The fact is that M. Eymieu has done his cause no good in the eyes of a scientific reader. His selections of men have not, in general, been made with care; indeed, they have not been made with ordinary knowledge. He has not scientifically gone to work to secure his information, as witness his uncertain results concerning the faith of Simon Newcomb. He has simply set about to support the belief of the uneducated or the half educated man of his own religious faith. It cannot be expected that he should have done for the dead what Professor Leuba did with respect to the religious beliefs of living scientists, but no one who has worked in the history of mathematics can fail to see that a much stronger case could have been made, and legitimately made, if the author had studied the problem with greater care.

It is evident to everyone that the most difficult thing to weigh in a scientific balance is the religious belief of mankind. The reasons are equally evident. One thing is clear, however,—that the study of the exact sciences no more tends to lessen this religious faith than the study of commerce, of civics, of sociology, or even of theology. The history of the exact sciences offers abundant illustrations of this fact, and evidence of a more convincing kind than that which M. Eymieu has adduced. Indeed, it would be a strange and inexplicable thing if scientific investigation should fail to show that mathematics, that branch of knowledge which is continually in touch with the infinite and is continually revealing the mysteries of the eternal, should fail to foster religious faith to an extent not reached by the other subjects of human study.

DAVID EUGENE SMITH.

The Early Mathematical Manuscripts of Leibniz. By J. M. CHILD. Chicago, 1920. iv + 238 pp.

THIS important work consists of translations of various Latin manuscripts of Leibniz found by Dr. C. I. Gerhardt in the Royal Library of Hanover about seventy-five years ago. These manuscripts were published by Dr. Gerhardt as parts of three works which he wrote on the origin of the differential and integral calculus,* and have long been known in their

* Historia et Origo Calculi Differentialis, a G. G. Leibnizio conscripta, Hanover, 1846.

Die Entdeckung der Differentialrechnung durch Leibniz, Halle, 1848.

Die Geschichte der höheren Analysis; erste Abtheilung: Die Entdeckung der höheren Analysis, Halle, 1855.

Latin form to scholars who cared to consult these sources. There is no denying the fact, however, that the present generation of students in this country has not been trained to look upon Latin as a medium of intellectual exchange, and even in England it is undoubtedly true that the language has lost enough of its former prestige to make a translation of such material as the early manuscripts of Leibniz a great convenience to anyone who is interested in the subjects treated. For this reason there can be no question as to the value of this work and as to the renewed interest which it will awaken in the problem of the actual contribution of Leibniz to the revealing of the laws of the calculus and to the invention of a convenient symbolism.

The first document translated is the Historia et Origo, which Gerhardt published in 1846. This article is written in the third person, in the style which American readers have recently seen in the autobiography of Henry Adams, and was probably intended for anonymous publication. The story is here told, in popular fashion, of the steps which led Leibniz to his discovery. His disturbance over the publication of the Commercium Epistolicum in 1712, a publication which everyone who has examined it must admit is not a judicial document, seemed to him to require an answer. For this reason he proceeded to relate the incidents of his early publication, at the age of twenty, of the De Arte Combinatoria (1666); of his interest at the same time in general questions of analysis, including the theory of finite differences; of his visit to London at the age of twenty-six; of his acquaintance with Huyghens; and of the gradual development of his ideas of the calculus. He summarizes his case against the British school in the following words:

"Since therefore his opponents, neither from the Commercium Epistolicum that they have published, nor from any other source, brought forward the slightest bit of evidence whereby it might be established that his rival used the differential calculus before it was published by our friend;* therefore all accusations that were brought against him by these persons may be treated with contempt as beside the question."

The second body of translated material consists of several manuscripts of the period 1673–1680. These relate to various topics bearing upon the calculus. For example, on November

* That is, by Leibniz himself.

1, 1675, he treats of moments about axes, giving none of the new symbols, *omn.* being still used instead of $\int$. Ten days later (November 11), the symbol $\int$ is used, x/d and dx are stated to be interchangeable, differentials of the second order are rejected, the problem is solved of finding a curve such that the rectangle contained by the subnormal and ordinate is constant, and Barrow's form of the differential triangle is used. Ten days later still, November 21, he speaks of a new kind of trigonometry of indivisibles, in which reference the editor finds the influence of both Pascal and Barrow. In June, 1676, he writes that "the true general method of tangents is by means of differences." In the following month (July, 1676) he writes, partly in Latin and partly in French, on the inverse method of tangents, and in November of the same year he sets forth a number of the basic laws for differentiation and integration. In July, 1677, however, he shows no evidence of ability to differentiate logarithmic, exponential, or trigonometric functions, but by 1680 he seems to have been in possession of the general theory which he published in the *Acta Eruditorum* in 1684.

This brief summary will serve to give some idea of the material awaiting the further study of scholars;—further study, because the world will not be inclined to accept as satisfactory the study given by either Dr. Gerhardt or Mr. Child. Dr. Gerhardt was a careful student, but he is shown by Mr. Child not to have been altogether judicial in his statements. As to the editor of the present work, two questions will occur to any reader who carefully examines his contribution. In the first place, has he approached the subject with the unprejudiced mind of a searcher after truth? Mr. Child has himself answered this question (page 229):

"I therefore set out with the determination to break down, if possible, the credibility of Leibniz as a witness in his own defense, when it came to unimportant details; then to show that he had opportunities for obtaining everything necessary to the development of the calculus, that he could not be expected to supply for himself by original work, without having need to know anything of the work of Newton; then to show that these sources of information were set out in a form far more suitable to the requirements of Leibniz than the work of Newton; finally, to clinch the matter, that the analogy of Leibniz's work was so close to these sources, that it was idle to suppose that he made use of any other sources."

It is impossible to read this confession of prejudice without being conscious of a slight feeling of, or akin to, that of amusement in Mr. Child's strictures upon Gerhardt:

"Never surely did any man have such a glorious opportunity as Gerhardt, in the whole history of scientific controversies; surely there never was an advocate who left himself so open to the attacks of the opponents."

It is to be regretted that Mr. Child repeats so often his ideas of the dependence of Leibniz upon Barrow. This repetition is partly due no doubt to the fact that the book is made up of essays that appeared from time to time in *The Monist*, little effort having been made to unify the presentation of the matter when combined in book form. It is also to be regretted that a more restrained style was not possible, since such a style would have carried much greater conviction than the one adopted. Probably the best approach to a perfect description of the working of a human mind in the reaching of a mathematical discovery is that given by Lord Moulton in his address at the Napier celebration at Edinburgh in 1914. There the trained intellect of a mathematician and an eminent jurist concentrated on giving a clear analysis of the development of a great idea, and the result of the analysis was a masterpiece,—delightful in style, free from any apparent bias, and convincing in its conclusions.

It is too much to expect, however, that we can all be Lord Moultons. Perhaps it is more human to find ourselves influenced by Carlyle as we see him in his thoroughly biased essay on Cromwell. But the reader will soon find that with all of Mr. Child's evident scholarship and painstaking research, and in spite of all our indebtedness to him for his excellent translation and the information which he often gives us, in the matter of style he is not particularly fortunate. Illustrations like the following will tend to convince the reader, however generous may be his inclinations, that the work was not carefully revised before publication:

"Does not this silence on the part of Tschirnhaus, the personal friend of Leibniz, rather tend to make Leibniz's plea, that his opponents had had the shrewdness to wait till Tschirnhaus, among others, was dead, recoil on his own head, in that he has done the very same thing?" (Page 29.)

"The work of Descartes, looked at at about the same time as Clavius, that is, while he was still a youth, 'seemed to be more intricate.'" (Page 37.)

"This, without either proof or figure, is a hopeless muddle. . . . Goodness knows what the use of it was supposed to be in this form!" (Page 61.)

"Neither Gerhardt nor Weissenborn tried to get to the bottom of these manuscripts, being content with simply 'skimming the cream.'" (Page 74.)

"Thus what is generally considered to be a muddle turns out to be quite correct. The muddle is not with Leibniz, it is with the transcriber." (Page 81.)

"This is of course nonsense." (Page 97.)

"I cannot get out of my head the suggestion that . . . ." (Page 110.)

"Is Leibniz trying to draw a red herring across the trail, the real trail that leads to Barrow's *a* and *e*?" (Page 128.)

Unfortunately, there are a large number of similar instances that will strike the reader's attention as he studies the pages of the book.

It is a rather low type of criticism that looks only for the misprints and inconsequential slips of the pen in the work of an author. When Mr. Childs remarks that "there is of course the usual misprint (in Gerhardt's work) that one is becoming accustomed to," he tempts the reader, however, to recall various instances of a similar kind in the work under review. Without wishing to call attention to these misprints in detail, the point may be illustrated by the cases of 15 for 16 (page 31), the period for a comma on page 74 (line 5), the absence of an interrogation point after the question in the note on page 101, and the date 1874 for 1674 as that of the second edition of Barrow (page 13).

That the book is a valuable contribution to the history of mathematics, however, is evident to anyone who gives its pages even a casual reading.

DAVID EUGENE SMITH.

An Enquiry Concerning the Principles of Natural Knowledge. By A. N. WHITEHEAD. Cambridge University Press, 1919. xii + 200 pp.

THE aim of this work is to illustrate the principles of natural knowledge by an examination of the data and the experiential laws fundamental for physical science. The modern theory of relativity has opened the possibility of a new answer to the question as to how space is rooted in experience and has