

23rd May, 1957.

My dear Henry,

Very many thanks for the 6th edition of your book "Mendelism and Evolution", which I have always so much admired. What you say in the preface is exceedingly kind, and I can assure you I very greatly appreciate it. I hope the reprint that Dover Publications are making of "The Genetical Theory" may be available before the Michaelmas Term.

I was so glad to see your last chapter on rapid evolution, and that while dismissing Wright's theory of drift, you point to the advantage which small isolated populations enjoy in rapid evolution towards local adaptations. I think it is most important that you have made this distinction so clearly, for students influenced by Wright seem invariably to confuse them, and indeed to confuse a number of other distinct factors *by changing their ground in argument.*

You will not mind if I make an annotation on a remark, presumably quoted from Darlington, on page 91, namely "The tetraploids themselves are of reduced fertility, owing to the abnormalities in chromosome-pairing to which they are subject". This of

course is true of some cases, or at least of some artificially manufactured tetraploids. It is far from true of the wild tetrasomic plant Lythrum salicaria, which I have worked with these many years, and on which a healthy plant sets something like a million seeds. As to abnormality of chromosome-pairing, there is room for differences of opinion about terminology, for Darlington I think shared the early prejudice, common among cytologists, to the effect that quadrivalent formation in itself constituted an abnormality. Of course, occurring in a diploid that is what it is, probably indicative of a translocation, but now that tetrasomic inheritance is somewhat understood and known to occur in such ^{common} wild plants as Lythrum salicaria and Lotus corniculatus, quadrivalent formation in them should be regarded as just as normal as pairing is in disomic material, for it is merely the pairing of homologous elements which exist in four different chromosomes.

The other class of tetraploids which comes to mind in regard to the generalization I am criticizing, is the amphi-diploids, like the American cultivated cottons, where pairing is apparently constant and regular, if Skovsted is to be believed, and inheritance is strictly disomic, while the frequent occurrence of duplicate factors confirms the ^{testimony of the} actual number of chromosomes counted in attesting their tetraploid origin.

As a critic of the over-confidence constantly shown by cytological workers, I have been amused, as perhaps you have been, with the progress of research on the number of human chromosomes. You may recall that 20 or 30 years ago diverse workers reported numbers, of which Painter's value of 48 survived longest in the literature, but others, especially odd numbers like 47, were influential in generating the theory, now known to be erroneous, that the human germ plasm lacked a Y. This theory is, I suppose, the basis of the criticisms which Penrose has been for years accustomed to put forward of the various pedigrees of Y-linked inheritance in man, which, from the nature of each family tree, is incapable of being verified by reference to other families, though a fair number have been reported with various traits.

Perhaps you have seen the latest contribution from a Japanese writer in the Proceedings of the National Academy of Sciences (Vol.43, No.3), in which he finds a number of Japanese with 48, a good many others with 46, as in the case of Tjio's Swedish material, and, curiously I think, only one with 47, so that it looks as though there was an unessential ancillary pair knocking about in some populations.

However, the really important point is that what I have long suspected is now undeniable, namely that many cytological

observations confidently reported are in fact worthless as scientific evidence.

When shall I see you again?

Sincerely yours,

P.S. You might care to see the enclosed which I am giving in Stockholm in August at the Int. Stat. Inst. Conference, where Luca Cavalli has organized a session on quantitative genetics. I hope what I say about Americans may do more good than harm; it will surely both shock and surprise some of them. Please let me have it back.