

# UNIVERSITY COLLEGE OF RHODESIA AND NYASALAND

## ZOOLOGY DEPARTMENT

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16th June 1964

Dear Mr. Davidson,

Thank you for your letter dated 14th May.

I should be pleased to send the information which which you suggest. I think the best thing to do is for me to send you the paper which I shall read in Rome, together with any more detail which I think might be useful. Would it do if the information <sup>is</sup> ~~was~~ in your hands by the end of August? This would insure that it <sup>is</sup> ~~was~~ as up to date as possible. I could let you have the Rome MS before that with a few notes at the end of August. I do have photographs of most of the hybrids of A, B, C & merus; of the testes etc, that is. I also have photographs of normal testes and vasa deferentia, stained, unstained and under phase contrast. These were taken to demonstrate my criteria for fertility when searching for sterile males in wild populations. I also have a certain number of photographs of chromosomes, both mitotic and polytene but it is still early days with this work and it is probably not too useful to mention it yet. In any case Mason may have more material of his own. I shall be mentioning the work which I have done at Chirundu on searching for females which have mated non-assortatively. Should I send this to Coz or to you? (As a matter of interest I have now examined the offspring of 116 females caught at Chirundu without finding any male offspring which could be described as sterile. I have also showed A, B, & C to be still present at Chirundu).

With regard to your proposed statement on the status of the forms, I find myself in a difficult position. Your statement starts "On present evidence...". As you know I have argued on this same evidence that the forms should be regarded as separate species on the basis of criteria of workers including Mayr, Cain & Mattingly. Mayr himself agrees that melas is a species (he has not commented on the situation since the other forms were revealed). If I accept the statement which you propose I must retract from my argued position, which I find it difficult to do as I have now even more evidence in its favour. I realize the difficulty fully. I therefore think that the best that can be done is to state your own position and to add a note that your opinion is not universally accepted.

The distribution list is <sup>almost</sup> complete as far as my records go, but contains a few errors I think. My record of B from Chirundu is left out, also B from Tinonganine. Is the B record from Uzumba yours? I have not found it there. I have not found A at Hippo Valley (See footnote below the caption of the second map in the paper in S.A J.med.Sci.). I have recorded spC from Swaziland and two other localities <sup>along</sup> the Lundi R. of S.R. A.merus I have recorded from Ndumu, Natal & de Meillon

recorded it (or rather supplied the evidence for believing that it was present) at St Lucia, Natal (In his book). In Iyengar's paper he provides evidence from de Meillon for its presence in Mocambique (I forget the name of the place & I don't have the paper with me). These records are as reliable as that from Aldabra. Mattingly in his WHO/MAL on his African trip mentions merus from Comores I think. I think you mentioned melas from the Ivory Coast when referring to Coz's work. There is also a record from Guinea (Konakri). I was very interested to see that you have recorded melas from Leopoldville.

Perhaps it would be a good idea to differentiate between genetic and other identifications for these distribution records?

I shall look forward to receiving a copy of your London communication in due course.

With kind regards,

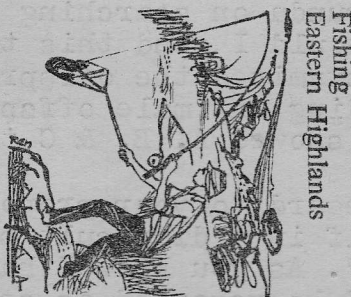
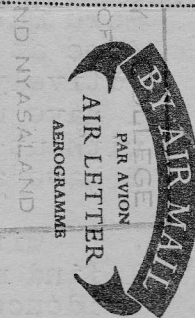
Yours sincerely,

*H.E. Paterson*

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\* Sassandra, Ivory Coast

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