DISCUSSION AND CORRESPONDENCE

"GENOTYPE" AND "PURE LINE"

THE widespread interest in the lectures on genetic problems now being given in this country by Professor W. Johannsen makes it worth while to point out certain diversities in the usage of terms introduced by him—diversities giving an appearance of disagreement where none exists. The fact that the present writer is partly responsible for any confusion thus caused impels the publication of this note.

The term *genotype* was introduced by Johannsen in connection with the term phenotype. The latter designates a group of organisms which in outward appearance seem to belong to one type, although in hereditary constitution they may actually differ greatly. *Genotype,* in Johannsen's usage, is not directly contrasted with phenotype, to signify a group of organisms that actually do possess in all respects the same hereditary constitution—though this is the sense in which some of us have been using it. It arose as follows. Organisms with hereditarily different constitutions must have different combinations of the determiners, called by Johannsen *genes,* that decide what the somatic characters shall be. They have, then, different typical combinations of genes. Johannsen calls the particular combination of genes that an organism has, its genotype. Or, without reference to genes, we might say that the genotype of any organism is the particular combination of hereditary features that characterize it. Thus, as employed in Johannsen's usage, genotype is an abstract term.

When a group of organisms all have demonstrably the same combination of hereditary characteristics, one can say that they have the same genotype, or that they belong to the same genotype. From this it is but a step to the employment of the word as a name for such a concrete group of organisms, all with the same hereditary characteristics. Following a bent toward concreteness, I have used the term in this way in my paper on "Pure Lines in the Study of Genetics in Lower Organisms."[1] Shull has done the same in his paper on the "Genotypes of Maize";[2] apparently this use of the term for a concrete, visible group of organisms is becoming general; for a term with this precise meaning is much needed. But this is not the usage of Johannsen.

Thus arise such differences as that shown by my own characterization of genotypes as "concrete realities," as "actual existences that strike you in the face," etc.,[3] when compared to Johannsen's statement that "we do not know a genotype," etc.,[4] and that this is a concept with which we cannot actually operate.[5] There thus arises an appearance of opposition where none exists. What I and some others have called a genotype is what Johannsen would call a group of organisms "identical in genotypical constitution." The usage recommended by the originator has of course the right of way.[6]

A diversity of usage likewise exists as to the expression "pure line." I have employed this to designate a genealogical series in which there arises no diversity in hereditary characteristics, either from within or from without; such, for example, as the series produced by the repeated fission of a single infusorian. Pure lines in this sense might be expected, from what we thus far have learned, (1) in cases of vegetative reproduction, (2) in at least some cases of parthenogenesis (where no reduction division occurs), (3) in case of self-fertilization of homozygotic organisms, (4) in case of inbreeding of a group of genotypically identical homozygotic organisms.

The pure lines investigated by Johannsen fall in the third group, and he employs their

[1] *Amer. Nat.,* February, 1911.

[2] *Amer. Nat.,* April, 1911.

[3] *L. c.,* p. 80.

[4] *Amer. Nat.,* March, 1911, p. 134.

[5] "Elemente der exakten Erblichkeitslehre," p. 130.

[6] Whether the word itself should be given up, in this connection, because it had earlier been used in an entirely different sense, is of course a different question.

characteristics as his definition for pure line. "A pure line may be defined as the descendants from one single homozygotic organism exclusively propagating by self-fertilization."[7] It appears that we need badly a term that will include "genotypically identical" series of forms arising in other cases than this one, so that it is difficult to give up the use of the term in this wider meaning.

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MOSQUITO ROMANCE

In the issue of Science for September 15, pp. 350–351, Dr. John B. Smith reviewed a book by Edward H. Ross—"The Reduction of Domestic Mosquitos." While Dr. Smith indicates that the book does not meet the general needs of those to whom the title is obviously meant to appeal, he intimates that it will be valuable "in warm climates." Other reviews of the book have appeared in terms of unqualified praise. The most recent of these is in the November number of *Entomological News*. Furthermore, the book has been well advertised among those who might need useful information on this now important subject. Under the circumstances the writer considers it his duty to protect fellow-workers by indicating the true character of the book.

The author restricts himself to the two principal house-mosquitoes of the tropics, *Stegomyia calopus* and *Culex fatigans,* and the problem of their control. But instead of facts we get an array of well-worn generalities, and, where he deals with the life histories of the insects, of pure fabrications. It would be a waste of valuable space to discuss this book *in extenso;* a few choice blossoms are culled herewith for the benefit of the uninformed.

The book is avowedly economic and biological, but, lest the reader think the systematic side is negligible, we quote the following: "Fabricius in 1805 first designated the 'tiger' mosquito, *Stegomyia fasciata,* although Villiers[1] had probably described the same insect before; Meigen called it *Stegomyia calopus* very soon after. In 1825 Latreille grouped mosquitos generally under the name Culicidæ, but only three genera were known, *Anopheles, Culex* and *Ædes.*" Alas for Meigen and for Theobald!

Chapter II. deals with "the life and habits of mosquitos." One of the first statements we find here is that "the hæmatophagous habit appears to be dependent on the presence, in the female, of the spermatozoa of the male." The author deduces this from the fact that all the females with blood in the stomach dissected by him contained spermatozoa in the spermathecæ. "From this it must be inferred that virgin females do not, commonly, take blood"—surely a simple piece of reasoning! We are then favored with some amusing speculative remarks on this unusual phenomenon. A most remarkable belief of the author is that the female *Culex,* after disposing of her eggs, seeks another male and, after being again fertilized, produces another raft of eggs, and then over again, apparently *ad infinitum.* This absurd belief is, of course, purely a product of the author's imagination and it is controverted by a formidable array of established facts, of which, however, our author is blissfully ignorant. Considerable space is taken up with the reiteration of this notion and the author returns to it again and again. "If a female lays a whole egg-raft or complete brood, she exhausts all the spermatozoa within her spermathecæ and then she must again cohabit with a male in order to be replenished. This is the reason why males are likely to remain in or resort to the places where the females commonly lay their eggs. For example, in houses, the males of the Culecines are commonly found in the water-closets. The females are attracted there by the seal-water,

[7] Johannsen, *Amer. Nat.*, March, 1911, p. 135.

[1] The author's name is de Villers. He described a *Culex fasciatus* in 1789. Independently Meigen, in 1804, and Fabricius, in 1805, described other mosquitoes under the same name. The last of these was the species here considered, but the name is preoccupied by the two earlier homonyms; consequently (*Culex*) *calopus,* the name under which it was later described by Meigen, had to be adopted. The genus *Stegomyia* was established by Theobald in 1901.