correspondence, for there were preliminary discussions. We all know who formulated the proposition, and I have authority which no one will dispute for saying that its author did not originate the plan, but simply formulated it as the result of the preliminary discussions between the members of our and of their special committee.

I can not, and *have not,* asserted that Professor Wilson originated the plan; but I think it safe to say that he knew of the plan before it was presented, that he approved it, presented it, and opposed the alternative plan of support without ownership, which was the preference of the Carnegie trustees. By all this Professor Wilson made himself its godfather.

In the passage quoted by Professor Wilson, the statement is made that 'they were asked on what terms they would consent to own and support it.' 'No such question,' says Professor Wilson, 'was asked or suggested in any of the official correspondence.' I did not pretend to give exact words, nor did I assert that the question occurred in the official correspondence. It is a mistake however to say that this correspondence did not suggest it. It did suggest it to me, and I think my statement fairly summarizes the attitude assumed on our side.

If Professor Wilson asked or suggested support that involved '*an obvious necessity*' of ownership by the Carnegie Institution, and if he has never objected to such ownership, but has objected to support that did not involve ownership, the objection to my words cannot be very serious.

C. O. WHITMAN.

CHICAGO, October 14.

THE MARINE BIOLOGICAL LABORATORY AND THE CARNEGIE INSTITUTION.

TO THE EDITOR OF SCIENCE: In your article in SCIENCE, September 19, 1902, on the 'Carnegie Institution,' you make statements in regard to this laboratory on which I beg to comment. You say that 'the corporation of the Marine Biological Laboratory is a corporation composed chiefly of those who have carried on research in the laboratory.'

Pardon me if I express doubt as to the exactness of this statement. The corporation has three hundred and fifty-two members. Of these sixty-five are residents of Boston or its vicinity, and most of them are personally known to me. Very few of them have ever carried on research in this laboratory. They have aided the laboratory by donations, but not by work. I think a large per cent. of those who have carried on research in this laboratory are members of the American Society of Naturalists. A comparison of the lists of members of that society and of the corporation shows that but seventy-one (about twenty per cent.) of the corporation belong to the society; further, that the society has but half a dozen female members, while one hundred and seventeen (about twenty-four per cent.) of the corporation are women. Still further, over fifty per cent. of the corporation give no university or college address, but simply town, street and number. Persons holding university or college positions generally give their official addresses. All these facts tend to confirm me in the opinion that the corporation is not 'composed chiefly of those who have carried on research in the laboratory.'

In the past several attempts have been made to secure to this laboratory large financial support, but on every occasion we have been told by those to whom appeals have been made, that the defects in our business organizations were deterrent to those who might otherwise contribute. We were told that before acquiring endowment, land and permanent buildings, all property should be vested in a smaller and more select body. What our advisers have told us in the past, the executive committee of the Carnegie Institution has but repeated. The matter of support by the Carnegie Institution was considered at two largely attended trustees' meetings, and it was voted unanimously to recommend to the corporation that on a promise of support by the Carnegie Institution, the corporation should convey its property to that institution.

At the annual meeting of the corporation, August 12, 1902, a deed conveying the property was read, and a motion was made in-

structing and empowering the treasurer to so convey the property.

You moved that the following amendment be appended to the motion: 'That the corporation of the Marine Biological Laboratory request the trustees of the Carnegie Institution to consider the possibility of assisting the laboratory without making it a branch of the Carnegie Institution.' A large majority voted against this amendment. It was made a second time in a slightly altered form, but received still less support.

The original motion was then put before the meeting, and by an overwhelming majority it was

VOTED: That the Treasurer, D. Blakely Hoar, be and he is hereby authorized and instructed to execute, acknowledge, and deliver, in the name and behalf of this corporation, the deed which has just been read, conveying to the said Carnegie Institution, all and singular, the properties of this corporation, and also any and all other documents of title in the opinion of counsel necessary or expedient fully to vest the title to such property in said Institution.

You and two others cast the only opposing votes. Yet, in your article in SCIENCE you state that 'It was the preference of nearly all the members of the corporation that the laboratory should be assisted by the Carnegie Institution without being made a branch of it.' I do not know on what this statement can be based.

In another part of the same article you say, 'the director and other scientific men serve the laboratory without salary.' The director, yes. The other scientific men, no. All members of the staff who need or wish it receive remuneration for services rendered.

In 1901 the salaries of the scientific staff amounted to $2,625. The income was $8,448.22, so that the salaries were about thirty per cent. of the income. In 1902, the salaries were $3,700. This, of course, does not include the curator, collector, janitors, boatmen, etc. These are the figures given by the treasurer.

Before the annual meeting of the corporation, the question was freely discussed, whether teaching would be continued under Carnegie Institution control, and how that would affect the numerous small salaries now paid.

It is to the credit of these men who receive salaries that when they were called on to consider the advancement of the laboratory, they forgot their salaries and helped to form the great majority in favor of Carnegie Institution control.

EDW. G. GARDINER,
Secretary.

MARINE BIOLOGICAL LABORATORY,
WOODS HOLE, MASS.

I TRUST that Dr. Gardiner will permit me to reply briefly to his remarks:

1. I am not correctly quoted in his first paragraph, as may be seen by reference to my article (p. 461 above). Instead of saying that the members of the corporation are 'chiefly' those who have carried on research in the laboratory, it would have been more accurate if I had said that 'the chief members of the corporation' or 'nearly all those who attend meetings of the corporation' have carried on research in the laboratory. The inexactness appears to be rather slight.

2. I think I was correct in stating that 'It was the preference of nearly all the members of the corporation that the laboratory should be assisted by the Carnegie Institution without being made a branch of it.' The members have never been permitted to make known their real preference. Professor Whitman, the director, and Professor Wilson, the chairman of the executive committee, who both voted for the transfer, have stated in SCIENCE that (to quote the latter) 'An organization similar to the existing one would be preferable if compatible with adequate financial support.' If Dr. Gardiner had quoted the second as well as the first half of my sentence, it seems to me that the matter would have been sufficiently explained. I continued 'but the alternative was placed before them of giving away the laboratory or losing the large support of the Carnegie Institution and perhaps witnessing the establishment of a rival laboratory.'

3. My statement that 'the director and other scientific men serve the laboratory without salary' is correct. Dr. Gardiner and I myself are among the many scientific men who have so served the laboratory. Dr. Gardiner has given a large part of his time to it for many years. Should the Marine Biological Labora-

tory become a branch of the Carnegie Institution and should Dr. Gardiner be retained as secretary, he should receive a salary.

Dr. Gardiner sends his letter to the 'Editor of Science,' but addresses me personally. The editor of SCIENCE, as representing the policy of the journal, is responsible for the acceptance of my article for publication, but not for the opinions expressed in it.

J. MCKEEN CATTELL.

COLUMBIA UNIVERSITY.

ORANGE COUNTY MASTODONS.

MR. GORDON will, I trust, pardon me for saying that he is mistaken in supposing that the bones of the last three mastodons discovered in Orange County were found in their proper relative positions. The Schaeffer specimen was scattered over about thirty by fifty feet and the greater portion of three legs was never found. The Monroe specimen is sadly incomplete and there is reason to suppose that part of it is a hundred yards away from where the tusks were discovered. Finally, the entire hind legs of the otherwise fine animal at Yale have never been recovered. There is also a specimen at Vassar that I believe came from the vicinity of Newburgh, and this too is incomplete.

It is possible, however, that Mr. Gordon has reference to the Peale specimens, and these, I believe, were fairly complete. If it is to these that Mr. Gordon refers, the mistake is on my part. F. A. LUCAS.

WASHINGTON, D. C.,
October 10.

SHORTER ARTICLES.

THE BITTER ROT DISEASE OF APPLES.

ON July 10, of this year, Mr. R. A. Simpson, an agent in the employ of this laboratory, called our attention to the fact that the bitter rot spores which infected the apples in his orchard at Parkersburg, Ill., seemed to come from canker-like formations on the limbs of the apple trees. The bitter rot was first observed by him July 9. An examination of the trees on which the rot had appeared showed that in almost every instance it was possible to trace the infection to such a canker. The tracing was comparatively an easy matter, as the first lot of infected fruit usually occurs distributed in the form of a cone, with its apex towards the top of the tree. Although it seemed probable from Mr. Simpson's discovery, which was verified and extended by us several days later, both in the orchard at Parkersburg and elsewhere in Illinois and Missouri, that a causal relation existed between the cankers and the bitter rot disease of the apples, it was not thought sufficiently well proven at that time to warrant publication. Examinations of the cankers showed the presence of pycnidia containing the characteristic pale bitter rot spores, likewise of numerous spores of *Sphæropsis malorum*, of a species of *Alternaria* and spores of several other fungi. In the cultures made from numerous cankers *Glœosporium fructigenum* appeared in every instance.*

At first conidia borne free on short hyphal branches appeared in the pure cultures, and later on the pink masses of spores usually found on diseased fruits. When kept for some time, the fungus in these pure cultures produced perfect perithecia and asci. Mycelium which produces perithecia and asci when transferred to fresh apple agar, will continue forming perithecia, the latter appearing in such fresh cultures seven to eight days after the transfer. Inoculations were made into the bark of healthy apple trees about the middle of July, with spores from pure cultures obtained from the cankers. At the same time apples were inoculated with these same spores. In the course of a week the infected apples showed every sign of the bitter rot disease as found out of doors. Inoculations were likewise made with *Glœosporium* spores taken from apples recently attacked in the orchard, both into healthy apples and into growing apple branches, at the Missouri Botanical Garden. Inoculations into the branches were made by making shallow cuts through the bark, and inserting a needle point covered with spores into the cut. Control cuts were made for every inoculation, distant but two to three inches from the infected cut. At first little difference was noticeable between

* Most of the cultures were made by Mr. Geo. G. Hedgcock, assistant in pathology.