works if he would, Mr. Carr next undertakes to show that the Indian is known within historic times to have built similar though smaller works. Arraying a mass of testimony from the old and even later writers, sufficient in quality and quantity, he succeeds in doing this.

There is one natural objection to his conclusion. While some, or most it may be, of existing mounds should be traced to early generations of the red Indian, or of races on his plane, he does not admit that it is supposable that another race, possibly of higher grade, may have built other of the mounds.

We suspect that the truth of this last proposition is to rest on other investigations than Mr. Carr has yet touched. Manifestly, that the Indian could have built the mounds does not prove that he did; and, even if it be proved that some of the mounds in question can be directly traced to him, it does not follow that others may not have been built by a different people, since mound-building cannot be confined historically to any single people or any single continent.

Perhaps Mr. Carr has thrown the burden of proof upon the opposers of his theory, since it may be fair to argue that there is no necessity of supposing another race to account for the mounds. Granting that Mr. Carr establishes his point from the external evidences of the mounds, there yet remains a test for his theory in the contents of the mounds. Mr. Carr acknowledges this shortcoming of his argument, and promises in due time to examine the question from the testimony of the skulls and relics of workmanship, as well as from evidences of parallel custom, which can be drawn from the records of the exploration of the mounds. These, it seems to us, are to be the final tests. It is clear that history cannot settle the question, but archeological investigations may. We suspect that Mr. Carr wrongly estimates the comparative value of the two methods in a question of this kind. He says that the investigators who have given rise to the views which he combats have been "practical explorers, who have brought to the investigation a certain number of facts, chiefly cumulative in character, and who have not as a rule been possessed of that measure of historical information which is necessary to a correct interpretation of these facts." It is indisputable that the historical evidence accumulated by Mr. Carr may be helpful; but the fact still remains, that this evidence must be viewed in the light of the archeological results. It may be safe to grant all that these historical evidences prove; but arguments respecting the origin of the mounds, based on them, become inferential, and may or may not accord with the archeological demonstrations. There can be no question which is to be the ultimate tribunal.

SIDGWICK ON FALLACIES.

Fallacies: a view of logic from the practical side. By ALFRED SIDGWICK, Berkeley fellow of the Owens college, Manchester. New York, *Appleton*, 1884. (International scientific series.) 16+375 p. 16°.

IT does not often fall to the lot of a reviewer to find so little to praise in a book by so clever a writer and clear-headed a logician as the author of the treatise on fallacies, which has appeared in the International scientific series. What most obviously calls for complaint is its want of adaptation to the main purpose for which, by its publication in this series, and by the explicit avowal of the author in his preface, it seems to have been designed; namely, to be of profit to the general reader. No reader who has not become familiar with the technical language of logicians, and even with many phases of logical controversy, is at all likely to follow our author with sufficient interest to so much as comprehend what he is talking about, much less to carry away a clear and lasting impression of important truths. Not that much knowledge of logic is presupposed; but the discussion is so full of abstractions and subtleties, of nice distinctions which we are presently told are no distinctions at all, and identifications of things we had supposed very unlike and which we are presently told we would better keep apart as of old, that if we add to the intangibility of such questions the difficulty, for novices in logic, of promptly seizing the precise force of the terms which are necessarily employed, we cannot expect any very valuable results from their perusal of the book before us.

But, in point of fact, it is not to tyros only that the book will be a disappointment. There is much balancing of views on nice points of language, and every now and then a most refreshing bit of sarcasm, for our author has a keen eye for all sorts of logical weakness; and there is often plain talk about the practical limitations to which we are subject in the search for truth. But there is an extraordinary absence of decision and concentrated statement, — qualities indispensable to the success of a work of this kind. On almost every point the author comes to the conclusion that little or nothing which is useful can be said about it. With

this conclusion we are not prepared to express a disagreement; but we feel quite convinced of the unprofitableness of reading three or four hundred pages of particularly uninteresting matter to arrive at it.

There are two reasons why it seems especially ungracious to speak so slightingly of the value of Mr. Sidgwick's book. In the first place, almost every page bears evidence of the author's logical power and literary cleverness; and many passages are really good and valuable. There is an excellent chapter on the burden of proof; the remarks on the variation in the meaning of words, and many other detached discussions, are admirable; and the author is always refreshingly severe on the subject of baseless metaphysical speculation. It is pleasant, too, to come upon such human, unscholastic ways of putting things as we are frequently treated to. Thus, on p. 128: —

"For, besides the real danger of platitude, there is an opposite danger to be avoided; namely, that of unduly and vexatiously stopping an argument to have the terms explained. Without wishing exactly to defend those who made Socrates drink poison, one still cannot help recognizing that there is a limit, beyond which the laudable desire for definiteness loses its value, and becomes a hindrance and a snare. There is something so fatally easy in the attitude of a sceptic or mere questioner. Any child can keep demanding explanations, any man sufficiently stubborn can delay the most important truth by pretending not to understand its import. An obstructive policy of this kind requires no great intellectual power; and, when adopted solely for obstructive purposes, it demands, as much as any thing, a rule of urgency. Life is not long enough for exhaustive explanations."

And on p. 289: —

"Nothing could well be more confusing than an attempt to apply the cumbrous machinery of the syllogism to arguments met with in real life. And whoever has tampered with his mother-wit by substituting for it a clumsy logic depending on elaborate mnemonics, must, no doubt, pay the penalty in loss of power, so long as the mischief remains."

In speaking of the methods of induction, as stated by Mill, the author judiciously remarks, —

"Since there may possibly be, in some quarters, a disposition to take these methods for more than they were probably intended to be worth, there will perhaps be some use in reminding the reader that it is the guarding against the danger to which each method is liable, that is in every case the all-important circumstance, far more so than the mere employment of this or the other method."

And a clever hit is made in introducing these methods: —

"While, as their author himself (and more lately, Professor Jevons) expended labor in showing, none of these is, except in an ideal sense, completely satisfactory" . . .

The other reason for one's dislike to condemn the book as a whole is, that the author's faults are so largely the *défauts de ses qualités*. His mind is so open to every argument that can be urged on either side of a question, that he finds it much harder than ordinary mortals do to come to a decision; and he is so conscientious in his attempt to tell the reader the whole truth, that he gives some measure of approval to any view that has the least proportion of truth in it. This scrupulousness is most annoying and obstructive when he deals with the definitions of his terms. Here we have to watch a long process of painful labor, sometimes over very simple matters, almost always with very little result. It is, of course, a vulgar error to suppose that a scientific definition ought to be so framed that no doubt can arise as to any individual case being comprehended under it. Scientific men well understand by this time, that, however we may frame our definition, there will always be a strip, more or less narrow, of debatable ground along the boundary. But Mr. Sidgwick is alone, we may hope, in going a step farther, and carefully making his boundary run in such a way that the debatable ground shall be co-extensive with the whole territory. This peculiar excess of refinement, which so often interferes with the effectiveness of our author's work, strongly reminds one of two recent important works on ethics and economics, and almost demands the coining of the adjective 'Sidgwickian' to describe it.

Of logical errors there are few, if any, in the book; but the author occasionally illustrates his own doctrine of the difficulty of establishing a charge of fallacy, due to one's inability to know how a given argument was intended to be understood by its proposer. Thus, in the quotation discussed on p. 259, *et seq.*, we can but regard the criticism as captious. If the passage is an example of false analogy at all, it is so in a very mild degree; nor are the two examples on p. 264 strikingly in point, if at all. And this leads us to mention one final criticism on the work, in so far as it is intended to be practically useful. There are very few illustrative examples, and a notable absence of any discussion of the fallacies which have actually played a part in the history of intellectual progress. The author does not familiarize the reader with the dangers of fallacious reasoning by concrete instances, or stimulate his interest by pointed discussions involving the applications of principles rather than the principles themselves. It would be time to write a book in the spirit of this one, when everybody had

become as good a scientific thinker as Faraday or Darwin; but to-day, while fallacies of the crudest kind are rampant in every field of discussion, from religion and party-politics to biology and political economy, something less ethereal and impalpable than this statement of the necessity of philosophic doubt would have been far more useful.

RECENT PROCEEDINGS OF SCIENTIFIC SOCIETIES.

Academy of natural sciences, Philadelphia.

July 8. — Professor Angelo Heilprin described a new trilobite from Walpack Ridge, about ten miles north of the Delaware Water-Gap. The tail-piece, which was the only part of the animal found, indicated an individual some six or seven inches or more in length, and clearly demonstrated its relationship to the genus Phacops, sub-genus Dalmania. Among its faunal associates were Phacops Logani, P. (Dalmania) pleuroptyx, Acidaspis tubercularus, Spirifer macropleura, Atrypa reticularis, Strophomena punctulifera, S. rhomboidalis, Orthis subcarinata (or O. multistriata?), Merista sp., etc. The horizon is that known as the Stormville shales (lower Helderberg), evidently the equivalent of the Delthyris shales of the New-York geologists.

Philosophical society, Washington.

April 26. — Prof. J. R. Eastman reported the discovery of a mass of meteoric iron at Grand Rapids, Mich. An analysis by Dr. F. W. Taylor gave: iron, 94.54; nickel, 3.81; cobalt, 2.40; insoluble, about .10; total, 100.85; specific gravity, 7.53. —— Mr. William H. Dall read a paper entitled 'Certain appendages of the Mollusca.' —— Mr. J. S. Diller read a communication on the volcanic sand which fell at Unalashka, Oct. 20, 1883, and some considerations concerning its composition. The substance of this communication has already appeared in *Science*. There ensued a general discussion of the nature and properties of volcanic dust, and of the theory which ascribes recent peculiar meteorologic phenomena to the dust ejected from Krakatoa. Capt. C. E. Dutton argued that the formation of volcanic dust particles by the bursting of bubbles tends to give them a somewhat definite general size, and does not produce a large amount of dust fine enough for indefinite suspension. The opposite view was maintained by Prof. H. M. Paul, and was sustained by Mr. Diller, who said that the microscope revealed no limit to the fineness of the Krakatoan dust. The higher the magnifying-power applied, the greater the number of particles visible; and this relation extends to the limits afforded by the capacity of the instrument. Professor Paul thought the violence of the Krakatoan explosion was competent to charge the atmosphere at very great altitudes, and considered the fineness of the dust a sufficient explanation of its indefinite suspension. Mr. William B. Taylor suggested that electricity might be an efficient cause of suspension. It is a common phenomenon of volcanic eruption; and dust particles charged with the same kind of electricity as the earth would be repelled not only by one another, but by the earth. The period elapsing between sunset and the red afterglow testifies to the great altitude of the phenomenon; and at such altitude the air is not only very rare, but is anhydrous, and the discharge of electricity is impossible.

May 10. — Mr. G. H. Williams of Johns Hopkins university addressed the society on the methods of modern petrography, classifying them as chemical, mechanical, optical, and thermal, and explaining their several functions. —— There followed a symposium on the question, 'What is a glacier?' Mr. I. C. Russell defined a glacier as an ice-body originating from the consolidation of snow in regions where the secular accumulation exceeds the loss by melting and evaporation (that is, above the snow-line), and flowing to regions where loss exceeds supply (that is, below the snow-line). Mr. S. F. Emmons defined it as a river of ice, possessed, like an aqueous river, of movement and of plasticity. In virtue of plasticity, it adapts itself to the form of its bed. The *névé* field is the reservoir from which it derives its supply of ice, and the initial impulse of movement. Until the *névé* moves from its wide and shallow bed into a narrower and deeper one, and thus gives outward proof of the plasticity of the ice of which it is composed, it does not become a glacier. It may become crevassed, and it may carry blocks of rock on its surface without losing its *névé* character. Mr. W. J. McGee said that the phenomena of glacier ice and *névé* belong to a graduating series, and can be only arbitrarily discriminated. He regarded as artificial and incompetent, classifications depending on acclivity of the ice-bed, on constriction of the ice-body, on ability to sustain bowlders, and on rate of motion. All things considered, the most satisfactory line of demarkation is the snow-line. Mr. William H. Dall discriminated masses of ice moving in a definite direction from fields of ice practically stationary, restricting the term 'glacier' to the former. A glacier is a mass of ice with definite lateral limits, with motion in a definite direction, and originating from the compacting of snow by pressure. Prof. T. C. Chamberlin said that the subject illustrated the fact that hard and fast lines belong only to nomenclature, whereas nature is characterized by gradations. The true distinction in this case is not structural, but genetic. There is an area of growth and an area of waste to every glacier. It is only superficially that the area of growth coincides with the *névé*, and the *névé* field is accurately defined only on the summer day of maximum waste. Capt. C. E. Dutton said that his intended remarks had been anticipated by Professor Chamberlin. Definition can