

15 December 1930.

Professor E.B. Poulton, F.R.S.,  
Wykeham House,  
OXFORD.

Dear Professor Poulton,

I have just received from Oliver & Boyd a complimentary copy of C.B. Williams's book on "The Migration of Butterflies". It seems to be a magnificent collection of facts and the author has had the patience to tabulate all that can be gleaned from very numerous and necessarily very incomplete observations. I should never have had the patience to do this without having a theory to test, and it is, in this sense, all the more to Williams's credit that he has done so much, apparently without this stimulus.

I must confess, though, that I found his theoretical discussions rather slight and inconsequent. Perhaps this is too severe a criticism on a work which is primarily a compilation of facts, by an author who evidently feels that very much more abundant observations are needed before we can hope to read any more sense into them.

If you are reading the book or have recently read it perhaps you would give me your opinion as to the argument he uses on p. 412 against migration away from seasonally uninhabitable areas. There may be some point in his argument which I have missed, but, as he says that the view frequently put forward as an explanation, I cannot help thinking that he ought to have put forward his objections more carefully and at greater length.

With respect to the Painted Lady, (it is certain that (p. 420) "after one or two generations in Europe the species dies out during the winter"? ~~And~~ if it usually disappears completely after a short time in Northern Europe, is this true also of Mediterranean countries and of Mediterranean Africa? I put the point, because, not being an entomologist,

I do not know on what evidence one could be sure, in any district, that indigenous broods were not appearing every year, if migrants were also fairly frequent; and the existence of a zone in which indigenous broods are reared regularly, though reinforced irregularly by mass migrations from the south, would seem to alter wholly the theoretical aspect of the problem. For, outside the tropics, the phototropic stimuli acting on an individual during its lifetime must be unbalanced, and it would not be unreasonable to conjecture that from this cause all insects much influenced by light would tend to lay, on the average, further to the South, (or North) than the point at which they emerge. If in any insect a strong phototropic tendency, which might be valuable to it in quite other ways, were combined with swift and powerful flight, the aggregate Southward drift of the species might, I suppose, be very considerable, without attracting attention, and such a tendency would both render the instinct for occasional Northward mass migrations beneficial to the participants in them, and supply the indigenous stocks of the Southern swarm centres with the germ-plasm derived from previous migrants.

This suggestion, which perhaps is not new to you, does not imply that in years of great abundance the migrating swarms should not occasionally overshoot the mark and appear as temporary immigrants in regions where they are bound actually to die out, and not merely which they can breed in and vacate singly. But it does imply a zone in which the larvae can thrive, and in only the Northern edge of which wintering is impossible, and throughout which individual movement has a Southward bias.

Naturally I should be tremendously interested to hear whether you consider this line of thought should be worth pursuing, or if any serious difficulties to it occur to you.

I must apologise for again writing so soon, but the problem has the fascination of novelty to me, and who else should I write to if my head is full of butterfly problems?

Yours sincerely,

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Professor E.B. Poulton, F.R.S.,  
Wykeham House,  
OXFORD.

Dear Professor Poulton,

Some time ago Prof. Boycott asked me if I could possibly take over the extensive stocks of the snail Helix nemoralis, which Stelfox had been sorting out in Ireland during several past years. I was a good deal attracted by the proposition, since it was already clear to me that the polymorphic snails might provide critical material for extending the conclusions as to Mendelising polymorphism which I had found from the dominance phenomena in some of the polymorphic butterflies, and in the grouse locusts.

My main difficulty was that the large number of cages required to maintain these stocks, or to breed adequate numbers of offspring to test viability would entail more time than I could spare, to do justice to them. It is, however, possible that I could obtain the help of one of the laboratory assistants here, to carry out the routine work, say a half day weekly, for quite a moderate charge, and that with this help I could myself find the time needed for the purely genetical work of classifying, mating, preserving the material, etc.

I believe I could meet the running expenses if I could raise

a Grant of £20 a year, and I should then be willing to meet the cost of cages and fitting up their accommodation. I wonder if this work would be of sufficient interest to the Oxford Evolution Fund, to be thought worth such a grant.

I enclose Professor Boycott's last letter, from which I gather that Stelfox is not now in urgent need of a new home for his stocks, but would be willing to supply material suitable for the determination of relative viability of the different genotypes.

The theory of the experimental determination I have in view is given in my recent paper on polymorphism in the American Naturalist. It was followed by a very valuable paper by E.B. Ford. If as I suspect, and <sup>as</sup> has been shown for the grouse locusts, there is a definite deficiency in viability of the homozygous dominants, it will prove that in Nature the dominant patterns must be advantageous, in order to maintain the equilibrium of the frequencies, and in relation to the frequencies observed in Nature, it would supply, what at present we wholly lack, a measure of the net intensity of the selection in its favour, in the complete ensemble of the selective actions to which the animals are exposed in Nature.

The case of Papilio polytes in which the non-mimetic <sup>male</sup> ~~male~~ like female is recessive, is strongly suggestive of the same phenomenon, and in this case the advantage of the dominant

colouration had already been inferred by students of mimicry before it was known to be dominant.

Yours sincerely,