eighths 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 superseding 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 $2\frac{1}{2}$ feet would make a yard, 4 miles a league, 5 feet a fathom, 625 acres a square mile, 1,953,125 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 decimetre"; "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 *inhibibility* 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 αἰ.*" Such expressions are natural to Chicago journalists, yet, emphatic as this is, we soon find the *κτῆμα ἐς αἰ.* 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 main-spring 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 anecdotal, 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 "anecdotal," 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 well-nigh 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 secondary 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 prestigation. 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 P₁, P₂, P₃, etc., indistinctly recognized.

The suggesting object, S, has these same predicates, P₁, P₂, P₃, etc.

Hence, S is of the kind M.

This is hypothetical 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, words "all . . . are . . ."

But in the second . . . Algebra of Logic covered syllogism only, no one could doubt . . . d to perform without it the extremely complicated pieces . . . ng which it can work out by purely mechanical process . . . any instances of reasoning of this sort can be got from rea . . . As Dr. Schröder points out, it is not strange that this kind . . . seldom attempted at a time when it was almost impossible of accom . . . 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 sociologist 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 ~~work~~ 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 it 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 geodesics 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 Chaldaea. 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. I.

—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 false 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 threat 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.

LARSEN 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 scolding 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 uncommoh, 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 official 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*, v.

Dascom Greene (1825-1900) was graduated in 18 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.

BOSTON UNIVERSITY.

The following are the courses for the present year, at Boston University, under the direction of Prof. B. P. Bowne and Dean Huntington.

PSYCHOLOGY. Thought studied as a fact; its forms and laws investigated; Current Theories expounded and criticised. Five hours.

LOGIC. Thought studied not as a fact, but as an instrument of knowledge. Investigation of the laws, forms, aims, and methods of mental activity. Five hours.

THEORY OF KNOWLEDGE. The study of thought as a process supplemented by the study of knowledge as its product. Knowledge defined, and the conditions, subjective and objective, of its validity investigated. The claims of scepticism, agnosticism, etc., considered at length. Three hours.

METAPHYSICS. Modifications of ontological and cosmological ideas in the light of rational criticism. Four hours.

PHILOSOPHY OF THEISM. The logical value and foundation of Theism considered. Four hours.

HISTORY OF ETHICAL THOUGHT. Christian Ethics. Text-book and lectures. Five hours.

PHILOSOPHY OF ETHICS. Critical and constructive review of ethical theories. Psychological questions as to the nature and origin of moral faculty ruled out as irrelevant. Two hours.

HISTORY OF PHILOSOPHY. From Descartes to the present time. Five hours. The Philosophical Club, organised in 1886, has since that time maintained stated meetings for the furtherance of its members in philosophical studies.

Last year, under the auspices of the University, a special course of five lectures on Educational Psychology was given before large audiences by William T. Harris, LL. D. The topics treated were as follows:

1. Introspection contrasted with external Sense Perception.
2. Mental Pictures *versus* General Ideas.
3. The Logical Constitution of Sense Perception.
4. Physiological Psychology.
5. The Psychology of Mathematics, Aesthetics, and Ethics.

The courses are for single terms only.

B. P. BOWNE.

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The courses are unified and thorough. A voluntary course in the History of Philosophy is given; and advanced courses will be offered this year in Modern Philosophy from Descartes to Kant, and in English Ethics from Hobbes to Stephen. The instructors are Professors Griffin and Emmot.

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