Lunds Universites Ars-Skrift, 1865.—From the University.

Schriften der Königlichen Physicalisch-œkonomischen Gesellschaft zu Königsberg, 1865–66.—From the Society.

Proceedings of the Boston Society of Natural History, Vol. X. —From the Society.

The following additions to the University Herbarium were announced:—

From the Royal Garden, Kew—Plants collected in Africa, by the late Dr Burchell.

From the Royal Garden, Kew—Ceylon Ferns, collected by G. H. K. Thwaites, Esq.

From Miss Goodsir—Collection of Dried Plants from Australia, India, Arctic Regions, &c.; also Specimens illustrating the Varieties of Leaves; which belonged to the late Professor Goodsir.

From the Rev. W. W. Kirkby, Red River Settlement—Collection of Plants from Mackenzie River and Fort Simpson.

From Henry H. Calvert, Esq., Vice-Consul, Alexandria—Collection of Plants from Erzeroum.

The following Donations to the Museum at the Royal Botanic Garden were announced:—

From Major Yule—Whip, Purse, and Pocket-Book, made ot Lace Bark; also Dolichos Pods.

From Mrs Little, Singapore—Skeleton Leaves.

From Professor Archer—Collection of 50 Species of Cones.

From Dr John Foulerton—Cones of Wellingtonia, collected under the trees in the "Mammoth Grove," California.

From Henry H. Calvert, Esq., Alexandria—Mussana Bark.

From Alex. Craig Christie, Esq.—Model of *Saxifraga granulata*, prepared by himself.

From Mr Coughtry—Specimens of Wigan Coal and Fossils.

From Rev. Buchan Wright—Fruit of Luffa, Soap Berries, and other Seeds.

From a Visitor—Large specimen of *Delesseria sanguinea*.

The President delivered the following Opening Address, "On Pure Hybridisation; or, Crossing Distinct Species of Plants:"—

The various papers and publications given to science and the world in recent years, by Darwin and others, have directed the attention of all botanical observers to the changes which have been, and which may be effected on the existing species of plants; and those who reflect on the diversity of the vegetable kingdom as displayed in the grandeur of the various forms which compose the primeval forests of the torrid zone, or in the no less diversified but homelier forms of our temperate climes, must be attracted with the statement that, throughout all past time, change —slow but incessant—has passed on everything that now has life; in so much that we see no more the things which were in the things that do appear. So, at least, holds Darwin, whose observations for general accuracy, so far as they are open to scrutiny, stand well the test of investigation, though beyond that limit they diverge, as he himself admits, into speculations which, however logically deduced, all of us are free to adopt or reject as we are or are not convinced by them. I acknowledge that I believe much of the Darwinian theory—more now than I once did—and my grounds for doing so will be afterwards stated. As, however, I have been asked by a high authority (in reference to the paper I read to you in March last), whether I adopted the Lamarckian views, which form the germ, if not the basis, of the Darwinian doctrines, I reply unhesitatingly, No—not in their beginning or their ending—though where the latter is, Mr Darwin is perhaps as much at sea as any one of us. But lop off that beginning and ending—above all, lop it off as regards his views of the animal creation—and there remains in that great work, "The Origin of Species," a body of botanical philosophy, so well sustained by the author's own generally accurate observations and wonderful discoveries, that it constitutes the most valuable contribution ever made to botanical science, and marks an epoch in its annals more brilliant than any yet attained. This is no inflated eulogy. For the last quarter of a century I have myself devoted every spare hour of my professional leisure, and for the last seven years (when free from professional yoke), my leisure almost entirely, to similar pursuits; and, as a humble labourer in the same field during all that time, I have some claim to be recognised as capable of forming an estimate of what has been discovered and achieved by Darwin, and given to the world in that great work, and in his scarcely less wonderful book on the "Fertilisation of Orchids," and his papers read before the Linnean

Society.* He has not only accomplished great things by himself, but he has aroused attention, and stirred up other admirably qualified observers to extend his researches, and, it may be, has thus led the way to other no less startling discoveries. Among those now in the field I would especially instance Naudin and Wichura, whose published researches in this department have stamped them as physiologists of no mean order, as close and discriminating observers, and generally just and sound in their conclusions. I may perhaps take occasion, in giving some of my own experiences, to point out how far they harmonise or conflict with theirs.

In the paper I read before the Society in March last, which related mainly to that form of hybridising generally known as *muling*, and cognate matters—all akin, no doubt, but for the most part *outside* the proper province of hybridisation—I intimated my intention of afterwards submitting to you another paper limited to the latter branch in its purer and simple form. I have now some things to mention which may sound as a thrice-told tale; but as they have reference to my own experiences, and as I desire to lay them truthfully before you, I trust they will neither overtax your patience, nor be without some value to those who may have already entered to some extent on the important field of observation. Let such as have the taste, and the means and appliances to boot, never be discouraged. Nature has many mysteries to unfold. She has fixed rules—some so plain that he who runs may read; and she has exceptions to these rules. Look at the wonderful provision she has made for the fertilisation of the orchids, and look at the no less marvellous modes she has adopted for the same end in the dimorphic forms of the genus *Primula*, and also in some forms of the genus *Linum*—of all which Darwin was the grand discoverer. I was myself almost a sceptic in the results obtained by him till I tested the statement he enunciated in the former genus by actual experiment, and found it true. I had, before he wrote, been myself at work among the species of the genus *Linum*, and while I found some of them tractable and open to self-fertilisation, I found a disturbing element among others for which I could not

* Journal of Proc. of Linn. Soc., vol. vi. pp. 77, 151; vii. p. 69; viii. p. 169.

account, till I found it cleared up by Darwin in his *dimorphic* discovery. To a mind like his, ready to follow out by untiring research every perplexing cause which baffles the expected result, one discovery followed and perhaps suggested another, and it may be that the most brilliant of all yet awaits him. Let us follow in his wake; and though few are so constituted or so gifted as to attain to like successes, there is much for all to do. There is romance in the pursuit, and laurels to be gathered by every acute, industrious observer. If he make no grand discovery, he may zealously endeavour, and assuredly he will succeed in improving our common flowers, fruits, and vegetables, and, what is still more important, our cereals and grasses. I never myself sought any high flight; but with an experience co-extensive, perhaps, with any living experimentalist, I had been both blind and stupid not to have obtained some sparks of more light, and a better acquaintance with nature's modes of working; though I did not give, as perhaps I ought to have done, more publicity as I went along to that little knowledge which I acquired. I feel the loss, as many things which I observed might, if noted then, have been useful to me now; and, unfortunately, many things which baffled me and called for solution have passed from my mind. I must, therefore, as respects the past, keep within the record of my written notes, and from these cull only such as may be of use to others.

Holding, as I do, that the first object of all papers on subjects laid before a society like the present is to communicate instruction, I hope the Fellows of the Botanical Society will not consider the present Address unworthy of their attention, but that they will bear with me while I state the rules I observe and the means which I take to ensure success in my experiments.

1*st*, I long held it to be of vital importance to have the separate plants intended for the parents in the cross, even though both were hardy, put under glass, and I still recommend it; for by doing so you heighten the temperature —an important thing—and you can better secure against the interference of winds and insects; and though Darwin holds the former of small account, I have reason for differing from him. But in the height of summer, pollen may be taken from an outside plant to cross an inside one, and *vice versa.*

2*d*, I hold it not enough merely to emasculate the intended seed-bearing flower, I take off every petal, for the petals attract the insects, which seem guided more by their optics than any sense of smell. This act of emasculation in some cases I perform long before the expansion of the bloom, for in many plants—as, for instance, in the *Papilionaceæ*, some of the *Rosaceæ*, and *Compositæ*—self-fertilisation may and does often take place in the unopened flower. This is not all. I sometimes put a *gauze* bag over it; if I do not, the mutilated bloom may not escape that most troublesome of all insect pests, the humble bee, which in his unwieldy flight may come across it by pure accident. But for the most part now I make clean work of it, and remove all other expanded flowers on the seed-bearing plant, and allow no kindred one to be near.

3*d*, Do not be in a hurry to effect your cross; wait till you find that the stigma is fully developed. In many plants this is shown by a glutinous exudation on the summit, as in the orders *Ericaceæ*, *Onagraceæ*, &c. In other orders, such as *Geraniaceæ* and *Malvaceæ*, it is indicated by the feathery expansion and recurvature of its separate divisions.

4*th*, The next thing is to obtain properly ripened pollen grains from the male plant. This is done by carefully watching when the anthers burst, otherwise the insects may be before you; and so active are they, especially on such favourite food as the pollen of the *Rubus* tribe, that, to get it at all, I have found it necessary to encase the opening blooms in muslin bags till the pollen was ripe and ready for use. Do not use, as is generally recommended, the camel-hair pencil, which, applied often and indiscriminately, may and often does convey, with the foreign, some insidious grains of native pollen, which, however few, are prepotent, and wholly neutralise the former. Take, where that can be obtained and afforded, the entire bloom of the intended male, and give the slightest brush with all its anthers over the stigma, or all the stigmas, if more than one, of the intended female. I will give my reasons for this by-and-by. You may use for experiment, in some cases, the *long*, and in some the *short*, stamens. To those of the proper dimorphic form I have made some allusion already; they occur in the species of *Primula* and in some of the species of *Linum* (as to both of which see Darwin's remarkable papers in the Proceedings of the Linnean Society). Such stamens, at least two long and two short ones, occur in the two orders of the Linnean class *Didynamia*, on which I may have a suggestion to offer hereafter. In cases where the anthers are few—as in the Linnean classes *Diandria*, *Triandria*, &c.—you may use small pincers—a bit of wire so twisted as to form that implement, and easily carried in the pocket, is by far the most convenient. I have used such an instrument all along, and find it better than any other form. In some tribes, the better to secure against invasion by insects, such especially as in some of the *Rosaceæ* having large discs, a muslin bag may be used so as effectually to exclude them. I use it constantly in the *Rubus* tribe immediately after emasculation, taking it off and replacing it after the cross, and keeping it on thereafter till the cross has set.

5th, In some cases it is a matter of some difficulty to procure, and when procured, of no less importance to preserve, pollen. In diœcious plants—such as *Aucuba*—a friend may have the male and you have, as we all have, the female in abundance. You would like to store that pollen till your female plant—generally later—comes into flower. Many hold that pollen cannot be preserved in a vital condition for more than one or two, or perhaps three, weeks. In a recent publication which refers to this matter—namely, Max Wichura's "Observations on Hybridisation" (of which a very lucid abstract, carefully digested and translated from the original German, by the Rev. M. J. Berkeley, is given in the January number of the "Journal of the Royal Horticultural Society"), that eminent author states that "the pollen of willows retains its potency for some time. In some cases pollen ten days old was efficient, while vitality was still further prolonged by steeping it in a solution of honey" (of which I have doubts). "Pollen," he adds, "of *Salix Silesiaca* eight days old seemed almost as potent as ever; in twenty-eight days the traces of vitality were very slight, while that of *Salix cinerea* had become weak in sixteen days." Now, I am not aware that there is less vitality in the pollen of willows than in that of any other family; and as many experimentalists hold kindred views to those here enunciated by Wichura, I deem it a matter of some importance to give you one or two instances of my own experience. I have carried in my pocket the pollen of *Rhododendron* again and again from six weeks to two months and upwards, and still found it potent. Of the Japanese forms of the genus *Lilium* I have kept pollen in an effective state in the same manner for equal periods. In fact, generally speaking, I have found the pollen of most plants remain good for similar periods. Having last year got the new and beautiful *Clematis Jackmanii* to flower, and being anxious to preserve its pollen as long as possible, I collected and stored it in its anthers in a simple pill-box. On the 4th of July 1866, I so gathered it and put it into a drawer of a cabinet in my own sitting-room, where it remained completely free from damp. On the 5th of June 1867, having first carefully emasculated a flower of *Clematis candida*, I crossed it with the pollen, then eleven months old, and from this cross I have this autumn gathered and sown eight well-developed seeds. Now, both parents are hybrids, with a large infusion of *alien* blood in them, so that here the vitality was put to its severest test. I notice this result here (somewhat out of place) to suggest the propriety of storing, and, if needful, of importing, pollen, which, if wrapt up in silk paper, might even, enclosed in a letter, reach this country still potent, by the overland route from India, or, after two or three months' voyage, from all parts of South and North America. Let collectors and friends in distant countries be instructed as to this, and we may soon have an improved progeny of the rarest things, even before such novelties from which they are derived have been obtained from their own seeds in this country.

6*th*, There is another matter of much consequence to be attended to in the crossing of distant species. I mean the times and seasons for effecting the cross. I do not find that any of those most experienced in the art, from Darwin downwards, have touched upon this point. It has been forced upon my attention for more than twenty years. I have found that I could, on some few propitious days which occur throughout the season, successfully effect crosses which I could not effect with all my care at other times. I adverted to this in the paper which I formerly submitted to you, and I again refer to it. There are some crosses which I have effected at such times, and which I would have tried in vain to accomplish at times less favourable. If you have, say, two plants of *Rhododendron*, one a tiny thing, to cross with a large species, or if you wish to attempt a cross between an Indian Azalea and a Rhododendron, watch for a propitious time. Such times occur, often few and far between, when there is less of sun than of that latent form of heat, which frequently occurs before thunder, from the air being more than ordinarily charged with electricity. Or they may occur in the spring season, when there is much ozone present in the atmosphere. The influence of this I have often found tell most favourably in promoting the germination of long-sown seeds. It was to the presence of ozone, or to some other form of electrical agency, that I attributed the almost simultaneous germination of some New Zealand seeds of a shrub which I got from that country under the name of "Black Maupan," a species of *Pittosporum*, which sprang up together on the morning of the 16th March 1863, after they had lain dormant two years and eight months. Such atmospheric conditions, to whatever cause they may be due, I have found not unfrequently to occur with the east winds of March and April; at which times I have seen many long-sown seeds spring up suddenly and unexpectedly. Seize upon all such seasons for difficult crosses. As to the time of the day, you will operate best from 10 A.M. till 6 P.M.

We shall suppose the cross now performed. Your next anxiety will naturally be to find out whether it has taken. Almost all experimenters have noticed that soon—I would say from six to ten days—an alteration is observed on the stigma and style. You will find the viscid matter on the former dried up while the latter has begun to shrivel. You will naturally conclude that it is all right, and that the fertilising influence of the pollen has now passed down into the ovary, and in some cases you may be right. But these appearances are deceptive, especially if you find the style maintain an erect position. I find, on glancing at the

"Gardeners' Chronicle" of the 19th October 1867, that this state of matters had been observed last summer by the editor of that publication, and described in his leading article of that day. He there observes:—"We have ourselves, in following some experiments on cross-breeding this season, noticed that the stigma becomes changed—withered almost immediately after contact with the pollen, even if no perfect seeds be produced." That is a correct statement, in my opinion. I did not, however, note the withering effect to be just so immediate as he had observed it, though it might have been so in the *Epilobium* tribe to which his experiments refer. Another effect I particularly noted last summer was, that, in attempting to cross an Indian Azalea with a Rhododendron (which, however, in that instance failed), not only did the stigma and style decay, but the divisions of the calyx took on a purplish tint, and a honeyed secretion continued long to exude from the disk. Another still more misleading condition often arises, as is noticed in the same leading article of the "Chronicle:"—"The ovary will swell, the fruit will set, in some cases without any contact with the pollen at all, though of course no embryo is produced." Wichura has noticed the like result, and the following degrees of failure noted by him have so often occurred in my own experience, that I cannot do better than cite them in his own words, from the Rev. M. J. Berkeley's translation already alluded to, which I only alter according to my own experience:—"1*st*, The organs submitted to hybridisation (the stigma and style) soon wither, but do not in all cases soon fall off. 2*d*, The ovaries swell and ripen, but do *not* contain a trace of seed. 3*d*, The ovaries may seem filled [I say may seem partially filled], having in some instances the small protuberant swelling outside as if seeds were within, and yet no seed be there. 4*th*, Seeds are present, but small, languid, and incapable of germination. 5*th*, Seeds apparently perfectly developed, which do not germinate. 6*th*, Seeds which germinate, but the young plants are weak, and wither in a short time, dying off oftentimes after developing the seed-leaves. I have had all these conditions and results amply illustrated; and of the second of these results, I had, last summer, mortifying proofs in a muling operation I tried, by fertilising a flower of the new *Arabis blepharophylla* with my still newer *Draba violacea.* The cross, to all appearance, had taken; the seed-vessel swelled better than the others where no experiment was made, and while the valves of the silicules of the latter plants opened and showed no trace of seed in them, the siliquas of the former remained closed, showing by outward swellings that two seeds were certainly within. But I found on opening the ripe seed-vessels that there was no perfect seed in the interior, but only an abortive production." While Wichura's statements in the above instances of *failure* are consistent with what I have myself amply experienced, I cannot, from my experience, endorse the views he has propounded on some of his *successful* results. At page 72 of the above article in the "Journal of the Royal Horticultural Society," Mr Berkeley, commenting on Wichura's paper, observes—"Gærtner, indeed, supposes that in genera which are rich in species, there are some which have a *prepotent* influence when hybridising, so that in some hybrids the type either of the male or female prevails. Amongst the various hybrid willows, though the genus is so rich in species and so prone to hybridising, Wichura has never seen a prepotent type, and doubts Gærtner's statement, especially as he makes it in very qualified terms." Mr Berkeley very judiciously remarks that it is not easy to determine "by examination of types, whether a hybrid is more like the mother or father—the perfect distinction is subject in many cases to great difficulties, since very much depends on the subjective view of the observation; for, in consequence of the frequent intermelting of both characters, the one observer finds in a hybrid the maternal type, while another thinks the paternal type prevalent." By which I regard Mr Berkeley as very modestly dissenting from his author. Further on, at page 78 of the same journal, Wichura speaks out still more absolutely. "When both parents," says he, "belong to the same species, we cannot tell what part the male and female parent take respectively in the formation of the progeny. But dissimilar factors are united in hybrids, and an *intermediate* form is the consequence. The products which arise from reciprocal crossing in plants, unlike those which are formed amongst animals, are perfectly alike." I regret to differ from so great an authority as Wichura, and I must venture to demur to the doctrine in more decided terms than Mr Berkeley does. I have had many instances of hybrids taking sometimes to one side and sometimes to another—but most frequently to that of the mother. To those who, like myself, have made experiments with many genera, it would be needless to give instances. The converse is the rarer case—that is, where the paternal type comes out most marked. Yet I remember one eminent instance of a seedling *Veronica* (from the batch of seedlings from which I obtained *V. Andersonii*, *V. salicifolia*, *V. speciosa*), being so like the male parent *V. speciosa*, that I presented it to a friend in the belief it was purely and simply the latter species; but when it bloomed, it showed, by the longer spike and lighter and brighter colour of the flowers, and by their being a bright crimson instead of very deep purple (which is the colour of the flower of the *V. speciosa*), that the blood of the *V. salicifolia* was there. I can well understand that, as respects the family of willows, from their being so attractive to bees, and from their being naturally so prone to intermix (in so much that few can tell what is a species and what is a hybrid), Wichura has not much overstated the fact, and that a distinct intermediate form may generally be reckoned on.

I must dissent still more strongly from what Wichura lays down in continuation of the above passage at page 78, as to reciprocal crossing. "The products," he says, "which arise from reciprocal crossing in plants, unlike those which are formed among animals, *are perfectly alike.* It is of no consequence which is the male and which the female parent. It is, therefore, a mathematical necessity that the pollen-cells must have just the same part in the act of generation as the ovules." He follows up and amplifies this doctrine in a series of aphorisms which, he admits, are to be "considered conjectural, and require to be submitted to proof"—an admission for which he is to be commended, and all the more if he submitted to the like test the dogma on which they mainly rest. It appears to me that his statement had been suggested from his experience among the *Salices*—of all plants the most mongrel in a state of nature. Now, in all this Wichura appears to me to imply, that if a distinct intermediate may be formed, and is formed, by crossing A on B, so may an exactly similar intermediate be reciprocated by crossing B on A. And M. Naudin, in his experiments among the *Daturas*, enunciates the same belief, and holds "that there is not a sensible difference between reciprocal hybrids of two species." That distinguished observer, like Wichura, seems to have confined his experiments to herbaceous or soft-wooded plants. But, from a long and large experience among both hard and soft-wooded plants, I demur—1*st*, To the capability of the parents being in *all* cases made subject to such reciprocity; and, 2*d*, To the statement, where such reciprocity does hold, that the progeny are *perfectly alike*, whether A or B supply the pollen.

In my various crossings I have experimented on many hard as well as soft-wooded genera, and among the former I would particularly mention the species of Rhododendron. In these I have again and again been baffled in reciprocating a cross which on one side was comparatively easy to be effected. When the lovely and fragrant *Rhododendron Edgeworthii* first bloomed in this country, all were eager to see its beauty and perfume transfused into dwarfer and hardier forms. Some tried the cross by making *R. Edgeworthii* the female or seed-bearer, others by making it the male. I tried it in both ways, but all my efforts failed where I attempted the cross on the *R. Edgeworthii*. But while it could not be brought to bear hybrid seed, I had no great difficulty in effecting a cross from its pollen on *R. ciliatum*, another of Dr Hooker's beautiful Sikkim species, having all the desirable requisites of hardihood, dwarf habit, and free-flowering tendency; and, singularly, just as I had obtained and sent off blooms of this brood to lay before the Committee of the Horticultural Society of London, Messrs Veitch of Chelsea anticipated me in having a plant of this identical cross first exhibited before that Committee, which is now well-known and generally cultivated under the name of "Rhododendron Princess Alice." Now, neither I nor any one whoever tried it, so far as I know, ever effected the *inverse* cross of *R. ciliatum* on *R. Edgeworthii;* and if they did, the progeny would long ere now have appeared in nursery catalogues. There is yet another instance I may notice as an illustration of what I am now contending for. In my former paper I noticed, as an exception to a rule I had found almost general—viz., that European had great aversion to cross with Asiatic species—that I had, notwithstanding, effected such a hybrid by crossing *Rhododendron eleagnoides* (another of Dr Hooker's acquisitions, a tiny Sikkim species) on the European *R. hirsutum*, and of having sent the survivor of the two plants which came of it to Kew, which, Dr Hooker writes, dwindled away and died after being a few years in the garden; but by no possible means could I invert that cross, or get that same very interesting tiny yellow-flowered species, *R. eleagnoides* (a form of *R. lepidotum*), to submit to a cross from any species whatever.

I shall now advert to the second point which Wichura lays down as a fact, viz., that the progeny of reciprocal crossing, whether it is A on B, or B on A, are precisely alike. While my past experience goes with what I observed last summer, it may perhaps suffice to give the latest instance. Having, through the kindness of Dr Hooker, obtained seeds of a beautiful new Californian Arabis (*A. blepharophylla*) with large fine rose-tinted flowers, I felt desirous to infuse that colour into some of the other kinds I possessed. After trying it on several, especially on *A. albida*, in vain, I at last effected a cross—a reciprocal cross—between it and *A. Soyeri*, a white-flowered kind, something like the *A. albida*, but with glabrous foliage. Of the cross *A. Soyeri* on *A. blepharophylla* I have raised six plants, the product of two very largely developed seed pods. These plants are alive and healthy, and promise an improved vigour over either parent. That the cross was sure, I had the best proof, from there being no seeds in the normal pods of the seed-bearer. Of the inverse cross from one weakly seed-pod I raised one plant, which, after maintaining a sickly existence for some two months or so, has died off. But while this last cross was equally certain as the others, like it, the plant had more of the mother than the father in it. In fact, I have oftener found the maternal type most marked in hybrid progeny. I have various crosses effected between distinct species of Rhododendron, where, while the male manifests his presence, the female type prevails. I have it in *R. Jenkensi* crossed by *R. Edgeworthii*, *R. Caucasicum* by *R. cinnamomeum*, and the hybrid from this latter cross crossed again with *R. Edgeworthii*, and especially the Sikkim species *R. virgatum* crossed with another of my hybrids, *R. ciliatum* by *R. Edgeworthii*—all having more the foliage and the aspect of the mother than the father.

I have another hybrid of the same *R. virgatum*, the female parent crossed, I believe, by *Rhodothamnus Chamæcistus*, a tiny procumbent plant of 3 inches, but all set with flower-buds—not, as in the male parent, at the tips of the shoots, but as in the female, at the axils of the leaves. I have stated my belief that the *Rhodothamnus* is the male parent, but I cannot do so confidently, from the tallies having got into confusion—the specimens being planted out. But as some plants were obtained from that cross, and as this is the smallest, I regard it as likeliest to be the true progeny; and the cross being an extreme one—a mule, in fact—it is open to question. But as I have this season effected still more extreme—certainly more unlikely—crosses in that family, where there could be no miscarriage, you may, I think, take it as true in the meantime. I could overwhelm you with proof. Darwin, at page 333 of the last edition of his "Origin of Species," has observed the above tendency. "When two species," he says, "are crossed, one has sometimes a prepotent power of impressing its likeness on the hybrid; and so I believe it to be with varieties of plants."

Naturalists of the highest note—Gærtner, Kolreuter, Naudin, and Wichura—are far from being at one on the subject of *variability*, as Darwin has shown, especially as relates to crosses—1*st*, between species and species; 2*d*, between species and varieties; and 3*d*, between mongrel offspring. But this is a complex subject; and when such high authorities are not at one, and Darwin admits that he cannot reconcile them, it is manifest that the case is still open to further probation. In dealing with the views of Gærtner (to whose testimony he deservedly accords great value (page 331), Darwin says that Gærtner, whose strong wish "it was to draw a distinct line between species and varieties, could find very few, and, as it seems to me, quite unimportant, differences between the so-called hybrid offspring of species and the so-called mongrel offspring of varieties. And, on the other hand, they agree most closely in many important respects. The most important distinction is, that in the first generation mongrels are more variable than hybrids; but Gærtner admits that hybrids from species which have long been cultivated are often variable in the first generation; and I have myself seen striking instances of this fact. Gærtner further admits that hybrids between very closely allied species are more variable than those from very distinct species, and this shows that the difference in the degree of variability graduates away. When mongrels and the more fertile hybrids are propagated for several generations, an extreme amount of variability in their offspring is notorious; but some few cases, both of hybrids and mongrels, long retaining uniformity of character could be given. The variability, however, in the successive generations of mongrels is, perhaps, greater than in hybrids." So reservedly does Darwin deal with a subject on which the opinions of many others could be brought to bear; but as they are not all concurrent, and not unfrequently conflicting (which they may well be from the various subjects experimented on), he has said, with commendable moderation, all that can be said on the subject.

From you, gentlemen, I respectfully claim the same kind indulgence which Darwin has shown to the testimony he has had to deal with, in judging of the views I have offered, and am now to offer, on the experiments which I mean to lay before you. But ere I enter upon them, it is necessary to make some remarks on that form of *dimorphism* which occurs among many plants—in the Linnæan classes from *Pentandria* up to *Decandria*—in having very generally one if not two pairs of stamens shorter than the other stamens in the same flower. And the same dimorphic form often occurs in even a more marked degree in many plants of the class *Tetrandria*. It is also the distinctive character of the two orders of *Didynamia* to have two long and two short stamens.

As observed in my former paper, it is now seventeen years since my attention was drawn to the long and short stamens, but to the latter more particularly in some muling operations there alluded to, where, by using them, I crossed that large species of Rhododendron, *R. cinnamomeum*, on the pigmy *Rhodothamnus Chamæcistus*. I refer to these short stamens again, as the means by which I succeeded in effecting some extraordinary crosses which, I confidently believe, but for their use and my improving a propitious time, would have been utterly impracticable. As I have said, I at first worked only with short stamens. These I use in all cases where I wish to cross a large on a small species. I have now found that the converse holds, and use the long stamens where I wish to cross a small on a large species. In all extremes I use the *longest* or *shortest* pair of stamens as the case demands. The short pair is generally well distanced by the others—the longest pair is often not just so much in advance. There is often an intermediate pair of short stamens, which in cases less extreme are exceedingly serviceable, but there are seldom such intermediates among the long ones. My reason for the use of these short, intermediate, and long stamens is intelligible enough. If I wish to cross a large on a small species, the smallest-grained pollen being in the short stamens, I take the pollen of these stamens of the *large* plant as best fitted to send tubes through the stigma and style, to fertilise the ovules of the *smaller* species, and so effect the cross on it; and so, *cæteris paribus*, with respect to the other forms. I shall restrict the instances I am now to cite to the last few years, noticing,

1. *Cases of Crossing with Short Stamens.*

The first cross I shall notice is one I have already alluded to—viz., *Rhododendron virgatum* with my own hybrid rhododendron *B.* (*R. ciliatum* crossed on *R. Edgeworthii*); and as this cross is memorable and instructive in several points of view, it is proper to give its history. On April 20, 1864, I find from my note-book that "I took off all expanded blooms of *R. virgatum*, and removed the stamens from all unopened ones on the plant, there being none left for self-fertilisation; done in fine sunshine—west wind—with three short anthers of *B*"—*i.e.*, the hybrid male, being the identical cross which produced Veitch's rhododendron, Princess Alice. Of this cross I ripened four capsules of seed, which I sowed on January 28, 1865, and, with some failures, got up by December that year seven nice healthy plants, all of which, however, save one, I lost by an accident. That one plant is now setting for bloom—not at the axils, as the female parent (*R. virgatum*), generally shows, but at the extremities of the shoots, as in the male (*R. ciliatum*), crossed by (*R. Edgeworthii*). But as I have had occasion to observe already, the type in all else is more that of the *female* than of the *male* parent. By the mother's side this plant is a hybrid, by the father's it is a mongrel, and yet it has a fair share of vigour in it. As in its sexual aspect, so in its height, it is that of the mother. A few *cilia* are noticeable on its leaves, but it has none of the tomentose or dense hairiness of the male parent; and so in this also it partakes most of the glabrous foliage of the mother. Again, this doubly-crossed plant, and the crosses which produced it—all extreme—show how such crossing may hasten on the reproductive or flowering state. Never in all my experience have I seen or heard of rhododendrons offering bloom at two years of age. I have rhododendrons now fifteen years from seed which have never shown the slightest tendency that way, though ten and twelve years I would consider about the mean at which they attain their flowering state. If by such crosses the like precocity can be generally secured, practical florists may turn them to some account in their profession. I am now dealing with *hard-wooded* shrubs, where there is in general more fixedness of structure and habit, than in those on which the physiologists I have cited have chiefly experimented, and which are less liable to be modified by the manifold influences which affect the more pliant and shorter lived herbaceous *genera*.

2*d*, The next cross in the rhododendron tribe effected by the short stamens to which I would direct attention is very recent, and one with which I took the utmost pains to prevent miscarriage. The beautiful *R. jasminiflorum* of Java, with its delicious perfume and its long tubular five-lobed flowers, of snowy whiteness, so like *Erica Aitonia*, so like, too, in form and fragrance, the sweet-scented jasmine, and so unlike all its own congeners, is the subject of it; and as I regard this cross of some scientific as well as of some practical value, I shall offer no apology for giving you full particulars. I made it the subject of attempted crosses by many of its own tribe—all of which failed except two, which, by the way, afford a good illustration of what I alluded to in my former paper of the *sympathies* of plants, and perhaps, too, of natural selection, though whether it be in the mode which Darwin regards as leading to diversity of species I cannot positively assert, yet I think it is worthy of his consideration. While it rejected many of its legitimate brethren of the rhododendron tribe pure and simple, I was somewhat surprised that it took kindly with my hybrid *B* already noticed, *i.e.*, *R. ciliatum* crossed by *R. Edgeworthii*, a hybrid of the first degree, having large flowers of 3 inches diameter, perfumed, and also of snowy whiteness. After the bloom had been long emasculated, on April 17, 1867, I effected the cross with the *short* anthers of the hybrid *B*. The cross took admirably—the seed-pod swelled, and was pulled fully ripe about 12th July last. On the 15th of that month I sowed the seeds. For the purpose of comparison, I sowed a pod of its own plain native seeds, which I had gathered previously, and had, in fact, sown some ten or twelve days before I sowed the cross. These are both now up. While the native seeds have produced a fair show of feeble plants, the crossed seeds have come up in more than double the number of plants, doubly vigorous in growth and habit, and with leaves so much larger than those of the normal form as to remove all doubt about the verity of the cross.

3*d*, The next illustration I have to give you is of a small-foliaged *Indian Azalea*, 18 inches high, which I crossed with the tall and robust shaggy-foliaged *Rhododendron Edgeworthii*. Two things more unlike in every feature from which to effect a union can hardly be imagined. Yet, with the short anthers—and it was with the *very shortest* I could find on *R Edgeworthii* that I effected it—the cross, after careful emasculation, was done on 6th May last. The seed-pod swelled to its due dimensions, and appearing to be ripe, I cut a slice *off* it, and sowed the seeds so early as the 13th, and the residue on 28th September last, and I have now got up one or two plants. If I shall be so lucky as to bring it to maturity, the progeny of this cross (one never before accomplished, perhaps), should be a sweet-scented Azalea, having a rose variegation like the female parent, a novelty in its tribe; for though the *Azalea sinensis* has been crossed by rhododendrons, I am not aware of any authentic cross, or cross of any kind, between the rhododendrons and this proper Indian Azalea.

4th, I have still further a cross of the same nature between another Indian Azalea and the *Rhododendron jasminiflorum*, the latter being again the seed-bearer; and I here refer to it mainly as showing another tendency of this rhododendron towards natural selection, or rather perhaps of *sympathy* between it and remote species, if not genera, for the azaleas have till lately been regarded as a separate tribe from the rhododendrons. The cross was effected in August last, when it again rejected its more natural allies, and formed a union with the Indian Azalea, a late rose-coloured spotted variety, a seedling of my own raising. The seed-pod of this cross is now at maturity.

5th, But I have now to call your attention to a cross in this same family bearing on Darwin's doctrine of *natural selection*, or of *sympathy*, in a still more remarkable manner, which I effected last summer between that most gorgeous of all the rhododendron tribe—namely, the lovely white, large-flowering, sweet-scented *R. Aucklandi* of Dr Hooker—and an Indian Azalea, the latter being the seed-bearer. I made the cross on two separate days on two separate blooms, carefully emasculated some time before; and on the same Azalea I tried other crosses with several of the rhododendron tribe—viz., with a fine form of *R. arboreum*, *R. Edgeworthii* pure, and the above hybrid seedling *B* (*R. ciliatum* by *R. Edgeworthii*). But while every one of these failed, the crosses by *R. Aucklandi*, which were effected respectively on 30th April and 1st May, took most kindly. Both pods swelled; and the seed-pods, though green, appeared to be sufficiently ripe when I pulled them. I counted the seeds in one of these pods, and found them to be about 324, all finely formed, but, I fear, too green to vegetate freely, though some which I sowed appear to be coming up. I cannot vouch for this cross being effected with the *shortest stamens*, for the stamens with which I effected it were kindly sent to me from another source, as I did not myself possess the male plant; but as I invariably select the *shortest* for such crosses, my firm belief is that I had so selected these in this instance, and I had a plentiful supply of all lengths to choose from. In the above cases of crossing a *small* with a *large* species, I hold firmly by the opinion that but for the use of the *short stamens* I could not have succeeded. I have few recorded instances of having extended my experiments with them far into other families. I certainly tried the *Pelargonium* in a plant I had of the beautiful white-flowered Madame Vaucher. I fertilised a bloom with its *two shortest* stamens, which, however, were very little shorter than the remaining ones; and from the three seeds which germinated I raised two fine plants, far more compact and somewhat dwarfer in habit than the parent, having the flowers equally fine, and elegantly thrown up above the plant. But the short stamens of this section of the *Geraniaceæ* are very little shorter than the others, and I therefore cannot rely much on the results as establishing the hypothesis I contended for in my former paper—namely, that where all other things are equal, a cross or simple fertilisation with the short *stamens* tends to *dwarf* the progeny. I still, however, adhere to my belief in this hypothesis. The instances I have given support this other *hypothesis*, that by the use of the short stamens you may cross a large on a small kindred species—a result which, without them, you might not affect.

2. *Crossing with Long Stamens.*

I have made fewer experiments with the long stamens, but I have one before me now no less remarkable, perhaps, for its far-reaching result than any I have alluded to as done with the short stamens. It is a cross which I effected on the tall *Rhododendron formosum*, fertilised with a scarlet-flowered Indian Azalea, on the 11th June last. The seed-pod is finely developed, but I have taken care in this instance to avoid pulling it too early. And I may here notice, once for all, that to obtain the seeds of a cross—especially if it be *extreme*—sufficiently ripe, you must allow a longer time for it than for the ripening of the normal seeds on the same plant.

In all the above crosses I had, perhaps, less an eye to accomplish a purely scientific experiment than to effect a beneficial result; for, after all, it is the *quid sit utile* which those for whom this paper is mainly intended, will have most in view; and, in my estimation, science is best promoted when she is made to minister to some useful end.

The following experiment among the species of *Clematis* illustrates my view of *sympathy* as well as of *antipathy*, and, I would add, of *unnatural selection*:—Having many years ago (long before the Messrs Jackman, who have accomplished such wonderful results) been myself working on the members of this genus, I thought of making another experiment on it, with a view to infuse a richer colour into a new and larger-flowering progeny; and, as I have observed already, I managed successfully to cross with pollen, kept for eleven months, the beautiful four-petaled *Clematis Jackmani* on a thirteen-petaled flower of the fine *C. candida.* But it is of a cross on Messrs Jackman's smaller, but no less beautiful, *C. rubro-violacea* I am now to speak. Though, like its congener *C. Jackmani*, it sometimes shows five or even six petals, it is in its general type a four-petaled flower. With a view to improve it in this feature, I crossed it also with pollen of the large-flowered *Clematis candida*, taken from a bloom having seventeen petals, though this *Clematis*—a French hybrid, I believe, from *C. lanuginosa*—is in its normal state a six or eight petaled flower. Though I crossed two flowers, after careful emasculation, I only gathered three seeds, but these all of unusually large dimensions. After the cross had taken, I left the normal blooms on the crossed plant to their fate; and though visited by insects innumerable, and though the native pollen was abundant, not one native seed, or any, except the three produced by the cross, were ever formed on the plant; and the singular thing was that, with its own native pollen, abortive on itself, I successfully crossed the fine double-white flowered Chinese *C. Fortunei;* and a cross more prolific in the seeds it yielded I have not seen in the tribe before. I know not the parentage from whence this *C. rubro-violacea* was derived, though I believe it to be a mongrel with none of the *Fortunei* blood in it; yet mark how kindly the latter took with it—another instance of remarkable *sympathy*. Although I have no record of it, I think I failed to get *C. rubro-violacea* to reciprocate *this* cross.

In all these instances of *sympathy* and *antipathy*, and especially in this section of the natural order *Ranunculaceæ*, there is something apparently so inexplicable, that I can only concur with what Darwin has observed in his paper on the existence of two forms in the genus *Linum*, where, in summing up the good gained by the inevitable crossing of the dimorphic flowers, and numerous other analogous facts, he says, that these all lead to the conclusion that some "unknown law of nature is here dimly indicated to us." This law, when discovered, may disclose more mysteries, tending, perhaps, to the wider divergence of species, with constitutions and habits better fitted for the climates and localities in which they may be cast, as well as for subserving the purposes they are intended to fulfil in the economy of nature. In looking at *Ranunculaceæ*, with their innumerable male and female organs (and the same thing occurs in the *Myrtaceæ*, most of the *Rosaceæ*, some of the *Hypericaceæ*, and in many other families and tribes), the idea was long ago suggested to me, that each separate row, from the outer to the inner circle of the stamens, might have some separate function, just as I believe that the long and short stamens have their separate functions; and with the view of testing the matter, I had the last summer begun experiments with these *outer* and *inner* stamens; but other aims and objects interfering, I gave up the experiment after I had begun it on these Clematises.

But to make success certain, it is my custom, as I have already stated, in crossing any of these polyandrous flowers, to take the entire bloom of one kind, and lightly to come over, with all its anthers, the stigmas of the flower to be crossed, and leave nature to make her own selection. In referring to the *Rubus* tribe and its species, I am reminded of an intention I expressed in my former paper of perhaps returning to them afterwards. I again experimented upon them last summer. But though I tried various crosses among them, and reciprocated the cross, I had no success in any, except between the *R. biflorus* and the *R. Idæus*, and that only where I made the latter the seed-bearer. And to make sure of either event—success or failure—I had the *Idæus* early potted and put under glass, emasculating every bloom I meant to cross; and for more security I stripped off all other flowers—nay, more, I put the emasculated flowers under fine gauze bags, to ward off the invasion of insects. When ripe for crossing I removed the bag, and on effecting the cross, I replaced it. In this way I succeeded in ripening three berries of the cross *R. Idæus* by *R. biflorus*, of which I sowed the seeds between the 5th and 16th July, though as yet none have vegetated. But *R. biflorus* stubbornly rejected a reciprocal cross. Again I tried both of these on *R. rupestris*, and the latter on them; and though *R. rupestris* showed some sympathy with *R. biflorus*, in a slight tendency to form seeds, these came to nothing. In all these attempts I applied, as I have said, all the anthers of the male flower.

I cannot quit this part of the subject without offering some additional suggestions to those of you who wish to act on any hints I have in my power to give:—

1*st*, If your desire be to *hasten* the flowering condition of plants, I recommend you to cross *violently*. That is to say, where the allies are not too near akin, and, above all, in the case of mongrels; for nature, ere she gives up, always makes a violent effort to reproduce.

2*d*, If you wish to make your hybrid flower more *freely*, as well as *early*, adopt the same advice.

3*d*, By following it, you will find that you have attained a further advantage. Your plant will remain longer in bloom, because most mongrels, especially those among herbaceous or soft-wooded plants, to which these suggestions apply, are impotent to produce seed, or nearly so, and in such cases the blooms remain long upon the plant. I have another idea, not sufficiently tested, however, in reference to the first point among hard-wooded as well as soft wooded plants, that all such as ripen their seeds more quickly than others (some among the rhododendron tribe ripen seed in half the time that others take) will reach more quickly their flowering state.

Lastly, as to *fruits*, on which, however, I have only partially experimented, I entertain the belief that we are on the eve of a revolution. I think that by judicious and persevering crossing we may not only transfer the delicious aroma of one to another, and communicate hardier and more abundant bearing habits to the hybrid progeny, but further, especially in stone fruits, such as peaches, plums, apricots, &c., we may, in addition to these advantages, increase the size of the fruits and diminish the size of the stones; and among *vines*, get rid of, or greatly diminish, the number of the seeds. All this I hold to arise from that law of nature by which she not merely strains her efforts to reproduce (to which, however, she has assigned a limit), but extends it when these have failed to make provision for her creature's wants. These views gather strength from what has been already done; and I may especially allude to what Mr Standish of Ascot has achieved among grapes, of whose extraordinary results an interesting account is given at p. 135 of the "Journal of the Royal Horticultural Society" for July 1866.

In conclusion, permit me to observe that, while my aim has been, in all the experiments I have brought before you, rather to achieve something useful and practical than to test the theories which Mr Darwin and others, especially the continental *savants*, have been so much engrossed with, I cannot refrain from making some remark on the results and the conclusions which some of them have come to while prosecuting a series of crossing operations, namely, that such crosses do and must eventuate in sterility. M. Naudin seems, like Wichura, as already observed, to have limited his experiments chiefly to herbaceous or soft-wooded plants. Among such, especially among Calceolarias, I too have often found myself brought to the *terminus* of bitter and hopeless sterility. I remember one instance where I had reached a perfect monster for size in that tribe, but except in that particular it had no other desirable property. Determined, however, to improve it by crossing, I found on trial I could make nothing of it, and on examination I found its stigma was a hollow tube, and that its anthers were hard masses, and contained not one particle of pollen. Man may run into such mistakes, but he cannot thence conclude that unviolated nature does so. Speaking from a general recollection which does not admit of my specifying instances, I have often found among hybrid seedlings some of a vigour which, in that respect, were in advance of either parent. May not such often occur in nature, and, as a naturally selected parent becomes the progenitor of a hardier and more vigorous race (which having in it, according to Darwin's views, a tendency to diverge), may it not culminate after the long lapse of time into a distinct species, and even annihilate the weaker one which gave it being? so that, in nature's crossing, may not fertility and vigour take the place of sterility and weakness, into which she so generally dwindles when modified by man's device?

Ere I close, I beg to express, as indicated at the beginning, how far my own ideas harmonise with Mr Darwin's on at least some of those doctrines propounded by him in the "Origin of Species." 1*st*, I am humbly but firmly of opinion that that portion of the creation which we inhabit came from the Creator's hand a completed work. 2*d*, That there were no separate or successive acts of creation apart from, or as a sequence of, those mutations or convulsions through which this globe has passed. But I no less firmly believe, differing from Mr Darwin, that it so came clothed with a vegetation of innumerable types and forms, different, it may be, from those that now appear, but from which these last, much varied, changed, and modified, are nevertheless legitimately descended. 3*d*, That differing again from Mr Darwin's belief, expressed summarily at p. 570 (last edition), "that animals have descended from at most only four or five progenitors, and plants from an equal or lesser number," I nevertheless, as respects the latter (for with the former I have nothing here to do), agree with him that there may, and most likely (through the vast lapse of unknown time which, it must be admitted, is involved in this earth's history) has been a great divergence of species, whether arising from natural selection, or descent with modification, or other causes. We see the laws by which such divergence may arise in operation around us now. I have no sympathy with those who, objecting to one part of his doctrines, wilfully shut their eyes to the grounds and facts on which the broad basis of his theory is laid, the truth and value of which, to my mind, is in no way shaken by all that is hypothetical being swept away. Holding the belief in innumerable vegetable forms simultaneously evoked into being with creation, all the *divergence* of species and *diversity* in character for which Darwin contends may, I think, have arisen from the causes assigned by him. And as for their dispersion, this, I humbly think, is reasonably accounted for from the various causes, detailed by him, briefly summed up in the following passage:—"The process of diffusion," he observes (p. 390), "may often be very slow, being dependent on climatal and geographical changes, or on strange accidents, or on the gradual acclimatisation of new species through the various climates through which they have to pass, but in the course of time the dominant forms will generally succeed in spreading."

I had intended to have offered some remarks on the very interesting paper of M. Naudin on hybridism, and had, in fact, written down some views with that intention; but as I have already exceeded all due bounds, I must for the present leave that intention unfulfilled.

I. *Abstract of Observations on New Zealand Plants.** By W. Lauder Lindsay, M.D., F.R.S.E., F.L.S., &c.

These observations refer exclusively to *flowering plants* collected by the author during his excursions in the province of Otago in 1861. The *species* commented on belong to the following Natural Orders and Genera:—

1. Ranunculaceæ
   - †Clematis
   - Ranunculus
2. Cruciferæ
   - Nasturtium
   - Cardamine
   - Lepidium
3. Violarieæ
   - Viola
4. Portulaceæ
   - Claytonia
   - Montia
5. Hypericineæ
   - †Hypericum
6. Lineæ
   - Linum
7. Geraniaceæ
   - Pelargonium
   - Oxalis
8. Rhamneæ
   - Discaria
9. Leguminosæ
   - †Carmichaelia
10. Rosaceæ
    - †Acæna
11. Crassulaceæ
    - Tillæa
12. Droseraceæ
    - Drosera
13. Haloragaceæ
    - Haloragis
    - †Myriophyllum
    - Gunnera
14. Myrtaceæ
    - Myrtus
15. Onagrariæ
    - †Epilobium
16. Ficoideæ
    - Mesembryanthemum
    - †Tetragonia

* The paper itself is reserved for future publication.