tion the facts regarding *Holothuria* Linnæus, 1758, as given by Gill and Poche. There is no question that the ignoring of these facts has placed us, who are specially interested in echinoderms, in a serious dilemma. Either we must refuse to follow the International Code or we must attempt, not only to substitute an unfamiliar name for the familiar *Holothuria,* and introduce a series of regrettable changes into the nomenclature of ascidians and echinoderms, but we must undertake to replace the colloquial English "holothurian" with some other term. Of these two evils it seems to me that the former is decidedly the lesser, and at the expense of consistency I propose to continue to call "sea-cucumbers," holothurians. Of course, if the International Commission on Nomenclature, in its proposed list of genera to be unchangeably adopted, assigns *Holothuria* to the ascidians rather than to the echinoderms, I shall not stand out against that decision, but meantime I shall sincerely hope that they will agree that an exception to the application of the code is wiser than a consistency which involves such difficult, one might almost say impossible, changes in nomenclature.

And I am confirmed in this attitude by certain facts either ignored or overlooked by Gill and Poche. Aldrovandus and other pre-Linnæan writers used *Holothuria* in the commonly accepted sense, as have all writers since 1766. Jäger in 1833 refers to the *twelfth* edition of the "Systema Naturæ" as the first in which true holothurians are included in the genus and he virtually bases his revision of the genus on that edition. Far more important than this, however, is the fact that if we assign *Holothuria* to the ascidians, it is by no means easy to decide what name shall replace it for sea-cucumbers. Fortunately for euphony's sake, it almost certainly will not be *Bohadschia,* as both Gill and Poche assert. A hasty survey of the literature between 1760 and 1830 shows that the case is quite involved. Mr. Austin H. Clark has called my attention to the fact that a very plausible argument may be made for *Holothusia* Barbut, 1783, Plate 6! Incidentally it should be remarked that there are many zoologists (Drs. Gill and Fisher among them) who will hold that *Holothuria* is properly a siphonophoran genus, and not ascidian, as Poche claims. Careful consideration of all the facts satisfies me that the attempt to radically change the usage of such familiar names as *Holothuria, Actinia* and *Salpa* can only make confusion worse confounded and, until an international congress of zoologists has voted that this shall be done, I for one shall continue to use the names in the commonly accepted sense.

Hubert Lyman Clark
Museum of Comparative Zoology,
Cambridge, Mass.,
September 28, 1907

Errors in Tower's "An Investigation of Evolution in Chrysomolid Beetles of the Genus Leptinotarsa"[1]

In reading over this work, I have noticed a few minor typographical errors and one rather more important biological misstatement or misconception in regard especially to one species of the group, all of which, however, do not affect the work as a whole. The biological misstatement or misconception is unfortunate, as it is founded on innumerable observations, and thus would tend to indicate carelessness in this respect on the part of the author. But I have no doubt that it was due rather to an oversight.

The typographical errors will be stated first:

Page 53,	¶ 2,	line 3,	*specimens* = *species.*
68,	3,	4,	*32* = *22.*
166,	2,	28,	*how* is repeated.
169,	3,	1, 3,	*histonic* = *historic.*
253,	2,	7,	*habitat* = *habit.*
294,	2,	3,	*ohers* = *others.*

In writing of the ontogeny of larval color patterns on page 147, Dr. Tower directly implies three larval instars to *Leptinotarsa signaticollis* Stål, and indirectly so to the species *diversa* Tower and *undecemlineata* Stål. On the next page, and those following, the same fact is implied in regard to the other species of the group, including the common *decem-*

[1] Publication No. 48, Carnegie Institution of Washington, 1906.

lineata Say, and colored figures of the three larval stages are given on plate 17, facing page 146, of more than ten of the species. Elsewhere (pp. 164, 219) general statements to this effect occur. Three larval instars are therefore implied for all, or almost all, of the species of the genus. And it is to this statement, in so far as it concerns the species *decemlineata,* that I desire to call attention.

Rearings of this species, both in nature and the laboratory, carried on in Georgia in 1906 and in Ohio in 1907, showed in both places four larval instars, all of which were distinct, and which have been described.[2] These rearings involved a total of not more than seventy specimens, and while this is very small in comparison with the large total reared by Tower, I can not think otherwise than that they represent the average for the species, and were not exceptions. All of the lots were small and under normal conditions, and the rearings were made especially with the view of determining the duration and number of the larval instars, so that errors in observation were eliminated. As Dr. Tower had other objects in view, I believe his observations in this respect were faulty, at least one ecdysis being overlooked in the larval development of *decemlineata;* and if in that species then as well perhaps in the others, though I am not concerned with them here.

A. ARSÈNE GIRAULT

WASHINGTON, D. C.,
September 16, 1907

EVEN PERFECT MEASURING IMPOTENT

THE attention of geometers should be directed to a remarkable article by Dr. R. L. Moore, of Princeton, whose extraordinarily elegant proof of the redundancy of Hilbert's axioms first appeared in *The American Mathematical Monthly.*

The new article, in the *Transactions of the American Mathematical Society,* Vol. 8, No. 3, pp. 369–378, July, 1907, is also a perfecting of the work of the Hilbert school, but reaches new results so unexpected, so profound as to be nothing less than epoch making.

We knew that the so-called laboratory method for mathematics, the "measuring" method, was rotten at the core, since mathematics is not an experimental science, since no theorem of arithmetic, algebra or geometry can be proved by measurement.

Our argument was sufficiently cogent: that the theorems of mathematics are absolutely exact, while no human measurement ever can be exact.

But Dr. Moore shows that even granting the impossible, granting the super-human power of precise measurement, we could not thereby ever prove our space Euclidean, ever prove it the space taught in all our text-books.

The title of his article is: "Geometry in which the Sum of the Angles of Every Triangle is Two Right Angles." But, omitting the Archimedes assumption, if this postulate be substituted for Euclid's, there results a geometry not necessarily Euclidean. Nevertheless, no human being confined therein could ever distinguish it from a Euclidean space even though he were supplied with instruments which could decide for him whether any two sects were exactly equal.

The Euclidean space would contain other points, points ideal or *ultra* as regards this "angle-sum" space.

But, most extraordinarily, no *ultra* point is ever between two ordinary points.

GEORGE BRUCE HALSTED

GREELEY, COLO.

SPECIAL ARTICLES

PLANKTON FISHING OFF THE ISLE OF MAN[1]

DURING recent years a good deal of attention has been paid by naturalists in various parts of the world to the *quantitative* distribution of organisms in the sea. It is obvious that exact information in regard to such a matter may be of enormous importance in connection with the fishing industries. Notable methods of work, and instruments for

[2] Girault and Rosenfeld, *Psyche,* XIV., 1907, pp. 47–52.

[1] Read before Section D (Zoology) of the British Association meeting at Leicester on August 6, 1907.