

DEPARTMENT OF

ZOOLOGY AND COMPARATIVE ANATOMY,

UNIVERSITY MUSEUM, OXFORD.

April 1st. 1939.

My dear Fisher,

How very kind of you to have made an analysis yourself. It is beautifully clear, and I need hardly say how interested I am in it.

As rather a long time had passed with nothing from Stevens, and six weeks ago he promised his section within a fortnight, I sent him a letter to hurry him up. I hope he was not offended to have it pointed out that we must get it soon! ^{So his letter} ~~It~~ arrived on Friday. I sent it on to Dowdeswell the same day to go through, with a request that it should come back by return. I am now writing up the summary and short discussion, which I could not well do till I had the mathematical analysis (the rest, including map, tables &c, is complete). I will then send it to Stevens to see if he approves, and tell him to hand the whole over to you straight away.

I shall be most grateful if you will make any alteration you think fit, and/or point out anything to me which you think I ought to rewrite. At the same time I will return your analysis to you which, by then, I will have digested. Now that yours is available, that of Stevens ought to take second place. Will you do what seems right in this matter, may I leave it to you?

Another point: there is a piece of research which I rather feel I ought to do, and should be so glad of your opinion.

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It relates to melanism in moths. Feeling dissatisfied with all current explanations of its spread in industrial areas, I have maintained that this is due to a greater viability conferred by those of the melanin-producing genes which have spread, and that they have failed to do so in ordinary circumstances owing to the loss of cryptic pattern - which matters outside, but not inside, such districts (for details, see Biol. Rev., 12, 481-9).

In several species the difference in viability concerned is so great that it has struck a number of breeders independently who were not on the look out for it. Now this ought to be put on a more exact basis, I think. Unlike most differential viability concerned in evolution, the difference here appears to be so great that it ought to be clear in an ordinary series of experiments. One should breed melanics and non-melanics (a) in good conditions and (b) in competition in bad ones - e.g. insufficient food - on a careful basis.

No fully recessive melanics have ever spread, and many of these are clearly of low viability. So I believe that those used for this work ought to be heterozygotes. Even yet, the homozygotes will not have had much chance for improvement in nature. Having felt this all along, I was excited by the conclusions of Brett (1937: J. Genet., 34, 307-23), and astonished that he does not seem to see their implication. He studied the dark forms of Hemerophila abruptaria, which is becoming fairly common (only) in the London area. This is a simple dominant (& ^{Here is a rarer dark form close to} probable modifiers). He finds 'no difference'

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between the viability of the heterozygous melanics and the normal form, but the homozygous melanics he found less viable (though not visibly distinct from the heterozygotes).

Doubtless a considerable difference in viability could exist without showing up obviously in his experiments. Since the dark form has spread, it has some advantage: as it is, polymorphism/ should be established;-but will the homozygote improve?

However, do you think viability studies on melanics would be worth while?

Again, many thanks for all you have done re the Scilly Isles.

Yours sincerely,

E. B. Ford -