

NEDERLANDSCHE GENETISCHE VEREENIGING

SECRETARIS:
Dr. A. L. HAGEDOORN
SOESTERBERG

POSTREKENING 43150
TELEFOON 517

Soesterberg, Holland 15th July 1945

Prof. Dr. R. A. Fisher

Dept. of Genetics

Univ. of CAMBRIDGE

England

Dear Colleague,

Many thanks for your note. So far, in trying to find out what our friends and colleagues in England have been doing, and how they have weathered those five years, I found them all active and well.

Thanks for the promise of separates; it might be necessary to send them through diplomatic channels, I am afraid.

As I have been working on dominance and on the origin of novel characters I am especially anxious to find how your experiments with backcrosses to *Gallus ferrugineus* (*bankiva*) turned out. Wasn't it terribly difficult to breed those beasts in captivity when the genotype became practically that of those pesky *G. bankivas*?

As you may know I do not accept the idea that *G. bankiva* has been "the" ancestor of domestic poultry; there are several reasons, I think, for the hypothesis that the first chickens domesticated were of another species with naturally strong "homing" instincts, ^{i.g.} *G. Soeneratti*, the *bankiva* colouring and other qualities being derived from species crossing afterwards. Our own poultrywork bears out your expectations very clearly, especially as far as the "barring" is concerned (one of the best genes for the purpose as this gives us quantitative differences in genotype which are visible in the effect of the gene). Breeding back wild housemice to dominant yellow did not change the colour of the yellow animals although there was a marked change in another

respect.

It will surely interest ~~you~~ to know that we succeeded in producing a strain of mice in which albinos and buffs interbreed, albinism being dominant to buff colour. We get clearcut 3:1 ratios, in the hybrids no trace of pigment can be discovered; this is, I think, a very good illustration of the effect of a gene on the action of another one. I think Goldschmidt's views, your own and mine are closely identical, but I don't think you ever discovered my 1911 paper on "Autokatalytical substances" in Roux' series or my book on the Relative value of factors in evolution (1921) My fault, for not drawing ^{your} attention to my writings!

When I first showed how the absolute numbers of individuals in a generation ~~must~~ occasionally cause a chance reduction of the potential variability of the group (what you call "variance") and called attention to this as to a major cause for specific purity, I must have been too early. I think you greatly helped to make the idea go down. Its applications in plantbreeding, but especially in animalbreeding technique have, I think, only very partially been realized, so far.

There is one point on which you and I are not in agreement. I have the impression (from your pre-war writings) that you are too apt to accept gene-substitutions as caused by mutations. I had a very good example of the origin of a double recessive novelty (aabb from the cross AAbb x aaBB) closely resembling a mutation. It is generally exceedingly difficult to make the differential diagnosis, and you have to be on the lookout for such things in advance, but I think in this case there is no doubt whatever. I will send you this paper on the waltzing ferretas soon as it appears.

To puzzle out the possibilities of practical applications of our views on dominance in plant and animal breeding and eugenics is quite an amusing thing. When the war immobilised us in Soesterberg I had great fun in writing a thickish book on the importance of genetics for medicine (which will be published in two languages) and an extensive plant- and animalbreeding

NEDERLANDSCHE GENETISCHE VEREENIGING

3. book.

SECRETARIS:
Dr. A. L. HAGEDOORN
SOESTERBERG

POSTREKENING 43150
TELEFOON 517

I hear from Hammond that he has been so kind as to arrange a second edition of my book on "Animal breeding" while I was forced to remain out of touch with the rest of the civilised world. I had been worrying whether anybody had ever read it.

We were very fortunate in being able to keep on working with our animals at Soesterberg, even at great odds. As there was no communication whatever, trains, automobiles and even bicycles having been stolen, I was cut off from the Institute of preventive medicine in Leiden, and had to keep everything home. But we pulled things through all the same, mostly rats, mice, pigeons, poultry and beans, and we hope to get better facilities pretty soon.

So far I have heard from Haldane, Grüneberg Hammond and Winge (Kopenhagen). Do you happen to know how Grew weathered the war?

When you have time, please give me an idea of what experimental work you are doing just now, and who are working with you. As soon as feasible I want to come over to Cambridge to reestablish contact.

Very truly yours:

A. L. Hagedoorn