

6th. May, 1930.

Prof. J.S. Huxley,
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Strand, W.C. 2.

My dear Huxley,

I am extremely glad that you think well of my book, and want to thank you especially for writing so quickly and kindly about it. The importance which you and Haldane attach to it - and there are no two opinions in this country to which I would attach more weight - gives me much pleasure, but not a little embarrassment, for if I had had so large an aim as to write an important book on Evolution, I should have had to attempt an account of very much work about which I am not really qualified to give a useful opinion. As it is there is surprisingly little in the whole book that would not stand if the world had been created in 4004 B.C., and my primary job is to try to give an account of what Natural Selection must be doing, even if it had never done anything of much account until now. It struck me there was a great deal untouched in this line of country besides much confusion due to past neglect to be cleared up.

As you have seen I have often been tempted beyond these austere limitations and, judging from your letter, I shall be still more tempted in future. I should love to talk over sexual selection in relation to monogamous territory birds, some time when we can get together. Will you be saying anything about it in your broadcast lectures? You must tell me when we meet if you are with me as to the origin of sexual preference, and as to the very sweeping argument of the first chapter.

About hormones from different species there is a point I specially want to ask you about. I understand that there is a good deal in common chemically, just as there is in the chemical composition of the bones or the milk, but does it go further than this? Or rather is the evidence strong enough to say that it goes any further?

After I had read Nicholson's paper on Mimicry Poulton sent me an early one of his, I think from 'Bedrock', developing a very similar standpoint, but I believe that all of Natural Selection that matters is intra-specific rather than inter-specific, at least I think that all that I deal with in Chapter II should be classed as intra-specific.

One thing I much regret is not mentioning Haldane's work in the preface as an example of the mathematical ground-work in biological problems which seems to me so much needed. Perhaps I should have mentioned Bernstein in the same place.

Many thanks for your other points and for reminding me to stir up Jourdain again.

Yours sincerely,