

is capable of neutralizing the particular toxin to which it owes its origin, if this is subsequently introduced into the blood. In this theory a specific combining relation is assumed to exist between various toxic substances and the secondary atom-groups of certain cellular elements of the body. The atom-groups which, in accordance with this theory, combine with the toxin of any particular disease germ, Ehrlich calls the 'toxicophoric side chain.'*

The fact that the toxin produced by the tetanus bacillus has an elective affinity for the cells of the nervous tissues seems to be well established. The wonderful toxic potency of this toxin is shown by the researches of Kitasato and by those of Brieger and Cohn (1893). According to the last-named authors the chemical reactions of the purified toxin show that it is not a true albuminous body. When injected beneath the skin of a mouse weighing fifteen grams, in the dose of 0.00000005 gram, it caused its death, and one-fifth of this amount gave rise to tetanic symptoms. The lethal dose for a man weighing seventy kilograms is estimated by Brieger and Cohn to be 0.00023 gram (0.23 milligram). Comparing this with the most deadly vegetable alkaloids known, it is nearly six hundred times as potent as atropin and one hundred and fifty times as potent as strychnin. Ehrlich's explanation of the origin of antitoxins is opposed by Buchner and others. According to Buchner the antitoxins are to be regarded not as reactive products developed in the body of the immune animal, but as modified, changed and '*entgiftete*' products of the specific bacterial cells. He insists that they do not neutralize toxins by direct contact, but only through the medium of the living organism.

On the other hand, Ehrlich insists that the antitoxin neutralizes the toxin directly, in a chemical way, and that such neutralization occurs when they are mixed in a test-tube, even more effectually than when they are injected separately into the body of a suscep-

tible animal. The experimental evidence appears to me to be in favor of Ehrlich's view, but neither time nor space will permit me to present this evidence or to review the experimental data upon which Ehrlich bases his side-chain theory. The reader is referred to Professor Aschoff's work for a full discussion of the subject. Certainly Ehrlich's views are entitled to great consideration, but it is evident that his theory, however plausible it may appear, especially to chemists, is far from being established upon a reliable experimental basis. For us, the numerous facts which have been brought to light by his painstaking researches have a far greater scientific value than his '*Seitenkettentheorie*.'

GEO. M. STERNBERG.

DISCUSSION AND CORRESPONDENCE.

SOME MATTERS OF FACT OVERLOOKED BY PROFESSOR WILSON.

PROFESSOR WILSON seems to think that the general scientific public is in danger of getting 'a wrong impression' of the situation at Wood's Holl from my article in SCIENCE of October 3; and in order to prevent this he offers some criticisms and insinuations which, I think, may produce a worse impression than the one he desires to correct. Let me say, therefore, to begin with, that our different standpoints and opinions have been, and will doubtless continue to be held on perfectly friendly terms.

Professor Wilson has favored merging the laboratory in the Carnegie Institution, and he has insisted very strongly that the independence of the laboratory would not be thereby endangered in any essential respect. This view was naturally seductive, for what friend of the laboratory would not welcome a permanent support which could be had without the sacrifice of a single principle or condition of vital importance? The financial difficulties under which we have so long labored predisposed all to accept relief and forget the risk. The assurance that there was no real risk from the one who had carried on most of the negotiations for our side, and the conditions proposed by the Carnegie committee all tended to allay doubt. Our organization was to remain essentially as

* Quoted from the writer's 'Text-book of Bacteriology,' second edition, 1891.

it is, our work was not to be interfered with, we were to direct the policy of the laboratory as hitherto, and our needs in the way of land, buildings, boats, libraries, etc., were to be provided for; in short, we were to have a permanent laboratory with staff and equipment for work throughout the year, a laboratory that would rival the best in the world. So bright did the prospect appear to Professor Wilson that he could speak of it as '*beyond the dream of avarice.*' With all my faith in Dr. Wilson's sagacity, I cannot escape the suspicion that he has been under the spell of some trance-like illusion, which, for the time being, excludes a calm consideration of 'matters of fact.'

If the latest communication from the Carnegie committee does not dispel the illusion, I do not know what will. This communication has gone to all our trustees and will probably be announced at the proper time. It is sufficient to say, that it conclusively confirms the position I have taken, namely, that the laboratory should remain forever independent, but always ready for cooperation and always grateful for such support as its work may deserve.

This is the main point of my paper, which Professor Wilson criticises in a spirit that seems to me to fall a little short of amiable; but I hope I am mistaken in this.

As the matter now turns, we may rejoice that our trust and our mistakes have not been confounded by the Carnegie trustees; and we are most deeply indebted to their wisdom, frankness and generosity. It is now, I believe, needless to follow Professor Wilson further on this point, as he has been answered by the communication above mentioned more effectively than by any arguments that I could offer.

There is just one incident bearing on this point, which I wish to recall as a significant matter of fact. After our corporation meeting, August 12, a petition was drawn up by one of the members and presented to Professor Wilson for approval. That part of the petition which concerns us here was as follows: 'We, therefore, hope that the trustees of the Carnegie Institution may find it possible to support the Marine Biological Laboratory in the manner proposed, *without requir-*

ing it to become a branch of the Carnegie Institution.' Professor Wilson read the petition, and at once declared that he was willing to sign it. When the petition was presented a few days later, Professor Wilson, for reasons that need not be given here, declined to give his signature, and the petition was consequently abandoned. The incident is significant as showing that at that time Professor Wilson was willing to endorse a preference for preserving the independence of the laboratory. I believe every member of the corporation would have been glad to sign such a petition, had it seemed safe and proper to do so. The fact throws light on the situation as a whole, and as it is no secret, I feel justified in bringing it forward.

I regret that Professor Wilson does not seem to approve of the publication of my paper in SCIENCE. I felt that the time had come for me to remove the misunderstanding in regard to my position. I stated the situation as I understood it, and frankly avowed my desire to preserve the independence of the laboratory. I submitted the paper to a number of the trustees and finally to Dr. Billings, who consented to its publication. Professor Wilson stigmatizes my view as 'pessimistic' and closes with a reference to past criticisms of the laboratory which might well have been omitted as wholly unprovoked and uncalled for. This is the most unkind cut of all, that a friend of the laboratory should thus covertly countenance its calumniators.

One point more. Professor Wilson objects to my saying that the plan of acquiring the laboratory as a condition to supporting it did not originate with the trustees of the Carnegie Institution. I stated the matter as I understood it and as I still see it. Professor Wilson was not the only one on our side who at first had a hand in determining events.

We have been repeatedly told by the Carnegie committee that they should have preferred to recommend support without ownership, and one of them distinctly stated in Professor Wilson's presence that it was the '*emergency*' placed before them which led them to the proposition finally made to us. It is little to the point to refer to the official

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-