

January 22, 1947

Dr. M. Demerec
Biological Laboratory
Cold Spring Harbor
Long Island, New York

Dear Dr. Demerec:

After a long telephone conversation with Lindegren, I have agreed to make a few minor changes in my discussion of his paper. I am enclosing the revised discussion and am sending a copy of it to Lindegren.

I want to make very clear to you that I object strongly and seriously to the efforts Lindegren has made and continues to make to squelch all criticism of his work. He tried to argue me out of publishing my criticism by maintaining that my main point has been experimentally answered. The "experimental answers" he speaks of are not in his paper, and he finally admitted this, but then said they are in another paper not yet published. Nevertheless, he wished me to delete my criticisms because of these "answers" which I have not yet seen. Frankly, I have no confidence in his judgment as to what constitutes valid evidence and I am unwilling to delete my criticism unless I see the evidence myself. You will recall that he, and apparently Spiegelman, maintained his present revised manuscript answered my criticisms which they prevailed upon you to delete. When I saw the paper - which Lindegren never sent me though Spiegelman said he would - I found their claims were totally unsupported. I don't like the smell of the whole business.

In my conversation with Lindegren just now, I made him promise to make clear the source and nature of the evidence he cites in answer to my criticism, particularly to state that the evidence is not in the paper I criticized or in any that has yet appeared in print. I am depending on you to check and see that he fulfills that promise. Otherwise, it would be only fair to let me have a chance to reply to his answer which, if it follows the line he told me over the phone, is, in my opinion, ridiculous and a violation of elementary scientific principles.

I realize that the volume must go to press quickly and that this bickering is holding it up. On the other hand, the issues involved are fundamental for the maintenance of honesty and freedom in scientific publication. I am sure you do not want the C. S. H. Symposia to run the risk of being involved in that basic issue. Lindegren's methods of attempting to suppress free criticism are, in my opinion, as reprehensible

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as Fascist to Communistic censorship, and I only hope that the others who criticized Lindegren's paper at Cold Spring Harbor have not had their criticisms suppressed. I say this to you because I know you would not want to be involved in such suppression, and I certainly do not wish you to be.

Believe me, I would not write to you in this way if I did not think the matter were extremely important.

With very best personal regards,

Cordially yours

T. M. Sonneborn

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Enc.

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