

XII. *An Account of some Experiments on the Fecundation of Vegetables. In a Letter from Thomas Andrew Knight, Esq. to the Right Hon. Sir Joseph Banks, K. B. P. R. S.*

Read May 9, 1799.

Elton, April 25, 1799.

THE result of some experiments which I have amused myself in making on plants, appearing to me to be interesting to the naturalist, by proving the existence of superfœtation in the vegetable world, and being likely to conduce to some improvements in agriculture, I have taken the liberty to communicate them to you.

The breeders of animals have very long entertained an opinion, that considerable advantages are obtained by breeding from males and females not related to each other. Though this opinion has lately been controverted, the number of its opposers has gradually diminished; and I can speak from my own observation and experience, that animals degenerate, in size at least, on the same pasture, and in other respects under the same management, when this process of crossing the breed is neglected.

The close analogy between the animal and vegetable world, and the sexual system equally pervading both, induced me to suppose, that similar means might be productive of similar effects in each; and the event has, I think, fully justified this

opinion. The principal object I had in view, was to obtain new and improved varieties of the apple, to supply the place of those which have become diseased and unproductive, by having been cultivated beyond the period which nature appears to have assigned to their existence. But, as I foresaw that several years must elapse, before the success or failure of this process could possibly be ascertained, I wished, in the interval, to see what would be its effects on annual plants. Amongst these, none appeared so well calculated to answer my purpose as the common pea; not only because I could obtain many varieties of this plant, of different forms, sizes, and colours; but also, because the structure of its blossom, by preventing the ingress of insects and adventitious farina, has rendered its varieties remarkably permanent. I had a kind growing in my garden, which, having been long cultivated in the same soil, had ceased to be productive, and did not appear to recover the whole of its former vigour, when removed to a soil of a somewhat different quality; on this, my first experiment, in 1787, was made. Having opened a dozen of its immature blossoms, I destroyed the male parts, taking great care not to injure the female ones; and, a few days afterwards, when the blossoms appeared mature, I introduced the farina of a very large and luxuriant gray pea into one half of the blossoms, leaving the other half as they were. The pods of each grew equally well; but I soon perceived, that in those into whose blossoms the farina had not been introduced, the seeds remained nearly as they were before the blossoms expanded, and in that state they withered. Those in the other pods attained maturity, but were not in any sensible degree different from those afforded by other plants of the same variety; owing, I imagine, to the external covering of the seed

(as I have found in other plants) being furnished entirely by the female. In the succeeding spring, the difference, however, became extremely obvious; for the plants from them rose with excessive luxuriance, and the colour of their leaves and stems clearly indicated, that they had all exchanged their whiteness for the colour of the male parent: the seeds produced in autumn were dark gray. By introducing the farina of another white variety, (or, in some instances, by simple culture,) I found this colour was easily discharged, and a numerous variety of new kinds produced, many of which were, in size, and in every other respect, much superior to the original white kind, and grew with excessive luxuriance, some of them attaining the height of more than twelve feet. I had frequent occasion to observe, in this plant, a stronger tendency to produce purple blossoms; and coloured seeds, than white ones; for, when I introduced the farina of a purple blossom into a white one, the whole of the seeds in the succeeding year became coloured; but, when I endeavoured to discharge this colour, by reversing the process, a part only of them afforded plants with white blossoms; this part sometimes occupying one end of the pod, and being at other times irregularly intermixed with those which, when sown, retained their colour. It may perhaps be supposed, that something might depend on the quantity of farina employed; but I never could discover, in this, or in any other experiment, in which superfoetation did not take place, that the largest or smallest quantity of farina afforded any difference in the effect produced.

The dissimilarity I observed in the offspring afforded by different kinds of farina, in these experiments, pointed out to me an easy method of ascertaining whether superfoetation (the

existence of which has been admitted amongst animals) could also take place in the vegetable world. For, as the offspring of a white pea is always white, unless the farina of a coloured kind be introduced into the blossom, and, as the colour of the gray one is always transferred to its offspring, though the female be white, it readily occurred to me, that if the farina of both were mingled, or applied at the same moment, the offspring of each could be easily distinguished.

My first experiment was not altogether successful; for the offspring of five pods (the whole which escaped the birds) received their colour from the coloured male. There was, however, a strong resemblance to the other male, in the growth and character of more than one of the plants; and the seeds of several, in the autumn, very closely resembled it in every thing but colour. In this experiment, I used the farina of a white pea, which possessed the remarkable property of shrivelling excessively when ripe; and, in the second year, I obtained white seeds, from the gray ones above mentioned, perfectly similar to it. I am strongly disposed to believe, that the seeds were here of common parentage; but I do not conceive myself to be in possession of facts sufficient to enable me to speak with decision on this question.

If, however, the female afford the first organised atom, and the farina act only as a stimulus, it appears to me by no means impossible, that the explosion of two vesicles of farina, at the same moment, (taken from different plants,) may afford seeds (as I have supposed) of common parentage; and, as I am unable to discover any source of inaccuracy in this experiment, I must believe this to have happened.

Another species of superfoetation (if I have justly applied

that term to a process in which one seed appears to have been the offspring of two males) has occurred to me so often, as to remove all possibility of doubt as to its existence. In 1797, the year after I had seen the result of the last mentioned experiment, having prepared a great many white blossoms, I introduced the farina of a white and that of a gray pea, nearly at the same moment, into each; and as, in the last year, the character of the coloured male had prevailed, I used its farina more sparingly than that of the white one; and now almost every pod afforded plants of different colours. The majority, however, were white; but the characters of the two kinds were not sufficiently distinct to allow me to judge with precision, whether any of the seeds produced were of common parentage or not. In the last year, I was more fortunate: having prepared blossoms of the little early frame pea, I introduced its own farina, and immediately afterwards that of a very large and late gray kind, and I sowed the seeds thus obtained in the end of the last summer. Many of them retained the colour and character of the small early pea, not in the slightest degree altered, and blossomed before they were eighteen inches high; whilst others, (taken from the same pods,) whose colour was changed, grew to the height of more than four feet, and were killed by the frost, before any blossoms appeared.

It is evident, that in these instances superfoetation took place; and it is equally evident, that the seeds were not all of common parentage. Should subsequent experience evince, that a single plant may be the offspring of two males, the analogy between animal and vegetable nature may induce some curious conjecture, relative to the process of generation in the animal world.

In the course of the preceding experiments, I could never

observe that the character, either of the male or female, in this plant, at all preponderated in the offspring; but, as this point appeared interesting, I made a few trials to ascertain it. And, as the foregoing observations had occurred in experiments made principally to obtain new and improved varieties of the pea, for garden culture, I chose, for a similar purpose, the more hardy varieties usually sown in the fields. By introducing the farina of the largest and most luxuriant kinds into the blossoms of the most diminutive, and by reversing this process, I found that the powers of the male and female, in their effects on the offspring, are exactly equal. The vigour of the growth, the size of the seeds produced, and the season of maturity, were the same, though the one was a very early, and the other a late variety. I had, in this experiment, a striking instance of the stimulative effects of crossing the breeds; for the smallest variety, whose height rarely exceeded two feet, was increased to six feet; whilst the height of the large and luxuriant kind was very little diminished. By this process, it is evident, that any number of new varieties may be obtained; and it is highly probable, that many of these will be found better calculated to correct the defects of different soils and situations, than any we have at present; for, I imagine that all we now possess, have in a great measure been the produce of accident; and it will rarely happen, in this or any other case, that accident has done all that art will be found able to accomplish.

The success of my endeavours to produce improved varieties of the pea, induced me to try some experiments on wheat; but these did not succeed to my expectations. I readily obtained as many varieties as I wished, by merely sowing the different kinds together; for the structure of the blossom of this plant

(unlike that of the pea) freely admits the ingress of adventitious farina, and is thence very liable to sport in varieties. Some of those I obtained were excellent; others very bad; and none of them permanent. By separating the best varieties, a most abundant crop was produced; but its quality was not quite equal to the quantity, and all the discarded varieties again made their appearance. It appeared to me an extraordinary circumstance, that, in the years 1795 and 1796, when almost the whole crop of corn in the island was blighted, the varieties thus obtained, and these only, escaped, in this neighbourhood, though sown in several different soils and situations.

My success on the apple (as far as long experience and attention have enabled me to judge from the cultivated appearance of trees which have not yet borne fruit) has been fully equal to my hopes. But, as the improvement of this fruit was the first object of my attention, no probable means of improvement, either from soil or aspect, were neglected. The plants, however, which I obtained from my efforts to unite the good qualities of two kinds of apple, seem to possess the greatest health and luxuriance of growth, as well as the most promising appearance in other respects. In some of these, the character of the male appears to prevail; in others, that of the female; and in others, both appear blended, or neither is distinguishable. These variations, which were often observable in the seeds taken from a single apple, evidently arise from the want of permanence in the character of this fruit, when raised from seed.

The results of similar experiments on another fruit, the grape, were nearly the same as of those on the apple, except that, by mingling the farina of a black and a white grape, just as the blossoms of the latter were expanding, I sometimes ob-

tained plants, from the same berry, so dissimilar, that I had good reason to believe them the produce of superfoetation. By taking off the cups, and destroying the immature male parts, (as in the pea,) I perfectly succeeded in combining the characters of different varieties of this fruit, as far as the changes of form, and autumnal tints, in the leaves of the offspring, will allow me to judge.

Many experiments, of the same kind, were tried on other plants; but it is sufficient to say, that all tended to evince, that improved varieties of every fruit and esculent plant may be obtained by this process, and that nature intended that a sexual intercourse should take place between neighbouring plants of the same species. The probability of this will, I think, be apparent, when we take a view of the variety of methods which nature has taken to disperse the farina, even of those plants in which it has placed the male and female parts within the same empalement. It is often scattered by an elastic exertion of the filaments which support it, on the first opening of the blossom; and its excessive lightness renders it capable of being carried to a great distance by the wind. Its position within the blossom, is generally well adapted to place it on the bodies of insects; and the villous coat of the numerous family of bees, is not less well calculated to carry it. I have frequently observed, with great pleasure, the dispersion of the farina of some of the grasses, when the sun had just risen in a dewy morning. It seemed to be impelled from the plant with considerable force; and, being blue, was easily visible, and very strongly resembled, in appearance, the explosion of a grain of gunpowder. An examination of the structure of the blossoms of many plants, will immediately point out, that nature has some-



thing more in view, than that its own proper males should fecundate each blossom; for the means it employs are always those best calculated to answer the intended purpose. But the farina is often so placed, that it can never reach the summit of the pointal, unless by adventitious means; and many trials have convinced me, that it has no action on any other part of it. In promoting this sexual intercourse between neighbouring plants of the same species, nature appears to me to have an important purpose in view; for, independent of its stimulative power, this intercourse certainly tends to confine within more narrow limits, those variations which accidental richness or poverty of soil usually produces. It may be objected, by those who admit the existence of vegetable mules, that, under this extensive intercourse, these must have been more numerous; but my total want of success, in many endeavours, to produce a single mule plant, makes me much disposed to believe that hybrid plants have been mistaken for mules; and to doubt (with all the deference I feel for the opinions of LINNÆUS and his illustrious followers) whether nature ever did, or ever will, permit the production of such a monster. The existence of numerous mules in the animal world, between kindred species, is allowed; but nature has here guarded against their production, by impelling every animal to seek its proper mate; and, amongst the feathered tribe, when, from perversion of appetite, sexual intercourse takes place between those of distinct genera,\* it has, in some instances at least, rendered the death of the female the inevitable consequence. But, in the vegetable world, there is not any thing to direct the male to its proper female: its farina is carried, by winds and insects, to plants of every different

\* This is said to be the case with the drake and the hen.

genus and species; and it therefore appears to me, (as vegetable mules certainly are not common,) that nature has not permitted them to exist at all.

I cannot dismiss this subject, without expressing my regret, that those who have made the science of botany their study, should have considered the improvement of those vegetables which, in their cultivated state, afford the largest portion of subsistence to mankind and other animals, as little connected with the object of their pursuit. Hence it has happened, that whilst much attention has been paid to the improvement of every species of useful animal, the most valuable esculent plants have been almost wholly neglected. But, when the extent of the benefit which would arise to the agriculture of the country, from the possession of varieties of plants which, with the same extent of soil and labour, would afford even a small increase of produce, is considered, this subject appears of no inconsiderable importance. The improvement of animals is attended with much expence, and the improved kinds necessarily extend themselves slowly; but a single bushel of improved wheat or peas, may in ten years be made to afford seed enough to supply the whole island; and a single apple, or other fruit-tree, may within the same time be extended to every garden in it. These considerations have been the cause of my addressing the foregoing observations to you at this time; for it was much my wish to have ascertained, before I wrote to you, whether in any instance a single plant can be the offspring of two male parents. The decision of that question must of necessity have occupied two years, and must therefore be left to the test of future experiment.