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

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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.

the purpose of capturing and measuring the organisms present, have been devised by some of the German investigators, and these will always be associated with the name of Professor Hensen, of Kiel, to whom very great credit is due for the ingenuity and scientific enthusiasm with which he has conducted the investigation. It is very important that any criticisms which are required should be brought forward before further researches have been made and before further material has been accumulated, and that any imperfections or limitations in the methods employed should be recognized.

The Hensen methods are based upon the assumption that the distribution of the plankton, or assemblage of minute floating organisms, in the sea is so uniform over wide areas under much the same conditions that total populations can be calculated from relatively very small samples. [Examples were given of several of the conclusions arrived at as the result of such calculations by Hensen and his fellow workers.] The correctness of these conclusions, it will be noticed, depends entirely upon the assumed uniformity of distribution and upon the adequacy of a small number of samples as representing the whole area. Some criticisms have appeared which are based upon imperfections of the instruments—variations in the nets employed and such matters. These imperfections can be obviated or allowed for; but I wish to bring before your attention a much more fundamental difficulty, namely, the marked irregularity or want of uniformity in the distribution of the organisms. At the time (five years ago) when I served as a member of the Ichthyological Research Committee, I was much struck by the evidence obtained of irregularity in distribution of marine organisms and of the inadequacy of small samples taken at considerable distances apart in either time or space. It has been a matter of common observation amongst naturalists that many of the larger organisms, such as Copepoda, occur in swarms; and this not merely around our coasts and in the narrow seas, but also in the open ocean. [Recorded instances were given.] *Trichodesmium*, again, is found in the Indian Ocean occurring in enormous profusion over narrow bands. At the last meeting of this association, Dr. G. H. Fowler gave some interesting results he had obtained in regard to irregularity of distribution in the open Atlantic. These and some other results which have been obtained, I believe, are unfortunately not yet published.

Convinced of the fundamental importance of such work, I spent the greater part of last summer vacation in experimenting day after day with various plankton nets under similar and under varying conditions in a limited sea-area off Port Erin in the Isle of Man—with results that were startling in their diversity. It was obvious that the plankton was at that time very unequally distributed over the depths, the localities, and the dates. It seemed clear that one net might encounter a swarm of some organism which a neighboring net escaped, and that a sample taken on one day might be very different in quantity from a sample taken under the same conditions next day.

I stopped this series of observations on September 17. After a few days of wind a spell of quiet, calm weather followed, during which I took some tow-nettings both inside Port Erin Bay and outside, both in the day and at night, and all of these differed entirely in character from the gatherings of the previous weeks—being composed mainly of *Chætoceros* and other diatoms. When the weather broke again, at the end of September, another abrupt change took place, and gatherings taken at the beginning of October showed very few diatoms, but many Copepoda. It is evident that if any observer had been taking quarterly or even monthly samples of the plankton in that sea-area, he would have obtained very different results, according to the exact date of his visit. On three successive weeks about the end of September he might have found evidence for as many different far-reaching views as to the composition of the plankton in that part of the Irish Sea. How it can be supposed that hauls taken miles apart and repeated only at intervals of months, or even weeks, can give any sure foundation for calculations as to the population of wide sea areas, I fail to see.

These conclusions need not lead us to be discouraged as to the ultimate success of scientific methods in solving what may be called world-wide problems, but they suggest that it might be wise to secure by detailed local work a firm foundation upon which to build, and to ascertain more accurately the representative value of our samples before we base conclusions upon them.

I do not doubt that in limited, circumscribed areas of water, in the case of organisms that reproduce with great rapidity, the plankton becomes more uniformly distributed, and a comparatively small number of samples may then be fairly representative of the whole. That is probably more or less the case with fresh-water lakes; and I have noticed it in Port Erin Bay in the case of diatoms. In spring, and again in autumn, when suitable weather occurs, as it did last year at the end of September, the diatoms may increase enormously, and under such circumstances they seem to be very evenly spread over all parts and to pervade the water at all depths; but that is emphatically *not* the case with the Copepoda and other constituents of the plankton, and it was not the case even with the diatoms during the present spring.

With the view of testing plankton methods still further, at another time of year, I devoted a month this spring (March 28 to April 27) to a systematic exploration, from the S. Y. *Ladybird,* of the sea off Port Erin at the southwest corner of the Isle of Man. We worked on 23 days and obtained 276 samples, an average of 12 per day. [Particulars were here given of the localities, the methods and the various nets used.]

All the gatherings obtained are now being worked up in detail, and the results will be published in the Lancashire Sea-Fisheries Report during next winter by Mr. Andrew Scott and myself.

One or two broad features of the collections made were obvious. In the earlier part of the time, up to about the middle of April, diatoms were abundant, and nearly all the gatherings had a greenish tinge. During that period the plants were more abundant in the bottom waters, and the animals at the surface. Day after day we found that the two closing vertical nets hauled up from 20 fathoms to 10 fathoms were of a brownish-green color and contained (especially the Nansen net) an abundant gathering of diatoms. The surface nets during this time contained more Copepoda. On April 15 and 19, however, when the change in plankton was taking place, the diatoms were found to be mainly on the surface and the Copepoda below. As an example of wide distribution I may cite April 10, when the nets gave consistent results all the afternoon at three localities north of Port Erin, the diatoms being in all cases more abundant at the bottom and the Copepoda on the surface.

We were fortunate enough on one occasion to obtain incontrovertible evidence of the sharply defined nature of a shoal of organisms, forming an instructive example of how nets hauled under similar circumstances a short distance apart may give very different results. On the evening of April 1, at the "alongshore" station III., north of Port Erin, off the "Cronk," one mile out, I took six simultaneous gatherings in both surface and deeper waters. Two of the nets were the exactly similar surface tow-nets which I have called B and C. At half-time, as the result of a sudden thought I hauled in B, emptied the contents into a jar, and promptly put the net out again. This half gathering was of very ordinary character, containing a few Copepoda, some diatoms and some larvæ, but *no Crab Zoëas.* At the end of the fifteen minutes, when all the nets were hauled on board, all the gatherings, including B, showed an extraordinary number of Crab Zoëas rendering the ends of the nets quite dark in color. B was practically the same as C although B had only been fishing for seven minutes. It was evident that at about half-time the nets had encountered a remarkable swarm of organisms which had multiplied several times the bulk of the catch and had introduced a new animal in enormous numbers. Had it not been for the chance observation of the contents of B at half-time, it would naturally have been supposed that, as all the nets agreed in their evidence, the catches were fair samples of

what the water contained over at least the area traversed—whereas we now know that the Zoëas were confined to, at most, the latter half of the traverse and may have been even more restricted. Under these circumstances, an observation made solely in the water traversed during the first seven minutes would have given a very different result from that actually obtained; or, to put it another way, had two expeditions taken samples that evening at what might well be considered as the same station, but a few hundred yards apart, they might have arrived at very different conclusions as to the constitution of the plankton in that part of the ocean.

It is interesting to note that enormous numbers of *Oikopleura* "houses" covered with diatoms were present in some of the gatherings; and the abundance of the diatom *Thalassiosira Nordenskioldii* was phenomenal. We have some reason to think that there has been an exceptional flow of cold water from the north into the Irish Sea this spring and that may account for the presence of this northern diatom which has not been found in our region before.

As an example of two surface nets hauled together which gave much the same quantity of plankton, but where the gatherings differed widely in their nature, I may give the details of April 13. [Slide shown and details explained.]

The bearing of such observations as these upon some recent speculations as to the fish-population of the sea, and even as to the amounts of food-matters present in the waters of large areas, is obvious. Nothing in the economics of the sea could be more important than such speculations in regard to what I have proposed should be called the "hylokinesis" of the ocean, if we could be certain that our conclusions are correct, or even that they are reasonably close approximations.

It is possible to obtain a great deal of interesting information in regard to the hylokinesis of the sea without attempting a numerical accuracy which is not yet attainable. The details of measurement of catches and of computation of organisms become useless and the exact figures are non-significant, if the hauls from which they are derived are not really comparable with one another and the samples obtained are not adequately representative of nature. If the stations are so far apart and the dates are so distant that the samples represent little more than themselves, if the observations are liable to be affected by any accidental factor which does not apply to the entire area, then the results may be so erroneous as to be useless—or worse than useless, since they may lead to deceptive conclusions.

My view in brief is: (1) That we must investigate our methods before we attempt to investigate nature on a large scale, (2) that we must find out much about our gatherings of organisms before we can consider them as adequate samples; and (3) that we must make an intensive study of small areas before we draw conclusions in regard to relatively large regions such as the North Sea or the Atlantic Ocean.

W. A. Herdman

A SIMPLE ELECTRIC THERMOREGULATOR

The advantage of electricity over gas for heating paraffin baths, incubators, culture chambers, etc., in laboratories is well known. Electric thermoregulators for use in connection with such apparatus have appeared from time to time; but, as far as I know them, they are all more or less complicated or expensive.

The electric heating coil and regulator devised by Professor E. L. Mark[1] and used with success in the laboratories of the museum of comparative zoology at Harvard University ever since, costs between $25 and $30 for each bath. The expense of this device excludes its use in many laboratories, especially those in which quite a number are desired for individual use.

It was for the purpose of heating a small paraffin bath with a sixteen-candle power incandescent lamp that I first devised an electric thermoregulator. Later modifications of this piece of apparatus resulted in two forms, a mercury regulator and a glycerin or air regu-

[1] Mark, E. L., "A Paraffine Bath Heated by Electricity," *Amer. Nat.*, 37 (434): 115–119, 3 figs., February, 1903.