

UNIVERSITY COLLEGE OF RHODESIA AND NYASALAND

ZOOLOGY DEPARTMENT

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Mr. G. Davidson,
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Gower Street,
LONDON W.C.1.

Dear Mr. Davidson,

Thank you for your last two letters.

With regard to the first, I must point out that you asked me to comment on your manuscript. This I did. Naturally I did not expect you to accept my views unless you were convinced that they were correct. My last sentence, I feel, makes this plain. For these reasons I feel that your second sentence was a little unfair.

In any case, I yield to no one in my admiration for Ernst Mayr. My difference with him is on a small point only. If you take the trouble to test my "dogmatic" views against Mayr's writings, I feel quite confident that you will find no clash. I should be delighted if all workers in this field were to follow the writings of Mayr.

Am I so dogmatic? I have always argued the case for each of my views, and I am certainly prepared to change them if stronger and more convincing evidence is presented for alternative views. However, up to now, the evidence appears to me to be for the conventional population genetical and evolutionary stand of workers such as Mayr, Dobzhansky and Cain.

I was very interested in Coz's results with A & B sympatric populations. They were so remarkable that I decided to look at the situation at Chirundu where, as you know, spp. A, B & C occur. So far I have examined the offspring of 29 females caught in huts. All the males dissected were normal as judged, in each case, by the general appearance of the internal genitalia and the detection of sperm in the vasa deferentia. I shall soon have the results of a much larger batch.

I shall look forward to seeing Coz's paper on evidence for hybridization between melas and "FW A. gambiae". Such evidence could be convincing. An earlier paper on the same subject left too many loopholes to be satisfactory.

Of course I have noted the distribution of genes determining dieldrin resistance in Africa. I have not yet attempted to explain them as I cannot do so on the available evidence. I am sure that the situation will be understood when we get more information.

I shall send you eggs of sp. C as soon as I am able to. My new colony is not yet established. I go to Uzumba only occasionally and then for short periods as I collect larvae with which to attempt colonization.

Yes, I have come across Spielman's work. I am very interested in the work on the pipiens complex which I consider more difficult to interpret than that on any other complex known to me. One thing I am sure of is that the situation in N. America is very different from that in Africa.

Yours sincerely,

H.E. Paterson.

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