29 September 1944

My dear Henry,

I just want to send a summary of what I have done further with Dominula. You will recall that, using a constant death-rate (best estimated at about 16% daily), one can get for each catching-day an estimate based on the proportion of recaptures and the probable numbers of marked moths then flying. These estimates were rather consistent at different dates of the same year, differing considerably from year to year, but giving me some embarrassment in indicating maxima later than would be expected from the catching evidence, especially in 1941 and 1944, the two years of high abundance.

I next wanted to use those estimates to supply approximate curves showing numbers probably flying at different dates in each year, not that I supposed that the true curve would really be smooth, for emergence must be "jumpy", but because it seemed evident that one's estimate for any one date would be improved by taking account

See Dominula P. for calculations

of experience at neighbouring dates, and because I wanted to get some idea of total emergence for each year. With this in view, I supposed that the numbers flying on successive dates could be sufficiently represented by a parabola on the logarithmic chart, or (what comes to the same thing) by a normal curve if the absolute numbers were plotted, and used the combined evidence of the four years to estimate the curvature of these similar parabolas, which in an inverse sense measures the average duration of the flying season. Using 15% mortality (I should like later, if I can, to rework it all for 16), the value obtained may be well expressed by the relationship between the total number of flying days in the season and the maximum number flying on any one day, and my estimate makes the former 18.384 times the latter. This is perhaps a little higher than might be guessed, and certainly the estimate(though impartial) has no great claim to precision. As, with 15% mortality, each insect emerged has on the average 6⅔ flying days, this is equivalent to estimating

that the total number emerged is 2.8[illegible]6 times the maximum number found flying together. Obviously this ratio must exceed unity, and I think it might well be twofold, though nearly threefold seems a little high. I enclose a graph showing the parabolas fitted to the four years, all being, as you see, similar and similarly situated. the two years with lower numbers, 1942 and 1943, having earlier maxima falling well within the range of observation, while, of the other two years, 1941 indicates a maximum considerably later than the last catch and 1944 only shortly before the last catch, when the moths were, by direct observation, certainly scarce. In 1944, therefore, the maximum is not greatly over estimated, my estimate being 3767 flying simultaneously, though the duration may be, so that my estimated total emergence 10670, is quite probably high. In 1941 my estimated maximum is 3084, but my estimated number at the last date of capture is only 2417, so that I presume that the total emergence was considerably less than the number, 8755, arrived at by this process.

Year	Maximum	Total days' flying	Total emergence	
1941	3083 9	58236	8735.4	Certainly too big
1942	931.7	17594	2639.1	
1943	356.2	6726	1008.9	
1944	3766.9	711 4	1670.	Probably too big

Of course, I may be quite wrong in guessing that the figure indicated, 2. 326, is really high, but I certainly think that, although 1941 and 1944 were years of much greater abundance than 1942 or 1943, yet thefind that the apparent maximum comes near to the last the end of the season does mean that the total emergence in 1941 and 1944 has been overestimated relatively to the "earlier" years 1942 and 1943.

The year 1942 seems fairly typical, as the numbers, being neither very high or very low, and there was a long series of observations, though with on the whole small catches. In consequence, four of the estimates are infinite, due to there being no marked moths captured. The non infinite estimates are, therefore, on the whole below the true

values. As I thought you might like to discuss this year somewhere in more detail, I have risked spoiling the diagram by showing, for this year only, the empirical estimates at each date of capture by points surrounded by circles, connected with the estimate on the smooth curve drawn by taking all dates of capture into consideration. Two of these empirical estimates are almost on the curve, one is slightly above it, and six below it, but I have drawn the diagram to try to show the way in which the estimated smooth curve is a compromise among the empirical estimates, the highest of which (599.6) is quite near to the estimated maximum (631.7). In spite of the [illegible] small catches, I have no doubt that in this case we have a tolerably accurate notion of the course of population change during nearly the whole month. In consequence, the estimate of total emergen cannot, I think, be greatly in error.

Of course, I have available, and should be glad to send you, any other details of the calculations with which I have not encumbered

this letter. Apart from the great importance, as I feel, of getting some estimate of total emergence, these smooth curves are rather artificial in the sense that they are bound to be inaccurate in detail, and the work done on them has not been very profitable unless it leads to a resolution of the difficulty that in the two abundant years the greatest numbers should appear to come so unsuitably late in the season

I hope your affairs prosper. I have recently been consulted about the Readership in Statistics and Directorship of the Statistical Institute at Oxford, and hope that someone interested in biological applications may be appointed. I am sure, at least, that you will be prepared to co-operate in guiding whoever is appointed in the matter of what he can do to forward the application of statistics in biological research.

Sincerely yours