

# UNIVERSITY COLLEGE OF RHODESIA AND NYASALAND

## ZOOLOGY DEPARTMENT

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Professor E. B. Edney

5th January 1964.

Dear Mr. Davidson,

Many thanks for your letter and MS which reached me in the middle of my recent move to Salisbury. My apologies, in any case, for the delay in answering it.

I am certainly very pleased that we are in such excellent agreement on most points. I can't help thinking that it should be possible to narrow down the points of difference even further. In the hope of helping to achieve this I have discussed several points in some detail below.

Perhaps I could start by explaining why I am so unenthusiastic about the possibility of A and/or B having multiple origins. As I pointed out on pages 40-41 of my recent paper, sympatric speciation is enormously improbable and for it to occur independantly more than once is astronomically remote. From your paper I gather that you now agree with this point, but you substitute the suggestion that sp.A (or sp. B) could have evolved independantly on several occasions by geographical speciation. I feel that the improbabilities involved in the arrival at a single species by convergent evolution on several occasions are just as great as in the case I dealt with and, I am sure you will not easily find an evolutionist who will support you in this view. It seems to me that you are trying to argue from the position that mating type is determined by very few genes. I maintain that we are not in a position to say this though it may be true. This is because you cannot be certain that you are not dealing with groups of closely linked genes, which as you know behave like single genes. You appear to be so set on a polyphyletic origin for these two forms because they have an unusual distribution pattern. I would suggest that our knowledge of the distribution of these forms is so rudimentary that distribution patterns are a very poor grounds to justify the introduction of so revolutionary a theory as the polyphyletic origin of individual species. With regard to our knowledge of the distribution of A & B all we can be certain of ~~xxx~~ are the positive identifications. For example I know that on Mauritius A, B, & SW A.gambiae occur but I cannot say that C does not occur.

I am also not in favour of your suggestion that A & B may be semi-species. I think a very strong case can be made for dropping the semi-

species as an evolutionary category (it is not a taxonomic category) since it is used in two senses. Mayr's original sense was fairly clear cut but the emended definition is ambiguous. Few would doubt that the mallard and pintail are good species of duck yet in nature they hybridize to some extent (about 1 hybrid per 1000 wild birds is found in these species). When two subspecies meet they will hybridize freely so that the distinctive features of the two populations ~~are~~ are lost. The difference between these categories are clear cut: two species can become sympatric without loss of specific integrity - any hybridization occurring between them is so rare as not to undermine the specific integrity of the two species populations. With subspecies, <sup>however</sup> the two meeting populations do lose their individual characteristics because the hybridization is so extensive. Now Mayr wishes to distinguish a further case in which a little more hybridization occurs than in, say, the case of the pintail and mallard but still not sufficient for the two meeting populations to lose their integrity; i.e. they still meet his definition of a species. Then there is the matter of recognizing ~~species~~ natural hybrids by Coz. On p40 of my recent paper I have pointed out the difficulties facing us at present in our attempts at recognizing hybrids in nature. Every population of animals has in it a proportion of sterile males. How does Coz distinguish these from hybrids? Has he studied the sterility rate in allopatric populations of both A & B? if so how did he arrive at the conclusion that he was studying allopatric populations? This is not quibbling: these are essential points to be considered before his results can be used as evidence for hybridisation in nature. The important point is that species A & B retain their integrity when sympatric, thus meeting the essential definition of a species. Incidentally the African forms of <sup>house</sup>flies cannot be regarded as semispecies since the evidence quoted by Mayr is wrong. My own work on houseflies over the last 9 years does not support Sacca's with regard to the degree of sexual isolation between them. I do not think that enough critical work has been done on the human lice to justify quoting them as an example.

We must never forget that at present the importance of the work in which we are engaged is mainly practical. I feel very strongly therefore that every effort should be made to avoid confusing the applied worker with theoretical arguments. It is for this reason that I took a strong stand on the status of the members of this complex as soon as the critical evidence became available. It is ~~because of~~ this effort to keep the position as simple as possible for the applied man that leads me to criticize views such as the possibility of multiple origins for species A & B and the designation of these forms as semispecies. The ordinary entomologist working on malaria can't define a species with any precision let alone tell you what a semispecies is. Though I would never suggest suppressing anything for practical reasons I do feel that we should make every effort to keep our theoretical arguing about finer details apart from results which are essential from the practical point of view. I was led to criticize the possibility of multiple origins for A & B because several people had used this possibility as an excuse for playing down the value of this work. I am sure you agree with me that the sooner we can get the malaria entomologists to think in terms of the forms rather than simply A. gambiae the better for progress in malaria work in Africa.

One final point: I would strongly urge you not to designate the Uzumba form "E" but to call it "C". Hadjinicolaou and I feel that this designation of yours will lead to a great deal of confusion. Some people will use C for the new form some will use it for the E. Coast SW form. I strongly urge you to keep to the convention we have followed up to now of referring to the West Coast form as A. melas, the E. Coast form as SW A. gambiae and restricting the lettering to the freshwater-breeding forms. This system is understood by everyone and I cannot see any justification for changing it with resulting confusion.

I am beginning to doubt that I shall be able to obtain funds to get to London in July. It is also in the middle of our second term. I think I shall concentrate on trying to get to the Parasitology conference in Rome. I hope you will be at this conference as well.

We certainly had difficulties with colonizing sp.C and had the same experiences as you did with regard to feeding. We lost the colony during the move to Salisbury in the 9th generation. I must now start a new colony.

With my best wishes to you for 1964. I shall look forward to hearing whether you are able to agree with me on any of the points I have raised.

Yours sincerely,

*H.E. Paterson.*

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