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.'

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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.

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