Sveriges Utsädesförening

TEL. SVALÖF 19

Svalöf 23/5/34.

Dear Professor Fisher,

I must thank you for offering me the genetical appointment at the Galton laboratory and I shall be very pleased to accept it. I assume that the appointment will date from the first of October next, as I shall be in Sweden until sometime in September.

I read with great interest the points which you raised about my last letter. I quite agree that Sewall Wright has shifted about a bit during the last few years but I was referring to his most recent paper in the American Naturalist. It seems to me that there is more in his line of thought than the mere presence of components of the reactions in excess, which, of course, is somewhat of an argumenta~~tive~~ry cul-de-sac. Both Rasmusson's case and Wright's argument, as I read it, turn on the fact, (or should I say supposition?) that even though the reaction be controlled solely by the gene in question, if we ~~treat~~ consider that gene as a catalyst, there is no ground for believing that the speed of the reaction will be simply proportional to the "concentration" of the gene. It seems quite feasible, and indeed probable, that the addition of an equal quantity of the gene will not double the speed of the reaction but increase it to a somewhat less extent. Still I must defer discussion on this point until I see you, as it is quite possible that I have got hold of the thing the wrong way about.

About the remarks on the suppressors which I made, I don't think that you quite got my point. If we suppose, and I think that we may legitimately do so, that more than two reactions involve the same substrate, then the speeding up of one reaction may be due to the slowing down of another, since, presumably the speed of a reaction is at least partially proportional to the concentration of the substrate. This, as I said, would involve an effect on some other character, which, I believe, is what Schultze found.

I appreciate your criticism of Harland's writings. He also adopts a rather critical attitude towards Haldane's pointing out that cotton is a polyploid. This has been fully established by Skovsted's cytological work and Harland tries to dispute it on genetical grounds. Since polyploidy is more simply understood and detected cytologically than genetically, Harland seems to be taking a rather doubtful attitude, which, to my mind, indicates that he does not altogether understand the genetical behaviour of polyploids. He admits himself that they have cases of duplicate factors but denies that this supports the idea of polyploidy as they have many other factors which are not duplicated. As I said in my last letter, it is quite likely that, if the polyploid be old enough, or it come from a cross between distinctly different types, like the Raphano-Brassica hybrid, it might very likely show genetical behaviour approximating to that of a diploid. This seems to be the case in cotton, while the occurrense of even a few duplicate factors indicates the possibulity of rather unusual behaviour of modifiers in some cases, at least.

Finally I should be very grateful if you would send me references to the most important works on the genetics of those animals which you are breeding. I should like very much to get to know some more about the detailed results obtained with some of the animal genetical material before I commence my new duties. It will save a lot of time later on for me to have such details at my finger tips.

Yours sincerely,

K. Mather.

P.S. If you intend to have any cytology done, and the snails might prove very interesting from that point of view, it will be necessary to have an incubator of some kind. We can of course, always discuss this matter later on, if you think it best. I should have to see the apparatus to be really able to judge its suitability.