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

Further work is being done on the inheritance of this taste deficiency, its linkage relations and its physiology, and these results will be reported in detail in the near future.

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GETTING THE STUDENT TO USE HIS OWN INTELLECT

PROFESSOR WILLIAM A. RILEY in his timely address as chairman of the Section of Zoology of the American Association for the Advancement of Science reverts to the advice given by Huxley in 1869 that, "if the great benefits of scientific training are sought, it is essential that such training should be real; that is to say, that the mind of the scholar should be brought into direct relation with the fact, that he should not merely be told a thing but made to see by the use of his own intellect and ability that the thing is so, and not otherwise."

In discussing the relation of this advice to the present educational situation Professor Riley incidentally raises a question without answering it. The question is how far laboratory work as now carried on in secondary schools and colleges is essential to the purpose of inducing students to use their own intellects. This topic has recently aroused a good deal of interest among teachers of science and is one which deserves serious consideration.

The usual way of conducting laboratory work is to put into the hands of the student a laboratory manual or sheet of directions which he must follow in order to produce the expected result. Subsequently he reads an assignment in a text-book and listens to a lecture. If this is the best possible way to get the student to do his own thinking it fully justifies the expenditure of money, space and time needed for the equipment of the laboratory and the conduct of laboratory courses. If it is not the best way and classroom demonstrations, lectures and text-books will serve equally well, then the expenditure is not justified.

Possibly the question might be answered conclusively if the same kind of subject-matter were presented to two classes equal in numbers, age and intellect but working according to the two different methods and at the end of a definite period tested with regard to their ability to solve problems involving application of what they had learned. Failing such evidence there may yet be some profit in discussing the probable relative advantages and disadvantages of the two methods in the light of experience both educational and practical.

In actual life more than one kind of thinking is needed. The successful chauffeur or cook must be able to see relations between concrete, material things and swiftly draw the correct conclusions. To me the probabilities are in favor of this ability being developed and strengthened more effectively if the chauffeur or cook actually drives a car or prepares a meal than if he simply watches a demonstration or hears an explanation. A combination of the two methods would be still more effective. Similarly if, in teaching science, we as educators wish to develop and strengthen the habit of thinking with the aim of producing particular effects on concrete material, a combination of individual laboratory experience with demonstration and explanation is advantageous. If children are being taught botany with the object of becoming better gardeners or farmers there is a real advantage in individual observation and experimentation with seeds and seedlings over demonstration with charts and models. A future practical chemist will benefit more by handling apparatus and reagents than by watching some one else do the same thing.

When the educational problem is less definite, when science is being taught not with the immediate aim of training for a specific occupation but of developing general mental powers, of establishing a scientific attitude, of giving practice in dealing with abstract as well as concrete ideas and of enabling the student to understand everyday happenings, what are the advantages, if any, of individual laboratory work? Will, for example, powers of observation and comparison be developed better if each student is supplied with specimens of plants or animals and asked to make a list of similarities and differences or if the teacher points them out on a chart or model? Which will leave a more lasting impression of the fact that heat causes gases to expand, for the children to make bubbles rise through water themselves or to watch the teacher do it?

The answer seems obvious. The secret of the greater effectiveness of individual laboratory work lies in the increased motivation. Most young people love to be active and to bring about results by their own efforts. Interest is greatly enhanced if the problem is sufficiently simple for them to devise their own method of solving it. This has the additional advantage of helping to establish the habit of self-reliance.

I believe it is a fact that some laboratory courses do not stimulate initiative and interest and I believe that this is because the laboratory directions make no appeal to the student. They are carried out simply as a matter of routine in order to prepare for the lecture or quiz which is to follow. In other words, the chief consideration has been the logical development of subject-matter rather than the stimulation of the student's initiative and powers. The reverse