April 13 19

12 Egerton Place
S.W. 3.

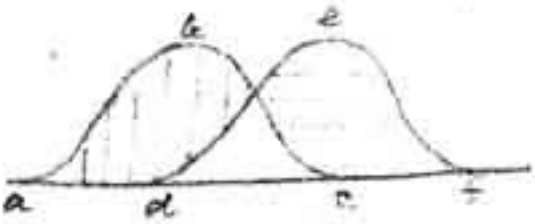
Dear Fisher,

Thanks for reading and returning my paper. I have not yet fully digested your remarks, but we evidently do not see eye to eye. This may be want of clearness on my part, or muddling, or both.

I think we are using the words in the same sense, but am not quite sure. By a mutation I mean a change in the gametes from one generation to the next. You say "are not mutations essentially centrifugal?". Certainly not in my sense. But here my words may not be happily chosen. My postulated mutations always take place in pairs. Now if both gametes

become more like each other on Segregation, and if this proce proceeds indefinitely, surely I am right in saying that in time the whole of the gametes will become identical, and will be like the mean gamete before this process began. Hence I call this kind of mutation centripetal. In one sense no doubt one of the two mutations will be centrifugal and the other centripetal. I do not see why a random mutation adds to the variance necessarily. Take a tall man, and if a true mutation occurs such that his descendants are shorter than they would have been without such a mutation, and the variance of the group will be diminished.

I cannot follow your argument about the leaf. A species consists of a number of different genotypes. How can it affect the ease with which one genotype reproduces its kind if another genotype is more or less different from it? It would be no more difficult for an elm to produce two kinds of leaves from two genotypes than it is for an elm and an oak to produce two kinds. When we know, as we do sometimes, that in the past a whole series of forms between the two has existed in the past, why have the intermediate forms died out? In this respect the leaf is a good example, as it is hard to think it is [illegible] of any consequence.



A species consists of a number of genotypes. Measured by any character these are normally distributed about a mean. That is, to say the characters would be so distributed, a b c, in a uniform Environment. When Evolution is taking place, I imagine that a new group of genotypes, d e f, is formed. But why does natural selection cutting off the part shaded /// necessitate the adaption of the part shaded = ? Why do we see Sometimes

get the new forms added without the distinction of the old, in a character like leaf-shape? Or the old forms selected out, without new forms added, and a consequently a greatly restricted range of differentiation? Perhaps we do, but I believe not. Bateson agreed that the sweeping up of species into heaps was an outstanding puzzle, and I think my Father felt the same.

The series of allelomorphs you give from Wilson is not the kind I had in mind, and I ought to have made this

clear. I was thinking of forms in nature and such a series rarely occurs in nature. My allelomorphs differed but little, and not widely as in this instance. Perhaps I ought to have postulated that the differences between the characters are correlated between both the differences between the allelomorphs. You say "is it not probable that there are really more than 5 "[allelomorph]" perhaps a large number divided into five classes". Here you are, I think, nearer what I am aiming at: In nature we might have a "light dun" species, and that-

would have a series of multiple allelomorphs differing but slightly from each other in regard to colour.

You say accepting Wilson's scheme "which implies that colour mutations of the kind do not take place". But such a scheme does not negative the possibility of slow and slight changes in any one of the five primary colours.

The heaping up of species, or the disappearance of intermediate forms, and the creation of new genotypes in the direction in which selection

is act of seem li me primary
puzzles not get faced, and
what I have been trying li
do is li face them.

I hope li get away on
a holiday next week, which I
need. I shall put this all out
of my head for a bit. I
will look at your letter and
write when I get back.

Yours sincerely

L Dawson

April 13 1919

Dear Fisher.

I return your paper which I have read with interest, such parts as I can understand. Don't let Mac Bride frighten you from trying to publish. His criticism to me was that the numbers were too small to found a theory on. That point can, I think, be met with the degree of confidence with which the conclusions are stated. I see no harm in suggesting a theory on a basis

inadequate for anything like
proof. I should think the numbers
are sufficient to show that it
is highly improbable that there
are 2 distinct categories of
twins. But for the 2 spermatozoa,
that can only be suggested very
tentatively. By the by, Someone
has been writing on twin calves,
called by the strange name of
free martins, I think. Have you
seen it? It might have a bearing
on your work. Bateson told me
about it. I don't see why a sperm

Cannot split into 2 identical spermatazoa just as well as an ovum. Could not your results be due to 2 identical spermatazoa fertilizing 2 distinct ova? It is always said that twins of different sex dont resemble each other at all. You dont discuss that point. The numbers are very few, but it would be interesting if possible to know the truth. People see what they look for. Hence people see likeness between twins. But when people are struck with the dissimilarity between twins, they only look for differences. Hence the general appreciation of the

differences of twins is not a test of their real differences, as it exaggerates the similarity at one end and the dissimilarity at the other. I wonder if this is the psychological explanation of the idea of 2 classes of twins.

I am writing separately about my own paper received last night.

Yours sincerely,

C. Darwin

107·89 | 114·13 | 97·13
78 | 78 | 78
298 | 361 | 191
234 | 338 | 156
648 | 493 | 353
624 | 468 | 312
9145 | 96·25 | 98·81
78 | 78 | 78
134 | 187 | 238
78 | 156 | 234
565 | 314 | 441
546 | 312 | 392
19 | 2 | 49

1·383
1·39 | 1·463
1·245 | 1·245
1·173 | 1·172
1·240 | 1·240
~~1·243~~ | 1·305
7·733 | 7·809
1·289 | 1·301