2 May 1931.

Major Leonard Darwin, Sc.D.,
Cripps's Corner,
FOREST ROW,
Sussex.

Dear Major Darwin,

I wouldn't be sorry to publish something of the kind you suggest, and Science Progress would be a distinctly good place, but I did start writing with a view to repairing what is something like an omission from my book. The trouble is, as you see, that while I look, earnestly and diligently enough, for objections to the theory of Natural Selection the opponents of that theory always fob me off with objections to Lamarckism or to evolution in general, so that I can really find scarcely anything that has any logical place in my book, in spite of the mass of anti-selectionist literature.

I do think, by the way, that you must be reading too much into my first chapter. Your letter reminded me of something that C.G. said in his review, to the effect that my argument, had it been brought forward before Mendelism was known, would have disproved selection theory. Now I don't think this is true at all, and as I must have been

responsible for the misapprehension, it is up to me to track it to its lair. Tell me if you think I am wrong, but I do not imagine that your father would have found anything new or interesting in my argument, except where I bring in facts which he did not know and had not guessed, and their consequences to an argument with which, without these facts, he would have felt perfectly familiar. The consequences of blending which I emphasise do not stop natural selection from working, for, with sufficiently high mutation rates, a supply of heritable variation can be maintained, and I think I could answer your challenge on this point sufficiently to show that your father did believe in these enormously high mutation rates, (almost every individual a multiple mutant) in man, and in the domesticated animals and plants; but that in the case of wild animals he long kept his mind open to the possibility that they might be for long periods practically invariable and only be made to vary occasionally by changes in their environment. What his final view was on this I do not know, but I have no doubt that after 1860 Wallace did something to persuade him that wild populations were not so constant as he would have been ready, formerly, to admit. Wallace could have produced evidence only of somatic variability and could not have proved that it was heritable.

There seems to me nothing whatever illogical in the theory that heritable variability is maintained by such very frequent mutation, and so the material provided for Natural Selection to work upon, but this ~~view~~ theory does open the door to the view that the particular causes to which each mutation must be due (as your father frequently insists) might be also important causes of evolutionary change. And though your father could find, I think, very little observational evidence that this was so, against much in favour of the efficacy of selection, yet this possibility he steadfastly kept open. This strengthens my confidence that he had a perfectly clear grasp of the argument I have set out in its essentials, though he might well have preferred other words, and that he was not merely the patient plodding accumulator of observations which one legend makes him out to be.

The case strikes me as remarkably similar to that of Carnot's principle which is the basis of all thermo-dynamic reasoning. For Carnot developed his theory in terms of the view that the quantity of heat remains unchanged, not knowing that it was quantitatively convertible with work. In bringing in the conservation of energy instead of the conservation of heat Clausius Wisely saW that the principle of

Carnot's reasoning was untouched, though it led to somewhat different consequences; for Carnot's reasoning was right on the observational facts known to him; and I am sorry that I should have let the point be missed that your father's reasoning seems to me to have been right, even where his premises were wrong.

On the second point you raise as to your father's theory of heredity, would you agree with me if I said that he would have welcomed a view of heredity which could have included reversion, but that, in the absence of such a view he was willing to accept the fact of reversion provisionally perhaps, as a principle independent of heredity and possibly due to some entirely distinct mechanism?

Thanks for your point about the lower animals, I must bear it in mind. Certainly Berg seems to think it rather impudence for such a wretched creature as a free-swimming Tunicate to have such a fine "test".

To revert to my original purpose, if I can ever produce anything good enough to stand as a review of the difficulties and criticisms raised against selection theory, how do you think such a chapter would go in a German translation? It would be, in its nature, much more provocative, at least to reviewers in a hurry, than the rest of the book, and my wife has just made the scandalous suggestion of using it

as a preface, as this is the only part they are likely to read. I have just had a letter from a German anthropologist, working in America, who wants to translate the book into German, quite probably he will fail to find a publisher, but, while the project is under discussion, I should like to know how you felt as to adding something of the kind I have sent you, with perhaps some boldish speculations on the rapidity of adaptive variation in the formation of the great classes.

Yours sincerely,