

OXFORD
BB167.

5, APSLEY ROAD,
OXFORD.

Thursday

[Early March, 1942]

My dear Fisher,

I enclose the Gallton
Lecture, which I hope you will find
on the right lines. You know
that I shall be most grateful for
any alterations which you think it
right to make. I am not quite
certain if the official title of the
lecture is correct; or what I have
said about it in the first sentences
of the introduction will be just as you
would wish. Please adjust these
points if necessary. I take it that
a Summary is appropriate, and have

Supply me.

Unfortunately, though Williams' additional data has brought up his numbers a bit, the excess of Melanics does not yet reach significance. I think I must work at the point myself and try showing the law, which was successful in the industrial case. I think it important to show beyond doubt that the Melanics are actually the hardest in form in which they have never established themselves.

The paper as I sent it covers the 4 main subjects discussed ~~in~~ in the lecture: - Selection in Athanas

grossulariata, the chemistry of pigments
in certain insectic forms, the
distribution of flavones in the Pieridae
with reference to their classification,
my families of Boarmia expansata
demonstrating the superior viability of an
industrial melanics - and the general
discussion of this situation.

I find that, to cover these
in detail (and they seem worth putting
out so), I have occupied ^{my} full
10,000 words, and (I am afraid) a
little over. I trust not too much.
But I fitted into the lecture two other
subsidiary points (1). Families of
Staphora menchica in relation to the high

race. You may remember that (as
already shown by others) the situation
is one of a chief factor basis, but
the form has been subject to genetic
modification. I had thought it
interesting to get some idea how
many factors had been employed in
order to produce such a race. Owing
partly to the difficulty of the
indistinguishable ϕ 's, the results are as
yet rather inconclusive. However, I
could supply a short section, saying then
I said, if you think it worth while -
and if space allows!

The other subsidiary point -
(that of the curious Pieris napi families)
I raised rather in the hope that someone

might have an explanation, and to stimulate discussion. Personally I feel that in this instance, as no conclusion could be reached, and ^{as} the particular ^{work} was chiefly not my own, it is perhaps better omitted in a formal paper. You will see that I have so phrased the statement in the introduction as to allow for this.

As explained in the lecture, I used ~~the~~ ^{used} the fact-cross in breaking down selection in Abraxas. Perhaps I ought to point out that ~~probably~~ the matter of selected heterozygotes was the more appropriate in building it up, because of the greater variability in

in heterozygous expression: or is this
clear enough? Perhaps you would
judge on the point - (I have just
noticed I have not mentioned it).
With + selection one could be less
certain which are the extreme forms
in homozygous than in heterozygous
latic: with - selection, it would
be impossible to know which
are the extreme forms in homozygous
granularis.

If you can run to a
coloured plate, as you kindly
thought, it may be that the
firm who do the reproducing like

to have the painting made themselves
(as they do for the Trans. Inst.
Soc.): unless they do it direct;
without a painting. In either case
they would require the insects, which
I have ready. If they require a
painting, and don't want to do it-
themselves, I could get one done at-
once.

Thank you once again
for all your kindness in regard
to the Gallon Lecture. I do
greatly appreciate it.

At the moment I have

my father ill in bed in my
house. He came for a three
days visit and went down
with influenza and complications
(bronchitis &c). He makes some
difficulties in a bachelor establishment,
as you will be able to imagine!

With kind regards to Mrs. Fisher,

Yours sincerely,
E. B. Fryer.