Personal memo from
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Bert Wills

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Thank you for the book.
I'll guard it carefully.
(Better not leave it on the open shelf if it's so scarce).

I found Johanssen's first use of "gene" in English, although (see p. 143) he entertained some speculations about genes as chemical entities, you can see how his caveats on p. 153 pose a daunting challenge to the "simplicisms" of Beadle + Tatum. They had to say enzymes were different from other characters, they were the primary products of genes. I don't think Garrod ever touched that. I'll elaborate in my preface. Cheers.

Josh

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make hypotheses as to the nature of heredity and variability. Darwin has somewhat exaggerated the scientific value of breeders' testimonies, as if a breeder *eo ipso* must be an expert in heredity. As to the principle of pure lines it has been occasionally vindicated by German authors, *e. g.*, K. v. Rümker, that pure line breeding is a thing old and well known. This is quite true; nearly sixty years ago L. Vilmorin not only emphasized in a lucid manner the importance of pure breeding, but he even tried a little to use his experiences theoretically. But it can not be denied that the principle of pure lines, as a true scientific analytical implement, as an indispensable *method of research in heredity*—not merely as a questionable and, at any rate, unilateral and insufficient method of practical breeding—is a novelty from recent years. Had this analytical principle been used in the times of Darwin, or had it even been appreciated in due time by the biometric school, certainly the real bearing of selection might long since have been rightly understood also by the practical breeders of pure strains.

The genotypes may then be characterized as something fixed and may be, to a certain degree, parallelized with the most complicated molecules of organic chemistry consisting of "nuclei" with a multitude of "side-chains." Continuing for a moment such a metaphor, we may even suggest that the genes may be looked upon as analogs of the "radicals" or "side-chains." All such ideas may as yet be premature; but they are highly favored by the recent researches of Miss Wheldale.

The fixity of a genotypical constitution in question is the conception arrived at by Mendelian and pure line work. Hence there is a *discontinuity* between different *genotypes*. This discontinuity has been energetically contested by several biologists, among whom Woltereck may be pointed out as an important representative. In his very interesting report on experiments with Daphnias, Woltereck indicates, as said above, that *selection was as yet ineffective;* moreover he describes *a case of discontinuous alteration* of type (mutation), and his ex-

Johanssen 1911 Am Nat. 45

only say that this case does not seem incompatible with Mendelian views. It must also be borne in mind that certainly there have been very *many genodifferences* between the differing races intercrossed in Castle's experiments. Hence these experiments are really operating with highly poly-heterozygotic F_1-generations. And how great influence upon dimensions (of ears and other parts of the body) those color-determining genes may have exercised can not be easily determined.

As to beans, it is proved that genes, effective in color-reactions, may also have great influence upon the dimensions and forms. So in my crosses a special factor, which makes yellow color turn into brown and causes violet to be turned into black, has a very marked influence upon the size and form of the beans in question. Here exact data are not necessary; the instance exemplifies the two incident matters of fact, viz., that apparently simple "dimensional" or meristic characters may be determined by *several different genes*, and that one sort of gene may have influence upon *several different reactions*.

Then it seems that Mendelian analysis is proceeding in a very prosperous way; but there may be even very narrow limits for this analysis: the entire organization may never be "segregated" into genes! But still there is much to do in carrying through the genotype-conception as far as possible.

As to cytological researches the genotype-conception is as yet rather indifferent. Certainly the process of segregation must be a *cell-action* intimately connected with division. But all the innumerably detailed results of the refined cytological methods of to-day do not elucidate anything as to segregation. It seems to the unprejudiced observer that the much-discussed cytological phenomena of karyokinesis, synapsis, reduction and so on may be regarded rather as consequences or manifestations of the divisions, repartitions and segregations of genotypical constituents (and all other things in the cell) than as their causes. This view is applicable even in those cases

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