

29 September 1931.

Major. L. Darwin, Sc.D.,
Cripps's Corner,
FOREST ROW,
SUSSEX.

Dear Major Darwin,

I don't think I ever remember having more letters from you than I cared to deal with, and certainly not now while I am still partly bedridden, though I have been promoted, and toddle round the room on a constitutional every afternoon.

About McDowell, he took the minutes at our meetings at Winchester and is, I think, chief science master at the College. He said he gave the senior boys a course in Eugenics in time allotted to Biology. I think he is quite earnestly anxious to help, though he often gets hold of the wrong end of the stick, about as often as not in any intricate argument. I believe I could review his book if you would like me to, without dereliction of other duties, in fact this is an exceptionally good opportunity.

I had never thought of the plants in quite that way, though I think I know the phenomenon your niece referred to.

So the analogy of dominance struck me as new and illuminating. If I have the point right you may have plants growing in a salt marsh with fleshy leaves and stunted growth, while others of the same species, growing in soil or fresh water, are tall and thin-leaved. The succulent habit of growth is characteristic also of whole species confined to salt situations, but when the succulent plants are transplanted and grown alongside fresh-water forms they revert to the fresh-water habit. Even closer still, Turesson has shown, with I forget what species, that some of the succulent plants are merely modified, while others, growing in the same site, are genetically succulent in that they and their offspring maintain the habit in culture. In this case the plants with halophytic genotypes serve to show that the habit really is adaptive, and enjoys a selective advantage, which is, however, gained by other members of the same species, by an adaptive modifiability. I take your point to be that the adaptive modifiability must have been acquired by the selection of modifying factors, which alter the differential reaction of the plant to its environment, without affecting its habit in fresh water, just as the dominance modifiers alter its reaction to a particular mutant gene, and that the selection in favour of these modifying factors is only effective in individuals containing the mutant gene, or growing in the

salt situations. This method of adaptation favours the cohesion of the species, whereas the alternative method adopted by plants which are thick leaved wherever they are grown, would tend to favour its fission; and if the salt marshes were very extensive, and largely isolated, might ultimately produce a distinct halophytic species. Let me know if I've got the point right as you only give me a hint in your letter.

I am glad that you like Ford's book and I am inclined to hand on your criticism anonymously, though I think the analogy he wishes to draw is a good one. Indeed it is much the same as the dominance analogy as expressed above. Still it is a pity to confuse the terminology, especially as we already have a confusion of the same kind when we use 'modifications' for environmentally induced differences, and then speak of modifying factors, meaning genetic factors having small or conditioned effects.

I had a letter from Ford by the same post as yours, in which he gives me an amusing account, though at second hand, of the population discussion in Section D. Huxley was good enough to speak in my place, giving an account of my views, and this seems to have stung MacBride into some wrathful allusions to the valuelessness of the opinions of mathematicians vis-a-vis biologists. At this there was applause.

Baker loudly shouted "Shame!" and there was applause from another section. Ford expresses great indignation against MacBride, the more so, I think, because MacBride evidently has some support, at least for this part of his views. I feel myself that it is very well worth while to be abused if it gets my book read and criticised.

By the way, I was very much surprised to hear from the Oxford Press that the Natural Selection book has sold even better than my one on Statistical Methods did. It was so long before I heard from them that I had quite made up my mind that it was one of those books which everybody praised and nobody read, and would have no influence on biological opinion. I think this is still its danger, but the sales must mean something.

I am glad you mentioned Blacker's book, because you will not want me to write a rival to it, but perhaps something supplementary, if this seems desirable later. You do not mention if you have formed any opinion respecting Mallet's circular on Eldon Moore, so I expect you do not want to be bothered with this question. Lord knows, we ought to be able to spare you some of the responsibility for the doings of the Society.

Do not hesitate a moment to write on any point which interests you, nor to refrain if the spirit does not

move you.

Yours sincerely,