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T. Woolnoth



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V. *On the Production of Hybrid Vegetables ; with the Result of many Experiments made in the Investigation of the Subject. In a Letter to the Secretary By the Hon. and Rev. WILLIAM HERBERT, D. C. L. F. H. S. &c.*

Read December 21, 1819.

MY DEAR SIR,

IT was your wish, that I should address some further communications to the Horticultural Society, concerning the production of *Hybrid Vegetables*. It would, perhaps, have been more advisable, to have waited till points, that are still doubtful, could have been accurately investigated ; but, according to your request, I will report the progress of my experiments, and explain my ideas on the subject. It opens a wide and interesting field : the vegetable kingdom may certainly be greatly enriched by artificial intermixtures, and I think that new plants may be so formed, which will be capable of reproducing themselves, as distinct generations, by seed, in the same manner as natural species. I am aware of the opinion of our worthy President, who conceives that the production of a fertile offspring, from two plants of species supposed to be distinct, is in itself a decisive proof, that the parents are really of one species, and that Botanists have been in error concerning them ; or, in other words, that fertility is evidence, that the plant is not really hybrid, but produced by the intermixture of two varieties of one species. I am, however, satisfied, from the progress I have already

made, that several plants, which I have raised, are not only, in the fair sense of the word, hybrid, but also fertile ; and if they should perpetuate themselves by seed, without reverting to the form of either parent, they will be entitled to be considered by the Botanist as distinct species.

If it is meant only that a fertile offspring may be supposed to intimate, that the two parent plants have branched out from one common stock since the creation of the world, I am fully disposed to admit the truth of that position ; but I should go much further, considering that many species, which we cannot now, by artificial means, prevail upon to intermix, have also descended from one original ; and I doubt very much whether such a multiplication of distinct species may not also have taken place in the animal and insect tribes ; but, to produce an intermixture between species that may have so diverged, the will of the animal must consent, while that of the plant need not be consulted.

I suspect that in the early periods of the world, there existed only the distinct genera of plants, or heads of families, not, however, exactly according to the present divisions of Botanists ; who, indeed, are perpetually at variance with each other, as to the features which are sufficient to constitute a variety, a species, or a distinct genus. The lapse of centuries and diversity of soil and climate have probably wrought the most wide and permanent distinctions between vegetables, that have originated from a common stock, possibly even between the arborescent Ferns of the Andes, and the herbaceous inhabitants of our forests ; but I should neither decide, if I found it impossible to produce a fertile offspring from the intermixture of any two plants, that they must have been

distinct from the commencement of the world ; nor, if I did succeed in procuring it, that they must not at present rank as distinct species of vegetables ; I should merely think, as the Botanist would probably have already decided, that they were referable to one genus, or family. I apprehend the whole mystery to be this ; that, in the progress of the distension of genera into various species, some species have retained such affinity as to admit of easy artificial intermixture ; others have preserved so little affinity as to render their union more difficult ; and others have departed so widely from their prototype, that the art of man cannot now reconcile them, and that, to bring them together again, it would be necessary to tread back or reverse the process of the centuries that have worked their difference. To which may be added, that in some plants the variation may be striking in many external appearances, and yet less considerable in the parts of fructification than in other species, of which the leaf or corolla may be more similar. If it be admitted, that diversity of species could have been produced by variations of soil, temperature, or humidity, it will be readily understood that such diversity might have been further multiplied by hybrid intermixture, as the species were brought together by the natural progress of their diffusion.

Mr. KNIGHT has raised fertile Strawberries from the mixture of the Chili, the Carolina, and the Scarlet Strawberries, which some persons had deemed to be distinct. But this division was probably erroneous, for we find that they will intermingle naturally, and become confounded by seminal variations, without any artificial impregnation, and therefore it was a pretty clear case that they had been improperly

separated. On the other hand, Mr. KNIGHT has failed, as yet, of producing a fertile plant between the American and European Strawberries : but are we to conclude from that circumstance, that plants, so nearly allied as the Scarlet and Alpine Strawberries, were from the creation of the world distinct, and at the same time hold that all the African Gladioli, between which (permanently dissimilar as they are in appearance) I can raise fertile intermixtures, were originally one species? Such an opinion would be so paradoxical, that it would require to be supported by very clear proof; and yet it would be difficult, by experiments, in any manner to confirm it.

It is not even true that all mules amongst animals are entirely sterile. There are well attested instances of the fertility of the mule between the ass and the mare; and mules between the gold-finch and canary-bird, the distinction of which no naturalist could deny, are frequently known to breed; and I understand that all the more beautiful mules are produced by a second cross with the canary-bird. I have known the mule siskin couple with a canary-bird; and I understand that the mules between two very distinct birds, the common pheasant and the silver pheasant, have proved fertile. The analogy, therefore, of animals, if such could be relied on, in considering the generation of vegetables, would not completely establish fertility as the test of a common origin. The organs in mule animals are said to be perfect, and the cause of their rarely breeding is not ascertained, though it is perhaps a sluggishness of constitution, as the equine mule is said to be more fertile in warmer countries. I suspect also, that the plants which thrive in a high tempe-

nature, will intermix more readily than those of colder regions. Strawberries, especially Hautbois, in their natural state, are often sterile.

Many plants, which Botanists have considered distinct, are certainly not so; as, for instance, *Ixia* (or *Tritonia*) *crocata*, of which seminal varieties have been erroneously named *I. squalida*, *I. miniata*, *I. fenestrata*, and *I. deusta*; *Ixia flexuosa* and *I. polystachya*, are the same; *Babiana stricta*, *B. villosa*, *B. sulphurea*, and *rubro-cyanea*, are not distinct; I have had a natural seedling from *Babiana sulphurea* with a pale eye like that of *B. rubro-cyanea*. I raised from the natural seed of one umbel of an highly matured Red Cowslip, a Primrose, a Cowslip, and Oxlips of the usual and other colours, a Black Polyanthus, a Hose-in-Hose Cowslip, and a natural Primrose bearing its flower on a Polyanthus stalk. From the seed of that very Hose-in-Hose Cowslip, I have since raised a Hose-in-Hose Primrose. I therefore consider all these to be only local varieties, depending upon soil and situation. I have raised a powdered Auricula and a *Primula Helvetica* from the seed of *P. nivalis*; and I have raised a *Primula Helvetica* also from *P. Viscosa*. I therefore esteem these Swiss Primulas to be local varieties of one species. The *Violas* are proved by cultivation to have been too much divided. The great Hearts-ease, which adorns Covent Garden market, under the name of *Viola grandiflora*, is found all yellow in Craven in Yorkshire, under the name of *Viola lutea*; with large dark purple flowers, without any yellow, in the neighbourhood of Moor-rig, above the falls of the Tees, in the county of Durham; and with mixed purple and yellow flowers,

under the name of *Viola amœna*, in Weardale, a few miles from the last named place. The seeds gathered in Teesdale from the dark purple Hearts-ease once produced a dirty purple and yellow flower in my garden. These are therefore only local varieties, which by their uniformity in their natural abodes have misled the Botanist.

I believe the Orange, Citron, Lime, Lemon, and Shaddock, to be varieties of one plant. I do not, however, consider that Mr. KNIGHT's experiment* has proved the Almond and the Peach to be one species. The Peach is extremely similar to the Almond, with the exception of the sweet pulp, which may be, very probably, the effect of cultivation; and, if any amelioration of the pulp could be produced in seedling Almonds, I should incline to think that a long course of cultivation might have improved the Almond into a Peach. But the production of a fruit resembling a Peach, from an impregnation of the Almond with a plant so very similar, only shews that in an intermixture between two plants, which have such close affinity, the type of the male (as is frequently the case) has been very conspicuous; and this, even if the Peach had been known to have grown wild, with a sweet pulp, before the deluge, would not have surprised me.

The science of the Botanist, at the best, is very unstable, because it is entirely a science of conjecture, liable, at all times, to be overset by the test of cultivation. He carefully observes in plants the features that are least liable to variation; and by their means is enabled to subdivide the Classes, Genera, and Species of vegetables; but experience sometimes shews that the features, on which he relies, are

* Horticultural Transactions, Vol. III. page 1.

very variable. Rhododendron and Azalea belong to two classes, widely separated by the number of the anthers, which is the characteristic feature of those classes ; yet they are found to breed so freely together, and accord so exactly in the seed and capsule, that it can scarcely be doubted that they have branched from one original stock. Indeed, the Azalea is reported to have been occasionally seen with ten anthers, instead of five. At the same time, I doubt the possibility of intermixing the Alpine Rhododendrons with those of Pontus, India, and America, although united in one genus.

The most distinguished Botanists are perpetually at variance with each other, as to the subdivisions of the vegetable system ; nor has any precise meaning been affixed to the terms by which they are known, as a guide to their labours ; and, if we ask what is an Order ? What is a Genus ? What is a Species ? What is a Variety ? We shall find the answers very unsatisfactory. The most rational interpretation of the terms, I think, will be found as follows : *An Order*,—all the genera or original stocks which have general affinities to each other, though not such as to warrant a belief that they have branched from one stock. *A Genus*, all the species which have peculiar affinities, distinguishing them from all others ; and which, I think, render it probable that they have branched, since the creation of the world, from one original. *A Species*,—a race of plants that will, in the present state of the world, perpetuate itself without varying in essential particulars, so as to confound itself with any other. *A permanent or local Variety*,—that which will perpetuate itself in a particular form, if kept in its native soil or situation, or at a distance from all other varieties ; but which would, other-

wise, confound itself with them. *An accidental Variety*,—that which cannot with certainty be perpetuated by seed in any situation. I do not believe that a better definition for the purposes of science can be given; and, if Botanists attended to it, their classifications would not be liable to such perpetual variety and contradiction: but, at all events, the experience of the Cultivator must always have weight to supersede the conjectural decisions of the Botanist.

The colour of the flower is one of the most uncertain features; and yet, if immutable through successive generations, it is just as sound a botanical distinction as any other, whilst pubescence is a feature to which more weight is attached, and yet we sometimes find it as variable as colour. For instance, as to *Colour*, the beautiful *Convolvulus varius*, figured in the *Botanical Magazine** as a variety of *C. purpureus*, is very similar to that plant in every thing but the colour of the flower. The seedlings of *C. purpureus* vary with every shade of purple, red, and white, having always five spots at the mouth of the tube, but no variation of colour is ever seen in the different flowers of the same seedling. *Convolvulus varius* has the opposite peculiarity; the plants which I have raised, through twelve or thirteen generations, differed not in the least from each other, but it would be difficult to find two flowers upon any plant exactly alike in the marks of colour, but they never have the five spots in the tube; and the natural ground is a sulphureous white, with the inside of the tube pale purple, and the flowers are irregularly streaked with dark blue, in infinite diversity. Sometimes an accidental flower, like a run Carnation, will have

* Plate 1682.

the ground entirely blue, with streaks of a deeper colour, but no instance of a plant raised from its seed, with all its flowers alike, or spotted at the mouth of the tube, has been seen; and, therefore, it appears to be as truly a distinct species, as if it had been distinguished by any other permanent feature. Permanent colour is also a principal feature, in distinguishing the species of *Anagallis*. On the other hand, as to *Pubescence*, the lovely *Echites suberecta*, does not appear to vary in colour, but in a pot of seedlings, I have found one with the stems hairy, whilst the rest have them smooth. If that difference had been found in a wild specimen, with some variation in the colour of the flower, the Botanist would have named it a distinct species, until the experience of Cultivators had shewn the difference to be uncertain.

With respect to the fertility or barrenness of mule vegetables, there is some mystery which I cannot at present and perhaps never shall be able to develop. All the mules I have raised between the African *Gladioli* have proved exceedingly fertile, indeed certain of producing good seed from every flower; yet the *Gladioli cardinalis*, *blandus*, *tristis*, *hirsutus*, and *recurvus*, from the intermixture of which some of those mules have been produced, are plants exceedingly dissimilar to each other, and could not possibly be considered as varieties of one species. I have found no difficulty in crossing one of the mules produced from these, with any third species: but the European *Gladioli* (which have not winged seeds, and ought to be considered as constituting a distinct genus) have as yet refused to breed with the African sorts. On the other hand,

the hybrid *Crinum Govenium*, figured in the *Horticultural Transactions*,* has not produced any seed, either from impregnation with its own dust, or with that of other *Crinums*, although I applied that of *C. speciosissimum* and *C. brevifolium*. Its stigma was, however, quite perfect, and furnished with the usual tubular fringe, and the particles of its pollen, examined with the microscope, though not very abundant, appeared to be furnished with the viscous juice which I consider to be the proof of fertility. I did not think of examining the interior of the germen till it had begun to turn yellow, but it appeared then, as if there had been a deficiency of the embryo seeds in the germen, and therefore an incapacity of bearing seed. Next summer I shall examine the germen while it is fresh, and try whether the pollen of this hybrid *Crinum* will fertilize any other. If the mule *Crinum* should prove to have no embryo seed at all in its young germen, its apparent sterility would be at once explained.

The American Azaleas do not produce seed abundantly in this country, and one reason for this is that they are frequently entirely deficient in pollen, in consequence, perhaps, of the dryness of our atmosphere or soil in spring, compared with that of America. In the second week of last May I was desirous of impregnating *Rhododendron Azaleoides*, which had no pollen of its own, with that of some Azalea, but I could not find a single Azalea flower that had any pollen. I touched its stigma therefore with the dust of *Rhododendron Catawbiense*. The capsules swelled, and in August they appeared in fair progress to ripen the seed, but owing to the extraordinary drought (the plant not having been

* Vol. III. page 187.

watered) the pods fell off; but they had remained on long enough to shew that the germen had been apparently fertilized; those to whose stigma the dust had not been applied, having withered long before. From this it should seem that the ovarium is not defective in that mule, and that it would probably be fertile in America. My own mule *Rhododendrons* have pollen, though not abundant, and I think I should have obtained seed from them this year, if their roots had not been injured by too much water in the pots.

I suspect that the germen is very speedily fertilized when the dust has touched the stigma; but I doubt whether, after being fecundated, it is closed against any further impression. I have a pot full of seedlings from a pod of *Crinum Capense*, of which the stigma was touched first with the dust of *C. crubescens*, and several days after with that of *C. scaberrimum*; and by their present appearance I think they are intermediate between the mules produced by the dust of either separately; but it is impossible yet to speak with confidence. Mr. KNIGHT has assured me that by touching the stigma of a smooth Cabbage with the dusts of a curled and of a red Cabbage, he had given both the curl and the red colour to the seedlings; but I am uncertain whether both dusts were applied at the same time. It would be very desirable to ascertain by experiments how long after the stigma had been touched with pollen, a second impression could be given. The summer before last I wished to try the possibility of crossing a plant of *Paneratium litorale* which had twenty-one buds on a stem, with *Crinum*,* *Nerine*,

* See *Botanical Magazine*, 2113, and 2124.

and Coburgia. The first flower had expanded before I had taken out its anthers, and though I could not distinguish any dust on its stigma, wishing to make my experiments with certainty, I immediately cut the flower off so low, as even to take off the summit of the germen, which I thought I had destroyed, the embryo seeds being partially exposed. The anthers were successively taken out of the twenty other buds, to which various uncongenial dusts were applied in vain; and the only seeds produced were from the germen of the flower which I had so cut off, and I raised true plants of *Pancreatium litorale* from them. This proves the fecundation to have been speedily effected, unless the germen could have been fertilized by dust having actually fallen into it accidentally when I cut off the flower. I have been unsuccessful in obtaining mule *Convolvulus*, *Hibiscus*, and *Turnera*, and I attribute this to some difficulty in ascertaining the right moment for impregnating fugacious flowers. I have opened the buds before expansion, to take out their anthers, but the result has been a failure of seed. I did raise one mule between the red American *Convolvulus sepium* and *Convolvulus candidans*, but it was very weakly, and died.

Last summer I took the anthers out of two flowers of *Alstræmeria pelegrina* and touched their stigmas with the dust of a white seedling variety of the same species which stood by it, and those two were the only flowers of either of the plants which failed to produce seed. I can only explain this circumstance, by supposing that the stigmas when touched might not have been come to maturity, and that the dust upon them might have become too dry

to have any effect before the stigmas were ready to receive its impression.

I believe it is an error to imagine that pollen will always retain its fertilizing powers for months, if kept dry ; on the contrary, I have found it quickly lose its virtue entirely so the moment it became dry. The pollen consists of minute vesicles filled with a juice which is very visible by means of a microscope. The vesicles soon become dry, and though they retain their form, no juice can then be pressed from them ; nor have I ever obtained seed by means of any dust that was not fresh from the flower.

In an attempt to fecundate the English Heaths with the dust of the African sorts, I was defeated by finding that the dust was shed upon the stigmas so long before the flowers expanded, that the anthers could not be taken out effectually without cutting into the bud at so early a period as to destroy its growth. The most likely cross would have been *Erica cerinthoides* with the dust of our *E. tetralix*, but *E. cerinthoides* does not make seed at all with me. I found the same difficulty with the Crocusses. The African tubular Heaths, on the contrary, do not cast their dust at all, unless the anthers are touched by a pin or the proboscis of a strong insect, which makes them spring asunder. I have therefore been able to obtain mules from them without taking out the anthers. It has been conceived that the African Heaths consist of different genera, which might be distinguished by the shape of their pods : but I have found no difficulty in intermingling species with different shaped pods, which proves that such a division would be erroneous ; and I am

quite satisfied that it is unsafe to rely upon the outward shape of the ripe capsule as a generic distinction when its internal structure and the form of the seeds agree. I have mules from the long-podded *Erica ampullacea* and *E. Jasminiflora* with the round-podded *E. vestita-coccinea* and *E. hybrida* or *cylindrica*. I have also mules from *E. Shannonia* with *E. gemmifera* and with *E. tricolor*, of *E. ampullacea* with *E. gemmifera*, and all the seedlings of a similar impregnation are alike amongst themselves, and would at once be pointed out, by a person acquainted with the African Heaths, as new species extremely unlike their parents. These have not yet flowered. I have not yet obtained any mule between tubular and campanulate flowering Heaths, but I have not made many attempts. I think such difference of form much more likely to constitute a true generic distinction in the family of Heaths than that of a longer or rounder capsule. The unwillingness of the African Heaths to shed their dust, unless touched by a strong insect or humming bird, must render them very likely to be fecundated by the dust of neighbouring sorts: and if the hybrid offspring should prove fertile, like that of the *Gladioli*, it will be evident how it comes to pass that the species of African Heaths are so multiplied, whilst the European sorts continue unalterable. I am confirmed in this view by the information I have received, that the different African species are very local, and not at all diffused over the face of the country.

Seeds, as it is well known, are originally existing in the germen, and during the time of the expansion of the flower, as the stigma advances to maturity, which often takes many

days, the germen and seeds continue to grow, and in some genera they increase exceedingly in size before the stigma becomes perfect, soon after which period the germen generally ceases to grow, and, unless it receives the congenial dust, it will fail. The difficulty is to explain how the seed can have power to draw from the plant the nourishment which is necessary for its growth, to a certain point, and yet be unable to continue to derive the support which is requisite for bringing it to maturity. I suspect the fact to be, that as long as the style remains fresh, the seed receives a portion of its nourishment by a return of sap from the style, and stigma; and thus continues to advance rapidly in growth, without any fecundation: but I apprehend that, during that period, it is only that part of the seed, which is to form the cotyledon, or seedling leaf, that grows, and that the actual germ of the young plant does not exist completely till after the fecundation of the stigma, when I conceive it to be actually formed by an union of the substance transmitted through the vessels of the style, and that which was already within the cotyledon, and thus to partake of the type of both parents. If the fecundation only gave the embryo a stimulus to excite it to draw nourishment, as I have heard suggested, the type of the male would not be conspicuous, as it is in the offspring, nor do I see how it could at all be impressed. I am led to this opinion the more, by finding that seeds apparently perfect, may be produced where the stigma has not been touched with pollen, or with pollen from a plant not sufficiently allied; but that on opening such seeds, there is a total deficiency of the germ, the seed being an inert lump, which cannot vegetate. The sprout or germ in the fleshy green

seeds of *Crinum*, *Nerine*, and the true, or occidental *Pan-
cratium* (for those of * Europe, and Teneriffe are a distinct
genus, with small black seeds, like *Narcissus*) is like a sharp
skewer lying in a narrow cavity, and as it grows, the sharp
pointed end pierces the fleshy substance of the cotyledon,
and entering into the earth it deposits at its point the young
bulb into the ground, at some distance from the seed; nor
can it be conjectured from the outward appearance, on
which side of the seed the sprout will issue. I have had from
Nerine (*Amaryllis*) *undulata* seeds even of unusual size,
though not always exactly of the natural form, when the
stigma had been deprived of all the pollen, but they never
vegetated; and after a certain period, they turned yellow
and decayed. In like manner I have been repeatedly de-
ceived, by imagining I had obtained hybrid seed, having

* I do not think there would be any chance of obtaining mules between the
occidental *Panocratiums*, and those of Europe, Teneriffe, and Asia. The Euro-
pean sorts might perhaps breed with the Asiatic *P. Triflorum*. (*Verecundum*
of the *Botanical Register*, Plate 413) which has from twelve to thirteen small
round seeds in each cell of the young germen, that are probably similar to those
of the European kinds. It is very singular, that although that plant is abun-
dant, and apparently indigenous, in Bengal, it has not been known to ripen its
seed there. I can only account for this, by supposing it to be a native of shady
hills to the North, from whence its seeds may be brought down into the meadows
of Bengal by the floods. I mentioned in a former communication † the diffi-
culty attending its culture here. I have since succeeded in flowering it with
certainty, by leaving the bulbs dry during part of the winter, and starting them
in a warm but completely shaded situation, early in the spring, when the flower
stems and leaves sprout together. It will not, at any period of the year, bear
exposure to the sun.

† Horticultural Transactions, Vol. III. p. 190, note.

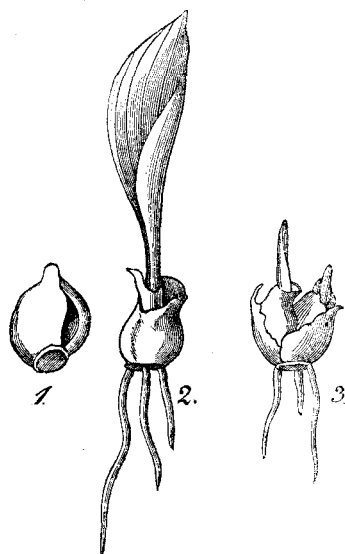
touched its stigma with dust from plants of other genera ; but finding them always fail, I cut several open as soon as they were ripe, and instead of the sharp pointed sprout within, I found an empty cavity where it should have been placed. It appears therefore that the cotyledon of the seed may be brought to tolerable perfection without fecundation, but that the germ of the seed will not be formed without the quickening dust of the male. In the same manner, I apprehend, that the egg of a bird, which has not been fecundated, contains all that is analogous to the cotyledon, and serviceable for the support of the embryo in its earliest stage ; but that the embryo itself is not completely formed, from want of the union of another requisite substance.

I have lately had an opportunity of observing what appears to me to be a singular phenomenon in nature. Having had reasons to consider, that the plant, known and figured in the *Botanical Magazine* (Plate 1419), as *Panocratium Amboinense*, belonged to a distinct genus, I was anxious to see its seeds, which had never been perfected with me ; and I had written to a friend at Calcutta, to request that he would send me the perfect capsule and ripe seeds. I received for answer, that the plant never produced seed at Calcutta, but that Dr. ROXBURGH had once seen it ; and from some resemblance to that of *Crinum*, he had called the plant *Crinum nervosum*, in the *Hortus Bengalensis*. I had, however, this autumn, an opportunity of seeing the seed, from a bulb I had given to Lord MILTON, which stood on a back flue in a very shaded situation ; and to my great surprise, instead of real seeds, it had produced perfect tunicated

bulbs. The flower stem was unfortunately broken off by an accident before the capsule appeared to be quite ripe ; and on opening it three bulbs were taken out by Lord MILTON's Botanic Gardener, Mr. COOPER, who supposed them to be seeds not perfectly mature, and laid them (as I had generally advised him to do with *Crinum* and *Pancreatium* seeds) on the surface of the mould in the pot of the parent plant. I regret exceedingly not having had the opportunity of opening the capsule myself, but Mr. COOPER, on whose accuracy and intelligence I have found every reason to depend, asserted positively, that there was no other body in the seed vessel (for I suspected that the bulbs must have been attached to withered seeds, that had sprouted prematurely in the capsule), and that the three bulbs were placed exactly in the usual manner of seeds, for which he had mistaken them. Lying in my stove upon the earth, they soon struck fibres into it, as a common *Hyacinth* bulb would have done ; and after some time, a young leaf sprouted out from the centre. The only peculiarity in which these little bulbs differed from offsets was, that the two outer coats were split on one side ; I pulled off the outer coat of one of them, without injuring it, and it has sprouted since ; and (which is very remarkable,) another of them, before it sprouted, produced within the outer coat, which shrivelled, an offset as big as itself ; or rather, it divided itself into two twin bulbs of equal size, as old bulbous roots often do.

The annexed figures represent the young bulbs above described. 1. is the bulb as first taken from the capsule. 2. the same after it had vegetated, the outer coat having

been stripped off sometime before, for the purpose of examining it; and 3, is another just beginning to sprout, containing twin bulbs within the outer coat, which is withered.



This is the first instance I have known, or heard, of an embryo, either in the vegetable or animal kingdom, drawing its support directly from the parent, without the intervention and assistance of an intermediate body, such as the cotyledon, the yolk of the egg, or the placenta, to afford it nourishment. I have indeed found one *Oxalis* from the Cape of Good Hope (very much resembling, if not the same as, the *Oxalis glandulitega* of the *Paradisus Londinensis*, Plate 66) to be viviparous; but in that plant the young germ is furnished with the usual cotyledon, though the seedling leaves are fully expanded when the seed (which is ejected, like that of Balsams, by a jerk) falls upon the ground; that amounts, therefore, only to a habit of premature vegetation in the seed, and

is not at all analogous to the singular production of perfect tunicated bulbs in the seed-vessel without the intervention of any alimentary seminal substance. The plant hitherto called *Pancratium Amboinense* has never manifested any disposition to bear bulbs on the stem or leaf, like some *Liliums*; nor, as far as I know, has any instance been observed of such plants as are otherwise bulbiferous, producing bulbs within the seed vessel.

The particles of the pollen of *Amaryllis*, *Crinum*, &c. seen through the microscope, are vesicles in form like Cucumber seeds, but more plump, and less acute. The stigma of *Amaryllis* proper* is covered with long, slender, transparent tubes; that of most *Crinums* with short conical tubes, exactly like miniature cones of a volcano. By these, the vesicles of pollen are arrested; and, from the manner in which I have seen torn and empty vesicles adhering to them, I suspect that, by some power of contraction, the tubes may break them; but at all events the particles are full of a clear liquid, which looks, in the microscope, like the finest white honey, and it seems that this liquor must be conveyed by the tubes into the style, and through its vessels to the seeds, which are severally attached, by a sort of umbilical chord, to its base. I attempted, by examining transverse sections of the styles of *Crinum* and *Amaryllis* in the microscope, to ascertain whether there were three separate communications from the three lobes or corners of the stigma to the three cells of the seed vessel; but I could not discern *any* communication, the whole appearing to be a spongy substance, so minutely reticulated, that its pores could not be clearly de-

* See Botanical Magazine, Plate 2113.

finer by my strongest magnifiers. Perhaps the examination of styles in a drier state, especially the larger styles of *Liliums*, may furnish a more satisfactory result. It is possible that the very minute vessels in the style will only permit the passage of such particles of pollen, as agree in shape with the vessels themselves; and that by such a wise precaution the dust of plants, which are not closely allied, may be excluded from fecundating the seed. It might be easily ascertained, by cutting off two of the lobes of the stigma of *Gladiolus*, whether the one remaining would be able to fertilize the whole seed-vessel, or only one of its cells; and even by cutting off part of that lobe, whether the remaining half would fertilize all the seeds in a cell, or only a portion of them. I do not believe it has been ever ascertained with respect to the plants that vary most from seed, as, for instance, Larkspurs, whether varieties of different colour are produced from seeds that lie in the same cell of the capsule.

A late observation gives me further reason to suppose that a second impression may be conveyed to the seed, and that dust falling upon different parts of the stigma, may fertilize different seeds, probably in different cells. Having been absent from home the second week of August last, I had directed my gardener to take the anthers out of three flowers on a weak stem of a seedling *Crinum Capense*, and to impregnate the first with the dust of *C. brevifolium*, the two latter with that of *C. defixum*. This was done accordingly, but he had neglected to take the anthers out of the latter flowers before their expansion, and a little of the pollen came off upon his fingers, though he trusted that none had touched

the stigma. On my return home the day after the last flower had been touched with the dust of *C. defixum*, having an opportunity of using the pollen of *C. speciosissimum*, I superadded it on the stigma of the last blown flower, which alone was still fresh. The three flowers produced seed; and the result is, that those of the first pod, which were impregnated with *C. brevifolium*, have produced plants quite distinct, but like to each other, with bright green leaves, deeply channelled quite to their point; whereas natural seedlings of *C. Capense* have their leaves glaucous, and flat on the inner side, without any channel. The second pod contained only four large seeds, of which one has not yet vegetated, and perhaps will be found to contain no germ; the other three seeds produced plants, of which two are bright green, so roundly channelled near the base, as almost to form a hollow cylinder by the meeting of their margins, but they become flat near their points, and are undoubtedly the hybrid offspring of *C. defixum*; but the third plant has flat glaucous leaves, and appears to be a natural *C. Capense*, produced in the same capsule by some particle of the dust that had escaped: as the pods of *Crinum* become one-celled before maturity, it could not be ascertained whether these seeds were formed in different cells originally or not. The last capsule has produced three green seedlings, channelled near the base only, but much less so than the former; also five or six very slender and weakly plants with flat green leaves; and one vigorous plant, with leaves coming nearer to the natural appearance of *C. Capense*. It is therefore pretty evident that different sorts of dust can act upon one capsule.*

* February 18, 1820. The mule *Crinums* raised last September, having been

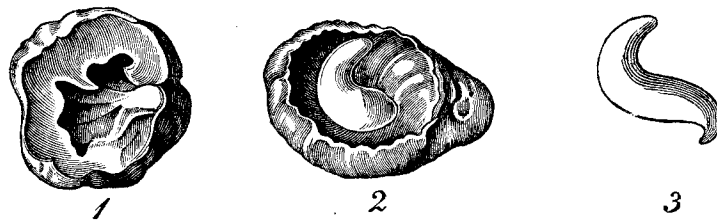
There are above forty known species of *Crinum*, with considerable diversity of form, stature, and colour ; and I have reason to think that *C. Capense* will not refuse to breed with any of them, as it mixes indifferently with those that have spherical or columnar bulbs, reflected and radiate or more companulate corollas ; and if joint impressions can be con-

recently singled out and examined, I am enabled to state some farther particulars concerning them. Those from pod 1, *Crinum brevifolio-Capense*, are quite uniform, and have formed no spherical bulb under ground, but have the slender columnar stem of the male parent. From pod 2, the two seedlings that were evidently *Crinum defixo-Capense*, thrive fast, as above described, and have formed small bulbs, in which respect they also conform with their male parent ; the third seedling, which had the semblance of a natural glaucous *Crinum Capense*, retains that appearance, the young bulb having split into two plants (in the same manner as the young *Pancratium Amboinense* mentioned before), but both are very weakly. From pod 3, the three first noticed seedlings are vigorous, and decidedly *Crinum speciosissimo-Capense*, and conformably with the habit of the male parent, have acquired minutely scabrous margins to their leaves, and have already formed bulbs as big as a goldfinch's egg ; the seedling, described as being vigorous, apparently a natural *Crinum Capense*, thrives also well ; but the other plants from that capsule, became like natural seedlings of *Crinum Capense*, though they were very weak, and all died in the winter except two, which are still very small and slender ; it looks as if the small quantity of the natural pollen which must have fallen upon the stigma, had conveyed its main impression to one seed, and barely supplied to the other seeds the basis of life, but not in sufficient quantity to enable them to germinate with vigour. The fourth seed in the second capsule of *Crinum Capense* impregnated by *Crinum defixum*, on examination this morning, proved, as I had conjectured, to be empty, and furnished with no germ or embryo. I have sent it, together with a natural seed of *Crinum defixum* to shew the proper form and position of the germ, to the Society ; it appears to me that this maturation of all that part of the ovula, which is necessary for the support of the embryo, without the existence of the embryo plant itself, when the congenial pollen had been wanting, is an

veyed, as seems to be the case, by blending the pollen of different sorts, the variety of produce may be almost incalculable.

I mentioned in a former communication,* that I had obtained a mule between *Crinum Capense* and *Pancratium distichum*; but I have since looked upon the plant with great suspicion, because I have never before or since succeeded in intermingling species of these two genera, which I considered fundamentally distinct, according to my own division

important point in the natural history of the formation of all living things, and, as far as I know, heretofore only observed amongst birds.



No. 1. of the above figures represents a section of the mule seed of *Crinum Capense*, wanting the internal germ or embryo; No. 2, represents the natural seed of *Crinum defixum* cut open, to shew the position and form of the germ, and No. 3, is the figure of the germ taken out of the seed. I may take this opportunity of mentioning, that seeds of *Crinums*, especially the African *Crinum giganteum*,† though large and firm when gathered, become soon after soft and mouldy in the driest atmosphere. I have discovered that this circumstance was owing to their having been insufficiently ripened, and have stopped the progress of the decay, either by laying them in the shade upon moist earth, or on a pot of moist sand upon the flue, covering them for a time with a glass, by which means I at last succeeded in obtaining a seedling from *Crinum giganteum*.

* Horticultural Transactions, Vol. III. page 196.

† Botanist's Repository, Plate 169. *Amaryllis ornata* β. of the Botanical Magazine, Plate 923*.

of the Amaryllideæ, as given in the *Botanical Magazine*,* and I am now persuaded, from the appearance of the plant, that the seed must have been fertilized several days after the application of the dust of *Pancreatum*, by the accidental escape of the dust of *Crinum canaliculatum*.

I was desirous, last August, of impregnating a mule *Gladiolus* with the dust of *Watsonia fulgens* and of *Amaryllis crocata*, thinking that the true occidental *Amaryllises* were, from the form of their stigma, capsule, and seeds, more likely to breed with *Gladiolus*, than with those species of the *Crinum* family, which have been confounded with them under the name of *Amaryllis*. The *Gladioli* in pots having done flowering, I chose the two last flowers that remained to expand in the open borders, having destroyed the buds that immediately preceded them, so that at the time of their expansion, there was no other fresh *Gladiolus* flower, nor any appearance of fresh pollen in the garden. The result is, that both flowers produced small distorted pods, containing a few seemingly good seeds. I have sown them; but I expect to find that they were fecundated by some particles of *Gladiolus* dust, from the half withered flowers, brought by the wind or bees, because I have no expectation of being able to intermix genera which are really distinct, and the experiment was made with a view to a negative † result.

* *Botanical Magazine*, 2113, 2121, and 2124.

† February 9, 1820. The various mule *Gladiolus* seeds obtained last summer, were sown in separate pots, and placed in the stove. All except those supposed to have been produced by the dust of *Watsonia* and *Amaryllis*, sprouted long ago; three or four of those from *Watsonia* began to grow sometime after; but

An idea is somehow prevalent, that if vegetable mules are fertile, their offspring will revert to the similitude of the original female parent. This appears extremely improbable, and, if true, almost inexplicable ; I have not yet flowered a sufficient number of seedlings from mules to speak confidently from experience, but I have no reason, as yet, for believing it. The only one of my seedlings from Johnson's *Amaryllis Reginæ-vittata*, which has yet flowered, was in every point precisely similar to its hybrid parent, which seems thereby to perpetuate itself as a distinct species. Great caution will be necessary with respect to the supposition of seedlings from mules reverting to the likeness of the original parent, because it is quite certain, that mules which are fertile may be fecundated by the dust of either, and especially of the female, parent ; and, if it is at all within reach, the dust may be brought, unperceived, by the wind or insects. The bees were so busy last summer amongst the different *Gladioli* that were growing in the same quarter of my garden, that if some of the seedlings from my mules were to appear like the original female parent, I should have no confidence that they might not have been accidentally fecundated by its dust, especially as the stigma of *Gladiolus* begins to expand before its anthers.

those from *Amaryllis* continued dormant, in four separate pots, till yesterday, when one seedling made its appearance, and the other seeds appear to be in motion. The seedlings are all in too young a state to judge from appearance of their parentage ; but if the last should prove to have been produced by the dust of withered *Gladiolus* flowers, brought by the wind, it will seem that such obsolete pollen produces not only imperfect pods, but seeds, of which the vegetation will be more tardy.

Mules are not often produced naturally, at least in Europe, (except in gardens, where plants are brought together, which could not otherwise have mingled) perhaps because all the combinations that were likely to occur in the native situations, have been made centuries ago, and have taken their place in the catalogue of species; whereas local species, which may have been produced by diversity of soil or climate, are often brought in contact by cultivation. The only sterile mule, in a wild state, of which I have heard, is *Centaurea hybrida*, which grows upon a hill close to Turin, where it is supposed to be produced by the frequent intermixture of two species of *Centaurea*, and to bear no seed itself. *Ranunculus lacerus*, also sterile, has been produced accidentally at Grenoble, and I believe near Paris, by the union of two *Ranunculi*; but this occurred in gardens. I cannot learn that any attempt has been made to touch their stigmas with their own dust, or that of one of their respective parents; or to ascertain whether there is any deficiency in the pollen or ovarium.

I have already mentioned that the stigma of Liliaceous plants is furnished with a fringe of transparent tubes, by which the juice of their own dust is probably taken in; but I have occasionally observed in *Crinums* one or two such tubes on the side of the style, and even close to its base, which is concealed within the long tube of the flower: and I see no reason to doubt the possibility of the germen being fecundated through their means, if the stigma were cut off, and the liquor expressed from the dust were to reach them. On examination of the flower of *Morea tricuspis* in a microscope, I discovered, near the base of the opposite petal,

exactly such tubes, as the part which is called the stigma in Iris and Morea appears to be furnished with ; and it seemed to me that, in that flower, all the more substantial part of the corolla might be considered as acting the part of a style.

The genus *Crinum* lends itself most willingly to the production of mules. In addition to *C. Govenium** or *Zeylanico-Capense*, *C. erubescens-Capense*, and *C. scaberrimo-Capense*, before mentioned,† I have now *C. brevifolio-Capense*; *C. defixo-Capense*; *C. speciosissimo-Capense*; *C. toxicario-Capense*? *C. Americano-erubescens*; *C. Americano-brevifolium*; *C. cruento-erubescens*; and *C. exaltato-scaberrimum*; which last will probably surpass the whole genus in beauty, as *C. exaltatum* (allied to *C. pedunculatum*) is loftier than *C. amabile*, and bears forty white flowers, while *C. scaberrimum* has decidedly the finest individual flowers of the genus, though they are not usually numerous; unfortunately, I have not yet had any plant of *C. Capense* in flower at the same moment with *C. amabile*, and therefore have not been able to attempt a mixture between them.

I have many seedlings from Johnson's mule *Amaryllis Reginæ-vittata*, and I have some mule *A. equestri-vittata*,

* *Crinum Govenium* has very short peduncles, but they are not visible without tearing off the spathe, which sits remarkably tight; the right name of its male parent, (*Amaryllis ornata* of the *Botanical Magazine*, Plate 1171) is *Crinum Zeylanicum*, and not, as I before stated, *C. Moluccanum*. The confusion arose, from Dr. ROXBURGH's having annexed a drawing of *C. Moluccanum* to his description of *C. Zeylanicum*, and the bulbs having been sent repeatedly from Calcutta, under the wrong name; Mr. KER's quotation ‡ in the *Journal of Science and Arts* is therefore right.

† See *Horticultural Transactions*, Vol III. page 196.

‡ See *Horticultural Transactions*, Vol. III. pages 189 and 190.

superior to Johnson's flower, in size and colour, with the longer tube of *A. equestris*. They make seed pretty freely, and I obtained a further cross by impregnating one of them with the pollen of *A. rutila* and *A. fulgida*. The bulb of *A. rutila* and *A. fulgida* is always surrounded by a crown of blind offsets, and the mule seedlings obtained from their dust began at five or six months old, to produce young bulbs, and every one of them has now several strong offsets adhering to the bulb; these will be a valuable acquisition.* I have

* February 18, 1820. Twenty-four bulbs having been lately sent by me to the Society, each being an offset from a different seedling of a mule *Amaryllis equestri-vittata*, crossed again with *A. rutila* and *A. fulgida*, I think it desirable to give a more particular account of them, as, from their rapid growth and increase, their free habit, and probable beauty of blossom, they are likely to become favourites in every collection, and to supplant *A. crocata* and *A. equestris*, which, though they thrive in a cool stove, appear equally impatient of too much heat and too much cold.

In March 1818, being desirous at all events of obtaining some seed from a splendid scarlet and white mule *A. equestri-vittata*, I touched the stigma of its four flowers with the dust of *A. rutila* and *A. fulgida*. I believe all the stigmas were touched with the pollen of both, but to one or two the dust of *A. rutila* was applied a day before that of *A. fulgida*, and the others were touched first with that of *A. fulgida*. The anthers had not been taken out, and before the flowers withered, the natural dust of the plant was purposely super-added, to make more sure of ascertaining whether the plant was fertile. Four capsules were ripened, each being furnished with three cells. The seeds of each cell were kept distinct, and sown in April. They vegetated vigorously, and in a few months they began to shew evidently the type of *A. fulgida* and *A. rutila*, by the production of offsets. On their examination, a few days ago, they had all, with the exception of one plant, from five to above twenty offsets. The seedling, which has no offsets, is unlike the rest, and has clearly the leaf of *A. equestri-vittata* simply, and must be the only seedling produced by the subsequent addition of the natural pollen. It was raised from the same cell with others that shew the type of either *A. rutila* or *A. fulgida*. I apprehend that

seedlings of *A. rutila* with the dust of *A. fulgida*, approaching as nearly as possible to the *A. miniata* of the *Botanical Magazine*, (Plate 1943), which is not, however, that of RUIZ and PAVON.* I have also seedlings from *A. fulgida* with *A. rutila*, and from *A. Reginae* with *A. crocata*. Seedlings of *A. rutila*, of *A. crocata*, and of *Cyrtanthus purpureus*, (which has been called *A. purpurea*,) have flowered with me at little more than two years old. Seedlings of *A. vittata* do not flower till they are at least seven or eight years old; but that from the mule *A. Reginae-vittata* flowered at the intermediate age of three years and a half, which is worthy of note, as it appears to be an intermediate specific habit. African Gladioli will flower often the second season from the seed; Sparaxides and *Ixias* frequently the first, and then stronger than when the roots are older. I have had a seedling *Sparaxis* produce nearly forty flowers on a strong branching stem, at nine months from the sowing of the seed.

Of Gladioli I possess the following mules; *G. blando-cardinalis*, *G. cardinali-blandus*, *G. angusto-blandus*, *G. tristi-blandus*, *G. floribundo-blandus*; *G. cardinali-angusto-blandus*; *G. tristi-hirsutus*; *G. ringenti-tristis*, and *G. versicolore-hirsutus*. I have this year seeds from further intermixtures, and mules may probably be obtained with endless variety of colour. These mules flower most beautifully in

those amongst the seedlings which have a strong purple stain at the base of the leaves, will prove to be the offspring of *A. fulgida*, though some may perhaps have partaken of a joint impression. Plants with green and with purple stained leaves have proceeded from the same cells, those with green leaves are probably the offspring of *A. rutila*.

* *Flora Peruviana*, Vol. III. page 57.

the open border, in a mixture of sand and peat, in patches amongst the Azaleas. It is perhaps best to take up the bulbs, and dry them, when the seed is ripe; but I have left African Gladioli unmoved for several years, in the border. I have never seen the least approximation to each other in the natural seedlings of *G. blandus*, *G. tristis*, *G. cardinalis*, *G. hirsutus*, and *G. recurvus*.

The only mule *Pancratium* I have raised, is between *P. rotatum* and *P. distichum*; I had about a dozen very fine seeds, but only one vegetated, and the rest, though above an inch in length, proved to have no germ.

Besides the mule *Rhododendron*, figured in the *Botanical Register* (Plate 195,) I have two that were produced by the accidental fecundation of a white-flowered Azalea by the dust of a *Rhododendron Ponticum*, that stood by it: one of these has produced fragrant whitish flowers very abundantly, having generally nine but occasionally ten or eight anthers. I distinguish it by the name of *Rhododendron hybridum enneandron*. I raised, three years ago, twenty-four plants from *Rhododendron Ponticum*, touched with the dust of *Azalea Pontica*, the plants having been forced in the stove, to make the experiment certain; but the seedlings were very weakly, and all died before the winter. I have young seedlings since raised, which should be mules; but I cannot rely upon them, because the dust of *Rhododendrons* is often carried by the wind to a great distance.

I have a very ornamental mule *Oenothera* from *Oe. glauca*, and (if my memorandum is right) *Oe. suffruticosa*, which produces seed and seedlings like itself.

I have not made many experiments with *Passion-flowers*,

but I think that several of them would intermix; perhaps the purple-fruited *Passiflora edulis*, and the scarlet *P. princeps*, with the hardy *P. cærulea*: this I intend to try next year.

Many species of *Pelargoniums* are known to intermix freely, and can scarcely be kept distinct in their generations. I found no difficulty in blending even the simple-leaved *P. lanceolatum* with *P. citronodorum*; but I have failed in every attempt to intermix them with either *P. tricolor* or *P. zonale*. The beautiful mule known by the name of *P. ignescens*, which derived its fine colour from the dust of *P. fulgens*, appears to be generally sterile, but I am told that it has produced a few seeds, probably from the pollen of another plant. *P. ardens*, though figured in *LODDIGES' Botanical Cabinet*,* as a native of the Cape of Good Hope, is known to have been produced in England from an intermixture of *P. lobatum* and *P. fulgens*. In the same book Johnson's mule *Amaryllis Reginæ-vittata* is also unaccountably represented † as a foreign species, under the name of *A. spectabilis*.

Much yet remains to be ascertained on the subject of hybrid intermixtures, and the propagation of new mules offers an interesting and inexhaustible source of amusement. I think that I could even make some of the natural species, by attending to their affinities; for instance, I think I could produce the curious *Gladiolus quadrangularis* (or *abbreviatus*) by an intermixture of *G. tristis* and *G. Cunonius*; and I have produced a plant hardly distinguishable from *G. versicolor*, by the union of *G. tristis* and *G. hirsutus*. I have a natural, though I believe unrecorded, Australian species of *Goodia*,

* Vol. II. Plate 139.

† Vol. II. Plate 159.

(which I call intermedia) so exactly intermediate between *G. lotifolia* and *G. pubescens*, that I cannot doubt the possibility of obtaining a similar plant by their intermixture.

An attention to hybrid productions will be found to confirm the general accuracy of the Linnean system, while it will tend to correct many errors that have been made by Botanists, and shew the necessity of paying greater attention in the classification of plants to the seed and internal structure of the capsule.

The figures of *Gladiolus tristi-hirsutus*, *G. tristi-blandus*, and *G. ringente-tristis*, annexed hereto, will afford a specimen of what may be done in hybrid productions, but I have later mules of the same genus, more exquisitely speckled, and with higher colours. Those which have been blended with *Gladiolus cardinalis* are amongst the most beautiful and conspicuous plants in my garden, and produce a blaze of bloom in the American borders, after the *Rhododendrons* and *Azaleas* are passed.

I hope that this report of the progress of my experiments, however imperfect, may throw some light upon the mysteries of the vegetable kingdom. I must refer those who may wish to intermingle species of *Amaryllideæ* to my division of the genera, in the forty-seventh Volume of the *Botanical Magazine*, now publishing.

I am, dear Sir,

Yours very truly,

WILLIAM HERBERT.

Spofforth, November 29, 1819.

POSTSCRIPT.

I have omitted to state the few observations I had made respecting the prevalence of the male or female type in hybrid vegetables. It appears that where there is a difference between the anthers and filaments of the parents, the mule may be expected to follow the male in that respect ; and on the other hand, where the distinction lies in the style or stigma, I believe that that part of the mule will be found conformable to the female parent. *Amaryllis Reginae* has the stigma less divided and more clavate than any other known *Amaryllis* ; the mule *Amaryllis Reginae-vittata* has the more trifid stigma of *A. vittata*, the female. I have seen no other mules from plants which differed in the form of the stigma or style. The mules raised from *Azalea*, impregnated by *Rhododendron*, have the ten anthers of *Rhododendron*, the male, or nearly that number. The hybrid *Crinum Govenium* has the filaments curved, like those of *C. Zeylanicum*, the male, which are much more bent than those of *C. Capense*, the female, parent. The filaments of *A. fulgida* are in pairs of three lengths ; those of *A. rutila*, a species closely allied to it, are, like those of *A. Reginae*, more irregular, sometimes of four different lengths, and sometimes of only two alternately. The mules from *A. rutila* impregnated by *A. fulgida*, have the filaments in three pairs, like their male parent. The form and colour of the leaves and corolla in mules, appear to be generally intermediate, partaking of the habits of both parents, and probably uniform or variable, according as they are more or less apt to produce seminal varieties. In the

genera *Crinum* and *Amaryllis*, the bulb of the mule appears to take strongly after the male in form and colour ; the lower part of the leaves also takes the colour of the male ; and in *Gladiolus* the sheath that incloses the bottom of the leaves and stalk, seems always to follow the marking of the male species. I have as yet seen nothing that militates against these observations ; and I think there will be found a prevailing, though probably not an invariable, disposition amongst hybrid vegetables, to conform with such laws.

With respect to the fertility of mule plants, I am disposed to think that where there is a marked difference in the form of the corolla of the parents, the mules will be sterile, and that where the distinction lies in the leaves, in the colour of the flower, in pubescence, and other such particulars, the offspring will be fertile. The genus *Crinum* consists of three divisions, those with narrow radiate or reflected petals, as *C. toxicarium* ; those with broader and more campanulate petals, like *C. giganteum* ; and those with a funnel-shaped corolla, like *C. Capense* ; *C. Americanum* being, however, intermediate between the two former divisions, and *C. flaccidum* between the two latter. I imagine that although all the species of *Crinum* may breed together, the mules produced between plants with the campanulate corolla, and those of either of the two other divisions, will be sterile. The difference of stigma in *Amaryllis Reginæ* and *A. vittata* has not however occasioned sterility in the plants produced by their union. The ovarium and even the cells of the germen of the superb *Crinum amabile* are so imperfect, that it appears, if not absolutely incapable, at least very unlikely to produce seed often, and I have no knowledge of its ever