ment will be added from time to time. In the words of *New Jersey Agriculture,* "thanks to Mr. Turner's vision and generosity, New Jersey will now have one of the largest and most complete dairy experiment stations in the world."

IT is reported in *Nature* that the Association of British Zoologists discussed at its general meeting the question of the payment of fees to zoologists for expert advice. It is well known, as Professor E. B. Poulton says in a letter on behalf of the council of the association, that a somewhat unscrupulous public takes for granted the good nature [and affluence!] of zoologists in requesting their professional help without offering payment in return. Whether it be a matter of the identification of a species or the delivering of a popular lecture, both demand the expenditure of time and energy, which the expert could have devoted profitably to his own purposes. The council's proposal is that, in the interests of their science, zoologists should demand fees for the work of identifying specimens and giving lectures. They say that such a demand would enhance the respect felt for the science, just as medical advice tends to be valued according to the size of the fee.

DISCUSSION

REDUCTION OF OXYLUCIFERIN BY ATOMIC HYDROGEN

SOME luminous animals, notably an ostracod crustacean, *Cypridina,* contain a substance, luciferin, which oxidizes to oxyluciferin in aqueous solution containing oxygen. In presence of a second substance, luciferase, luminescence appears. Luciferase acts both as a catalyst, accelerating the oxidation of luciferin, and also supplies molecules which may be excited to emit light by the energy of oxidizing luciferin. Oxyluciferin in water solution can be reduced to luciferin by various hydrogenation procedures. The mechanism has been fully discussed by Harvey.[1]

During attempts to excite luciferase to luminesce by the energy of recombination of hydrogen atoms we have observed that dry oxyluciferin can be reduced to luciferin. The apparatus was a modification of that used by Urey and Lavin.[2] Atomic hydrogen was produced in a high tension discharge tube at low pressure and drawn over the material exposed about 30 cm from the discharge tube. Dry oxyluciferin alone gives no luminescence when luciferase solution is added to it, but dry oxyluciferin first exposed to atomic hydrogen becomes reduced to luciferin, which can then be detected by luminescence on adding an aqueous solution of luciferase. No luminescence appears in the atomic hydrogen treated oxyluciferin on adding water alone but only if luciferase is present also, as is to be expected. This experiment confirms the results obtained by reducing oxyluciferin in water solution and makes it quite certain that the luciferin-oxyluciferin change is a dehydrogenation-hydrogenation reaction.

When dry luciferase and luciferin are exposed to low concentration of atomic hydrogen there may at times be observed a faint bluish glow which breaks into incandescence on raising the atomic hydrogen concentration. Dry egg albumen and dry powdered pill-bugs (*Oniscus*) exhibit a dull orange glow in low concentration of atomic hydrogen which may also pass into undoubted incandescence. The faint glow of *Cypridina* may be a luminescence or a low temperature incandescence since the containing vessel does become warm. Willemite and certain other substances show undoubted luminescence in atomic hydrogen.

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[1] *Bull. Nat. Res. Council,* No. 59, p. 50, 1927.
[2] *J. Amer. Chem. Soc., 51,* 3286, 1926.

"THE POSSIBLE RÔLE OF MICRO-ORGANISMS IN THE PRECIPITATION OF CALCIUM CARBONATE IN TROPICAL SEAS"

THE statement made by Dr. Werner Bavendamm, in the issue of SCIENCE for May 29, 1931, on the subject quoted above, compels me to make some critical comments. Much as I detest polemics I feel it necessary that those interested in the subject in question have the facts before them.

Dr. Bavendamm discusses at some length the observations in tropical seas with respect to calcium carbonate precipitation, and makes it appear that his findings are in conflict with mine. As a matter of fact, Dr. Bavendamm confirms practically all the results obtained in my own studies as reported elsewhere.[1] For example, (1) Dr. Bavendamm finds a very small bacterial population in the open sea. This confirms my findings and those of others. (2) Dr. Bavendamm shows that the bacterial population of muds like those off the Bahama Banks is relatively small as compared with those of soil populations. This also confirms my findings. (3) Dr. Bavendamm

[1] Publication No. 340, Carnegie Institution of Washington; Publication No. 391, Carnegie Institution of Washington, 1929.

states that investigators have used erroneously the term "calcium bacteria." This is a point which, among several, I emphasized particularly in my papers when I stated that the literature of bacteria shows that many different forms of bacteria have the power of precipitating calcium carbonate under the proper conditions.

In view of these statements by Dr. Bavendamm, I fail to see why he does not agree with what he calls my ideas and conclusions "based on experiments which were not sufficiently convincing." What Dr. Bavendamm probably means is that my experiments are not sufficiently convincing to him.

It appears as I read Dr. Bavendamm's statement that he bases all his views and attitude on the problem in question on his findings of bacteriological conditions existing in mangrove swamps like those on the coast of Williams Island. The fact that bacteria and other micro-organisms exist in large numbers and in great variety in mangrove swamps is not in the least surprising to me, nor can it be to any one who is acquainted with bacterial populations in such material as exists in mangrove swamps. To be sure, very little investigation of bacterial populations in such mixtures of organic and inorganic materials has been carried out, and it is highly desirable that much of this work shall be done, but this has little or no relation to the question as to whether or not the calcareous deposits of the earth's surface have been built up through bacterial action in the open sea.

If Dr. Bavendamm will consult my papers he will see that I have merely examined critically the possibility of calcuim precipitation in any quantity in the open sea through bacteria existing there, and with special reference to the Drew hypothesis. I have never claimed that calcium carbonate precipitation could not be effected in such a medium as the mangrove swamps, nor, if Dr. Bavendamm reads my papers carefully, will he find the conclusion that the physical-chemical method of calcium carbonate precipitation to which he referred was regarded by me as the only method of calcium carbonate precipitation in the sea. My main contention was, and still is, that no case has as yet been made out for specific forms of bacteria which have as their function the precipitation of calcium carbonate, and secondly, that no case has ever been made out for large-scale precipitation of calcium carbonate in the open sea, by the mechanism postulated by Drew and too hastily approved by geologists, generally speaking.

That many living organisms are concerned with the secretion of calcium carbonate has been emphasized by many biologists. That these may have been indirectly concerned with the accumulation of calcium carbonate deposits has also been emphasized by many investigators, but these facts have no bearing on the points originally made by Drew with which I took issue in my publications in this field.

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INHERITED TASTE DEFICIENCY

In Science for April 17, 1931, Dr. Arthur L. Fox, of the laboratories of the du Pont de Nemours Company, was reported as having found that certain persons apparently have no ability to taste para-ethoxy-phenyl-thio-urea. It was reported that 40 per cent. of the individuals tested could not taste the compound, while to the remaining 60 per cent. it was exceedingly bitter. I immediately wrote Dr. Fox asking for some of the compound with which to investigate the possible inheritance of this taste deficiency. This is a preliminary report of the occurrence of the condition in one hundred families.

First of all, I can confirm Dr. Fox's conclusion that the taste deficiency actually exists, and is not a matter of age, sex nor race. It is not dependent upon acidity nor alkalinity of the mouth. Those tasting it find it bitter, usually exceedingly bitter, even nauseating, while those not tasting it are unable to get any taste at all, even after rinsing the mouth with dilute acids or alkalis.

My results to date show 68.5 per cent. tasters, and 31.5 per cent. with the taste deficiency. I have tried it out in families, and the results of the first one hundred families are so conclusive that they are worthy of record. The taste deficiency is apparently due to a single recessive gene. It is not sex-linked nor sex-influenced. When neither parent can taste the compound, none of the children can taste it.

Dr. Fox tells me that the taste deficiency occurs in other compounds of the phenyl-thio-urea group as well. Di O-tolyl-thio-urea behaves somewhat differently from the others, and will be reported on later. For the present it is sufficient to establish the taste deficiency as a unit-factor recessive.

The results of the study of one hundred families are as follows:

		Children	
	No. of families	Can taste	Can not taste
Both parents can taste	40	90	16
One parent can taste, the other can not	51	80	37
Neither parent can taste	9	0	17

	Males	Females	Total	Percentage
Can taste	150	151	301	68.5
Can not taste	71	68	139	31.5