THE general board of studies of Cambridge University recommend that a University lectureship in physiological and experimental psychology, connected with the special board for biology and geology, be established for the term of five years from October next, and that the stipend of the lecturer be £50 a year.

PROFESSOR H. K. WOLFE has resigned the chair of psychology in the University of Nebraska. Dr. W. B. Pillsbury, now instructor in psychology in Cornell University, has accepted a similar position in the University of Michigan. Mr. F. C. S. Schiller, instructor in logic in Cornell University, will return to Oxford, having been elected fellow and tutor in Corpus Christi College. It is understood that Dr. C. E. Seashore, now assistant in Yale University, will be appointed assistant in psychology in the University of Iowa, and Dr. J. H. Leuba, lately fellow at Clark University, to a position in psychology in Clark University. Mr. S. I. Franz has been elected assistant in psychology in Columbia University.

PROFESSOR WILLIAM S. FRANKLIN, of Iowa University, has been elected to the chair of physics and electrical engineering at Lehigh University, filling the place vacant by the resignation of Professor Harding. Dr. John Marshall has been appointed to the chair of chemistry in the medical department of the University of Pennsylvania, vacant through the death of Professor Theodore G. Wormley.

MISS MARY E. PENNINGTON was appointed Thomas A. Scott fellow in hygiene in the University of Pennsylvania. Miss Bertha Stoneman, now at Cornell University, has been appointed to the chair of botany in the Huguenot College for Women in Cape Colony.

REV. R. E. JONES, of All Angels' Church, New York city, has been nominated for the Presidency of Hobart College, Geneva, N. Y.

PROFESSOR J. L. PREVOST has been elected professor of physiology in the University of Geneva. Dr. P. Francotte has been appointed professor of embryology and Dr. P. Stroobant professor of astronomy in the University of Brussels. Dr. J. J. Zumstein has been promoted to a professorship of anatomy in the University of Marburg.

DISCUSSION AND CORRESPONDENCE.

THE POTTER'S WHEEL IN ANCIENT AMERICA.

THE paragraph referring to American Ceramics, by Dr. D. G. Brinton, in SCIENCE, for May 21, 1897, page 797, containing the categorical statement that 'the device of the potter's wheel was (anciently) unknown in either North or South America,' should be noted as inadequate. Under the present knowledge of the subject, while referring, as a noteworthy substitute for the wheel, to a clay dish twisted by the Chillian Indians (Araucanians), so as to mould the clay ball resting in it (described in *Globus*, February 20, 1897), it would have been well to mention a similar device from the Southwest or Mexico, which, according to Professor Putnam, had been in the possession of the Pealody Museum at Cambridge, Mass., for sometime previously.

More uninstructive is it to ignore the *Kabal*, of Yucatan, a disc of wood caused to turn on a slippery board by the bare feet of the (present) Maya potter, while the clay sticking to the disc and revolving with it is thus made to mould itself symmetrically against the stationary fingers of the worker. This very noteworthy device, a primitive potter's wheel in the full sense, was observed and fully explained by the Corwith Expedition of the University of Pennsylvania to Yucatan in 1895. I illustrated it in 'Hill Caves of Yucatan' (Lippincott, Philadelphia, 1896, page 163), having previously described it to archæologists, in the *American Naturalist*, for May, 1895. A correspondence with Dr. Brinton upon the significance of the Maya word *Kabal* resulted in his failure to find the word in the Spanish dictionary of the Maya language, published at the monastery of Motul in 1576, upon which he argued, inconsequently I thought, that the device had been brought to Yucatan by Spaniards. On the other hand, the late Bishop of Yucatan and, I think, Captain Theobert Maler believed it to be indigenous, and I have as yet learned of no discovery of the *Kabal* device in Spain or among the Moors in Africa. Under these circumstances, whether final investigation shall prove the *Kabal* to have been of European or American origin, the general references above noted to the potter's craft in

the New World, omitting mention of this long unnoticed instrument (not yet described in duplicate, to my knowledge, anywhere else in the world), are unsatisfactory. The specimens attesting the interesting process, in the possession of the museum of the University of Pennsylvania, since 1895, cannot be ignored.

H. C. Mercer.

Indian House, May 23, 1897.

THE SIGNIFICANCE OF INTERNAL SECRETION.

The communication on the above subject published in Science for April 30th, by Mr. Albert Mathews, seems to me not only of interest, but of importance, because it indicates in a comprehensive way some of the directions in which our thoughts may move just now to advantage. Views not wholly unlike these of Mr. Mathews are hinted at in my 'Animal Physiology' (1889); but it has been especially in lectures to my most advanced class in physiology that, for ten years, I have been accustomed to insist on the bearing of the function of one part on that of another—a subject generally neglected in the books—and also the relation of the development of one tissue or organ as determined by another. Necessarily it was impossible, till more recent discoveries had been made, to indicate many of the ways in which this is brought about, and even yet we can do so but vaguely.

It was very natural, therefore, for me to hasten to read Mr. Mathews' communication to my class and to enforce its teaching by comparison with similar expressions of opinion in a paper entitled 'Experimental Cachexia Strumipriva,' published in the *Canadian Practitioner* in October (?) 1895. I venture to think that Mr. Mathews will find in this paper views as broad as his own, if not more so. To quote a single sentence: "No cell is so small, so distant from others, but that in some way it makes itself felt, and this is to me the most important lesson of all this recent development in physiology and medicine growing out of the study of the total or partial extirpation of organs, of transplantation, of feeding of glands, etc." The extension of the principle of the influence of the internal secretion to plants is admirable, in my opinion, and in this I am inclined to believe that Mr. Mathews is entirely original. However, while Mr. Mathews' views are broad they are apt, if taken alone, to lead to narrowness by their very exclusiveness. When he seeks to explain the co-ordinated life of plants in this way does he also remember the *protoplasmic continuum*, and when he would explain by internal secretion the co-ordination in movement, say, of one cell with another in simple invertebrates does he bear in mind the possibility of explanation through *molecular impact?* Life implies ceaseless molecular movement. Just now we are witnessing, in the medical world, the most remarkable development of chemical conceptions to explain pathological conditions that has yet taken place, but, as usual, with a narrowness that is evidence of the evil effects as well as the advantages of specialization. The doctrine of 'pangens' has always seemed to me a crude and unnecessary hypothesis, and I cannot believe that internal secretion *alone* will supply an adequate substitute, though it will assist to a better understanding of certain results in detail.

Nearly ten years ago I put forward a view in a paper entitled 'A Physiological Basis for an Improved Cardiac Pathology' (*Medical Record*, October 22, 1887), which, so far as I know, was then set forth for the first time in print, though it had been earlier taught in my lectures. This conception was more fully elaborated in 'The Influence of the Nervous System on Cell Life.' (*New York Medical Journal*, December 22, 1888.)

I endeavored to show that we were justified in holding that the nervous system exercised a *constant influence* over all cells, tissues and organs, either directly or indirectly, in every animal provided with such a system, this influence being the more important the higher the animal in the scale of existence. This theory of the constant influence of the nervous system over metabolism, etc., has, so far as I am aware, not been recognized or, at all events, taught by anyone except myself, till it was prominently brought forward last October by Professor M. Foster, the well-known physiologist, in his admirable Huxley lecture. It has since been publicly espoused by the distinguished neurologist Gowers, and will, I have no doubt, shortly receive the recognition which I have long felt it deserved.