

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.