dation there is a microbous growth which sets up interior disarrangements, * * * which eventually result after fecundation in the formation of a miniature plant. The strange thing about the matter is that the little plant may, when grown up, turn out to be greatly different from either the plant from which the pollen drifted or from the plant which caught and nourished the pollen on its stigma and then received the 'being' of the pollen in its ovary." The question of the ascent of water in plants is attacked, and the author seems sceptical about the existence of root-pressure and transpiration, while the famous spiral tendency is revived in connection with the ascent of sap and phyllotaxy. Evolution, natural selection and spontaneous generation are mentioned in a way that shows that there are still dark places into which correct notions of these phrases have not penetrated. As has been indicated, quotations from good authors are interspersed, but the result is rather like that which follows a mixture of ice cream and lobster salad.

It is hard to say what object this article can be conceived to serve. The distinguished metaphysician who has been the efficient head of the bureau so long, and may he long remain there, might perhaps be able to give an answer from the depths of his philosophic lore. No plain man can. Fortunately from the method of its publication, the indigestible mass of actinic rays, earthworms, Rothamsted experiments and circumnutation—in addition to the constituents already mentioned — cannot do much harm except to the naïve folks who think that government reports are a sort of gospel.

Seriously, although scientific men are becoming accustomed to the notion that pedagogical 'experts' have a plenary inspiration which gives them the right to discuss all subjects under the sun without studying them; and although they may simply smile when a psychologist speaks of the legs of a hydra, and opposes the sarcolemma to the germ-plasm, or attributes the upward growth of a stem to heliotropism, if he has something to say that compensates for the blunders; yet it does seem that these people might at least take the trouble to submit their manuscript to some *Fachmann* before publication. And the Bureau of Education might do well to investigate the training of its 'specialists' a little before employing them to write up scientific subjects.

University of Colorado. John Gardiner.

THE PSYCHOLOGY OF SUGGESTION.

To the Editor of Science: Permit me to make a few remarks in regard to the review of my book, 'The Psychology of Suggestion' by Professor Wm. Romaine Newbold in Science for June 24, 1898.

Professor Newbold contends against the truth of my second law, that of abnormal suggestibility. He brings the phenomena of *rapport*. "In states of heightened suggestibility," he writes, "suggestibility to suggestion has no significant relation to the mode in which the suggestion is administered, but rather to the *source whence it comes.*" (The italics are his own.) "Rapport," he says further on, "although not an inevitable, is perhaps one of the most constant traits of heightened suggestibility, and this Dr. Sidis' second law ignores." Now this is not true. *Rapport* is not a characteristic spontaneous trait of the advanced stages of hypnosis, it is itself due to a suggestion forced on the subconsciousness of the subject. Where the personal element is considered important, there the phenomena of *rapport* will naturally be frequent. Where, however, it is realized that hypnosis has little to do with the personality of the experimenter, *rapport* is *absolutely* absent even in the very last stages of hypnosis. Thus in none of my best subjects have I found the phenomena of *rapport*. *Rapport* had to be specially induced by most emphatic suggestions. This is simply due to the fact that in my experiments I have taken precaution to guard against all unconscious suggestions in general, and particularly against the 'personality suggestion.' The importance of the personal element, 'the source' in hypnosis is a widely spread, but an unjustified belief due, no doubt, to some lingering remnants of mesmeric theories. As a matter of fact, *rapport* is not spontaneous in hypnosis, it is induced by suggestion, and, like all other suggestions, depends on the conditions and laws of suggestibility.

Professor Newbold finds fault with my preliminary definition of suggestibility. I won-

der Professor Newbold does not see that the definition given in the first chapter is only provisional to start the work with, that the nature of suggestion and suggestibility is worked out in the course of the first part, and that the final definition is not arrived at before the end of the eleventh chapter.

A few words more before I conclude. Professor Newbold finds my physiological theory rather incorrect when confronted with Apathy's investigations. I do not find that my theory is to any extent shaken by Apathy's 'anastomosis.' Apathy's work may hold good for the nervous system of the lower invertebrates, but not of the cerebro-spinal nervous system and especially of the association areas. Apathy himself admits it. I am happy to say that the eminent pathologist, Professor Ira Van Giesen, accepts the same physiological theory, and in a special work will take up this point about Apathy and will furnish experimental data demonstrating the truth of the position taken by me in the book 'The Psychology of Suggestion.'

Professor Newbold's criticism is fair and candid, and one cannot help contrasting it with the virulent, almost personal, onslaught of those academic psychophysicists, especially of the Wundtian fold, who lack and neglect all knowledge of mental pathology and who attack bitterly any one who has the courage to proclaim openly the poor and sterile state, the trivial nature of the scholastic laboratory science of normal 'student psychology.'

BORIS SIDIS.
PATHOLOGICAL INSTITUTE OF THE
NEW YORK STATE HOSPITALS, NEW YORK.

CELLULOID FILMS.

TO THE EDITOR OF SCIENCE: My own experiments and sad experiences in the use of celluloid 'cut films' instead of glass plates for photographic purposes on long expeditions prompt me to write a warning to those who will read the note quoted in SCIENCE, July 22, 1898, page 106. If the advice given by Mr. Stillman were followed by scientists without further test I greatly fear that their return from a six months' expedition with numerous undeveloped 'films' safely stowed away for development at leisure would be made less enjoyable after a few hours in the dark room. Two years ago I made trial of some fresh films and thought them so superior to glass because of their lightness that I adopted them for use on a visit by bicycle to the astronomical observatories of Europe. I could not find ten dozen in stock in New York without taking some that were three months old. I was in Europe only three months, and during that time carried those films and a camera with other baggage on my bicycle for two thousand miles, on hot days buoyed through the 'slough of despond' by the expectation of having at least one hundred fine photographs of observatories and scenery. The camera was a familiar one, and I had had long experience in photography in America and in South and Central Africa with glass plates which had always proved successful. But, alas! when I returned to the States and at once proceeded to develop the films I could find only the faintest traces of the scenes which ought to have been there. There was every indication that the acids in the celluloid had destroyed the sensitiveness of the emulsion either before or after exposure. Since then experiments on 'films' of various ages and the questioning of professional photographers who have developed many thousands of these 'films' have confirmed my belief that *as a rule* they may be regarded as practically worthless after they had been made a year, and are very unreliable after six months. I mean by 'unreliable' that it is impossible to predict by the action of one plate what the time of exposure on another plate of the same emulsion ought to be.

Hence I conclude that one should be very cautious in adopting the suggestions of Mr. W. J. Stillman, from whom you quote, if the expedition is to last more than six months from the time when the plates were made; and in every case I should prefer to get fresh films every month and develop them as they are exposed.

HERMAN S. DAVIS.
COLUMBIA UNIVERSITY, July 22, 1898.

SCIENTIFIC LITERATURE.

Au Pays des ba-Rotsi. By ALFRED BERTRAND. Paris, France, Hachette et Cie. 4to. Pp. 333 and 10. 104 illustrations.

This volume, prepared in the handsome style of the famous French publishing house from