15 October 1931.

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My dear Ford,

Many thanks for Spooner's letter which I return herewith. As you say it is difficult to make much of the data as they stand. I had forgotten the incubator part of the story, but I suppose the tale that was brewing involved the induction of mutations by change of temperature. If so, this at least, makes a good opportunity for getting a decent bit of work done on the frequency and kinds of easily recognisable mutant genes in the wild population, which should be well worth doing, quite apart from the comparison, Incubator v. Room Temperature. I am glad they have such a level-headed man, as Spooner seems to be, on the job. If the incidence of homozygotes in the wild population could be included in their research, it would be very much to the good.

I have been thinking a little about the project of introducing Chevreuxi into an isolated situation where

it is not now found. If you introduced a line known to be free from the visible mutations it would, on our present knowledge, be a long time before an appreciable proportion of the resulting population would be brought even to the heterozygous condition by the accumulation of mutations, e.g. after a hundred generations, a mutation with a mutation rate of 1 in a million would be heterozygous in something less than 1 in 5,000.

There is another possibility which might work rather more quickly and that is increased adaptation to higher or lower salinity if the conditions are otherwise suitable and especially if better adapted competitors belonging to other species are absent. You do not mention other species in your letter but I presume they are the chief obstacle to its spread from the Plymouth centre.

Yours sincerely,

ENCLOS: 1.