

DESCRIPTION OF PLATE XIII.

- Fig. 1. *Stictodesmis australis*, . front view.
 2. & 3. ,, ,, side view.
 4. ,, ,, valve.
 5. *Plagiogramma costatum*, front view.
 6. ,, ,, side view.
 7. ,, *spectabile*, side view.
 8. ,, *constrictum*, side view.
 9. ,, *Atomus*, side view.
 10. *Omphalopsis australis*, front view.
 11. ,, ,, side view.
 12. *Amphiteras parvula*, . side view.
 13. *Amphiprora eximia*, . front view.
 14. ,, *lata*, front view.
 15. ,, *delicatula*, front view.
 16. ,, ,, side view.
 17. ,, *? superba*, . front view.
 18. ,, *nitida*, . front view.
 19. ,, *lineata*, . front view.
 20. ,, *? Clepsydra*, front view.
 21. ,, *? paradoxa*, front view.
 22. ,, *? undulata*, . front view.

All the figures are $\times 400$ diameters.

II. Experiments on the Fertilisation of Orchids in the Royal Botanic Garden of Edinburgh. By Mr JOHN SCOTT.

It was long and generally assumed that sterility, as variously manifested in the phenomena of hybridisation, had a special function to serve in the natural economy of organic beings; that in fact, the Author of Nature thus prevented the blending of specific types. This opinion was strongly opposed by the Hon. and Rev. W. Herbert; and in his well-known paper on Hybridisation, he adduced a remarkable array of disproving facts. So deeply rooted, however, was the counter opinion in the minds of the generality of naturalists, that these statements failed to convince them, and thus, until lately, the point at issue has been allowed to rest. The subject had not acquired a sufficient degree of importance in the minds of those who espoused Dr Herbert's view, to induce them to urge its validity; and this the more especially because its bearings were somewhat less accordant than the other view with that time-honoured,

and still by many anxiously-cherished belief, that species are the original units of creation. From this unimportant position it has now been brought out, and placed in bold relief amongst those questions of theoretical natural science, whose perfect solution, it is thought, will afford most important evidence to one or other of the opposed doctrines respecting the origin of species. Indeed we find it regarded by Professor Huxley as the *experimentum crucis* to the full acceptance of the doctrine of the "origin of species by natural selection." Thus in his "Essay on Man's Place in Nature," p. 106, he remarks, "Our acceptance of the Darwinian hypothesis must be provisional so long as one link in the chain of evidence is wanting; and so long as all the animals and plants certainly produced by selective breeding from a common stock are fertile, and their progeny are fertile with one another, that link will be wanting."

The illustrations which I am about to lay before the Society, though of a somewhat different nature from those above referred to by Professor Huxley, are nevertheless of high importance in their bearings on the evidence he requires. I refer to those cases in which certain individual plants under cultivation are absolutely sterile when impregnated with their own pollen, and yet, when reciprocally crossed with another species, each male and female sexual element is found perfectly capable of performing its function. Illustrations of this have been collected and recorded by Mr Darwin in his "Origin of Species;" and in his paper on the "Dimorphic Condition of Primula" a semi-analogous case is adduced from observations on the "long-styled" form of *Linum grandiflorum*. This latter form he finds perfectly sterile with its own pollen; nevertheless, by applying a little pollen from the "short-styled" form to the stigmas of twelve flowers, he "got eight remarkably fine capsules. The existence of plants," he well remarks, "in full health, and capable of bearing seed, on which their own pollen produces no more effect than the pollen of a plant of a different order, or than so much inorganic dust, is one of the most surprising facts which I have ever observed." In view, then, of such singular phenomena, not a few of us perhaps will be inclined to look somewhat suspiciously at that *experimentum crucis* where-

from Professor Huxley would have a decision as to the full claim Mr Darwin's theory of the "Origin of Species" has upon our acceptance. This, indeed, we can scarcely avoid doing, inasmuch as the above phenomena show us that sterility is occasionally a superinduced quality due to inappreciable though purely incidental individual idiosyncrasies. In the vegetable kingdom we have various manifestations of sterility; thus it occurs individually by a plant when self-impregnated producing no seed, both sexual elements being found good when treated reciprocally with another species, or by two individuals of a species producing much more seed when crossed than when self-impregnated;* or again, by one species of a genus fertilising another and producing fertile offspring, yet incapable of reciprocal fertilisation.† The arguments deduced from the different results in crossing and hybridising, though not entirely invalidated by these phenomena, certainly lose much of their force. The latter form, as it were, collateral links to that one *emphasized* as absent by Professor Huxley in the above extract, and thus indirectly connecting his broken chain of evidence, render perfectly admissible the belief that "Mr Darwin has demonstrated the existence of a true physical cause amply competent to account for the origin of living species."‡

I will now lay before the Society a few experiments on different species of Orchids, as farther elucidating certain of the above phenomena. Having occasion to require a few capsules from the Vandææ tribe, I carefully fertilised for some time various species which flowered in the Royal Botanic Garden here. From certain of these I had the following somewhat singular and anomalous results. On different plants of the *Oncidium sphacelatum*, I fertilised a number of flowers with their own pollen, yet in no case did I ever succeed even in causing the capsule to swell. The only external signs the flowers afforded of being affected by the pollen were the closing of the stigmatic orifice twenty-four hours or so after its application,§ and the

* Darwin's "Dimorphic Condition of Primula," Trans. Linn. Soc., 1862.

† Darwin's "Origin of Species," 3d edit., p. 280.

‡ Huxley's "Man's Place in Nature," p. 106.

§ Mr Darwin records the following very remarkable observation on the

slightly earlier withering of the flower. On the large and vigorous plant of *O. sphacelatum*, which, along with others subsequently noticed, Mr M'Nab has kindly permitted me to place before the Society, I impregnated between 100 and 200 flowers, yet every capsule aborted. I dissected the column of many of these flowers as they dropped off, and invariably observed an abundance of pollen-tubes, which in most cases I traced into the ovary. Thus if the function of the stigma be simply to excite the emission of pollen-tubes, and that of the style their conduction to the ovary, we have evidence of its accomplishment, and might then attribute abortion to some inappreciable change in the sexual elements preventing normal conjunction. It is useless, however, in the present state of our knowledge, to speculate on the cause of abortion in this as in many other cases; and I refer to it simply to show that in the above

closing of the stigmatic orifice in *Bolbophyllum rhizophoræ*—"Orchid Fertilization," p. 170: "After the flower," he remarks, "has remained some time open, the sides of the oval orifice of the stigmatic chamber close on and shut it completely—a fact which I have observed in no other orchid, and which I presume is here related to the much-exposed condition of the whole flower." This case, then, of Mr Darwin's differs from that above mentioned, and from all others which I have observed, in requiring no external stimulus for its excitation. After the application of pollen to the stigma, I have observed it in a number of species from different tribes; in absence of the pollen, however, it remained open to the last. There is thus in the closing, as here manifested, an almost conscious sympathetic action—a care that the grand end of vegetal life is not too readily frustrated by an over-sensitiveness of the stigmatic chamber, inasmuch as I find that when pollen is applied from widely distinct genera, the orifice does not move. As illustrating this, I applied pollinia from species of *Cypripedium* and *Asclepias* to flowers of *Trichopilia tortilis*; nevertheless, though pollen tubes were emitted, the stigmatic orifice did not close, which it invariably does eighteen hours or so after the application of its own pollen. In respect to the suggestion of Mr Darwin as to the special end subserved by the closing of the stigmatic chamber in *B. rhizophoræ*, I may remark that in many of the species exhibiting the phenomenon after the application of pollen, it is evidently subservient to a highly beneficial end in affording more genial conditions for the perfect development of the pollen-tubes than could otherwise be afforded in the naturally upturned and fully exposed condition of the stigmas of many of those species which exhibit the phenomenon. In others, however, it must be admitted that no such relation is evident; species whose stigmatic chamber closes being equally as well protected from physical injuries as others in which it remains open. This, however, may be simply due to inheritance with modification; at least it will be so regarded by those who believe that species are the modified descendants of previously existing species.

case it was neither due to the non-emission nor the non-penetration of the pollen-tubes, as some might otherwise be inclined to suppose.*

Having thus failed in fertilising *O. sphacelatum* with its own pollen, I determined to try crosses with other species. With this end in view, I first crossed it reciprocally with the neighbouring species, *O. altissimum*, under the impression that the probabilities for successful results would be inversely proportionate with the more or less immediate systematic affinities of the plants. In this my experiments have somewhat disappointed me; and the results of the above were the unexceptional abortion of every capsule. I next tried *O. graminifolium* with the pollen of *O. sphacelatum*, and succeeded in producing a capsule, which contained about one-fourth of embryonated seeds. I did not succeed *vice versa*. All the flowers thus treated dropped early. I may state that *O. graminifolium* does not appear from my experiments to be very susceptible of fertilisation with its own pollen.

From four flowers of *O. ornithorynchum*, impregnated with the pollen of *O. sphacelatum*, I got one fine seed-capsule, though I had never before succeeded in fertilizing this species with its own pollen; I have now a single capsule thus produced. On dissection of the crossed capsule, I was disappointed to find that it contained few seeds, and of these a great majority presented only a loose, transparent testa, entirely destitute of the embryo. The capsule, on

* Since I wrote the above, Mr Darwin has kindly sent me a copy of his interesting paper "On the Existence of Two Forms in several species of the genus *Linum*," where I find the following interesting observations on this point: "In contrast with the case of *Linum grandiflorum*," whose stigmas he previously shows scarcely at all excite the development of their own form pollen, "it is a singular fact that the pollen grains of both forms of *L. perenne*, when placed on their own form stigmas, though not causing fertility, yet emit their tubes, and these tubes I found, after an interval of eighteen hours, had penetrated the stigmatic tissue, but to what depth I did not ascertain. In this case the action of the pollen grains on their own stigmas must be due either to the tubes not reaching the ovules, or reaching them and not efficiently acting on them. He further states that in *Lythrum Salicaria* "there are three distinct forms, each of which produces two kinds of pollen; but neither pollen, when placed in its own stigma, causes fertility except occasionally and in a very moderate degree, yet the pollen-tubes in each case freely penetrate the stigmatic tissue."

the other hand, fertilised with its own pollen, though not quite so large as the above, was quite filled with seeds, of which about three-fourths presented an embryo. I did not succeed in impregnating *O. sphacelatum* with pollen of *O. ornithorynchum*, though the capsules thus treated at first showed symptoms of swelling. It is worthy of remark, that these two species, which have imperfectly fertilised each other, present colour differences, as well as great structural differences—*O. sphacelatum* having yellow, *O. ornithorynchum*, rose-coloured flowers. Now, though differences in colour are generally regarded as of little value in classifying, they seem, at least occasionally, to have an important influence in facilitating or otherwise the crossing of plants.* In such a genus, then, as *Oncidium*, where yellow so greatly predominates in the colour of the flowers, we may well look with surprise at this case of fertilisation, more especially on considering the very different results from the above experiments, with the comparatively—and in both respects—closely allied *O. sphacelatum* and *O. altissimum*.

The most successful experiments which I have made were with *O. divaricatum* var. *cupreum*, and *O. sphacelatum*. I impregnated six flowers upon the latter with pollen from the former; and I have now, as you observe on the plant before you, four fine capsules, nearly mature, as the result. I failed, however, in my attempts at crossing them reciprocally. I made also numerous attempts to impregnate *O. divaricatum* with its own pollen, and this as well upon those plants growing in baskets suspended in the hot-houses as those growing in pots. On one of the plants in the latter condition I have now, after many failures, succeeded in fertilising four flowers; the capsules, however, are very abnormal-looking productions. In one of them which I cut off for dissection, I did not find a single perfect seed; the walls of the capsule had alone been developed, and this more particularly around its apex, which, along with the column—likewise largely developed—gave it a very unnatural appearance. On the other hand, the results of experiments on plants growing in baskets, though the more natural mode of cultivating them, was, singularly enough,

* See Darwin's "Origin of Species," p. 293. 3d ed.

the unexceptional abortion of every capsule thus impregnated. The only indications of pollinic influence were an earlier fading of the flowers, and the closing of their stigmatic orifices, which I may remark is in this case effected by a gradual depression of the clinandrium, instead of a simple incurvation of the wings of the orifice, as occurs in *O. sphacelatum*, mentioned above.

Thus, in the above experiments, I have failed in crossing reciprocally any two species; nevertheless, I think I have satisfactorily shown an individual impotence of the sexual organs, in their mutual action, conjoined with a capability of normally performing their functions by the action of other species—and this in comparative disregard of recognised systematic affinities. I have yet, however, another case to state, in which we are afforded an illustration of this reciprocal action of species. It occurred with the *Maxillaria atro-rubens* and *M. squalens*. On the former of these I had made numerous attempts to fertilise flowers with their own pollen, yet in every case they proved abortive. I was thus induced to try pollen from another species. I accordingly took a few pollen-masses from flowers of *M. squalens*—certainly a very dissimilar species, but the only one in flower at the time—with which I fertilised eight flowers of *M. atro-rubens*. These have afforded me very successful results, as six out of the eight have produced well-formed capsules. I next tried pollen from *M. atro-rubens* upon flower of *M. squalens*, and have now succeeded in fertilising a capsule on the latter also. I may remark, however, that I have found this species perfectly productive when fertilised with its own pollen.

In this case, then, we are afforded an additional illustration to those extraordinary cases recorded by Mr Darwin, in which the male element of a specific individual, A, for example, will not fertilise its own female element, and yet fertilise that of another, B; while, on the other hand, the male element of B, perfectly self-effective, be it remembered, is also capable of fertilising that of A. Of such cases, I need scarcely remark, no explanation whatever has been offered, but indeed special observation has been as yet little directed to them. It therefore remains to be seen, whether or not the microscope can reveal any change in the

minute structure of the *genitalia* of such absolutely self-impotent individuals, in a comparative examination with others of the same species perfectly self-fertile, sufficient to account for such singular results.

The paper was illustrated by the plants experimented upon from the Botanic Garden, some of which still bore the capsules, while in others the seed-vessels were exhibited preserved in spirits.

III. *On some New British Lichens.* By the Rev. T. SALWEY, B.D., Vicar of Oswestry, and formerly Fellow of the Linnean Society.

1. *ASPICILIA AQUATICA*, *Köerber.*

Thallus crustaceous, smooth, of a pale ivory colour, and of a darker yellow internally, broken into distinct areolæ, of a somewhat spongy substance, which becomes firmer by age; the areolæ are separated from each other by a coarse, somewhat branny, hypothallus, of a dark-brown or blackish colour, which fills up the interstices, rising frequently to the level of the areolæ. Apothecia constantly immersed; disk black and contracted, covered at first with a greyish meal; the margin black and thickish.—Synonymes:

Aspicilia aquatica, Köerber, p. 164; *Porina chionæa*, Ach. Lich. Un., 311; *Thelotrema chionæum*, Ach. Meth. Lich., p. 131, t. 8, f. 2.

Acharius, who had at first placed this lichen amongst the *Porinæ*, afterwards removed it to the *Thelotremata*, and gives his reason as follows:—"Ob crustam tartaream in hęc genere (*Porina*) nunquam occurrentem, et defectum thalamiorum verarum, *Porinæ* species vix est, sed thallum ignoti cujusdam lichenis tantum offert; hisce rationibus ductus, eam nunc exclusi."—Syn. sub *Porinâ*.

This author does not appear to have met with the plant in its more perfect state of development, in which state it has been described by Köerber, who himself speaks of it as "rare," growing upon stones in brooks and rivers in elevated places; nor has it yet been met with in this country in the perfect state described by Köerber, but only in the state in which Acharius seems to have met with it. I have