School of Economics and Political Science, to which he was appointed in 1903, retains the readership in geography, to which, under its then title, he was appointed in 1902.

DISCUSSION AND CORRESPONDENCE

MENDELIAN PROPORTIONS IN A MIXED POPULATION

TO THE EDITOR OF SCIENCE: I am reluctant to intrude in a discussion concerning matters of which I have no expert knowledge, and I should have expected the very simple point which I wish to make to have been familiar to biologists. However, some remarks of Mr. Udny Yule, to which Mr. R. C. Punnett has called my attention, suggest that it may still be worth making.

In the *Proceedings of the Royal Society of Medicine* (Vol. I., p. 165) Mr. Yule is reported to have suggested, as a criticism of the Mendelian position, that if brachydactyly is dominant "in the course of time one would expect, in the absence of counteracting factors, to get three brachydactylous persons to one normal."

It is not difficult to prove, however, that such an expectation would be quite groundless. Suppose that *Aa* is a pair of Mendelian characters, *A* being dominant, and that in any given generation the numbers of pure dominants (*AA*), heterozygotes (*Aa*), and pure recessives (*aa*) are as $p:2q:r$. Finally, suppose that the numbers are fairly large, so that the mating may be regarded as random, that the sexes are evenly distributed among the three varieties, and that all are equally fertile. A little mathematics of the multiplication-table type is enough to show that in the next generation the numbers will be as

$$(p+q)^2 : 2(p+q)(q+r) : (q+r)^2,$$

or as $p_1:2q_1:r_1$, say.

The interesting question is—in what circumstances will this distribution be the same as that in the generation before? It is easy to see that the condition for this is $q^2 = pr$. And since $q_1^2 = p_1r_1$, whatever the values of p, q and r may be, the distribution will in any case continue unchanged after the second generation.

Suppose, to take a definite instance, that *A* is brachydactyly, and that we start from a population of pure brachydactylous and pure normal persons, say in the ratio of 1:10,000. Then $p=1$, $q=0$, $r=10{,}000$ and $p_1=1$, $q_1=10{,}000$, $r_1=100{,}000{,}000$. If brachydactyly is dominant, the proportion of brachydactylous persons in the second generation is 20,001:100,020,001, or practically 2:10,000, twice that in the first generation; and this proportion will afterwards have no tendency whatever to increase. If, on the other hand, brachydactyly were recessive, the proportion in the second generation would be 1:100,020,001, or practically 1:100,000,000, and this proportion would afterwards have no tendency to decrease.

In a word, there is not the slightest foundation for the idea that a dominant character should show a tendency to spread over a whole population, or that a recessive should tend to die out.

I ought perhaps to add a few words on the effect of the small deviations from the theoretical proportions which will, of course, occur in every generation. Such a distribution as $p_1:2q_1:r_1$, which satisfies the condition $q_1^2 = p_1r_1$, we may call a *stable* distribution. In actual fact we shall obtain in the second generation not $p_1:2q_1:r_1$ but a slightly different distribution $p_1':2q_1':r_1'$, which is not "stable." This should, according to theory, give us in the third generation a "stable" distribution $p_2:2q_2:r_2$, also differing slightly from $p_1:2q_1:r_1$; and so on. The sense in which the distribution $p_1:2q_1:r_1$ is "stable" is this, that if we allow for the effect of casual deviations in any subsequent generation, we should, according to theory, obtain at the next generation a new "stable" distribution differing but slightly from the original distribution.

I have, of course, considered only the very simplest hypotheses possible. Hypotheses other that that of purely random mating will give different results, and, of course, if, as appears to be the case sometimes, the character is not independent of that of sex, or has an influence on fertility, the whole question may be greatly complicated. But such complications seem to be irrelevant to the simple issue raised by Mr. Yule's remarks.

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Trinity College, Cambridge,
April 5, 1908

P. S. I understand from Mr. Punnett that he has submitted the substance of what I have said above to Mr. Yule, and that the latter would accept it as a satisfactory answer to the difficulty that he raised. The "stability" of the particular ratio 1:2:1 is recognized by Professor Karl Pearson (*Phil. Trans. Roy. Soc.* (A), vol. 203, p. 60).

PURE CULTURES FOR LEGUME INOCULATION

In the 1907 Report of the Biologist of the North Carolina Agricultural Experiment Station, Dr. F. L. Stevens and Mr. J. C. Temple report some work upon cultures of the nodule-forming organisms of legumes. The cultures used were obtained from the United States Department of Agriculture. The investigators have presented their data in such a manner that the value of pure cultures for inoculating legumes appears questionable and their conclusions emphasize their attitude of disapproval. In carefully reviewing their report, a very brief outline of which appeared in Science, Vol. 26, 1907, p. 311, I have been impressed with the fact that the inferences drawn by the casual reader would almost certainly be unwarrantably antagonistic to the use of pure cultures for inoculating legumes. The investigators' objections to the actions of cultures supplied by this department are briefly as follows:

A considerable number of the cultures hermetically sealed in glass were sterile at the time they were examined by Dr. Stevens and Mr. Temple. The misconception in regard to the viability of cultures distributed by the department at the present time could have been prevented by the insertion of a footnote explaining that since July, 1906, small bottles with wax seals have been substituted for small tubes hermetically sealed in the flame of a blast lamp. It is surprising to me that four out of seven of the old-style cultures examined by Dr. Stevens should have been sterile, as my own investigations previous to adopting this method for distribution indicated that about one half of one per cent. of the cultures sealed in this way in routine work would be injured or sterilized by the heat of sealing. The law of chance must perhaps be invoked to explain the discrepancy in our figures. It must be remembered, however, that the cultures spoken of at this time are the old-style liquid cultures, and that the cultures distributed since July, 1906, are not open to criticism of this sort.

It is surprising to me also to learn that during the multiplication period conducted in the practical manner outlined for use on the farm such great contamination should have become manifest. Two years ago I had small samples of these gross cultures prepared on the farm returned to me by farmers in various parts of the country for examination, the sample being taken and mailed to me at the time the culture was applied to the seed. This, of course, allowed for greater development of contaminations than would have taken place at the time the culture was applied to the seed. Even with this handicap about two per cent. of the cultures received from the farmers were apparently pure, and if contaminated the contamination was evidently very slight indeed. About sixty per cent. were contaminated, but not excessively so, it being easy in all of these cases to isolate large numbers of *Pseudomonas radicicola*. The remainder were in rather bad condition, although I doubt if ten per cent. of the entire number received were so seriously contaminated as to be worthless.

The description of the pot experiments conducted by Dr. Stevens and Mr. Temple is confusing. In the first place, the sterilizing of soil by heating is well known to injure the soil seriously, and, regardless of the condition of the nodule-forming bacteria introduced, it is an open question whether soil sterilized by heating would allow nodule formation until a normal bacteriologic flora and normal soil conditions generally had been reestablished. It is impossible to determine whether any