25th February, 1964.

H.E. Paterson Esq.,
University College of Rhodesia and Nyasaland,
Zoology Department,
Private Bag 167 H,
Salisbury,
S. Rhodesia.

Dear Paterson,

After building up our Uzumba Colony (species C) to quite large numbers after the Christmas holiday we are now unfortunately reduced to two females which have not yet parted with their eggs. I have been seeing quite a lot of Dr. H.L. Wolfe of late and he recommends me to write to you for a possible further supply of this strain. I understand that the possibility of field-collections becomes remote after next month until much later in the year. Have you established a colony in Salisbury yet? I would be more than grateful if you could oblige us with some eggs when convenient to you.

I have lately been trying to confirm the fertility of the F_1 males from the cross: group A male x Uzumba female. Though the testes show some apparently normal sperm, I had failed to get a significant F_2 generation except in one cross with Kisumu which was done at a time when the Uzumba colony may have contained group A individuals. I have now crossed several A strains to the Uzumba strain and each cross has given virtually nothing but male offspring. Mating these to virgin females both passively (leaving together in cages) and actively (artificial mating) has so far not produced viable offspring.

Thank you for letting me see your paper on Mauritius. I have little comment to make other than what is relevant in my previous letter. Your reference to our sterile male control method (Page 17) is a bit premature. Present cage tests indicate that some sterile hybrid males actually mate

more readily than the parent males i.e. that hybrid vigour is manifested in an enhanced mating capability. However, of course, we don't know if this would show itself in nature. Like you, I am somewhat sceptical of the potential use of this method to control such an insect as A. gambiae but not for the reasons given by you - more for general practical difficulties!

Incidentally have you been following the work of Andrew Spielman at Harvard on autogeny in the Culex pipiens complex? He has evidence of sympatric speciation characterized by autogeny and anautogeny. Hybrids are easily recognised but rarely seen, normally, in nature. In the laboratory cage hybridization is easy and no sterility apparent. However, the isolating mechanism has been known to break down in nature after a tornado when something like one-third of the mosquitoes caught showed evidence of hybridization. Could this sort of thing not be happening with A. gambiae from time to time?

Yours sincerely,

G. Davidson,
Reader in Entomology as
Applied to Malaria