INDICATIONS OF THE TRANSMISSION OF AN ACQUIRED CHARACTER IN FLAX

IN the great field of plant and animal growth, change of form and type through development and adaptation, evolution is always in evidence. No two individuals are ever quite identical. Darwin seems to have conclusively proved that the thought "once a species, always a species" is untenable. Since his epoch-making labors, most biologists are quite agreed that species, varieties and strains continually show changes of type and character; but how do these changes come about?

In all types where open crossing is possible, it is easy to understand that an endless number of combinations of form and character may arise through various unions of units of form, quality or character. Thus, through Mendelian studies and methods it is easy for the formalists, given to mathematical methods, to account for any number of noticeable forms and qualities. Hence, if followed to the ultimate conclusion along such line of reason, species, varieties and individuals again become fixed recurring entities which can not be changed or modified in actual nature except it be through the recombination of certain fixed hereditary particles.

Of late, plant and animal breeders and leading geneticists have held that these recombining parts or particles are either represented by the chromosomes, or are contained within them as the genes. The genes, as such, approach philosophical or hypothetical units of heredity. They are assumed to be carriers or bearers of hereditary characters. The whole scheme of thought is entrancingly interesting, and appears to work out so well for properly contrasted units of form or character that it can be looked upon as an almost certain basis for predicting the general type of progeny which may follow upon the development of any given set of cross-mating.

It is but fair to say that this line of investigations and thought carries very great hope for plant and animal improvement, including man himself. It is, perhaps, the greatest working hypothesis yet given to the biologist.

However, the old thought that acquired characters may be transmitted and do in fact account for evolutionary progress and development, need not be dismissed as wholly untenable. The Mendelian theories and formulas do not explain the origin of the genes, nor is it proved that these may not be subject to change. It is with this thought in mind that I call attention to the wonderful work done by many workers in immunization against disease in animals, and to the yet more pronounced work of procuring disease resistant and climate resistant plants. In these lines of work there rests evidence of progressive evolution that can not be accounted for through the hypothesis of permanent, never-changing chromosomes or by the genes which may be lodged therein. If these never change in substance or character, then there could never have been a beginning, hence no change or evolutionary development.

In my own work of developing numerous strains or varieties of wilt-resistant flax, these points become very plain. From numerous varieties of non-resistant flaxes, I have been able through constantly submitting the same to ever-increasing degrees of disease attack, to bring about a high degree of immunity to Fusarial attack, without any associated evidence of cross-fertilization. Equally pronounced results are indicated as against flax rust.

When these strains which have accumulated a certain degree of resistance to a fungus attack are crossed artificially upon non-resistant strains, they transmit a definite degree of immunity to the resulting progeny. Thus we are now able to develop and throw the resistance to Fusarial wilts through crossing into any type or variety of flax desired through artificial crossings of the resistant strains upon the non-resistant strains or varieties. Mendelianists have suggested that such resistance as we have thus procured is but accidentally selected or picked up because of "fortuitous field crosses." However, flax is a closely fertilized plant and in all our work I have not seen sufficient evidence of open crossing to account for the rapid accumulation of wilt resistance.

During many years of continuous planting side by side, it is possible to grow numerous varieties and selection strains without there appearing in the product any evidences of breaking up of the noticeable characters of flowers or of other morphological parts because of fortuitous crossing. We have grown hundreds of individuals from individual plants and from individual seeds for the distinct purpose of observing this feature. It probably does not occur in sufficient extent to in any way affect the results in accumulating wilt resistance which we have here obtained.

Our results indicate no other conclusion than that wilt resistance may be accumulated rather rapidly by a non-resistant strain or variety and that, when it is thus accumulated, it is transmitted from generation to generation through seed; and, further, that when once obtained to a certain degree, it can be fixed through artificial crossing.

This appears to be the final proof of the transmission of an accumulative quality. Besides, if such a quality of resistance can not be so accumulated in nature, how can we account for such features as the

gradual but rather rapid acclimation in corn? If such qualities do not arise in nature by or through gradual accumulation in association with constant crossing, how then did the first gene or unit of character arise? How did the first plant become resistant for any character?

I write simply to suggest that it is well for plant physiologists, ecologists and plant breeders to hold the open mind over against the thought of "Once a gene, always a gene."

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MECHANISM OF BUFFER ACTION IN SOILS

WHILE working on "The Rôle of Pectin in Jelly Formation" it was found that the buffer action of the pectin solutions was due entirely to the impurities in the solution and not to the colloidal properties of the pectin.

It previously had been assumed, while outlining the method for the attack of the problem involving a fundamental study of the mechanism of buffer action in soils, that the buffer action exhibited by certain types of soils was, for the most part, due to the colloidal content of the soils. This assumption was based on the fact that soils high in colloidal matter showed considerable buffer action while soils low in colloidal matter showed scarcely any buffer action.

In view of the results obtained with pectin solutions the plan of attacking the soils problem was changed so that now an attempt is being made to attribute the buffer action exhibited by the several soils to the impurities held by the colloidal fraction, perhaps by electrostatic attraction, double decomposition or neutralization of alkali with an acid or *vice versa*.

Some preliminary work has been done, using a Portsmouth loam, high in organic material. The colloidal fraction was separated and electro-dialyzed thus removing the greater part of the iron, aluminum, manganese, calcium, magnesium, sodium, potassium and other elements, as well as sulphates, phosphates and other acid radicles. As the electro-dialysis progressed samples were frequently withdrawn and their buffer action determined. It was found, during this preliminary work with this particular type of soil, that as the impurities were progressively removed from the colloidal organic fraction of the soil, the buffer action steadily decreased until, the impurities becoming negligible, the sample exhibited scarcely any buffer action.

As a result of this preliminary investigation the work is being continued, using various soil types with the hope of obtaining data sufficient to substantiate the claim that buffer action peculiar to soil types laden with colloidal material is not due directly to the colloidal properties of the soil but rather to the salts, metallic or acid radicles that are held by the colloidal fraction.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

A METHOD FOR OBTAINING INFECTIVE NEMATODE LARVAE FROM CULTURES[1]

CREEPING eruption, a human skin disease frequently encountered during the summer in some of the Southern areas of the United States, was shown by Kirby-Smith, Dove, and White[2] to be caused by third-stage nematode larvae. Later White and Dove[3] demonstrated that dogs and cats are concerned in the causation of the disease.

Much culturing has been necessary in the search for the adult worm of the causal parasite and in other studies in which infective larvae have been used. The useful Baermann apparatus was first employed to recover the infective larvae from the cultures. Later a still simpler method was devised which reduced very materially the time required. This latter method has been employed for a year and a half and has proved to be entirely adequate for the problem. An outline of it is given in the present article.

The method makes use of the fact, often observed, that the larvae of a number of parasitic nematodes as they approach the third larval stage and the close of the free-living period tend to migrate from the medium in which they are growing. The apparatus traps many of the migrating worms.

Convenient and sufficient equipment consists of crystallizing dishes 125 to 150 mm. in diameter, watch-glasses slightly larger than these dimensions respectively, Petri dishes 100 to 125 mm., test-tubes 20 by 150 mm., filter papers 9 to 12 cm., a spatula with a 4-inch blade, a test-tube rack, a three-quart boiler with cover, animal charcoal, and sterile water. Brief steaming in the covered vessel suffices for all sterilization that is needed.

The charcoal and the feces are properly mixed conveniently in one of the larger watch-glasses and transferred to the half of a Petri dish, with a moistened

[1] Read before the Washington Helminthological Society, April 16, 1927.

[2] Kirby-Smith, J. L., Dove, W. E., and White, G. F., "Creeping Eruption," *Arch. Dermat. and Syph.*, xiii, Feb., 1926, 137–173.

[3] White, G. F., and Dove, W. E., "Dogs and Cats Concerned in the Causation of Creeping Eruption." Official Record, U. S. Dept. Agr., Oct. 27, 1926, *V*.