but we may confidently expect the beginnings of such a genetic psychology in the future. At any rate, in this field, as in most other fields, progress and profit are increased by greater exactness and care, by more accurate and convenient apparatus and by shorter and more definite methods. These elements are the ones which experimental psychology is trying to introduce into the exploration of mental life. The fact that these methods are somewhat new in psychological work gives us the right to call a system of them a 'new psychology.'

Professor Stanley's claim that biology is the main standpoint of psychology is quite justified —if 'psychology' means the science of mental development. It must be remembered, however, that there is a fundamental difference in aim and method which marks off experimental psychology from the other mental sciences. Its object is to determine the fundamental laws of mental activity in the adult human being under ordinary circumstances. The change of the problem to child-study, to the development of the individual or of the race, or to abnormal circumstances, produces closely related sciences. All these sciences are inter-dependent. In fact, all these sciences—as Professor Stanley implies —are needed for a concrete, practical understanding of mental life; nevertheless convenience and clearness sometimes require that attention should be concentrated on one of them at a time.

E. W. SCRIPTURE.

NEW HAVEN, CONN., May 20, 1898.

FOSSIL FULGUR PERVERSUM AT AVALON, N. J.

ON page 682 of SCIENCE the quotation from Captain Swain, of the Avalon Life Saving Station, N. J., with reference to the casting ashore of *Fulgur perversum* is slightly inaccurate. I now quote from his letter the passage I read at the Academy that "the conchs in question come ashore only during a strong northwest (not northeast) wind that happens immediately after a northeast or a southeast gale, a northwest wind is the only kind that will bring heavy substances ashore, it seems to make the surface current offshore, and this creates an under current on-shore." I have no doubt that *Fulgur perversum* at the locality is raked out of a fossil bed a short distance offshore, and that this off-shore wind after the on-shore gales favors the tides and currents in doing so.

LEWIS WOOLMAN.

THE DEFINITION OF SPECIES.

I HAVE stated in this JOURNAL (N. S., VI, 329) that I believe the quantitative study of variation to be the most pressing problem of biological science. I have consequently read with great interest the papers by Professor Davenport and Mr. Blankinship, on 'A Precise Criterion of Species' (page 685 above). It seems evident that for the definition of species we should not depend on a 'type specimen,' the one first found, in the best state of preservation or the like, but should collate a considerable number of specimens taken at random, and when the traits can be measured give the averages and the mean deviations. Then, as Mr. Davenport explains, we have double-humped curves showing a tendency for the type to split up, and these are of the greatest possible interest to the student of the causes of the evolution of species.

When, however, Mr. Davenport proposes to use a given relation between the height of the smaller hump and the depression between the humps*—namely 100 : 50—as a precise criterion

* This relation depends not only on the distance between the apices, but also on the relative number of specimens of the two types, which, of course, has nothing to do with the difference between the types. There are other cases in Mr. Davenport's paper where the statements seem scarcely to take account of the complexity of the problems. It is meaningless to say that 'in some cases fifty per cent. or even more of the individuals will occur at the mode' and that in this case the curve is steep. The number of individuals at the mode depends on the unit of measurement selected, and the steepness of the curve is arbitrary. The 'half range,' defined as three times the 'standard deviation' (error of mean square), is a theoretically impossible point, and could only be determined approximately from thousands of specimens. Thus in Mr. Davenport's Fig. 9 the 'half range' of the right-hand curve is tripled by a single specimen. In all these cases Mr. Davenport neglects the probable errors which when reckoned show that his distinctions between species and varieties have no validity whatever. The data of Fig. 9 can be expressed by a curve with a single apex.

of species, I cannot follow him at all. Size and weight—the traits that can be measured—are especially dependent on the environment and variable within the same species. Varieties of dogs may not intergrade at all in size and weight, or in the relative dimensions of the skeleton, but this does not lead us to call them separate species. The cephalic index is one of the most important differentials in man, but the fact that it may not intergrade does not turn races into species. The conditions are far more complex than Mr. Davenport assumes them to be. A certain quantitative amount of intergrading may mean entirely different things under different circumstances, and the various differentials of a species may intergrade to very different degrees. It does not follow that the chief differential is that quantitative characteristic intergrading the least. It may be the teeth or the reproductive system or whatever serves most conveniently as a basis of classification.

My excuse for writing on the definition of species is that I hold it to be a psychological problem. In pre-evolutionary days the naturalist undertook to discover species that had been created ; now it is he who creates the species.* The problem is analogous to deciding how many colors there are in the spectrum; it may be held that there are three, or four, or seven, or two-hundred. There are, indeed, various criteria that may be used in the separation of species, of which the most important seem to be : (1) the phylogenetic history when known ; (2) hereditary stability and variability ; (3) the tendency to cross and the fertility of crosses, and (4) intergradation. The last named factor is not only quantitative, as in the cases given by Mr. Davenport, but also qualitative, and here the naturalist must try to use as his unit what the psychologist calls the 'just observable difference.' The degree of distinctness that shall constitute a species must, like the meaning of every word, depend on the best usage. As the usage of the best writers is compiled and given currency by dictionaries, so the usage of naturalists is compiled and given currency in a work such as *Das Thierreich*. The criterion given prominence by Messrs. Davenport and Blankinship should be carefully studied, but it is only one of many factors, and these must be distinguished and adjusted by the powers of observation and judgment of the naturalist. The definition of species is, as I have said, a psychological problem.

J. MCKEEN CATTELL.

*I fear that I am here sailing under Dr. Merriam's heavy guns. He has written : "The function of the naturalist is neither to create nor destroy species, but to recognize, describe and learn about those which nature has established." (SCIENCE, N. S., V., 124.) Innumerable coyotes, differing more or less, live or have lived, and Dr. Merriam, not nature, has established eleven species. Some other naturalist has created the coyotes.

SCIENTIFIC LITERATURE.

Contribution towards a Monograph of the Laboulbeniaceæ. By ROLAND THAXTER. Memoirs of the American Academy of Arts and Science. 1896. Vol. XII., No. 3. Pp. 189–429. 26 plates.

This is the second important memoir by Dr. Thaxter on Entomogenous fungi, the first being a monograph of the Entomophthoreæ. The very large number of these plants which are being brought to light by the keen observation and untiring industry of the author of this memoir is a surprise to any one acquainted with the literature of the subject.

As Dr. Thaxter states in the introduction, his study of Entomogenous fungi was begun with the intention of embodying in a single monograph all species truly parasitic on insects. But the number of species of the Entomoythoreæ were sufficient for a monograph of considerable proportions, and now the hitherto insignificant family of Laboulbeniaceæ has, under his indfatigable researches, grown to an order of formidable proportions, while several other groups of insect fungi remain yet to be investigated.

While a few of the members of the genus *Laboulbenia* have been known for nearly one-half a century, our knowledge of the development, sexuality and formation of the spores has remained very imperfect. This, together with the difficulty of defining the position of the family in relation to other thallophytes, has probably had much to do with the almost universal absence of treatment of these forms from text-books of fungi.