

INDIANA UNIVERSITY

BLOOMINGTON, INDIANA

DEPARTMENT OF ZOOLOGY

November 21, 1950

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Dear Sol:

I am deeply grateful to you for the frankness and fullness of your letter of the 15th and I both appreciate and enormously admire the qualities of the scientist at his best which shine through every page of it. Although I shall write you later after I have fully considered all the points you raise there are some things that can be said at once.

1. I had no way of knowing that Lindegren had misrepresented the part Doudoroff played in the "repetition"; nor did I know that Doudoroff was unwilling to have his name associated with the work, since it was in fact so associated in the CSH paper. Had I known the facts to be as you state them, I should certainly have been unconvinced of the significance of the "repetition", as I am about all of Lindegren's work. But how was that to be known?

2. Concerning the data to which you refer in Lindegren's book, I can make no considered comment until I have carefully examined the data in the light of your comments. At first glance, however, the point that 2:2 segregations were obtained in all 4 fully analyzed tetrads in presence of substrate, while in only 1 out of 9 "controls", doesn't seem in itself to lessen the validity of the claimed failure to confirm the earlier report of 6 out of 7 with all 4 positives. Concerning the full significance to be attached to the use of different cultures for "controls" and experimentals and for the original experiment and the "repetition", (although undoubtedly an example of poor experimenting), I should at present like to defer judgment.

3. With regard to the paragraph in the middle of page 2, let me repeat that I would not have given credence to Lindegren's statement if it had been clear it was about his own work only. I gave credence to it only on the basis of the association with Doudoroff's name, just as I give credence to those parts of Lindegren's work that were associated with your name.

4. With regard to Winge and Roberts, I was of course well aware of the point made in the middle of page 4, as your later reference to my course outline confirms. I can also see why you think the arguments from Doudoroff and Winge are in a sense contradictory. On the other hand, as a representative of those who "use this combination" on you, as you put it, let me try to explain my mental processes, irrational though they may seem to you. There are two major points of fact at issue: (1) In crosses of $M \times m$, does the presence of substrate result usually in the origin of 4 apparent M cultures? (2) Do cultures with the m gene have or lack the capacity of producing enzyme in response to presence of substrate? The "Lindgren-Doudoroff case" seemed to raise doubts as to the first fact and the "Winge case" raised doubts as to the second. To be sure, as you and I both realized, it was also difficult to see how the two cases could be reconciled. Nevertheless, with these two independent difficulties to be faced, it seemed to me that the plasma-gene interpretation was more seriously challenged than it would have been by either one alone. For, even if the two cases were irreconcilable, there was no way of knowing which one--if either--was to be taken seriously and which rejected. Hence, the need seemed to arise for demonstrating two things before the initial results and interpretations could be accepted: (1) repetition of the results with your material; (2) demonstration that your m gene was not merely one for slow adaptation. I hope this makes clear my (possibly peculiar) mental processes.

5. I think I have already covered above your points (a) and (b) on page 6. Concerning (c), it was of course difficult to have uninterrupted conversation at Columbus and I hope to have you visit me here at any time convenient to you.

6. The point of view you express in the first paragraph of page 7 is one with which I am in 100% agreement. I would subscribe to it without changing a word, and you should know that. Indeed I cannot see why you wrote it, unless you were referring again to my query about your silence for four years. If this was what prompted it, let me assure you that I had in mind no confession of sins; I referred only to your failure to comment on the "repetition" in any way. If you had reasons to doubt the validity of the repetition, as you have, it would have been extremely helpful to us on the outside to know that you did and why.

7. I am sorry your letter was painful to write. I welcome your straightforward, honest statement and I am deeply grateful to you for it. Your efforts have helped me to understand your views and on many of the matters you discussed I already share your viewpoint. When I examine further your comments on the data and your manuscript, I may share even more. Most of all I agree wholeheartedly with your appeal to further experiment; that was I thought the upshot of my comments in the Scientific American.

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8. After receiving your first letter, I wrote to Flanagan informing him that you disagreed with my statement and suggesting that you might wish to write a letter to the editor. Since you do not wish to do that, I'd be glad--if you agree--to write such a letter myself and to submit it to you for approval before transmitting it to Flanagan.

9. I don't know what you have in mind in your comment on "social implications" and I am genuinely sorry I caused you pain and depression. I tried only to convey my understanding of the present status of the work, namely that it requires clarification. I certainly did not wish to present any obstacle to your further work, for like many others I fully appreciate your superb qualities as a thinker and investigator. Moreover, I particularly regret having you feel that I have been guilty of injustice to you for I have myself often been on the receiving line and know very well how it feels. Nevertheless, I have tried not to be diverted from the main business of carrying on, as actively as possible, my own researches, in the faith that work will seek its own proper level in the perspective of time. I have gradually learned that the unpleasantnesses that occasionally occur are part of the price one pays for having one's work and ideas considered worthy of notice.

I have now received for distribution 350 copies of the article in the Scientific American. I intend to strike out, in every one of them, on page 34 the words "other investigators and they themselves have" and to replace these words by "the Lindegrens". I intend also to call attention to this change by writing on the title page "See correction on page 34". I should be grateful to you if you would provide me with a list of names and addresses of the people (up to 350) to whom you would like me to send these corrected copies. This, together with my proposed letter to the editor, is all I can think of at present in the way of attempts to rectify whatever possible unwarranted and unwished damage I may have done. I have also been asked to expand the article for a little booklet and will modify that section accordingly. Please let me know if there is anything else I can do now to lessen your depression.

I will write you again after I have studied the meat of your letter in relation to the passages in Lindegren's book, and your manuscript. The latter will be returned at that time. Meanwhile, I hope you will think in more concrete terms about coming here for a week-end visit. My older boy is away at college and his room will be available for you. What would please me most would be to have you arrive here before 8 p.m. on a Friday, prepared to present a discussion of your work and views to our group of workers on Paramecium. We meet in our laboratory every Friday night at 8. It is entirely informal--around a table, but with blackboard available. There will probably be frequent interruption with questions and comments as you talk and you would not be held to a rigid time limit. The speaker continues till 9:30 or 10 when we

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stop for light refreshment and then break up into smaller groups to continue discussion as long as we wish or can take it. Any Friday (except during vacations) after December 8 will be fine. I do hope you will arrange to come. This invitation is not an attempt to make amends; it is a sincere invitation from which we might all profit.

Cordially,


Tracy

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