H.E. Paterson Esq.,
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and Nyasaland,
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12th February, 1964.

Dear Paterson,

I don't think a little discussion on theoretical aspects of speciation (which you started, not I) will do anyone any harm. Am I not entitled to express my views or have I to accept the word of Paterson as the last on the subject, even to ignoring people like Ernst Mayr (in his referencr to semi-species)? As to confusing the field worker, I am quite convinced that the latter will never make use of the knowledge of the existence of a complex of species until he has the tools to recognise the members of the complex, namely morphological characters.

It seems to me that there are so many gaps in our knowledge of this A. gambiae complex that it is too soon to be as dogmatic as you are. I would prefer to keep a more open mind on the subject and await further facts; particularly with regard to the status of the A and B forms. These seem to be so intimately mixed, and both have been recorded from areas of holoendemic malaria, that differences in their vectorial capacity can only be slight. Apparent changes in the predominance of one or the other forms has occurred over the years in Upper Volta and Western Sokoto, for example. Whether this is a real change or due to chance, remembering that our colonies are usually

derived from very few mothers, remains to be seen. As to concrete evidence of significant hybridization would you not accept all (not just a few) the male offspring of a wild-caught female being sterile as good evidence? Three of Coz's thirty-eight females showed this, eleven showed two sterile, and one fertile, and five showed one sterile and two fertile males, though admittedly only three males were dissected from each female's family. I have now asked Coz to send me pickled testes for confirmation of this sterility.

Incidentally, Coz also has evidence of hybridization of melas and fresh-water gambiae (probably group A) near Sassandra, Ivory Coast. He has found larvae in this area with teeth on the pecten intermediate between those of gambiae and those of melas. I have just been hybridising Sassandra gambiae and Liberian melas for him here so that he can confirm his field finding.

Hybridisation of A and B is easily achieved in cages in the laboratory here. Just recently I put 50 males and females of an A strain from Man, Ivory Coast in a one-foot-cube cage with 50 males and females of a B strain from Kano, Northern Nigeria. They were left together for six days during which time they were given two feeds. After the second feed most of the females still alive were set up individually in tubes for egg-laying. ~~Some~~ 44 families were reared separately, and in just over half of them (55%) the males were sterile, indicating almost completely random mating. I know you will immediately say that such results acquired under such artificial conditions are meaningless, but are they? Some hybridization you would expect between fully reproductively isolated forms under such cage conditions, but surely not 55%. In sharp contrast to the case of hybridization of A and B forms in the laboratory, is the distinct difficulty in mass-mating either of these with either of the salt-water forms or mass-mating the two salt-water forms. As fro the C form, well we have never succeeded in mating anything with this form other than by the artificial method.

One final point on this A and B issue: if they have a single origin how do you account for the fact that resistance in both A and B forms is confined to West Africa, or has this no relevance?

I don't see your objection to Mayr's latest definition of a semi-species. To me the lack of complete reproductive incompatability after geographical isolation seems entirely feasible and dependent to a large extent on the period of time elapsing since geographical isolation and the meeting up again of the divergent forms. Until proven otherwise, I am quite prepared to believe that significant hybridization even with its attendant wastage due to sterility, occurs between A and B and perhaps even between salt-water and fresh-water forms at certain times of the year.

You may be pleased to hear that I have managed to change my E to C to conform with you, and have given artificial symbols to the East African salt-water form and to A. melas in my tables, but have referred to them by their full names in the text.

I am hoping to get to Rome in September but do not know for certain yet.

Yours sincerely,

G. Davidson,
Reader in Entomology
as Applied to Malaria.