

JOHN INNES HORTICULTURAL INSTITUTION

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6th. January 1942

Dear Professor,

Thanks for the comments on the Primula paper. I shall be extremely interested to see your account of the χ^2 calculation, as it does seem to be very close to my own method.

As regards the sheltering question, I of course know of your Amer. Nat. paper, which I saw when it was in preparation. My wording in the Primula paper was rather careless, I should have said "sheltered from the action of some forms of selection", and "Deleterious combinations of recessive (polygenes)". This was what I had in mind as my reference to internal balance shows. I had always viewed the question in Primula from the point of view of polygene inheritance, and so the possible ^{epi} clash, as a result of poor wording, with your findings had not crossed my mind.

I must look up the reference you give to polypoidy in the Amer. Nat.. The number in question has been removed from our library but I have taken steps to get hold of it.

Blackman called here today and he said that Waddington had acted in an entirely unofficial capacity in writing to you about the B.W.C.. Blackman, in fact, appeared to deplore Waddington's action and he added that the whole matter stands, as far as he is concerned, where it did when we spoke to him at Cambridge. He is taking our proposals up with his executive committee and, I believe, also with Salisbury's committee. He is also sounding the Linnean about their reactions to our proposals. So he seems definitely to be on a different line than Waddington. From his general manner I gathered that we shall get a very sympathetic hearing if he can manage it.

Oliver and Boyd have consented in a letter to me, to my using the tables from "Statistical Tables", provided that you and Yates agree. Yates has also agreed in a letter I received a few days ago. I have written to Oliver and Boyd to say that Yates has sent his agreement and that you have agreed in conversation, as, when I came to look into the matter, your letter concerned the use of tables from "Statistical Methods", which does not contain the variance ratio. I hope that you have no objection to this statement of mine to Oliver and Boyd.

Yours sincerely,

K. Clatter

P.S. I have just rescued the Amer. Nat. and looked at Warmke's discussion. He seems to consider that because in Polyploids recessives may be longer in becoming homozygous from certain heterozygous types, that hence they will be subject to less rigorous selection. This seems to me to confuse two very different things, the segregations observed and the "fitness" of the segregants, so I am very doubtful about his whole argument. He slides very easily over the question of mutation as a means of soring out "sheltered" genes, and I really doubt whether he understands the importance of this action.