24 June 1932.

Dr. E.M. East,
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Dear Dr. East:

I see that an ambiguity of wording in my letter of 17 May 1932, has misled you. I had asked of the apparently homozygous M_1/M_1 plant "whether it is supported at all substantially by the behaviour of other plants". By the last word I had meant individuals, but you read it as species, for you tell me (May 28) that you did not mean to suggest that there were other species of plants which behave in the manner of Lythrum. But, I was concerned with this, one plant thoroughly tested gave evidence of being a viable homozygote, ~~hence~~ have other plants less thoroughly tested, yet been tested sufficiently thoroughly in the aggregate to show that they also, or some of them, are homozygotes? Otherwise it is still possible and not at all far-fetched to suppose that crossing-over was ~~initiated~~ inhibited in one special individual, and that the mid factors are really lethal, in accordance with your previous findings.

I realise that the evidence for lethality always lay chiefly in the absence of such homozygotes as you now report, and that your interpretation, at least of Mrs. Barlow's data is on the whole better without it than with it. I have, however, still difficulty with Table 10 of your 1927 paper.

The shorts used in all the matings of Table 10 must be heterozygous for mid since their short parents SA, SC and SM seem all to have been tested and found free from M; at least I suppose these are the same plants which appear as the pollen parents of the progenies shown in Table V. They might conceivably be double heterozygotes in coupling though this is difficult to claim for the ♂ parents of progenies 12, 13 and 1308, since these received their mid genes from MA, which was tested in matings 1208, to 1216 of Table IX, and behaves there as a single heterozygote. The mid-parents used in Table X are also all heterozygous for mid, MA being used four times, and the other five coming from a long parent and the mid D 18, which has thrown several long offspring in crosses with mids, Table IV, and must therefore be a double heterozygote in repulsion. These 9 mids, therefore, are all by their origin or other tests apparently single heterozygotes, though by cross-overs some might be double heterozygotes in coupling. I cannot therefore understand

your suggestion on p. 413 that in the first four crosses the shorts might be 'homozygous' (either in the sense of true homozygotes or of double heterozygotes in repulsion) and that in the next five crosses both the mids and the shorts might be 'homozygous'. Unless I have misunderstood your notation the most that could be assumed would be that all the parents of Table X were double heterozygotes in coupling and this would give an expectation of 20.25 per cent. longs out of the total of longs and mids, against 13.2 per cent. observed. The deviation even from this theory is near the verge of significance and it depends on the wild plant MA being of the rather rare doubly heterozygous coupling type, of which its other matings give no indication.

I know that nothing can be more troublesome than to be bothered about old published work, especially as you are not continuing your researches on this species. Cases of polymorphism, however, seem to me of such importance for the application of genetic principles to evolutionary theory, that I am unwilling to give up the hope that I have in some way misunderstood the notation you have used, and that your observations are really consistent with the theory put forward, or with some modification of it.

I ought to say that the frequencies for stable equilibrium sent with my previous letter are those appropriate to the

1927 theory with two lethals. I made the further convention which seemed the most reasonable of those open to me, that all types produced equal numbers of viable seed, so that the lethal zygotes are regarded as replaced by an equal number of offspring from the same mother plant, though not necessarily from the same pollen parent. The lethal zygotes however are not very numerous, and I do not think it would make a great difference if one assumed the alternative that the lethal zygotes were not replaced at all, or thirdly that they were replaced by other offspring of the same parentage both on the mother's and the father's side.

The calculations were fairly laborious and I think the case of non-lethal mid factors, though it involves more genotypes, could be worked out more easily. If you should have a use for this I should be glad to see what could be done with it.

I am a little troubled by the fact that unless they are lethals there seems to be no need, from an evolutionary standpoint for duplicating the M factors. But obviously one cannot hope to understand the evolution of the system until its genetical structure is clear.

Yours sincerely,