the good ones in hearing, those in the senses of pressure and temperature, the accurate measurements of visual space, the measurements of the reaction-time, etc., have all tended to place experimental psychology on a high level and to furnish a foundation for a science of psychical measurements, or psychometry.

What is the reason, then, that we are doing second-rate work when we might do first? The trouble lies, it seems to me, in the lack of a proper training. We attempt to make experiments; but how many of us have received a practical training in the use of our apparatus? We make observations; but how many are familiar with the methods of observation and the computation of errors? We obtain tables of results; but how many know how to formulate the equation expressing those results? I know that, until I was brought face to face with the question of what to do with my figures when I had got them, it had not occurred to me to remedy my deficient training by a study of the methods of expressing results. We all of us daily use light, sound, heat, electricity, etc., in our experiments; but how many are familiar with the units and the methods of measuring these forms of energy? What a psychologist must have is a thorough course of training in psychometry, or the methods of psychical measurement.

Summing up, I would say that what we need in experimental psychology is: no quackery, little amateurism, a proper estimation of qualitative work as subordinate, a transformation of the qualitative into quantitative investigations, and, as the means of obtaining all this, a thorough laboratory training. E. W. SCRIPTURE.

Clark University, Worcester, Mass.

THE VESICLES OF SAVI.

IN the *Archives Italiennes de Biologie*, XVI., 1891, page 216, there is a reprint from the *Atti della R. Accad. dei Lincei*, VII., 1891, fasc. 6, of Dr. Alessandro Coggi's important notice of the development of Savi's "*appareil folliculaire nerveux*" in the torpedoes. Since Savi's announcement of his discovery of these peculiar follicles on the lower surface of the torpedo, 1841–44, an extensive series of publications has been made on the subject. The anatomy has received attention at the hands of Boll, Leydig, Kölliker, Max Schultze, Müller, and others; and the nature and functions have been variously determined. Leydig made the vesicles to be one of his three classes of organs for a sixth sense; Wagner supposed them to be electrical excitants; but the majority agreed in regarding them as tactile organs. In 1888, in my work on the "Lateral Canal System of the Selachia and Holocephala," published by the Museum of Comparative Zoology, it was proved that the vesicles belonged to the lateral system, as seen on the skates and sharks, and it was shown that they were not confined to the torpedo, but were found on such genera as Urolophus, Potamotrygon, and Disceus of the rays, where they were simple rudimentary remnants of the lateral canals. My conclusions are amply confirmed by Dr. Coggi from the embryology of torpedo, in the early stages of which he traces the ventral canals, as in embryos and adults of other Selachia. He finds various stages of canal disruption corresponding with those I had figured from the Batoids above mentioned.

Dr. Coggi's assertion that the hypothesis making the vesicles of Savi a special modification of the lateral line system was first brought forward by M'Donnell, 1864, is one to which I should take exception. It must be due to misunderstanding of M'Donnell's statements. That author enumerates five structures that "may be, or have been, confounded with different parts of the lateral line system," and he describes the last one of the five as "The bodies discovered by Savi in the torpedo (*appareil folliculaire nerveux*) — which last, however, may be related to the lateral line, as I shall afterwards attempt to show." This is sufficiently involved to make his meaning very doubtful. But to prove that M'Donnell did not advance the idea of identity of follicles and lateral lines we have only to turn to the penultimate paragraph of his article, where he classes the follicles with other tactile organs, and says that they, one and all, appear to be distinct from the system of the lateral line, which, he says, has more the appearance of a cutaneous excretive organ than of one of sensation (Trans. R. Irish Acad., XXIV., 1864, read 1862, page 161). Up to the present I have learned of no proof or assertion of identity of Savi's follicles and the lateral canal system previous to that in my work of 1888.

Respecting the utility of the follicles it may be added here that my conclusions are at variance with those of all who have heretofore discussed the matter, insomuch that I consider these organs to be practically without special function, and to represent only a transitory condition of the lateral system, intermediate between functional perfection, in the embryo, and ultimate more or less complete disappearance, during the life of the individual. As the organs are absent from particular species or from older individuals, and are rudimentary and irregular when present, this seems to me the only tenable conclusion. S. GARMAN.

Museum of Comparative Zoology, Cambridge, Mass., Feb. 29.

BACTERIA IN DRINKING WATER.

DR. W. MIGULA (*Centralbl. f. Bakt. und Parasitenk.*, Bd. VIII., No. 12, p. 353) makes a contribution to our knowledge of this subject which is really a new departure as regards the examination of drinking water. He points out that, although considerable stress has been laid on the examination of water for pathogenic organisms, there is no reliable rule to guide the hygienist in his examinations for the ordinary saprophytic organisms and their relation to the purity of water to be used for drinking purposes. Dr. Migula washes out small flasks with bichloride of mercury; then, after rinsing them with the water to be examined, he leaves a specimen in the flask, which is plugged with sterilized cotton wadding and covered with an india-rubber cap. It is not necessary to pack the flasks in ice, as it is assumed that if any of the organisms multiply they will all do so, while if the putrefactive organisms (those that liquefy gelatine) grow more rapidly than the others, independent evidence is obtained of the impurity of the water. Cultivations are made in flat glass dishes in order to save the time required in manipulating plates and tubes during the cooling process. After examining 400 springs, wells, and streams, the author has come to the conclusion that where there are more than ten species in any sample of water, especially when these are not species ordinarily met with, the water should not be used for drinking purposes. He found that in only fifty-nine waters was this the case, but that 169 waters contained more than 1,000 organisms per cubic centimetre, sixty-six of these having over 10,000 (forty over 50,000). From these figures it will be seen that some of the sources of supply would be condemned by the old method but would be passed by the new, and some condemned by the new would be passed by the old. Migula found in all twenty-eight species,