8th May, 1953.

Dear Dr. Boyd,

Thanks for your letter of May 4th on "The fitting of gene frequencies to data on Rhesus reactions." It is nice to hear from you again.

The standard deviations of the adjusted estimates of gene frequencies shown in the fifth column of Table 7 are the square roots of the diagonal elements of Table 9 e.g. the first standard error 0.86454 % is the square root of 0.747433 $(\%)^2$ as the left hand top diagonal element for R_2 may be written. I headed that table as "Multiplied by 10^4" in order to accomodate this adjustment of the corresponding square roots to percentage values.

Of course the unsatisfactory part of expressing these standards errors ~~per cent~~, is that it gives the impression that they are independent as they cannot possibly be with frequency estimates constrained to add to a hundred per cent. Hence the need for the covariance matrix. the sense of which is, I hope, made clear in the further note on the calculations of the frequencies in Annals of Eugenics. Vol. 13. pages 223-224. published early the following year where the use of the covariance matrix is further illustrated to calculate standard errors for aggregates of allelomorphs such as C and c.

I remember Henry Wallace well and think he has raised an interesting point. Early in this last war I worked through some hundreds of thousands of blood group donour forms, having in mind among other possibilities, that differential death rate between the ages of twenty and sixty might alter the blood group proportions in this age range. I got no clear results, as was perhaps to be expected in view of the fact that the British population is somewhat heterogeneous in the proportion of A and that considerable migrations from one part of the country to another can take place in a single generation, so that at any one place, such as Slough, donors in the fifties may not have been born in the same part of the country as donors in the twenties.

Another line of evidence has appeared with the MN factors, where it seems to be agreed that matings between heterozygotes do produce, on the average, more than half heterozygous children. This I suppose must mean elimination at an early age, mostly prior to birth. Of course it has long been suspected, e.g. by Levine that there might be considerable elimination by haemolytic reaction of heterozygous A and B children and if it is true there must certainly be countervailing selections in the viability or fertility at other ages.

I presume that the sex difference in the ratio A to O found both in my own data and in that of Fraser Roberts must be taken as an indication of differential elimination of those

genotypes differing in the two sexes.

I hope you will be attending the International Genetical Congress this year at Bellagio where I am looking forward to meeting a number of old friends.

Sincerely yours,