

Aug 13. 34.

CRIPPS'S CORNER,

FOREST ROW,

SUSSEX.

Dear Fisher.

When I hear of a book which I think that I shall like to ponder over I sometimes buy it on spec. This I have done recently with Mc Dougall's Religion & The Sources of Life, and am on the whole considerably disappointed. Yet I am glad to have read it, and if you ever want to dip into it, I shall always be glad to send it. He is evidently most anxious to prove that Lamarckism is true, which tells against the validity of his evidence. The same

is true as to his attitude to
Spiritualism. On this latter subject, it
seems to me that a kind of natural
selection goes on which results in only
those without the power of weighing
evidence fairly continuing in this line
of investigation, or rather a tendency
in that direction. F. Galton, I believe,
gave up that study because he was
not allowed or able to apply strict
scientific tests. As to Lamarckism,
I do not think Mr. Dugall understands
what were my father's opinions. He
says, p. 178, in effect that my father's
acceptation of Lamarckism necessitated
a belief that mind has "played a
dominant role in biological evolution".
I think my father would have said
that that was true if the phases of-

Darwinism, which he regarded as
silly, were accepted, whilst he himself
would make no such assertion, not
really knowing what mind is. I
do not think Mr. Dugali has a
correct idea of the trend of modern
thought in regard to Evolution.

I have heard nothing for more
than a year of Ford's idea of
writing on Darwinism, which I
stirred up in him. I have written
him a note on the subject, but as
yet have had no answer. I wonder
what you are doing about the
Annals of Eugenics. That would be an
easy job.

Do not trouble to answer this,
unless you would like to see

Mr. Dugali's book -

Yours sincerely

L. Darwin

13 August 1934.

Major L. Darwin,
Cripps's Corner,
Forrest Row,
Sussex.

Dear Major Darwin,

I am enclosing an offprint of a recent, rather polemical note on dominance and, what I think will interest you more, a proof of a letter which is appearing in the American journal "Science", which is a kind of first fruits of the experiment with poultry which I have now been carrying on for more than five years. I think I may say already that in, at least, 5 cases out of 7, the so-called dominants used in my experiment have proved themselves to be no dominants at all, when bred into the wild stock, but, like the case of crest and cerebral hernia, mentioned in my letter, had heterozygotes clearly distinguishable from both pure bred types.

Looking at what had been previously written on the subject, I found, as you will see, a very beautiful example of the way in which the earlier geneticists, in this case Davenport and Punnett, threw over, on the weakest possible evidence, the views held by your father and other early writers.

I am inclined to write a preliminary report of the experiment this year for the Royal Society, although it

will need two more years to verify my conclusions experimentally, and I rather want to rub in the blunders of the first years of this century, when a good many geneticists eagerly believed that the study of heredity had just then, for the first time, become really scientific.

Yours sincerely,

your letter arrived as I was signing this. Yes, I should quite like to see McDougall's book. When convenient I should be glad to have the proof back.

I have got papers from J. B. S. Haldane, Penrose (of the Darwin Trust), Fraser Roberts (of the Bunden Trust), "Student" (Gosset of Dublin), besides some work from this Laboratory for the first number of the Annals. But contributors were tardy, & I shall not get it out till October.