THE

PHYTOLOGIST:

A

POPULAR

BOTANICAL MISCELLANY.

CONDUCTED BY

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and the narrow leaflets, with their acute serratures, will generally sufficiently mark this species.

Weihe and Nees, the authors of this species, describe two varieties, one with white flowers and thick leaves, the other with red flowers and more flaccid leaves. The latter is indeed an extremely beautiful plant, with bright shining leaves and brilliant red flowers. It is in this variety that a few glands occasionally occur on the panicle. The two forms are, however, evidently osculant, and our Selborne plant holds this intermediate station. T. BELL SALTER.

(To be continued).

On the Theory of "Progressive Development," applied in explanation of the Origin and Transmutation of Species. By HEWETT C. WATSON, Esq., F.L.S.

BRITISH botanists, with some few honourable exceptions, would appear to entertain very limited ideas regarding the scope and objects of the science to which their attention is directed. The majority are content to acquire a moderate knowledge of plants and their names, or of the physical characters of parts (shape, proportion, colour &c.) in which their resemblances and differences may be detected. In itself this is doubtless an agreeable kind of study; and it is one, moreover, which so lightly taxes the mind, as to be within the grasp of moderate capacities; for even children can learn Botany thus far. But scarcely any exercise or stimulus is here given to those higher intellectual attributes of man, which are concerned in all trains of reasoning, and which lead to the knowledge of causation and dependance between the phenomena of creation. The study of plants, simply as physical existences, and of their resemblances and differences, on which technical classifications are founded, is an exercise of the same mental faculties which give origin to the restless and prying curiosity of the monkey. So far, the botanist is an intellectual Simia. He advances a step further, when he uses names and terms to express these existences and their similitudes or distinctions. And he ascends successively still higher in the intellectual scale, as the scope of his studies extends over the vital actions of plants — the influence of external agencies upon their growth and health — their relations to the rest of creation—and the mode, or laws, by which the present vegetation of the earth's surface has been substituted in place of a past vegetation, which was greatly dissimilar to that now seen around us.

In this present communication, it is my wish to make some remarks on the last of these subjects. Public attention has been lately directed to that subject, by a volume of considerable merit, published anonymously, under the title of 'Vestiges of the Natural History of Creation.' There is little, very little, of novelty in the book; yet it will probably make the subject more popular, more talked about, and even more thought about, than any previously published work has done. The 'Vestiges' has exactly the character and qualities which are required in a really "popular work." The style is remarkably good and readable—the subject is great and interesting—the illustrations are mostly found in those facts which have been made familiar by public lectures and elementary works — the leading argument of the whole volume, "progressive development," is single, and it is seldom lost sight of by digressions — plausibility is sought, and carried even to case-pleading, rather than any critical balancing of *pros* and *cons*—the reasoning is obvious and direct, leaning more to the superficial than to the profound. Thus, the reader finds himself interested and drawn onward; his mind is neither wearied by dulness, nor exhausted by any serious tax on its powers; he believes that he sees the whole argument or theory clearly made out and established; and he is self-flattered by the supposition of having thus easily acquired a new and important truth.

I allude here, of course, to the "general" reader, who is conversant with the natural sciences to that limited extent which may now be easily attained by attending lectures at a 'Literary and Scientific Institution,' or by the perusal of elementary treatises and other books expressly written for general readers. The judgment of those who have more thoroughly trained themselves in scientific investigation, will not be quite so favourable; although they may pronounce the work to be one of high merit in its class — namely, the class of "popular works." The pretensions to originality, and the success of the argument or evidences, will scarcely be acknowledged by parties who possess a sufficient knowledge of the natural sciences, to render their judgment worthy of much respect. Still, we may allow that the author has embodied an idea, not new in itself, in a more substantial-looking form than it had previously assumed; while he has also given a freshness and fulness to his principle, by tracing its application through many departments of science, and making each yield illustration or evidence in support of the theory.

The author's idea is, that in all departments of Nature — from the origin of a planet or a whole solar system, down to the production of a plant or animal, or any part of a plant or animal — there are such evident signs and proofs of a gradual and progressive development, that we may believe this to have been an original principle, or a law impressed in the constitution of our universe and of the beings by which it is peopled. He first takes up the condition of the solar system, before the formation of the planets, and traces the change of nebulous matter into the sun and its planetary satellites; all which, of course, is purely hypothetical.

In reading the past history of the earth, as unfolded to us by the researches of geologists, we rest upon grounds that are something more than hypothetical. It may be held a truth, inferred from sufficient premises, that the earth has undergone great changes, in the transition from its past to its present condition. There can be no doubt that the earth was formerly inhabited by plants and animals widely different from those at present existing upon it. It is probable, almost to certainty, that in the earlier condition of the globe, its plants and animals were those of a simpler ("lower") organization, than some of the others which followed them; although always, even to the present times, animals and plants of an equally simple organization existed in abundance, along with those of a more complex ("higher") organization. Such changes were apparently progressive, proceeding generally from the simpler towards the more complex types of structure: invertebrate animals preceding the vertebrate; fishes and reptiles preceding birds and beasts; cryptogamic plants preceding phanerogamic.

A question naturally arises in any thoughtful mind, while contemplating these facts in their stony or earthy records, how plants and animals were first called into being, and by what means the later species were substituted in room of the earlier species? It has been repeatedly suggested, that one or more species may have first emanated from inorganic matter, and that succeeding species may have been formed by mutation or metamorphose of the preceding species. This hypothesis is plausible, to say the least of it. If adopted as a true theory, it would account for much that is at present obscure or incomprehensible. It receives strong analogical support in those metamorphoses which are well known to take place during the progressive development of individual plants and young animals. And there are, moreover, some facts which bear so decidedly on the subject, as to assume almost the character of direct evidence in confirmation of the theory.

On the other side, it must be admitted, when our attention is limited to the plants and animals now existing upon the earth, that much more *primâ facie* evidence is found to countenance a belief in the permanent distinctness of species; and that, consequently, the great majority of naturalists do steadfastly hold to this belief. And we may likewise say confidently, that all the clearest, most readily tested facts, directly tend to confirm the axiom of "*omne ex ovo.*"

Against these admissions, it may be fairly contended, that the formation of a plant or animal, from unorganized matter, could only be expected in the case of very small and very simply organized species; and that it is precisely in these cases we find the doctrine of "*omne ex ovo*" to be itself incapable of proof. And as to the metamorphose of one species into another, it must be remembered, that the very definition of "species" comes in the form of a *petitio principii;* since the widest change ever seen, in the descendants of any plant or animal, would only entitle them to the name of "variety," according to recognized usage in the application of these terms.

The author of the Vestiges pleads the case of the minority; and I will now quote his views, as briefly as possible, in his own words; strongly recommending his whole volume to the attentive perusal of phytologists.

"The nucleated vesicle, the fundamental form of all organization, we must regard as the meeting-point between the inorganic and the organic — the end of the mineral and beginning of the animal kingdoms, which thence start in different directions, but in perfect parellelism and analogy. We have already seen that this nucleated vesicle is itself a type of mature and independent being in the infusory animalcules, as well as the starting point of the fœtal progress of every higher individual in creation, both animal and vegetable, We have seen that it is a form of being which electric agency will produce — though not perhaps usher into full life — in albumen, one of those compound elements of animal bodies, of which another (urea) has been made by artificial means. Remembering these things, we are drawn on to the supposition, that the first step in the creation of life upon this planet was *a chemico-electric operation, by which simple germinal vesicles were produced.* This is so much, but what are the next steps? Let a common vegetable infusion help us to answer. There, as we have seen, simple forms are produced at first, but afterwards they become more complicated, until at length the life-producing powers of the infusion are exhausted. Are we to presume that, in this case, the simple engender the complicated?" * * *

"I suggest, then, as an hypothesis already countenanced by much that is ascertained, and likely to be further sanctioned by much that remains to be known, that the first [second?] step was *an advance under favour of peculiar conditions, from the simplest forms of being to the next more complicated, and this through the medium of the ordinary process of generation.*" — pp. 204, 205.

"The idea, then, that I form of the progress of organic life upon the globe, is, *that the simplest and most primitive type, under a law to which that of like-production is subordinate, gave birth to the type next above it, that this again produced the next higher, and so on to the very highest*, the stages of advance being in all cases very small—namely, from one species only to another; so that the phenomenon has always been of a simple and modest character."—p. 222.

The author of these passages would seem to be slenderly acquainted with Zoology, and still less conversant with Botany. He has thus written under considerable disadvantages; for it is to these sciences he must turn in search of facts which bear upon the transmutation of one species into another, or the production of one species from another different one. Our concern is with matters botanical; and we cannot compliment the author, on the value of his botanical evidences, which are here copied in his own words.

"It appears that, whenever oats sown at the usual time are kept cropped down during summer and autumn, and allowed to remain over the winter, a thin crop of rye is the harvest presented at the close of the ensuing summer. This experiment has been tried repeatedly, with but one result; invariably the *Secale cereale* is the crop reaped where the *Avena sativa*, a recognized different species, was sown." * * "Perhaps those curious facts that have been stated with regard to forests of one kind of trees, when burnt down, being succeeded (without planting) by other kinds, may yet be found most explicable, as this is, upon the hypothesis of a progression of species which takes place under certain favouring conditions, now apparently of rare occurrence."—p. 221.

Assuming these to be veritable facts, it may be suggested to the author, that they overprove his theory. The change of the oat into rye, is a pretty wide generic leap. And I am not at all aware that a burnt forest is forthwith succeeded by trees nearest allied, in specific or generic characters, to those which have been destroyed. The phenomena are here scarcely those of "a simple and modest character," or an advance "from one species only to another." Had we been told that the Avena strigosa could be so converted into the Avena sativa, or that a burnt forest of Tilia parvifolia would be succeeded by another of Tilia europæa, the changes would have corresponded better with the theory. In a future communication, I will try whether Botany cannot yield some facts more applicable as tests of this theory. Meantime we may leave it an "open question," which is not to be answered in the negative too hastily. HEWETT C. WATSON.

Thames Ditton, March, 1845

On the proposed Change of Name in Lastræa recurva.
By WILLIAM WILSON, Esq.

WITH all deference to those who propose a change in the name of Lastræa recurva, I must say that I see no reason whatever for discarding it: on the contrary, I think it very *apt* and *expressive*, and *in perfect harmony with the use of the term in other cases.*

WILLIAM WILSON.

Orford Mount, near Warrington,
March 17th, 1845.

On the name of Lastræa recurva. By CHARLES C. BABINGTON, Esq., M.A., F.L.S., &c.

As in a recent number of the 'Phytologist' Mr. Newman has taken upon himself to express his belief that I am the author of the review of his 'History of British Ferns' in the 'Annals of Natural History,' (Phytol. ii. 26); and as, in the point now to be noticed, I fully agree in the opinion there expressed; I take the liberty of replying to the article by Mr. Bree in the last 'Phytologist,' (Id. 75).

My idea of the *botanical* meaning of the word *recurvus* is derived from the uses to which it is applied by the best botanists. For instance: Smith says "*recurva* or *reflexa*, curved backwards," (Intr. to Bot. 118.). De Candolle, "*Recurvus, recurvatus, reflexus*, réfléchi, courbé ou fléchi en dehors," (Theor. Elem. 478). Bischoff, "*recurvatus* und *recurvus*, zuruckgekrümmt, answärts oder ab wärtsgekrümmt," (Wörterbuch der beschreibenden Botanik,' 170). Martyn, "*recurvatum* folium. A recurved leaf. Deorsum flexum, ut arcus superiora spectet. [Linn.] *Delin. Pl.* — Bent, or rather bowed or curved downwards, so that the bow or convexity is upwards," (Language of Botany). Bertolini,—"*recurvata*, deorsum flexa, curva, ut convexitas arcus superiora spectet," (Prælectiones Rei Herbariæ, 274).

On the Theory of "Progressive Development," applied in explanation of the Origin and Transmutation of Species. By HEWETT C. WATSON, Esq., F.L.S.

(Continued from p. 113).

MY former communication on this subject was intended to have an introductory character only. Two questions arise on the theory of progressive development, as set forth in the 'Vestiges;' namely, first, Can plants originate from unorganized matter?—secondly, Can plants of one species, in any way, produce individuals of another species?

To both of these questions the author of the 'Vestiges' seems ready to give an affirmative reply. But his attempt to base this affirmation upon the ground-work of facts, unfortunately, must be pronounced a thorough failure. Overlooking the best part of the evidence which might be adduced in favour of this hypothesis, he stumbled upon two or three pretended facts, which had been published only to be scouted as absurdly improbable; and which, when rightly examined, are really not in accordance with the theory which he advocates.

To the former of these two questions, our existing knowledge of Biology seems inadequate to afford any satisfactory answer. We can neither assert nor deny that plants do sometimes originate from inorganic matter. The pre-existence of a parent appears always necessary to the production of those species of more complex organization, with the propagation of which we are best acquainted. Yet this constant fact may not hold true with other species of very simple organization. And it should be conceded to those who advocate the theory of progressive development throughout Nature, that only the simplest plants could be expected to originate wholly or solely from inorganic matter. In truth, he is more hasty than philosophic in his judgment, who can believe himself entitled to assert, that the simplest forms of vegetable life (say, for example, a Protococcus) never come into existence, unless by the development of germs which have first constituted portions of a parent individual similar to themselves. On this first question, however, I do not wish to enlarge here. It is unsettled, and likely long to remain unsettled.

The second question, bearing on the transition of species, may be taken under consideration independently of any reference to the origin of organic nature. In this consideration we are not restricted to those very simple forms of vegetable life, the diminutive size of which puts insuperable difficulties in the way of correct observations. A pervading uniformity is everywhere seen in the operations of Nature, which may warrant a presumption that the same rule will hold true here, alike in the complex structures and in the more simply organized plants—whether that rule shall ultimately establish or refute the idea of a transition of species. I use this term "transition," to signify the production of one species from another, whether it be effected by descent, or in any other mode. And my purpose here is to point out the kind of evidence, upon the validity of which a decision must be made, in forming our opinions upon the matter. This evidence may be conveniently arranged under three general heads: —

1. Inferences which have been drawn from the past history of the earth, and those changes in the character of its Flora which have been brought to light by geological research.

2. The tendency of species to vary; and hence the production of such intermediate and connecting links between different species, as would warrant a presumption that no permanently impassable limits are assigned to them.

3. Direct facts towards establishing the transition from one species into another.

First, then, it will be conceded that many species of plants formerly flourished on the surface of the earth, which were quite distinct from those now growing around us in their stead. Further, there is good reason for believing that none of the present species existed in those remote periods. And it seems highly probable, if not certain, that past changes in the earth's Flora were effected gradually; the whole Flora of any one period not being destroyed in the aggregate, to make room for another entirely different Flora; — but that species after species disappeared, species after species appeared, singly and successively; no total change occurring at once, unless as a local event, which would not implicate the general Flora of the earth.

It is extremely difficult to account for these changes, by natural means, unless on the hypothetical assumption that one species produced another, under changed conditions of climate or other circumstances. In rejecting that hypothesis, we are thrown upon the supernatural alternative of assuming, quite as gratuitously, a direct and oft-repeated exercise of Creative Power. But this latter assumption is not consistent with anything now seen in Nature, where all seems to proceed uniformly, in accordance with pre-settled laws. Still, gratuitous though it is, the supernatural alternative is the one generally received by the vulgar, and admitted — tacitly, at least — by men of science. The author of the 'Vestiges' found this impediment in his way, and he has accordingly penned some arguments against it, which I will quote in preference to stating my own ideas on the subject. The arguments apply to plants equally as to animals.

"It may now be inquired," he writes,—"In what way was the creation of animated beings effected? The ordinary notion may, I think, be not unjustly described as this, that the Almighty author produced the progenitors of all existing species by some sort of personal or immediate exertion. But how does this notion comport with what we have seen of the gradual advance of species, from the humblest to the highest? How can we suppose an immediate exertion of this creative power at one time to produce zoophytes, another time to add a few marine mollusks, another to bring in one or two conchifers, again to produce crustaceous fishes, again perfect fishes, and so on to the end? This would surely be to take a very mean view of the Creative Power — to, in short, anthropomorphize it, or reduce it to some such character as that borne by the ordinary proceedings of mankind." . . . . "Some other idea must then be come to with regard to *the mode* in which the Divine Author proceeded in the organic creation." — p. 153.

There is small likelihood that the stone tablets of Geology will ever yield an explanation of the "mode" by which the exchange of species was brought about in past eras. In the absence of real knowledge we take up an hypothesis which best accords with the facts, when we seek to explain past events by assuming, hypothetically, that one species changed into or produced another.

Secondly, we have to consider whether species are distinguished from each other by definite and permanent characters, or whether they vary to such a degree as may justify a doubt respecting the existence of impassable limits between them. For the present I must write of "species" as commonly understood by botanists, without attempting any rigorous definition of the term, which may hereafter be found to represent only a fiction of the human mind. Philosophical thinkers now regard the larger groupings of systematic Botany, orders and genera, in the light of conventional unions only. But almost all botanists believe species to be something real and permanent in Nature. The prevailing belief apparently is, that individual plants of the same species vary among themselves only within limits comparatively narrow; that they can be distinguished from those of different species by certain peculiarities of structure or form, which are technically called "characters;" that these characters are constantly repeated in their descendants; and that the distinctive characters of one species are never assumed by the progeny of another species.

It must be confessed, however, that there is much difficulty in reconciling this belief with the familiar fact, that in many genera the number and distinctions of the supposed species seem to depend pretty much upon the fancy of the botanists who describe them. Thus, in the genera Salix, Rosa, Rubus, Mentha, Viola, Festuca, Poa, Saxifraga, Cerastium, Hieracium, Polygonum, Myosotis and others, the number of species may be held optional with botanical authors. Such a remark may startle some of our great "species-botanists;" and yet, in the short table below, we have something very like a proof of its correctness. The table is intended to show the number of indigenous species in some of these genera, varying according to the author who describes and catalogues them.

	Salix.	Mentha.	Rosa.	Rubus.	Saxifraga.
Hudson (1791), ...	18	6	5	5	9
Smith (1824—8),	64	13	22	14	25
Lindley (1835), ...	29	9	17	21	24
Hooker (1842), ...	70	13	19	14	16
Babington (1843),	57	8	19	24	20
London Catalogue (1844),	38	8	7	34	16

Some few of the species were first discovered in this country during the present century; but these novelties will go only a short way towards making up the wide differences between Hudson and Smith. The grand cause of the varying numbers arises from discordant views about species and varieties; those forms which by one author are described for distinct species, by another are included together as varieties only of the same single species. I select the genera named above, as examples of uncertainty in numbers, because their described species are numerous. Equivalent differences will appear in other genera, where the species are few. Thus, Hudson's solitary (or, dubiously, two) species of Myosotis has now expanded into eight. His six species of Viola have been increased to ten, although they are now again reduced to six or seven. From his two species of Betula we have seen four made, and a fifth is now threatened under the significant sentance of "probably a distinct species." So, on we might go, with the species of many other genera. It will be borne in mind here, that the plants of Britain have been long and carefully studied by many able botanists; and it would hence seem impossible for such differences of opinion still to exist among them, unless the distinctions and limits of species were truly very uncertain — not to write, arbitrary.

The preceding examples are derived from plants in a state of nature. When brought under cultivation, and it becomes the interest or amusement of cultivators to increase and extend their variations, scarce any limit can be set upon the power of doing so. Our cultivated species of Pelargonium, Erica, Rosa, Fuchsia and Calceolaria, have now become respectively an undistinguishable intermixture of cross breeds and varieties. The changes brought about in long-cultivated fruits and vegetables seem to prove that varieties of a single species may differ quite as widely among themselves, as do other plants which are usually accounted distinct species. We have examples in the apple, pear, plum, gooseberry, strawberry and grape, among fruits; in the pea, potato and cabbage, among vegetables. To these we might add other examples in florists' flowers; such as the Dahlia and pansy, which have been so greatly run into varieties in the course of a few years past.

The numerous and still increasing variations in the species above mentioned, afford clear proofs that the progeny is not necessarily a copy of the parent, varying only in luxuriance or other slight and temporary character. In the course of generations some descendants differ so widely from their ancestral plants, as to appear like distinct species, when they are contrasted against other less changed, or unchanged, descendants from the same ancestors—or, at least, what are supposed to be such. We find, indeed, a conflict of opinion in some cases, whether the wild and the cultivated species have been derived from the same common stock, or whether they have been aboriginally distinct. Let us make a short series, in example of this, where the uncertainty respecting an original identity of stock will become greater and greater. It is generally agreed, I believe, that the wild thorny pear is the original stock of all our garden pears, various though they are. It is not quite so generally allowed, that the wild thorny crab of our hedge-rows is the true stock of the garden apples in their countless varieties. More doubt attaches to the wild sloe or the bullace (or both, as two forms of a single species) in the light of a common stock to all our plums of the garden. And very few botanists seem prepared to receive the wild cherry (*Prunus Cerasus*) as the real stock of the garden cherry (*Prunus avium*). Some of our Cerealia cannot be referred to any known wild stock: whether the original species has ceased to exist in a state of nature, or whether the long-cultivated varieties have lost resemblance to their original stocks, might be made a question which would not be likely to find any speedy solution in response.

With such examples before our eyes, we are bound to concede to the transitionists, that plants do possess a capability of wide variation from any one form which we may choose to select for the normal or typical form of a species. But are these variations sufficiently wide to give any probability that one species may pass gradually into another? As a reply to this query, I will now cite some few instances of admitted species being tied together (so to speak) by a series of intermediate forms.

According to the usual application of the term, it may be safely assumed that Geum urbanum and Geum rivale are two distinct species. They are easily distinguished by several well defined characters; and I do not recollect that any botanical authority has united them under a single specific name. Yet intermediate forms between them have long been familiar to botanical eyes, and which have usually been accounted varieties of one or of both the species above named. These intermediate forms have been commonly clubbed together, under the single name of "intermedium;" this name meaning a third species in the estimation of some few botanists, a variety in that of most others, or a series of intermediate varieties in the eyes and ideas of another and smaller section of botanists. The Geum intermedium is taken up as a distinct species, by our present great adopter and maker of dubious species, who writes, "If this plant is not a distinct species I do not know to which of the others it should be referred." There is, however, a strong objection against regarding the plant as *a* "distinct species," in the fact, that it is not one clearly defined form, with characters intermediate between those of two other well marked forms; but that it is really a group or series of intermediate forms, which run into Geum urbanum, at one end of the series, while approximating also to Geum rivale at the other extremity. Apparently, both species sport into varieties; and these varieties run so near together as to have been combined into one supposed third species.

We obtain another familiar example in the cowslip and primrose. Though some degree of doubt may have been expressed occasionally, the prevailing opinion has clearly been, that Primula veris and Primula vulgaris are truly distinct species. They are so dissimilar that every country-bred child can distinguish them with the greatest facility. They are extremely abundant in many places; and thousands or tens of thousands may be examined without any decided example being found which would indicate the transition from one towards the other species. Notwithstanding this, intermediate forms are occasionally seen, which exhibit a series of steps from the common primrose (*Primula vulgaris*) towards the cowslip (*Primula veris*), and which have usually been mistaken, in this country, for a different species (*Primula elatior*, of Jacquin). On the other side, there is a variety of the cowslip which makes a considerable step towards the primrose, in its larger, paler, and nearly flattened limb of the corolla. This latter is the Primula veris, var. *major*, of the London Catalogue. It has been supposed that those varieties of the primrose which approximate nearest to the cowslip, are hybrids or mule-breeds between the two received species. This conjecture may be correct, although the supposed hybrid wants one of the peculiarities usually expected in true mules; namely, that of sterility. (See Phytol. i. 9, 232, 1001).

It would not be difficult to adduce other examples of two reputed species apparently passing one into the other by intermediate varieties. But in the pages of a monthly periodical I can give only few examples in any detail. In most instances, perhaps, where two alleged species are thus connected by intermediate varieties, the distinctness of the two species is called in question for that very reason. Thus, in the eyes of some botanists, the cases would resolve themselves into examples of variation in single species, rather than instances of connecting links between two species. Teucrium scordioides passes into Teucrium Scordium, by a gradual variation of character; but the former is rightly deemed a dubious species. So also of Erica Mackaiana, a very dubious species, which may be traced, step by step, into a form scarce distinguishable in any way from Erica Tetralix. Betula glutinosa passes into Betula alba; Veronica humifusa shades into Veronica serpyllifolia; Rumex conglomeratus into Rumex sanguineus; Avena alpina into Avena pratensis; Festuca loliacea into Festuca pratensis; F. pratensis into F. elatior; F. elatior into F. arundinacea; Viola lactea into Viola flavicornis; V. flavicornis into V. canina; &c. &c.

From such facts as these — whether seen in the wilds, produced in the gardens, or recorded in books — are we not forced to concede to the transitionists, that the notion of permanently impassable limits between species, whether true or false in itself, wears rather a doubtful aspect at present? Still, we cannot altogether concede that the mere existence of wide varieties, or of intermediate forms between alleged species, will sufficiently warrant a presumption against the reality of such limits. Moreover, it is to be remembered, that some species stand isolated from all others by broad characters of difference which cannot well be supposed passable at a leap. The Linnæa and the Adoxa are examples of this among our indigenous plants.

We have still to inquire about direct facts towards establishing the transition from one species into another. This will be a difficult subject to treat, because the very definition of the term "species," as usually given, involves an assumption of non-transition; so that any case of real transition—supposing such a case to be adduced—would be set down simply as evidence to disprove the duality of the species. I must reserve this inquiry for another communication, lest it should extend the present paper to a length incompatible with the limits of the 'Phytologist.' HEWETT C. WATSON.

Thames Ditton, April, 1845.

A List of the Musci and Hepaticæ of Yorkshire.

By Mr. RICHARD SPRUCE, F.B.S.

As I am on the point of setting out on a Botanical expedition to the Pyrénées* and the south of Spain, and it is quite uncertain what length of time may elapse ere my return, I venture to solicit your insertion in 'The Phytologist' of the following list of Yorkshire Musci and Hepaticæ, which includes all the mosses that have been added to the Flora of the county since the publication of Mr. Baines's work. As a mere *list* of Yorkshire species, it is as complete as I have it in my power to make it, but the pressure of preparation for my intended

* My object in visiting the Pyrénées is to collect and publish the flowering-plants, Mosses, Hepaticæ and Lichens of those mountains. I hope to have the Phanerogamic portion of the collection ready for sale in London by the end of autumn; the accurate determination of the species of the Cryptogamia will be a work of time, but they will appear as early as possible after the flowers, and I propose to publish them in the style of Drummond's 'Musci Americani.' Those of the readers of 'The Phytologist' who have been in the habit of receiving specimens from me, will be able to form an idea of the manner in which my Pyrenean collections will be got up, and I much regret that the confining nature of my profession has prevented me from cultivating so extensive a botanical correspondence as I could have wished. I may add, however, that the specimens will be as perfect in every respect as it is possible to procure and to render them.

I contemplate, ere my return to England, to devote several months to the examination of Andalusia, and especially to the Sierra Nevada, with the same objects in view. The vegetable productions of this rich but imperfectly known country are more interesting than even those of the Pyrénées, and I have reason to anticipate the discovery of many novelties.

On the Theory of "Progressive Development," applied in explanation of the Origin and Transition of Species. By HEWETT C. WATSON, Esq., F.L.S.

(Continued from p. 147).

IN my former remarks on this subject, I left, for a separate communication, the "crucial" inquiry about any facts directly in proof of a transition of species, one into or from another. Theoretically, a species comprehends all the individual plants which are descendants (or might have been descendants) from a single progenitor, how wide soever their differences may have become in course of many descents. Practically, this idea of a species is utterly disregarded by the botanists who describe and give names to plants; scarce any of them ever trying a single experiment, in order to ascertain whether species A can or cannot be raised from the seed of species B. With botanists the practical inquiry is merely a search for some one or more physical characters, usually those of shape or proportion, sufficiently obvious to be readily seen in dried specimens, and sufficiently uniform to become marks whereby to distinguish the plants. If such characters can be found, the plants are described as distinct species; and this is done, even although only "a single specimen, and that none of the best," has been seen by the describer. That potent organ in the brain, called by phrenologists the "Love-of-Approbation," or (better name) "Love-of-Notoriety," stimulates many of our botanists to seek out even the most trifling differences, upon which to found a pretence for "making a new species," and giving it a name. This circumstance, together with the frequent change-naming and cross-naming of plants, has rendered it customary of late, to add also the surname of the botanist who first applied to any plant the technical name by which it is designated. This addition of the botanist's own name should have removed much of the uncertainty occasioned by changes and misapplications of names of plants. Unfortunately, by giving a powerful stimulus to the Love-of-Notoriety organ, the custom has tended greatly to increase the confusion and uncertainty of plant-nomenclature.

The consequence now is, that we have many gradations of species— so to speak. Some species are universally admitted distinct by all botanical authorities; as Betula alba and Betula nana. Other species are received as such by the majority, though questioned by some few; as Primula veris and Primula vulgaris. With regard to others, opinions may be held equally balanced or thereabouts; as Ranunculus aquatilis and Ranunculus circinatus. Many more are deemed varieties by the majority, while the minority (one, two, three, or more) describe them to be species; as Alchemilla alpina and Alchemilla conjuncta.

The step from those plants which are allowed by all to be simply varieties, into others which only very few botanists (perhaps only a single botanist) suppose to be distinct species, must be a very small step indeed. And once among these dubious species, we may ascend, step by step, from the least to the most generally admitted. A single step more, and we arrive at the universally admitted species. At which, of all these little steps, are we to find the impassable barrier between varieties and species? Where does the possibility of transition cease, and the impossibility succeed?

Notwithstanding a mere theoretical definition, never really applied by way of test to one species in a thousand, the assumed difference between species and varieties, the capability or incapability of transition, is simply conjectural—an unproved idea of the mind—a *petitio principii.* The assumption is so far a safe one, that it never can be disproved, never can be put to a test which would be conceded by its believers. Could any one raise a beech tree from the acorn of an oak, the botanists might fall back on their theoretical definition, and argue that the fact only proved the beech and oak to be varieties of one single species. While the transitionist, on the other hand, would feel himself entitled to put forward the fact as a confirmation of his views; namely, that one species could give origin to another different species.

No doubt so wide a transition as that of an oak into a beech, were it possible, would shake the faith even of the most unreasoning botanist. But it is equally an arbitrary assumption on the part of botanists, to say that a cowslip and primrose are proved varieties of a single species, if one can be raised from the seeds of the other. The distinction is one of degree only; the oak and beech being more dissimilar, the cowslip and primrose less dissimilar.

Still, the tendency of like to produce like, is so evident and decided throughout the best understood operations of Nature, that botanists may reasonably call on the transitionist to prove, if he can, that the exceptions to this tendency may extend so far as species. On the other side, the transitionist may plead that he should not be required to show cases of change between very dissimilar species; but that he creates a presumption in favour of his views, when he adduces instances of transition in plants which are held to be distinct species by botanists of acknowledged skill and reputation.

I must now become, temporarily, a sort of advocate for the transitionist, in adducing some examples which look very like cases of transition. Assuredly I can bring none so wide as the alleged conversion of the rye into the oat; which, I may safely assert, is credited by extremely few botanists. But facts of minor conversion are not altogether wanting; and if more diligently looked for, they might be found more numerous than is at present supposed to be the case.

Viola canina (*Linn.*) and Viola flavicornis (*Smith*). — The dog's violet is the commonest species of its genus in Britain. Being found under very different conditions of soil, shelter, humidity, &c., it runs into several varieties; so that the line between this one and allied species (so reputed) is drawn differently by botanical authorities. One of these (species or varieties, as opinions may run) is the Viola flavicornis of Smith—not the dwarf variety figured under this latter name in 'English Botany' (2736); but the one described in 'English Flora,' and specimens of which are preserved in Smith's herbarium. The V. flavicornis grows on open commons, and it presents several differences of physical character, when compared with the ordinary forms of V. canina which are seen in coppices and hedge-rows. The differences are not very strong, yet are quite as wide as those which are deemed sufficient to distinguish species in the same genus, or those in other genera. It has been stated, also, that these peculiarities remain unchanged in living specimens after removal into a garden. I have not found this stated fact to hold true with a plant brought into my own garden. An example of V. flavicornis was removed from a common in Surrey, into my garden, when flowering, in 1841. Being absent in the summer of 1842, I did not see it during that season; but in 1843 and 1844, it had assumed so much the size and shape of leaf, with other peculiarities which belong to V. canina, as to be barely (if at all) distinguishable from some forms of the latter, when pressed and dried. Moreover, I have raised plants in a flower-pot, from the seeds of a wild example of V. flavicornis, which came still nearer to the more usual form of V. canina than did the changed garden plant. In neither case, has the typical form of V. canina been fully acquired — perhaps, it was not to be expected so rapidly; but together with a series of wild specimens in my herbarium, they suffice as links of connexion between the two reputed species.

Polygonum maritimum (*Linn.*) and Polygonum Raii (*Bab.*) — The plant which is now becoming familiar under the name of Polygonum Raii, has been imperfectly known to the botanists of England for many years. About the year 1831, when a very young botanist, I was struck by the difference between this plant and P. aviculare, with which it had previously been associated; but the specimens then sent to the author of the 'British Flora,' were placed as a variety of P. aviculare, in the second or third edition of that work. In the fifth edition, it appears as a distinct species, under the name of P. Roberti; but the identity of our plant with the P. Roberti of the continent being doubtful, Mr. Babington has described it under the name of P. Raii. I am not aware that any botanical author has yet concurred with me in deeming it rather a variety of P. maritimum, than of P. aviculare. Those who do not believe it a variety of P. aviculare, hold it a proper species. P. Raii is technically distinguished by the few and unbranched nerves of its short ochreæ, the long internodes, loosely trailing habit and annual root. In P. maritimum the ochreæ are longer, with more numerous and branching nerves, the internodes very short, the root perennial, and the plant forming a suberect close bush. Yet the seeds of the true P. maritimum, collected in the Azores and sown in my garden, produced plants in 1843, which partook much of the physical characters of P. Raii from the shores of Britain. They had the loosely trailing growth and long internodes of P. Raii, though nearer to P. maritimum in their ochreæ; and they proved annual in this climate. Other examples, raised from the seeds ripened in 1843, had rather reverted back again towards P. maritimum in the drier and warmer summer of 1844; having their ochreæ larger, internodes shorter, and leaves broader and more coriaceous, than was the case in the examples of 1843. Further experiments will require to be made on these plants; but I may mention one circumstance which will show that the general appearance of my garden plants, of 1843, approximated to that of the British P. Raii. One specimen was sent by post to a well-known Professor of Botany, who has collected P. Raii in its native localities, with a request that he would name the specimen. His reply was "P. Raii." I wish that some kind botanist would send me ripe seeds of P. Raii, for a trial how near this could be brought to P. maritimum.

Lolium perenne (*Linn.*) and Lolium multiflorum (*Lam.*) — English agriculturists have latterly been sowing the Lolium multiflorum, which they call "Italian Ryegrass," instead of the better known L. perenne of Britain. That there is some decided difference between the two species, and that this difference is perpetuated by seed, may be inferred from the preference shown for the Italian ryegrass. The most conspicuous distinction between them, botanically speaking, occurs in the awned paleæ of L. multiflorum. Besides this, the spikelets are composed of more numerous flowers, whence the specific name; and the plant is usually of a paler colour and more upright growth. It has been stated, as a further distinction, that the L. multiflorum is annual, producing no "barren shoots." On examining this grass in sown fields, I have found a very large proportion of the plants corresponding with the alleged characters of the species; but I have also found among them examples in exception to each one of the distinctive characters in turn; some having the awns very small or obsolete; some having fewer flowers in the spikelets than L. perenne; some producing barren shoots, &c. About Midsummer, 1843, I transplanted a root from a sown field of L. multiflorum, into a small flower-pot; cutting down the flower-stems, and supplying the plant rather sparingly with water. It grew rapidly, soon filled the flower-pot with its roots, and again produced flowering-stems in September and October. The flowers were now less numerous than usual in the spikelets of L. perenne, and were scarcely awned at all. This same plant lived through the winter in the flower-pot, and was transplanted into the open ground in spring. In the summer of 1844, it grew into a strong tuft, producing many flowering-stems, with numerous flowers in the spikelets, bearing very short awns; also many barren shoots; the colour of the whole plant being equally deep green as that of L. perenne. It was scarcely distinguishable from L. perenne, except by its short awns—if this can be deemed a distinction, for L. perenne is occasionally awned in Britain. My observations and experiments upon this grass were intended to try the constancy of its distinctive characters; and thus the case is left short of full transition, although the changes went so far as to give a strong presumption in favour of the possibility of transition.

Primula veris (*Linn.*) and Primula vulgaris (*Huds.*)—In my second paper on the present subject, I cited some examples of two reputed species being so connected by intermediate varieties, as to cause difficulty in tracing any clear line of distinction between them. One of these examples was found in the cowslip and primrose, which are closely connected by intermediate varieties, usually called oxlips. These varieties occur under such circumstances as create a presumption that they are the offspring of one or both of the two species mentioned. I have lately proved by direct experiment, that the seeds of an oxlip, all taken from the same plant, at the same time, and sown together, will produce a mingled assemblage of cowslips, oxlips and primroses; the oxlips forming a series of intermediate forms, passing into the cowslips at one extremity of the series, and into the primroses at the other extremity. I hope shortly to publish a detailed account of this experiment, and shall therefore not give more exact particulars here. I had expected to obtain primroses and oxlips, but had not anticipated the occurrence of cowslips also. It is true, the recorded experiments of Herbert and Henslow might have led me to expect the result which appeared; but I may now confess a lurking suspicion that some unascertained cause of error had been at work in their experiments. And since Hooker, Babington, and other botanists still continued to describe the cowslip and primrose as two distinct species, I may presume that they were also sceptical on the point. Now I can see only a choice between two inferences; namely, that the cowslip and primrose are a single species only, or, that one species can pass into the other in two descents—the oxlip being the intermediate step. The experiments of Herbert and Henslow show the cowslip passing into the primrose in one descent.

Festuca pratensis (*Huds.*) and Festuca loliacea (*Huds.*) — For half a century past, it has been customary with British botanists, to describe the Festuca pratensis and F. loliacea as two distinct species. The difference between them has appeared so strong in the eyes of some botanists, as to warrant them in placing F. loliacea under another genus (Brachypodium). In Steudel's Nomenclator, which bears the date of 1841, they are entered as distinct species; as also in the Catalogue published the same year for the Botanical Society of Edinburgh. I had, however, seen some evidences that one could change into the other, before the Edinburgh Catalogue was published; and in the same year of 1841, I brought a wild root of F. loliacea into my garden. Though planted in close unworked soil, it had become a large tuft by 1843, and in the summer of that year it produced numerous flowering stems. Some of the stems retained almost exactly the character ("spiked raceme") which distinguishes the wild F. loliacea; while others of them had so far assumed the branched or panicled inflorescence of F. pratensis, that a botanist would assuredly have assigned them to F. pratensis, unless informed that they had been taken from a root of F. loliacea, or shown the intermediate forms, which were also produced from the same root. A root of F. pratensis, removed into the same garden, became in 1843 rather less like F. loliacea, than it was in its wild state; but in the dry summer of 1844, some of its panicles were reduced nearly into racemes. I have also seen these two reputed species pretty closely connected in a series of wild specimens, collected by Mr. Tatham, in the neighbourhood of Settle. *In this case,* F. loliacea appears to become F. pratensis simply by increased luxuriance, which is favoured by the free space allowed to it in the garden.

Tolpis umbellata (*Bert.*) and Tolpis crinita (*Lowe*).—Those characters which are sufficient to warrant the assignment of plants to two different genera, should be of a more important kind than are the characters which suffice only to distinguish two species of the same genus. In the Prodromus of DeCandolle, the Tolpis umbellata and T. crinita, though brought under the same genus, are assigned to different sections of their genus. These sections represent the genera of other authors, Drepania and Schmidtia, founded on differences in the pappus of the fruit, akin to those which separate Thrincia from Leontodon. In the year 1842, I collected specimens and seeds of Tolpis crinita in the Azores. The specimens corresponded with one from Madeira, which was given to me under the same name by Dr. C. Lemann, who has enjoyed the best opportunities for becoming well acquainted with Mr. Lowe's plants. The seeds were sown in my garden, and produced plants which I could refer only to T. umbellata. I communicated one of these living examples to Dr. Lemann, and he wrote me that the plant was T. umbellata; thus corroborating my own view of them, and showing that Tolpis (Drepania) umbellata and Tolpis (Schmidtia) crinita are not permanently distinct species — much less distinct genera. This instance, if so explained, may be considered a case of unnecessary "hair-splitting" in the formation of genera. Or, on the other side, the transitionist may argue that characters which have been deemed sufficient to separate genera, may be acquired and lost in such manner as should throw doubt on the supposed impassable distinctions of species.

Orchidaceous genera. — Mr. Schomburgk published a paper in the Linnean Transactions, to show that orchidaceous epiphytes, referred to three different genera by first-rate authorities in this order, could change into or produce one another. One of the plants "produced a scape with six flowers of Monachanthus viridis and two of the Myanthus barbatus, while a second scape of the same bulb had twenty-five blossoms of the Myanthus barbatus." The same combination of genera occurred on a second plant in another collection. A third plant produced the flowers of Monachanthus viridis at one period, and those of Catasetum tridentatum at another time. And on Mr. Bach sowing the seeds of Monachanthus viridis, one among the plants produced a scape with the flowers of Catasetum tridentatum. Here, also, it may be said that the plants had been incorrectly described as different species and genera. But the fact still shows that cases of transition can occur, where the differences were so wide that a first-rate botanical authority deemed the plants to be not only specifically, but even generically, distinct. In fact, nothing less than the actually observed transition would have caused botanists to unite the three into one species.

Among the cellular plants there are instances alleged, which, if correct, would establish the possibility of transition from one order to another. Perhaps, not much stress should be laid on these instances at present. I do not know that stronger examples than the preceding can be adduced from the vascular plants. Their tendency is in favour of the theory of transition; although, from admitting of a different explanation in each example, they do not yield unquestionable evidence in support of that theory.

I will not write more on the subject just now; though it may perhaps be desirable to add two or three pages more, on a future occasion, for a short summary of the leading arguments, on both sides of the question. I have curtailed argument as much as possible, under the idea that the reasoning faculties are so poorly developed in botanists (as a class—but with exceptions) that very few of them will feel any interest, or see any importance, in such an inquiry. The idea of its bearing in any way on the moral condition of the human race, will doubtless appear ridiculous before the eyes of nineteen in twenty botanists. But slender as may be his knowledge of plants, the author of the 'Vestiges' can see much farther than this into Nature and Nature's laws. HEWETT C. WATSON.

Thames Ditton, May, 1845.

Note on Luzula congesta, (Smith). By THOMAS BENTALL, Esq.

MR. BABINGTON, contrary to the opinion entertained by some other botanists, still considers this to be a distinct species; and describes it in his Manual under the name of Luzula multiflora, (*Lej.*) The characters by which Mr. B. distinguishes it from L. campestris, are the greater comparative length of the filaments, and the oblong (not reniform) seeds. The following remark is appended to the description:—"I introduce this as a species, in order to draw attention to the character which appears to distinguish it from L. campestris, that its constancy may be ascertained." It appears to me that there has been some misunderstanding connected with these plants. In the 'British Flora' it is stated that both grow together, which I believe is *rarely the case*, as L. campestris abounds most in open meadows and

Odonthalia dentata	Porphyra vulgaris	Rhodomenia palmata
Plocamium coccineum	Ptilota plumosa	Ulva latissima
Polysiphonia fastigiata		

On viewing the list it will be seen that the phanerogamous plants collected represent 33 natural orders, and amount to 79 species and one variety. The list of cryptogamic plants is by no means complete, partly from the short time allowed for the examination of the island and partly on account of many of the mosses and lichens not being in fructification. There were observed 7 species of ferns, 14 mosses, 4 Hepaticæ, 19 lichens and 14 sea-weeds, making in all 68 cryptogamic species. It will be remarked that the ferns are in the proportion of 1 to about every 11 of the flowering plants; and taking phanerogamous plants and ferns together, the latter will form nearly 1-12th of the species.

J. H. BALFOUR.

Glasgow, June, 1845.

Cursory Thoughts on the Philosophy of Botany. By EDWIN LEES, Esq., F.L.S., &c.

I HAVE frequently had to complain, either orally or in writing, of the contempt cast upon the "mere botanist,"—a favourite term used by professed philosophical writers, as if there was something paltry and senseless in the pursuit of Botany itself, technically considered;—something so very mechanical, that *thought* was never called forth by it, *reflection* never aroused, or *truth* sought for or arrived at. Such ideal degradation of labourers in other walks than their own, if not excusable, may be accounted for; but surely the unkind aspersion should not come from the practical botanist to his own brethren. Mr. H. C. Watson, has, in some of his late papers, however, rather unnecessarily fallen foul upon the humble yet perhaps not altogether inutile tribe of plant-collectors, who, as observers and recorders of "unconsidered trifles," are denominated hair-splitters, and species-splitting monomaniacs.* This seems rather unqualified language to apply to poor wandering simplers after rifling their stores! As Mr. Watson's name is so deservedly honoured among British botanists, I presume he has a license, like the heroes of old, to brándish his battle-axe on all sides without let or hindrance, though almost as much to the terror of friend as foe; but in his last flourish it has so nearly fallen upon my own toes, that if no one else calls out, I must.

* *Phytol.* Vol. ii. pp. 161, 219.

"What are we to say," observes Mr. Watson "about the frivolous attempts at species-making among the Rubi and Polygona in vogue at present, as among the Rosæ and Menthæ in former years?"* I adduce this sentence, though the last on the record, as rather coming home to myself from having laboured at what Mr. Watson thus by implication condemns, and object entirely to the spirit in which it is written. Why should it be esteemed more frivolous to attempt unravelling the intricate forms of the Rubi, than to sow primrose-seeds and make varieties of their produce? If Mr. Watson will really allow thought on the subject, by others as well as by himself, then I should be disposed to say, that not only were the observers of Roses in times past doing good service to Botany, but that the observers and describers of Rubi and Polygona, as well as the experimenters on the permanent characters of species in any family, are doing so now.† Mr. Watson's remarks tend to repress observation except in his own way; but surely knowledge is only to be obtained correctly by unrestricted observation on all sides.

But why this objection to "species-making,"—or rather the observation of minute differences in plants? If this minute attention be not given, do not the greatest mistakes arise? If, then, an individual plant differing from another in some particular point is not to be noted, why attend to species at all, or attempt to set bounds to them? Better at once say with Thomson, as we contemplate the flowery meadow and its grasses, "beyond the power of botanist to number" up their forms. But if Mr. Watson admits the discovery or designation of species to be advantageous, then why decry that attentive examination of them which every tyro in Botany has been taught it is important to attend to? But here we come to the *opprobrium botanicum*—the definition of species so carefully constituted as to form what Mr. Wat-

* Phytol. ii. p. 219.

† I wish botanical writers would exercise a little more candour and forbearance as well as due appreciation towards their compeers and fellow-labourers than is usually the case, and not at all events attribute any depreciating motive as influencing their labours—*if they can help it.* Yet alas! somebody or other has always to complain on this score. Sir James Smith murmured at Dr. Hooker's making nought of all his efforts on the willows, and the latter possibly thinks he may have been slighted in his turn. Dr. Lindley warmly reproached the friends of Sir J. E. Smith with not allowing him to participate in the spoils of the Rubi, as he says they "determined to keep the game in their own hands;" yet he himself with equal injustice denounced the school of Linnæus as an "incubus upon science," and as leading to no one useful purpose. Now Mr. Watson comes to the charge, blaming botanists for "love of approbation," or as seeking notoriety, and I, in my turn, grumble at his uncharitableness!

son calls an "impassable barrier between varieties and species." If this is not to be expected or attainable, then an arbitrary boundary must be proposed, subject to the influences of observation and experiment; and this really renders it expedient that at one time a variety should be named as a species, and at another a supposed species subside into a variety, just as the evidence before an observer preponderates one way or the other. If this be inconvenient to the systematist or botanical statist, it must be submitted to, till it has been decidedly shown what are the characters to distinguish species from variety in every natural family.

It is doubtless true, as remarked by Fries, that small is the difference that depends upon a hair, and yet a hair's breadth may be a sufficient line of demarcation between safety and destruction, and therefore not quite to be despised. But until botanists have decided what is absolutely essential to specific distinction, and what is not so, in every family, we may be justified, I think, in attending to minute characters, and noting them, until extended observation produces conviction of truth or error. But is not the variety of Nature's productions a source of the most ravishing delight, and the contemplation and examination of her numerous vegetable forms a pursuit well worthy of our attention, as giving rise to mental pleasure, and exercising the perceptive faculties? Our predecessors in the field, indeed, have only left us in our own country the gleanings of the harvest; but let us not rest satisfied that they have done all that can be accomplished, but carefully look out for ourselves. Some botanists appear displeased with Nature because she smiles at the rules of art, and hence they would, if possible, fetter her within their own definitions. In their capriciousness they will expand some genera agreeable to them with well-turned species, but others must remain locked up with all their inmates, and no liberty is to be allowed *them*. How many fresh delights have opened upon me since I studied minutely the characters of the Rubi, unchilled by the remark too often made on every hedge, that it is only Rubus fruticosus that is there! And as to the objection of an herbarium's containing too many specimens of varieties or supposed species, I am of opinion that it is only by the study of numerous specimens that a fair judgment of the claims of any species can be arrived at, and that it is injudicious to found a species upon a single specimen only.

I think also, that it is unfair to conteud sweepingly that botanists in general are guided in all they do by a "love of approbation" or notoriety-seeking. This is not *my* experience of my own botanical acquaintance, and many have I known whose love of Nature's beauties was as enthusiastic as it was modest, unassuming, and unaffected. Perhaps there may be occasionally professional aspirers, who, anxious to gain the top of the tree, may be careless of disarranging its branches, if the rustling they make only brings them into notice ; such a casual disturbance may knock the dry sticks about our heads, and call for Mr. Watson's reprobation; but such an annoyance from notoriety-seeking, if that be the only motive, is not likely to be of long continuance, nor are the whole body of practical botanists to be held responsible for it. Without insisting upon the principles of phrenological development in the matter, I should judge the feelings of the botanical rambler to be instigated first by the *love of novelty*, for this is common to us all, and to "range in fresh fields and pastures new," or gather for the first time, as Lucretius says, "new flowers," is exciting even to the uninitiated.

> "'Tis not for nothing that we life pursue,
> It pays our hopes with something still that's *new*."—*Dryden*.

The love of knowledge follows upon the excitement of novelty, and we hasten to understand what we have discovered ; and surely it is but cold comfort in return for our efforts to be told that instead of having progressed in knowledge, we only show our deficiency in reasoning powers, but have the bump of notoriety well developed ! It would, I think, be but charitable to infer that in most instances truth is sought after; for if a plant be found really not answering to recorded descriptions, I cannot but think it deserves to be noted, even if it eventually turns out that it is the description only which requires correction. Instead, therefore, of Mr. Watson's too sweeping condemnation of "species-making," as he terms it, I would propose a resolution by way of amendment, restricting all *young* botanists from publishing new names till they had studied the science for at least five years, and preserved their specimens for examination and criticism. But I think if a person has made any class, family, or genus, his peculiar study for upwards of five years, it is but fair to infer that he has found out something, and if so, let us by all means have the benefit of his labours, even if a change of names or a new species does result in consequence.

That the term *species*, as Mr. Watson suggests, requires a more extended definition, or recasting, may be correct ; or rather perhaps the *characters* on which a species is supposed to depend, are not the same in every family, and hence a too rigorous form of words will be in all cases inapplicable.* Certainly, I think this requires to be looked into, for if characters are employed to determine species which are variable in themselves, the fault rests in the employment of this exceptionable character. Thus the involucra were formerly employed to determine the species of the Umbelliferæ, and Œnanthe pimpinelloides was described to have a general involucre, while Œnanthe peucedanifolia had not. From this unimportant point being regarded numerous errors have arisen, and the two plants became confounded, and the former even erased from the British Flora by Mr. Babington; and yet their *roots* show them to be perfectly distinct, and this character is constantly available, and probably may be most discriminative in all the Umbelliferæ, the *roots* of which are most important to mankind, though in some other orders this character may be of no account. So that whatever may be asserted about the oat changing into rye, I think all the ingenuity of the greatest advocate of transmutation would not be able to effect the change of a parsnip into a carrot, or induce the Œnanthe phellandrium to become a celery. Only then find out the character that is really the most important in an order or tribe, and much doubt and confusion is removed, and we find indications of permanent boundaries in Nature *there*, at any rate.

In the rose tribe Nature appears most capricious; root, leaves, armature and fruit all fail us at need as unerring absolute characters; yet surely the *attempt* to discriminate between the variable forms that occur is not to be despised, because in the effort truth may be arrived

* Whatever theory may suggest, *practically*, botanists are right in separating as *species* plants of the same family that have *permanent palpable differences* in a wild state in *some particular character*. It is obviously impossible for a travelling collector to make experiments, and any assumption on his part could only be productive of error. Experimental botany should be considered a separate department, and let the experimentalist make his claims to regulate or modify specific nomenclature, as the lawyers say "without prejudice." With respect to *varieties*. there is perhaps more anomaly and ambiguity than even in species, since no weight appears to be bestowed upon the relative amount of variation, and thus almost every botanical author's "Alpha—Beta—Gamma"—is different to that of others, giving rise to whole columns of synonyms. Now this really requires emendation. *Transient* varieties, therefore, should be distinguished from permanent ones, and rules laid down for this purpose. A plant with an additional petal or two, a white-blotched or fissile leaf, or a white flower instead of a coloured one, though curious, is rather a *sport* or *luxuriation*, than a variety, and does not deserve to be estimated in the same manner as more important and continuing characters would, affecting the appearance of the whole plant. Hence varieties ought to be classed as *casual* or *permanent*.

at, and this would be an abundant reward of all past labour. Besides, in such a labyrinth a proposition must at first be made, and experience will eventually decide as to its correctness; but assumption without proof, that an alleged species is only a variety, ought to be reprobated in every case. The boundaries of species both in the genera Rosa and Rubus are not yet perfectly ascertained, and therefore I cannot agree with Mr. Watson that the attempt to ascertain these boundaries is "frivolous." Neither, if the usual definition of species will not apply in every family alike, is it philosophical to give up the term as useless and "fall into the transition-of-species theory." For in some families there may be and is a transition of one form into another to a limited but not a constantly progressive extent, just as the river winds in a thousand sinuosities to reach the ocean, its waters by evaporation again returning to the mountains to pass over the same windings as before. So in every tribe of plants, the seed more or less may have power to sport whether in leaves, flowers, or fruit, to an extent perhaps unknown or unascertained, but not unlimited; it can only go through the changes providentially assigned to it; in its seed again brought back to its old position. This is to be particularly borne in mind, for these restricted changes by no means oblige us to side with the never-ending transmutation theory. The *Vestigians* would infer that certain metamorphoses which we see confined in their range, prove former transmutations which we have not witnessed, and that to an unlimited extent. But this is most fallacious reasoning, for all the varieties, for instance, that horticulture shows us in the Dahlia or the geranium, even if exhibited in a wild state, could give us no just reason to believe that *something else* other than the seed of a Dahlia or geranium had given rise originally to them, and that they would eventually spur on to ulterior developments different from their present family appearances. Because Tilia Europæa and parvifolia may, as I believe is the case, be the same species under different phases of growth, and the character of the leaf in the lime may be therefore variable, it would be absurd to suppose that because we must alter our definition in *this* respect, our confidence ceases as to the Tilia really remaining one, and that we may rationally look out for something else arising from the transmutation of its roots, when it falls or is cut down.

It may be inconvenient to find that Nature does not respect our definition of species in every case, and that thus between the primrose and the cowslip she *will* sport into oxlips, stalked primroses, or red cowslips; but it being once established that it is so, from repeated observations, the difficulty ceases there, and we find that the oxlip cannot be placed as a permanent species, alternating as it does between, and producible from either the cowslip or primrose. Other families may be found to present similar anomalies, and let observation go on detecting them wherever they are perceivable, and thus we may eventually know the extent of Flora's sportive footsteps. But wherever these may lead us, let us not be afraid of finding out the truth, or attempt to repress observation as "frivolous" in any department, from the fear of our science becoming too complicated, or that it will oblige us to remodel our definitions. Would, indeed, that in numerous cases they were remodelled, for too often, it is not the thing itself that is obscure, but the dark cloud of obscure words in which its description is clothed! Here we have to grope as in a darkened gallery, where the windows have been purposely closed up for solemn effect, and we can only find our way by the aid of the friendly chinks unintentionally left open. This is too often the effect of a long laboured description.

But to come to an end of these "cursory thoughts," I cannot but remark, that whatever sports and floral variations may be detected by the experimentalist in Phytology to a bounded extent, we need not fear that the grand principle of the general identity and permanence of species can be broken in upon or materially disturbed. We may not in every case find the "impassable barrier" Mr. Watson desires between species and varieties, but we may detect the species that do vary, and like the oscillations of the pendulum, note the extent of their utmost variations. This will assist our judgments in doubtful cases; and instead, therefore, of checking observation from the idea that all is done that can be done in British Botany, I believe that much remains to be effected, and something perhaps to be undone. While, then, I would wish observers to be cautious, undogmatical, truth-seeking, and not unconscious of what others have done before them, I believe we shall only profit by an increase of observers and an increase of observations, which, whether arising from a "love of approbation" only, as Mr. Watson suggests, or from a love of science and truth, as I would myself sincerely hope and believe, is really of no account, if science ultimately progresses in consequence.

EDWIN LEES.

Henwick, near Worcester,
July 8th, 1845.

numerous seedlings of any umbellate variety of P. vulgaris coming into flower without variation from the parent form. As our native species and varieties of Primula were not sufficiently understood at the date of Professor Henslow's experiments, some doubt will unavoidably arise about it; and perhaps we should take the result as a suggestion rather than a proof.

HEWETT C. WATSON.

Thames Ditton, August, 1845.

Some words on "Species-making." By HEWETT C. WATSON, Esq., F.L.S.

IN the August 'Phytologist,' Mr. Lees has hastily taken to himself my incidental mention of the genus Rubus, among others, in example of the species-making taste now in vogue; and he has indited half-a-dozen pages of verbal vengeance against me, under the inspirations of the cap which he has supposed to fit his own head (Phytol. ii. 263). I can assure Mr. Lees, however, that there was no intention of alluding to him individually by the example; and that he is perfectly at liberty to read Salix, Poa, or any other be-species-ed genus, instead of Rubus, as an illustration of the remark, which had a general application to the practice of species-making on slight grounds, without reference to any particular individual whose taste may lead him to join the section of species-makers. I do not recollect that I ever publicly connected the name of Mr. Lees with any remark which could be fairly construed into the expression of a feeling at variance with those of good will and respect towards that gentleman. On some occasions, in epistolary or oral communications with other botanists, I have found it necessary to give them a hint against relying too implicitly on his botanical exactness, and some such hint may have been repeated to him. But I have not done this on slight grounds.

The immediate object of this paper, is to rescue my own printed remarks from the erroneous construction put upon them by Mr. Lees, and likely to be adopted by readers equally "cursory" as the thinker in the 'Phytologist.' It is not to "the observation of minute differences in plants" that I ever objected, but to the hasty practice of species-making, as soon as such differences are observed, although there may exist little or no other reason for supposing the plants to be genuine species. Mr. Lees adroitly enough turns the attention of his readers from this essential distinction, by a stratagem which would look more available in a legal pleader than in a writer on science. After imperfectly quoting my words "about the frivolous attempts at species-making," he puts an interrogation,—" But why this objection to 'species-making,'—or rather the observation of minute differences in plants?" And by thus connecting together two things so totally different, he is then enabled to hold me forth to his readers in the character of one who objects to the observation of minute differences, and who decries the attentive examination of species!

This is unjust towards me, individually, and not much less so towards those readers whose judgment would be distorted by such a strategic connexion of things quite dissimilar. There may be some egotism in the illustration, but I will appeal to my own practice in proof of the distinction. During several years past I have been in the habit of collecting examples of variation in plants from every available source, and several of these have been already put on record in books, or distributed as specimens for the herbaria;—but nobody has yet charged me with being one of the species-makers. The study of varieties, and the love of species-making, are thus completely dissevered in practice; and therefore the strongest objection expressed against the one custom, cannot justly be construed into any censure of the other.

I shall still venture to repeat my own conviction, that science is much impeded by the prevalent habit of raising varieties to the rank of species (as it is expressed), without first taking the pains to ascertain whether they merge into known species during cultivation or through intermediate examples. Things which are obscure and uncertain are thus equalized with those which are clear and certain, error becomes largely commingled with truth, and the difficulties of scientific definition are greatly increased.

On the contrary, I conceive that experiments have a decided tendency to promote science, by removing error, and by substituting certainty in place of obscurity. Suppose, for instance, I find a wild plant which is distinguishable from known species by some peculiarity which could readily be described after the manner of drawing a specific character. Two courses are open. I may at once invent a specific name, write a specific character, and publish the plant as a new species. Or, I may first diligently seek for other examples which will suffice to connect it with a known species, observe it when cultivated under different conditions of soil, and raise it afresh from seeds. The species-maker takes the former course; while the experimenter takes the latter—at least in the first instance. I do not think that the species-maker would here be manifesting the greatest love of truth, or the smallest zest for notoriety.

HEWETT C. WATSON.

Thames Ditton, August, 1845.

Plants collected in Westmoreland &c. in July, 1845.
By JOSEPH SIDEBOTHAM, Esq.

I SEND you a list of a few of the rarer plants collected during a short visit to the lakes of Westmoreland &c., in July, which may be interesting to some of your reades.

Thalictrum minus, var. β. *majus.* On the mountains above Patterdale.

Hypericum calycinum. Road-side near Brathay, in several places, probably escaped from a garden.

Saxifraga aizoides and *stellaris.* On the borders of most of the mountain streams, very fine on Langdale Pikes.

Saxifraga hypnoides, var. β. *platypetala.* In a ravine in Patterdale.

Lobelia Dortmanna. Rydal-lake &c., abundant.

Primula farinosa. This beautiful plant, which I here met with for the first time, grows plentifully on swampy ground and the borders of mountain streams.

Juncus filiformis. Derwent-water.

Carex rigida. Helvellyn, above Red tarn: the foliage was in a beautiful state.

Salix herbacea. In flower on Swirrel-edge, Helvellyn.

—— *reticulata.* Mountain above Brother's water, Patterdale.

Poa nemoralis. Stock-gill, Ambleside.

Allosorus crispus. Some of the mountain sides were completely green with tufts of this beautiful fern.

Asplenium viride. Wet rocks above Patterdale.

Hymenophyllum Wilsoni. In fructification in Patterdale, Stock Gill and Langdale Pikes.

Isoetes lacustris. In Rydal-lake.

Lycopodium selaginoides. Very fine and abundant on wet banks &c. Some specimens gathered on Loughrigg were four inches high.

Andræa alpina, Rothii and *rupestris.* Helvellyn &c.

Bartramia Halleriana. In fruit in a ravine near Brother's water.

Bryum crudum. Scawdale Fell, Patterdale.

——— *elongatum.* Abundant on the sides of mountains.