

Radham College,
Oxford.

September 26th: 1931.

My dear Fisher,

I am very relieved to hear that your operation has been successful. That is the great thing; I had been thinking of you with considerable anxiety. But this period of inactivity after it is one of the worst things one can have to encounter, as I know well.

I was delighted by your account of the Brit. Prot.:, and glad to find I have a supporter in thinking that sort of thing an undiverted waste of time. To tell the truth when it met here some years ago I was in Oxford the whole

time and never once went to a meeting!
I did so now, and spoke, because
Poulton wanted me to, and I would
willingly waste more time than that
if it were any satisfaction to him.
However I have not stuck it out,
though I may go up to the general
meeting on Tuesday, though I shall
not speak.

As a summary of
Evolutionary knowledge the proceedings
on Thursday struck me as either
funny or pathetic, according to one's
mood. All of the other people
were only given 20 minutes each,
so as to leave plenty of scope for
Fairfield Osborne. I was cut
down to only 10 minutes, and was
not told so until I had begun

Talking; so you can imagine how
much opportunity there was of getting
anything across! Osborne himself
went on regardless of time and all
other considerations, and the meeting
closed with a short sermon by
D'Arcy Thompson on Conic sections.

x Such curious things
have been happening in the course of
Mrs. Sexton's Gammarus work at
Plymouth Mass. I am going down
there for three or four days next
week and to see for myself. Julian
will probably drive me down. With
due respect to everyone concerned,
the results obtained down there
are not more curious than are the
theories advanced to account for them.

x Briefly, as far as I can

made out from the M.S. of a new paper they have sent me, the points are these: -

Seven distinct mutant genes producing Red Eye of some shade have now turned up in their stocks; ~~one at least has~~ ^{several have} occurred twice. All are recessive to the normal black eye. They are difficult to distinguish from each other, and two are phenotypically inseparable. However all are genetically distinct. This they have established without doubt; on mating any two different red-eyed forms the F₁ are red-eye black. They don't appear to be linked - "with one doubtful exception" as they say (I have not seen their data on that point). But linkage experiments on "mimic" and almost

minic genes present obvious difficulties when the chromosomes are not already mapped!

Now here is the curious part. With one exception all of these have turned up in the F_2 generation from wild individuals brought into the laboratory. None have appeared in laboratory stocks of long standing. They have all appeared in such a way as to make it apparent that one of the original wild individuals was heterozygous. They appear only to have bred from 44 wild individuals (22 pairs found mated in the Open) in recent years, though from these they have obtained vast numbers of descendants. Of these 44 wild specimens, 11 were heterozygous for red eye factors of various kinds!

But though "large numbers" (whatever that may mean) have been examined in the open, no red eyed specimen has ever been found in the wild state. ~~at least~~ That is in the last few years. However we have more exact information in 1916. In that year over 21,000 wild specimens were brought to the laboratory and examined by Mr. Sexton for eye colour. Not one red-eyed individual was found - and she is immensely painstaking and careful you know.

This makes Mr. Sexton think that these (adult specimens) mutate when brought into the laboratory; i.e. that the change stimulates mutation, but the new conditions when maintained do not. His breeding shows that half the offspring of these wild pairs

are heterozygotes, I am afraid we cannot regard that as a very critical point of view.

Now for a start I want to examine large numbers in the open. But though many of these red eyed forms are ~~at~~ at little disadvantage in the laboratory, one cannot say that they are not in the open. However one could easily get over that by bringing in large numbers of ♀s with a nearly mature brood and examining the young at hatching. As a sample of 44 produce 11 heterozygotes, it ought not to be a very big job to spot some red-eyed young in this way. But something surely must have happened to the population since 1916! These 22 pairs were taken from different parts of the

locality and at different times during the last few years. They therefore seem a random sample.

Gammarus cleveauxi
you know occurs only in brackish water in one locality (Chelson Meadow) at Plymouth. It is land reclaimed from the sea, and subject to ^{partial} flooding by sea-water. All together the conditions make it a most isolated habitat, to which the cleveauxi population is completely restricted - the species is not known to occur elsewhere in the world.

They tell me that the numbers of the population fluctuate very greatly in nature throughout a single year. As there are only about 3 generations per year in the

to open, this must often mean a great increase per generation (perhaps 10 fold or more). I should like them to take samples of the population and test the genetic conditions at different levels of abundance, ^{just} at the end of a rapid increase and of a decline.

It is rather a pity that they have spent so much time down there in getting these exact numbers to prove their 3:1 ratios, many times greater than are necessary, rather than use some of it in attacking the other interesting problems there are.

If any other points occur to you to be settled I should be glad if you could let me have a few lines before I go down. When

I have been there I will let you know
if there really proves to be anything
of interest.

You say you are tired.
I am afraid you will be even worse
if you don't get through to the end
of this letter.

With best wishes for a quiet recovery,

Yours sincerely,
E. B. Ford

Letter N.º 2.

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ZOOLOGY AND COMPARATIVE ANATOMY,
UNIVERSITY MUSEUM, OXFORD.

Saturday evening.

My dear Fisher,

After writing to you this afternoon I saw John Baker, who told me what happened at the Brit. Ass-ⁿ. This morning. Had you been there yourself I should have remained, as it was I had returned to Oxford. Hurley it seems had read your paper for you; I did not know he was going to, otherwise I would have heard it.

At the end of the morning MacBride got up and said that the opinions of J. Fisher are of no value since he is not a trained Biologist, and a mathematician's ideas are of no importance on the problem of population! At this there was applause. Baker loudly shouted "Shame", and there was applause from another section.

That MacBride is a damned ignorant pig-headed fool, I know; I know also that he is quite unscrupulous. I did

not know however that he has not learned
how to behave. To use those words about
a man who is himself unable to be
present, seems pretty unpardonable to me
in any circumstances.

But we all know what
MacBride's mentality is. Unfortunately he
is just an extreme type of a whole class
of Biologists with neither the inclination or
the capacity for critical thought.

I wish I had been there;
I feel pretty furious.

The very best of luck to you,
Ever yours,

J. B. Ford
