DR. FRANK THILLY, professor of philosophy at Cornell University, and Professor Madison Bentley, professor of psychology in the University of Illinois, will lecture during the summer session of the University of California.

DR. WILLIAM A. R. TAYLOR, now instructor in botany in the University of Pennsylvania, has been promoted to an assistant professorship.

MR. ARTHUR LEE DIXON, M.A., F.R.S., fellow and tutor of Merton College, University of Oxford, has been appointed Waynflete professor of pure mathematics in succession to Professor E. B. Elliott, fellow of Magdalen, who has resigned.

MR. ARTHUR LAPWORTH, D.Sc. (London), F.R.S., at present professor of organic chemistry in the University of Manchester, has been appointed to the Sir Samuel Hall chair of chemistry and to the directorship of the chemical laboratories.

DISCUSSION AND CORRESPONDENCE

GENETICAL ANALYSIS AND THE THEORY OF NATURAL SELECTION

IN my Toronto address I lately referred to John Ray as the first who laid stress on the sterility of interspecific hybrids. I was then writing away from books and must apologise for this slip. The passage in the *Historia Plantarum* 1686, 1, pp. 40 and 42, that I had in mind is probably the first in which anything approaching a genetical definition of species is attempted. Ray there lays down the excellent principle that forms which, though differing from each other, can be bred from seed of the same plant, should be regarded as of the same species. Not till the Linnean period, more than half a century later, did the cognate question of the sterility or fertility of interspecific crosses assume prominence.

Professor Osborn has expressed great vexation at the tenor of my address. After considering his remarks, I do not know that I can add much to what I have said. The divergence between the conceptions to which genetical analysis introduces us and the doctrines of which Professor Osborn has been so long a distinguished champion is indeed wide.

Paleontological observations have served a useful purpose in delimiting the outline of evolution, but in discussing the physiological problem of interspecific relationship evidence of a more stringent character is now required; and a naturalist acquainted with genetical discoveries would be as reluctant to draw conclusions as to the specific relationship of a series of fossils as a chemist would be to pronounce on the nature of a series of unknown compounds from an inspection of them in a row of bottles. The central tenet of Darwinism that species are merely the culminations of varietal differences, such as we find contemporaneously occurring, is not easily reconcilable with the new knowledge. It was my purpose once more to direct the attention of naturalists, especially geneticists, to this deficiency in the evidence, by no means without hope that it may be supplied.

Professor Osborn, in extenuation, suggests that my tongue ran away with me and that I could not have meant what I said. That defense, however, is not available, for I had taken the precaution which I understand he learned from Huxley, and I had prepared a written text. This, in all important passages, I followed verbatim, and it appears without serious modification in SCIENCE for January 20. I may even plead guilty to having spoken and written to the same effect on many previous occasions, and Professor Osborn will find the theme developed in "Problems of Genetics" (New Haven, 1913, and in my presidential address to the British Association in Australia (1914).

W. BATESON

MARCH, 1922

A SUGGESTION TO MR. BRYAN

I THINK most readers of SCIENCE must feel indebted to you as I do for reprinting W. J. Bryan's attack on Evolution. It may be true that only the psychologists will be able to find in it data of value to their science but to them the importance of this contribution of Mr. Bryan's must be large indeed. The rest of us welcome the diversion which it affords. A Don