

210.—NOTE UPON THE EFFECT OF HIGH PRESSURES ON THE VITALITY OF MINUTE FRESH-WATER AND SALT-WATER ORGANISMS.*

By A. CERTES.

I have the honor of presenting to the Biological Society the note which I deposited in the Academy at its session of March 17 last, on the culture under shelter of germs from the waters and sediments of the Travailleur and the Talisman. (*Comptes rendus*, No. 11, p. 690.) On this occasion I ask permission to give a brief review of some new experiments which I have made with various microscopic organisms by subjecting them to high pressure for a period varying from seven hours to seven full days. This communication will, I hope, tend to lessen, if not to cause to disappear entirely, the differences, more apparent than real, which Dr. Regnard pointed out at the last session between the conclusions reached by his experiments and by mine. Thus I fully agree with Dr. Regnard's opinion "that the infusoria of the surface of the sea could not, without slow acclimatization, live in the depths, and that for these parts, as for all the others, there must be an abyssal fauna." It is nevertheless true that our experiments differ, both as to the aim we have in view and the conditions under which they have been made.

As far as I am concerned I have made it my aim to find out by what processes organic matter has been reduced to an inorganic state in the great depths of the sea. After the expedition of the Travailleur, in 1881, I at first searched directly for "microbes" by examining microscopically the sediments obtained by osmic acid and treated with coloring reagents. Not finding anything by this process, I had recourse to the method of cultivation; but from the very beginning I was fully aware that it would not be capable of producing genuine "microbes" from great depths, like those from the material gathered by the Talisman, but that I would have to place these "microbes" under their normal conditions of physiological activity. It is difficult to produce these conditions, and it is only by way of experiment that I at first sought to ascertain the effect of high pressure on unicellular organisms, both infusoria and "microbes," which we find near the surface. I had also to study how to avoid sudden pressure and a sudden stoppage of pressure, which in nature occur only by way of accidents.

At my request, and by the kind intercession of Mr. Cailletet, Mr. Ducretet has slightly modified the regulations for using his apparatus.

* *De l'action des hautes pressions sur la vitalité des micro-organismes d'eau douce et d'eau de mer.* Paris, 1884. Translated from the French by HERMAN JACOBSON.

In the apparatus which I use there are two receivers and two manometers instead of one. The receivers are either isolated or placed in communication, just as it is desired, by means of a stop-cock, which allows to accumulate the pressure or to decrease it, without too many precautions, in the first receiver. One can then, by means of the stop-cock, transmit the effect which has been obtained from one receiver to the other, as slowly as is desired. Thus in all my experiments, except when I made a mistake, it has taken nearly half an hour to rise from 0 to 500 atmospheres, and *vice versa*. In order to reproduce still more completely the conditions of nature, I have endeavored never to exceed from 400 to 500 atmospheres, which represent the average pressure of the depths explored by the submarine expeditions.

If I have rightly understood Dr. Regnard's different communications, these conditions, except as regards the experiment of which he has given an account at the last meeting, differ very materially from those which he sought to produce. No wonder, therefore, that the results obtained by me differ from his.

By working in the manner indicated, and always taking care to determine beforehand the species of infusoria or small organisms which I subjected to pressure, I have obtained the following results:

At a pressure of 100 to 300 atmospheres, maintained for seven, twenty-four, forty-eight and seventy-two hours, certain organisms were killed; others came out of the apparatus as lively as they entered; others again fell into that state of latent life of which Dr. Regnard speaks. At 450 to 500 atmospheres the number of live organisms decreases, and that of dead organisms, or those which have fallen into a latent life, increases. In the first experiment—of which I have already given an account to the academy—the *Ohlamydococcus pluvialis*, when subjected for seven hours to a pressure of 100 to 300 atmospheres, all came out of the apparatus as lively as they were when put into it. The majority of the other infusoria had died. In a second experiment, prolonged for forty-eight hours, at a pressure of 300 atmospheres, fresh-water infusoria, such as *Paramecium colpoda* and *Vorticelles*, had fallen into a state of latent life; others had died. On the other hand, *Euplotes charon*, *Euplotes patella*, and *Pleuronema marina*, marine infusoria, had remained active. Other species, especially *Holosticha flava* and *Actinophrys*, had died.

In the last experiment which I made, fresh-water organisms were for thirty-six hours subjected to a pressure of 520 atmospheres. When taken from the apparatus most of the *Ohlamydococcus* appeared to have fallen into a state of latent life; some had died, and others were still active; but entirely green individuals had resisted in greater number than those whose chlorophyl had begun to assume a red color. In the same tubes I was able to show to two of your colleagues, a quarter of an hour after they had been taken from the apparatus, rotifers in full activity. The tardigrades, which had fallen into a state of latent life, revived more slowly. In all the experiments certain "microbes," which

were very numerous in the tubes, when subjected to pressure disappeared, others moved about as soon as taken from the apparatus.

It appears, therefore, that under the conditions which I produced, the effects of high pressure vary not only between the different species but also between the individuals of the same species. It seems, moreover, that it makes a great difference whether the pressure or stoppage of pressure is more or less rapid. It is therefore not impossible that with a stronger pressure continued for a long time no surface organisms would survive, but that they would all indiscriminately die. This should be proved by experiments.

I cannot pass in silence the effect of high pressure on the carbuncled, charred "bacterid." With Dr. Roux we subjected carbonaceous blood to a pressure of 600 atmospheres for a period of twenty-four hours. This blood retained all its virulence, and experiments made with it proved entirely successful.

It will be seen that in none of these experiments had I touched the problem of fermentation or putrefaction. The experiments made by Dr. Regnard with yeast seem to agree with what is already known regarding the sleep of the mycodermic cells which are found in sparkling and sugared wines. At a certain given moment these cells no longer decompose sugar, either because they have become subjected to the paralyzing action of carbonic acid, or because their food gave out, or, finally, because—as Dr. Regnard's experience also seems to prove—the pressure produced by the tension of gas hinders the fermentation. But it may also be asked whether other ferments, especially those possessing much body from great depths, do not obey other laws. This is the problem which at the present time engages my attention, and which I am endeavoring to solve.

211.—ON THE SCARCITY OF MACKEREL IN THE GULF OF SAINT LAWRENCE.

By Capt. J. W. COLLINS.

The results which have been obtained in the mackerel fishery of the Gulf of Saint Lawrence by American vessels during the present season, are clearly and forcibly set forth in the following notes and sworn statements of captains, for which I am indebted to Capt. Fitz J. Babson, collector of customs at Gloucester. These facts, which may well supplement those already submitted concerning the same subject, demonstrate in the strongest possible manner that so far from deriving any benefit from a participation in the fisheries of the Gulf, the vessels of New England have suffered a severe pecuniary loss by leaving our own waters. It is worthy of notice that of the reports obtained from ten vessels *three came out of the Gulf without a single barrel of mackerel*, and of the catch made by the other seven schooners, *only 50 barrels*