

deeply 2-cleft attenuate paleas are obviously awn-tipped. This plant, which may deserve specific reinstatement, but which seems to pass directly into the other varieties, does not occur in the bleaker habitats nor the more northern regions, like the headlands of Newfoundland and the coast of Labrador, where the others are found. Geographically, it is decidedly more southern: known in Newfoundland only along the sheltered river-banks; and on the mainland extending from Rimouski Co., Quebec, westward across the continent, south very generally on ledgy shores or slopes through northern and western New England and north-central New York, and locally to the Carolina mountains.

GRAY HERBARIUM.

ON THE MENDELIAN INTERPRETATION OF OENOTHERA CROSSES.

R. RUGGLES GATES.

IN a recent review of my book on Mutations,¹ East² takes occasion to repeat certain criticisms of the *Oenothera* work which have been reiterated in recent years with rather tiresome frequency. This criticism is to the effect that since it is known that in the *Oenotheras* a considerable percentage of the pollen grains, eggs and embryos frequently fail to develop, therefore it is impossible to draw any conclusions whatever from the abundant crossing experiments that have been made in this genus; unless, perchance the result happens (as it occasionally does) to be Mendelian. In the case of East, we are further assured that "no single fact discovered by those who have made pedigree cultures of the group, precludes a Mendelian interpretation." I venture to think that such a statement would only be made by one who had allowed his bias to outrun his discretion. It would further, I think, scarcely have been made if its author had first attempted to apply his idea to an explanation of the known facts.

¹ Gates, R. Ruggles, 1915. *The Mutation Factor in Evolution*, with particular reference to *Oenothera*. London: MacMillan. pp. xiv + 353, figs. 114.

² East, E. M. 1915. *RHODORA* 17: 235-237.

As the matter is one of some importance, involving as it does the whole question of the interpretation of mutations in certain aspects, it may be worth while to point out some of the difficulties which East has failed to see. He will then have the opportunity of explaining them on a Mendelian basis.

It would be quite impossible in a limited space to discuss all the classes of cases which do not conform to the Mendelian conception, but a few of them, belonging to one class only, may be pointed out.

In the first place let us consider what de Vries calls mutation crosses, such as *Oe. Lamarckiana* \times *rubrinervis* or its reciprocal. In such crosses the F_2 splits into the two parental types¹ and both breed true in later generations. We are assured by some that this can be explained as ordinary segregation, but for such an explanation the following assumptions must be made: (1) that *Lamarckiana* is heterozygous for the *rubrinervis* character, (2) that it breeds true both before and after the cross because the *rubrinervis* germ cells either fail to develop or fail to fertilize each other, (3) that about 50% of the *Lamarckiana* germ cells are *rubrinervis* in character, since *rubrinervis* usually appears with this frequency in crosses with *Lamarckiana*. Thus far the assumptions, though improbable are not impossible, and the fact that *Lamarckiana* may show 50% or more of sterility leaves the interpretation a loophole through which to crawl.

Now let us go a step further. If other flowers on the same *Lamarckiana* plant used to cross with *rubrinervis* are pollinated by *nanella* the dwarf mutant, the F_1 will again contain the parent forms *Lamarckiana* and *nanella* in widely fluctuating percentages, and the same result occurs in the reciprocal cross *Oe. nanella* \times *Lamarckiana*. We must now apply the above Mendelian hypotheses *mutatis mutandis* to these crosses, and assume that some 50% or more of our *Lamarckiana* germ cells are now *nanella*. The same must be done for all the other forms which show a similar behavior in crosses with *Lamarckiana*. This is of course absurd, for it assumes that 50% of the *Lamarckiana* germ cells are at the same time *nanella*, *rubrinervis*, *oblonga*, etc. The only way out of this difficulty that I can see is by the further assumption that when one crosses *Lamarckiana* with *nanella* pollen all the *rubrinervis* germ cells present obligingly disintegrate and disappear,

¹ The form of *rubrinervis* derived from this cross has since been found to differ in certain particulars and has been called *subrobusta*. But this does not alter the interpretation of the facts.

while when *rubrinervis* pollen is used the *nanella*-carrying eggs of *Lamarckiana* disappear. Will anyone be found willing to support such an hypothesis?

But the difficulties with a Mendelian interpretation of these crosses have only begun. How are we to account for the fact that both *Lamarckiana* and *nanella* from the F_1 of *Lamarckiana* \times *nanella* breed true? On the Mendelian assumption it must be because in these *Lamarckiana* plants the *nanella*-carrying germ cells all degenerate, either in the pollen or the egg cells or both. Otherwise when selfed they would produce *nanella* in F_2 . Is this degeneration a reasonable assumption when we know that in *rubrinervis* \times *nanella* some of the *rubrinervis* plants appearing in F_1 when selfed split out *nanella* in a ratio which is, in some cases at least, close to 3:1? That is, we know that *rubrinervis* plants which are heterozygous for *nanella* develop their two types of germ cells according to regular Mendelian expectation, and it would be, to say the least, highly improbable that the closely related *Lamarckiana* would behave in an entirely different manner and that its *nanella* germ cells (assuming that there are such) would degenerate.

Another fact which East must explain is this: Why is it that *Lamarckiana* \times *nanella* yields dwarfs in F_1 while *rubrinervis* \times *nanella* only yields them in F_2 , or in other words why is it that the first result is a mutation cross while the second is a Mendelian result as regards the dwarf character? So far as I am aware, no Mendelian has attempted to offer an explanation of this significant fact.

There is yet another fact in this connection which has not even been considered, still less explained, by the critics. This is that while *Lamarckiana* gives rise to the mutant *nanella*, *rubrinervis* has never been known to do so in all the extensive cultures. Is it unreasonable to connect these facts with those mentioned in the last paragraph?

The mutationist conception on the other hand, while it may not furnish a complete explanation, at least enables us to consider all these facts under a consistent point of view and does not lead to any of the absurdities which lurk in a Mendelian interpretation. Moreover, it offers an explanation of whole classes of facts which no Mendelian writer has attempted to explain. Let us consider this conception as it applies to the facts we have cited. DeVries has assumed that pangens, or if you like, factors for the differences between the mutants and their parent *Lamarckiana*, may exist in

three conditions, (1) labile, (2) active, (3) inactive; that, *e. g.*, the *nanella* pangen or factor for tallness is labile in *Lamarckiana* since that species can give rise to *nanella* through a mutation, while it is only active in *rubrinervis* since the latter can not give rise to *nanella*. In *nanella* it is considered inactive rather than absent. Correlated with this is the fact that, as we have seen above, *Lamarckiana* \times *nanella* splits off dwarfs in F_1 , while *rubrinervis* \times *nanella* splits them off in Mendelian fashion, *i. e.*, in F_2 .

Instead of the impossible and self-contradictory assumptions regarding degeneration of certain classes of germ cells or zygotes in the various crosses, DeVries made the one assumption that in the zygotes of, *e. g.*, *Lamarckiana* \times *nanella* either one or the other form or condition obtains ascendancy, to the complete exclusion of the other form in later generations. This view is at least self-consistent, which cannot be said of the Mendelian "explanation." If any Mendelian can suggest an alternative explanation which avoids the pitfalls pointed out above, we shall be glad to see it. We have shown at any rate that in the particular group of crosses considered above, the attempt to hide behind sterility as a means of offering a Mendelian explanation only leads to difficulties. So far as we can see, the Mendelian explanation fails hopelessly in all these cases and in others as well.

It will be time enough to consider East's other objections to the point of view of my book when the points discussed above have been cleared up.

AN OVERLOOKED ENVIRONMENTAL FACTOR FOR SPECIES OF PRUNUS.

ROLAND M. HARPER.

IN the March number of *RHODORA*, pages 66–70, Mr. Bayard Long reports finding *Prunus cuneata* on the southeast side of a creek or small river in the pine-barrens of Ocean County, New Jersey, especially on a gravelly railroad embankment in the creek swamp; and he discusses at some length the question of whether it can be native there, in view of the fact that no other stations for it are known within many miles.



Gates, R. Ruggles. 1916. "ON THE MENDELIAN INTERPRETATION OF OENOTHERA CROSSES." *Rhodora* 18, 198–201.

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