ART. VI.—A Consideration of the Terms "Species" and "Variety" as used in Botany, with Special Reference to the Flora of New Zealand.

By L Cockayne, Ph.D., F.L.S., F.R.S., Hutton and Hector Memorial Medallist.

[Read before the Wellington Philosophical Society, 22nd September, 1915, received by Editors, 30th December, 1916; issued separately, 6th July, 1917.]

LACK OF UNIFORMITY IN THE TAXONOMIC EMPLOYMENT OF THE TERM "SPECIES."

Notwithstanding the great progress made in botanical science during the last century, the present-day taxonomic conception of the terms "species" and "variety" is a heritage from the past which has been handed down with but little change from the time of Linnaeus. In other words, although Darwin revolutionized biological thought with his doctrine of organic evolution, taxonomic thought and procedure is even yet dominated by the dogma of special creation. Darwin prophesied that "Systematists will be able to pursue their labours as at present; but they will not be haunted by the shadowy doubt whether this or that form be a true species. . . . The endless disputes whether or not some fifty species of British brambles are good species will cease."

Unfortunately, the above prophecy is far from being realized. In every land where "species-making" is in progress the "lumpers" and the "splitters" are as hard at work as ever, but the latter are now in the ascendancy, perhaps partly because the stock of so-called "valid" species is rapidly declining, and partly because by many it is considered a more laudable action to "create" a species than a variety; but also there is being carried out a far greater amount of truly critical work than formerly.

That taxonomy should have proceeded on the old lines is really what might have been expected, the doctrine of evolution notwithstanding; for the taxonomist is working with species as they are, and not with what they may have been or are going to be. To all intents and purposes evolution has created distinct species many of which are presumably just as invariable as if they were the work of special creation.† and the taxonomist is not usually dealing with long rows of intergrading forms, but with fairly clear-cut groups of individuals, which are occasionally connected by intermediates, but for these latter are explanations available different from that of Darwin.

If one possessing a fairly wide knowledge of the actual plants critically examines the diagnoses of the species in almost any flora, it will stand out clearly that such species (and, for that matter, their varieties) belong to different biological categories—i.e., that there is more than one kind of taxonomic species. Those forming an extremely common class do not exist in nature as true-breeding entities: they may be considered ideas merely, or at best quite artificial groups of polymorphic forms. The diagnosis of such a species includes a number of distinct groups of individuals which,

* Origin of Species, 6th ed., pp 399-400, 1872
† Bateson strongly upholds the conception of specific distinctness, as opposed to the belief of those who consider that species are "impermanent groups, the dehuminations of which are ultimately determined by environmental exigency or fitness" (see Problems of Genetics, pp 10, 11, and 12, 1913).
though frequently differing greatly in appearance, each group having its peculiar characteristics, are distinguished by the possession of certain characters in common which appear to warrant the belief that such groups are of common origin. Species of this kind are of the utmost importance for phytogeographical and evolutionary reasons, and probably require increasing rather than suppressing. Modern taxonomy recognizes their utility, and speaks of them as “aggregate” or “collective” species. Aggregate species themselves, however, are of different biological values. The discussion of a few simple examples taken from the Manual* should make this statement clear:

_Pseudopanax crassifolium_ (Sol. ex A. Cunn.) C. Koch, as defined in the Manual, p. 235, does not exist in nature. The definition includes two supposedly† definite entities, the varieties _unifoliatum_ Kirk and _trifoliatum_ Kirk. It seems, therefore, incorrect to speak of having collected _P. crassifolium_, since this latter is an idea merely, for no plant can pass through two distinct life-histories at the same time. The entities are _P. crassifolium_ var. _unifoliatum_ and _P. crassifolium_ var. _trifoliatum_, and these names should be used in floristic lists or catalogues; or another method would be to consider one or other of the above varieties the “type” of the species, and name it _P. crassifolium_, while the other variety‡ would be known by a varietal name.

_Olearia ulcifolia_ Hook. f. has a variety _mollis_ Kirk (Manual, p. 286). In this case the description of the species is that of a true entity.§ so far as is known, from which the variety _mollis_ (which is not included in the specific description) differs so markedly that it can be recognized at a glance. But the latter also is a distinct true-breeding entity, which cannot be termed _O. ulcifolia_, but _O. ulcifolia_ var. _mollis_, or, by those who consider it a species, _O. mollis_.

Another class of aggregate is that in which there is said to be a long series of intergrading forms, the extremes of which, at either end of the series, are very dissimilar. _Apremu prostratum_ Labill. is a case of this kind. In the Manual, p. 205, there are briefly described a var. _a_, a var. _b_, and a var. _c_, _filiforme_. No plant answering to the specific description occurs in nature. Var. _a_ and var. _filiforme_ are the extremes, but neither, on account of the connecting intermediates, is accorded specific rank, although var. _filiforme_ comes true from seed. Here, then, the treatment adopted in the group of individuals comprising _A. prostratum_ is quite different from that

---

* T F CHEESMAN, Manual of the New Zealand Flora, Wellington, 1906. The name “Manual” used throughout this article refers to this work.

† The word “supposedly” is here used as it is quite likely that var. _unifoliatum_ may itself be an aggregate. There are certainly a good many differences in individuals of this variety, especially in the juvenile leaves.

‡ The “type” is the comparatively rare var. _trifoliatum_, since A. Cunningham (Flora Insularum Novae Zelandiae Praecursor, p. 214, 1838) describes the leaves as “arbors adulti ternata.” This plant is also the one figured in the Icones Plantarum, t. 583, 1843.

§ Cheesman (l.c.) speaks of “intermediates” between _O ulcifolia_ Hook f. and _O macrodonta_ Baker, but, as the “typical” form of both comes true from seed, the occurrence of such intermediates would not invalidate my statement. The intermediates might quite well be, every one of them, either hybrids or true-breeding entities, but experiment is the only test. Since _O ulcifolia_ and _O macrodonta_ are so nearly related that their inflorescences and flower-heads are virtually identical (Manual, p. 286), the believer in unstable, non-hybrid intermediates should certainly unite these two species as an aggregate under one name, made up of, say, _var. genitisma_, _macrodonta_, and _mollis_, and ready to include other forms of recognizable distinctness.
of the group comprising *Olearia silicifolia* together with *O. macrodonta*, where the extremes of the linear series of intergrading forms receive specific rank.

The case of *Veronica salicifolia* Forst. f may be next examined. The species, as defined by G. Forster in the *Prodromus*, probably originally referred to one or two plants at most,* in the *Manual* the meaning has been greatly extended, so that the species, according to Cheeseman, contains three named varieties, a typical form which is not strictly defined, an unnamed variety from the Kermandes,† and, in addition (Manual, p. 504), "numerous forms which appear to connect it with *V. macrowya*, *Diefenbachii*, *macrocarpa*, *ligustrifolia*, and others." What is meant in that sentence by the word "it" is vague, but it seems as if the type was thus indicated, while the varieties and intermediates are something different. At any rate, at one end of the series would be the "type" (whatever that may be), and at the other end one or other of the above species; or the whole of them, together with the "type," its varieties and the "intermediates," might constitute one huge aggregate.

Still another class of aggregate species is that large one said to be "variable," but where few or no varieties are defined *Agropyron scabrum* Beauv., *Dianthus semiannularis* R. Br., *Veronica pinguisfolia* Hook f., and *Celmisia discolor* Hook. f., along with many other species, belong to this category. Usually no plant answering to the description of such species exists in nature.

Finally, species may be highly polymorphic, but no mention be made of such "variability" Thus *Arcaea Sangusorbae* Vahl is not spoken of in the *Manual* or by Hooker in the *Handbook of the New Zealand Flora* as variable, and yet Bitter, in his recent memoir on the genus,‡ has clearly shown that the species is a most puzzling complex, while my preliminary experimental studies are suggesting that his numerous subdivisions are insufficient.

The great class of species is that which the layman practically concerned with plants—e.g., the gardener—alone recognizes as such; that is, those in which there is no manifest variation except such as is quite unstable and caused by the environment of the varying individual. The *Manual* contains many species of this character,‖ of which *Agathis australis* Salisb., *Dacrydium cypressinum* Sol., *Carmichaela gracilis* J. B. Armstrong, and *Celmisia bellidioides* Hook. f may be taken as examples.

The conception of a species as put forth by its author has been frequently modified not merely by other taxonomists, but by the author himself, and this without any statement to that effect Generally a fairly invariable species is thus transformed into an aggregate, but occasionally the opposite course has been followed The citation of the original author's

---

*Regarding Forster's type, see certain remarks by myself in *Trans. N. Z. Inst.*, vol. 48, p. 201, 1916

† Now *Veronica brevipulvosa*, W. R. B. Oliver.


§ In the case of certain species there is frequently a quite constant form in cultivation which owes its constancy either to being a microspecies or to all its members being the vegetative offspring of one individual. *Veronica Banksii* Cockayne is a case of this kind, as is also the ordinary form of *V. Diefenbachii* Benth.

‖ Of course, without actual breeding experiments it is impossible to know whether any species is really a true-breeding entity, so the word "many" can be taken for what it is worth.
name is then no guarantee that the species is that of the author in question. Thus \textit{Celmsia discolor} Hook. f. of the \textit{Flora Novae-Zelandiae} is not the \textit{C. discolor} Hook. f. of Kirk's Students' Manual. \textit{Veronica salicifolia} Forst. f., which, as stated above, was originally defined from one or at most two true-breeding groups of individuals, is certainly not the \textit{V. salicifolia} Forst. f. of any subsequent New Zealand botanical author.

Much more could be written regarding the non-uniformity of the term "species" as used, not in the \textit{Manual} only, but indeed in florae in general. As to how such inconsistencies have arisen, and why they are not only tolerated but perhaps necessary, demands, in the first place, a consideration of how the flora of New Zealand has reached its present stage; and, secondly, an historical examination of the species-question, and a consideration of the relations between a species and its subdivisions in the light of modern knowledge.

\section*{History of the Conception of the Term "Species" in the New Zealand Flora.}

The New Zealand flora has attained its present standpoint from the labours of many men, no two of whom have had exactly the same conception as to the limits of species and varieties, so that each has been more or less a law unto himself. Happily, one of the greatest taxonomists the world has ever seen, Sir Joseph Hooker, revised in a searching manner the work of the earlier botanists, while for many years he was virtually the sole author who dealt with New Zealand material. This led to a uniformity of treatment in his \textit{Handbook of the New Zealand Flora}, published in 1864-67, which otherwise would have been lacking. At the same time, it must be pointed out that Hooker had at his disposal dried material only, together with the notes of various collectors regarding variation, &c., which certainly must have been of very unequal value. In his conception of the limits of species Hooker, as is essential for any botanist dealing with the plants of the world, or of very wide areas, favoured the creation of large aggregates.* This, as already pointed out, is a reasonable and, in many respects, an excellent course to take; but, in order to make it available for really finding out the prevalent forms of the species, variational names are essential. These Hooker used to some extent, it is true, but the comparatively small amount of material at his disposal, the lack of knowledge regarding the great majority of the plants as they grew naturally, and an acceptance of that general belief, dealt with farther on, that varieties were not stable, made these Hookerian varieties, in many cases, of slight taxonomic value.

For example, \textit{Leptospermum scoparium} Forst. is split up into var. \textit{a}—erect, leaves lanceolate; var. \textit{b}, \textit{limifolium}—erect, leaves narrow, linear-lanceolate; var. \textit{y}, \textit{myrtifolium}—erect, leaves ovate, spreading or recurved; and var. \textit{d}, \textit{prostratum}—prostrate, branches ascending, leaves ovate or orbicular,

* See the classical Introductory Essay to the \textit{Flora Novae-Zelandiae}, which demands as much attention now as it ever did, notwithstanding that it appeared more than sixty-four years ago. Hooker's explanation of his wide conception of species is put forth at considerable length: it must be read in its entirety, and so here but few quotations are given. He also explains the standpoint of those admitting much smaller species, and is quite sympathetic, declaring that "there is much to be said on both sides of the question," and that truth can only be arrived at through the joint labours of workers of the two schools.
recurrved.* In no list of plants hitherto published, so far as I am aware, are these varietal names used, partly because New Zealand botanical authors have troubled little about varieties, and partly because the above do not represent true-breeding races. Kirk and Cheeseman both recognize these varieties of Leptospermum, but it is suggestive that the latter botanist assigns a habitat to var. prostratum only † This plant, however, there is every reason to believe is merely prostrate owing to wind or some complex of ecological factors, and so has no right to a varietal name in the sense in which the term "variety" is now generally used. I could easily send to Europe dried specimens of different forms of L scoparium, taken from shrubs growing in close proximity, which would at once receive varietal and perhaps specific names from any taxonomist monographing the genus and unacquainted with the species as they grow. At the same time, there are distinct and easily recognizable races of L. scoparium which come true from seed, but most of these as yet possess no distinguishing name.

This comprehensive view of species adopted by Hooker, although for many years religiously followed by the leading New Zealand taxonomists, is being rapidly departed from, since studies in a limited area, as Hooker early on explained,‡ force upon the observer the importance of minor characters. Thus it has come about that species after species has been added to the flora, especially of late years, which would have been included by Hooker, and, indeed, by nearly all New Zealand workers of a decade or two ago, in their aggregates. This multiplication of named groups has certainly in many instances simplified the work of the field botanist. For example, the acceptance of Haussknecht's arrangement of the genus Epilobium§ has made it much easier to place most of the individuals of this rather difficult genus into well-defined groups ¶ Yet so great was the prestige of Hooker that, although the above work appeared in 1884, its conclusions were not generally accepted until 1899, when they received the sanction of Kirk's Students' Flora. The recent action of Cheeseman in segregating a group of individuals from the aggregate Celmisia longfolia Cass under the name of C. Morgani‖ would have been accepted by few New Zealand botanists a dozen years ago, but it will certainly be welcomed by most at the present time.

Conception of the Terms "Species" and "Variety" in General.

If a number of definitions of the term "species" as formulated by eminent taxonomists, both in the pre-Darwinian and post-Darwinian periods be examined two essential differences of treatment stand out, the one

---

* Handbook of the New Zealand Flora, pp. 69-70, 1864
† Manual, p. 60
‡ "The local botanist looks closer, perceives sooner, and often appreciates better inconspicuous organs and characters, which are overlooked or too hastily dismissed by the botanist occupied with those higher branches of the science, which demand a wider range of observation and broader views of specialities." (Introductory Essay, p. xii)
§ C. HAUSSKNECHT. Monographie des Genus Epilobium, Jena, 1884
¶ HAUSSKNECHT when dealing with an extensive aggregate species—e.g., E puniceum Sol.—uses the term "form" for his subdivisions, and not "variety." Thus for the above species he describes six named forms, the names preceded by a, b, &c., each form corresponding to a species of some earlier author; (i.e., pp. 286-90). These "forms" of Haussknecht are evidently equivalent to the "varietes" of this paper, while his use of the term is contrary to ordinary taxonomic procedure
‖ T. F. CHEESEMAN. Description of a New Celmsia. Trans N Z Inst. vol. 46, 1914, p. 21
morphological and artificial, the other physiological and natural. Thus, according to the first conception, a species is a group of individuals which are distinguished by certain definite and unchangeable characters, and so differs from all other groups; but according to the second conception a species is a group of individuals resembling one another which on being bred together produce like individuals. This second definition holds that a species is a definite immutable entity, whereas the first is based, at best, only on a superficial examination of numerous individuals, and so permits the establishment of species which, as already explained, are not realities but ideas, the limits of which may be extended or reduced according to the whim of the taxonomist. Now, of recent years the careful experimental work of the Mendelists and others has clearly shown that if the breeding-test be accepted as the ultimate criterion of specific rank, species differing from one another in the most minute, perhaps outwardly unrecognizable, characters would have to be established by the thousand. That is to say, the main object of a flora, which is the ready recognition of individual plants, would be nullified. Therefore, since a true biological classification of individuals is probably taxonomically impossible, taxonomy is forced in no few cases to adopt such as is manifestly more or less artificial and to fall back upon the procedure of the pre-evolutionary taxonomists. But, as already pointed out, these investigators had no definite rules to observe, each was a law unto himself, and the species of one were frequently of quite different value from those of another, while it is even yet altogether a matter of opinion which of the two would be the more correct.

The classification of groups of individuals becomes far more complicated when groups smaller than species are to be constructed. The Mendelian gets over the difficulty by limiting his groups to the microspecies—i.e., to the "biotypes," as he styles them, which breed true; but such treatment is not generally suitable for floristic botany. Still worse is the abandoning of groups smaller than species, and, without any experimental test whatsoever, splitting up easily recognizable aggregate species into dozens of so-called species which no one but their describer can possibly recognize! Even the orthodox taxonomist, when he attempts intensive work and defines subspecies, varieties, subvarieties, and even forms, is not infrequently impossible to follow.* Leaving out of consideration the work of such extremists, and turning to those who confine their groups of individuals to "species" and "varieties," it is here that the greatest differences of opinion occur, for the "species" of one are the "varieties" of another, and so on. This does not matter greatly so far as concerns floristic botanists themselves, but it is otherwise for investigators in other branches of botany, who demand definite names for the plants they are dealing with, together with some clue to their relationships. It is important, then, to get some idea of what is meant by "variety," and in order to do so a brief consideration of this term in taxonomy, from the time of Linnaeus, and incidentally of "species" also, should be of service.

Linnaeus himself considered varieties as anomalies to be brushed aside as not worthy of the attention of the philosophical botanist. This curious attitude has not even yet been altogether abandoned, for in few floras

*A binomial, or even a trinomial, is a practical proposition, but if such names as Bitter's (i.e. pp 263-66) *Acaena Sanguisorbas* subsp. *soue-zelandiae* var. *rudisima* subvar *rubescensita* (not at all an extreme case) are to be used, then we had much better go back to the pre-Linnean practice of a brief description of the group, for it had the merit of telling something about the plant in question.
do “varieties” receive the attention they merit, while the somewhat contemptuous expression, “merely a variety,” is certainly not unknown.

Taking the pre-Darwinian taxonomists as a whole, the conception of a variety was of a collection of similar individuals which differed from the remainder of the individuals comprising the species in certain supposedly unimportant particulars, and, if it came true from seed for a time, would eventually revert to the “type.” Thus Abraham Rees, in *The Cyclopaedia* (1819), writing of varieties, states that “a little observation will prove how transient such varieties are and how uniformly their descendants, if they are capable of providing any, resume the natural characters of the species to which they belong.” By A P de Candolle and K Sprengel (1821)* the question of species and varieties is treated in a most illuminating manner “Species,” they write, “have existed as long as the earth has had its present form,” and “have maintained the same properties invariably” At the same time, certain properties, they assume, are subject to change—i.e., some properties are variable and others invariable. The variable properties supply the material for subspecies and varieties. The subspecies are defined as “such forms as continue indeed during some reproductions, but at last, by a greater difference of soil, of climate, and of treatment, are either lost or changed.” . . . “Varieties,” on the contrary, “do not retain their forms during reproduction.” The cauliflower is cited as a subspecies, and the variable colours, tastes, and other properties of kitchen vegetables, ornamental plants, and fruit-trees “show what varieties are” De Candolle and Sprengel further remark, “The scientific botanist must therefore be particularly attentive to distinguish permanent species from the variable subspecies, degenerate plants and varieties.” The authors conclude with this excellent advice which is certainly not inapplicable at the present time. “In order to decide respecting the idea of a species, an observation of many years, and of much accuracy, is often required, and the cultivation of plants from the most different climates in botanical gardens is in the highest degree necessary for their determination.”

Sir James Edward Smith in 1827,† writing of varieties, says, “We frequently indeed see new varieties, by which word is understood a variation in an established species, but such are imperfectly, or for a limited time, if at all, perpetuated in the offspring.”

John Lindley in 1832‡ defined a species as “a union of individuals agreeing with each other in all essential characters of vegetation and fructification, capable of reproduction by seed without change, breeding freely together and producing perfect seed from which a fertile progeny can be reared. Such are the true limits of a species, and if it were possible to try all plants by such a test there would be no difficulty in fixing them and determining what is species and what is variety.” And again, “It is probable that, in the beginning, species only were formed, and that they have since the creation, sported into varieties, by which the limits of the species themselves have now become greatly confounded.”

Apart from Hooker’s “Introductory Essay,” the most interesting statements regarding the matter under consideration, so far as New Zealand students are concerned, is that of G Bentham in his “Outlines of Botany,” which forms an introduction to Hooker’s *Handbook of the New Zealand

---

*a Elements of the Philosophy of Plants*, pp 96–98.
† *An Introduction to Physiological and Systematic Botany*, 6th ed., p 291
‡ *An Introduction to Botany.*
Flora (1864), a work in constant use by all investigators of the New Zealand flora for a period of thirty-five years. Here (p. xxiv) a species is described as "all the individual plants which resemble each other sufficiently to make us conclude that they are all, or may have been all, descended from a common plant. These individuals may often differ from each other in many striking particulars, such as the colour of the flower, size of the leaf, &c.; but these particulars are such as experience teaches us are liable to vary in the seedlings raised from one individual. When a large number of the individuals of a species differ from the others in any striking particular they constitute a variety. If the variety generally comes true from seed it is often called a 'race.' A variety can only be propagated by grafts, cuttings, bulbs, tubers, or any other method which produces a new plant by the development of one or more buds from the old one. A race may with care be propagated by seed, although seedlings will always be liable, under certain circumstances, to lose those particulars which distinguish it from the rest of the species. A real species will always come true from seed."

The above definitions of Bentham are clear enough, and show plainly that to him the ultimate test of a species was its capacity for breeding true. Bentham (i.e., pp. xxiv, xxv) also deals with "occasional or accidental anomalies" peculiar to one or a few individuals, which may prevent the species being "at once recognized by its technical characters." These "aberrations" he divides into two classes—viz., those for which some general cause may be assigned, and those of which the cause is unknown. To the first class belong changes due to various ecological factors, to use the modern term; while the second class includes those variations which some would now consider due to mutation, or to some factor acting on the embryo at an early stage of its development. Hybrids fall into Bentham's first class of "anomalies."

Biologically, if not taxonomically, the conception of species changed with the general acceptance of the doctrine of evolution, so that many considered them to be much less sharply defined than had up to that time been believed, while in no few instances they were thought to be not yet fully differentiated. According to Poulton, Sir E. Ray Lankester goes so far as to declare that "the 'origin' of species was really the abolition of species, and zoologists should now be content to name, draw, and catalogue forms" (Essays on Evolution, p. 62, 1908). Poulton himself considers that the usual diagnostic method of considering as species sets of individuals arranged according to certain characters fixed upon by the systematist, in a series without marked breaks, is not a sufficient conclusion, and he suggests that, in addition, the members of the group must interbreed and be of common origin. Romanes (Darwin and after Darwin, vol. 2, p. 231, 1895), after some discussion regarding the term "species," gives the following definition: "A group of individuals which, however many characters they share with other individuals, agree in presenting one or more characters of a peculiar and hereditary kind with some certain degree of distinctness."

Coming now to Darwin himself we find these words (The Origin of Species, ed. 6, p. 400): "Hereafter we shall be compelled to acknowledge that the only distinction between species and well-marked varieties is that

* The italics are mine.
the latter are known, or believed, to be connected at the present day by intermediate gradations, whereas species were formerly thus connected. . . . We shall have to treat species in the same manner as those naturalists treat genera who admit that genera are merely artificial combinations made for convenience." "This," concludes Darwin, "may not be a charming prospect, but we shall at least be freed from the vain search and undiscoverable essence of the term 'species.'"

Notwithstanding the last-quoted words of Darwin, the panstaking and epoch-making researches of Jordan, Mendel, de Vries, Bateson, Johannsen, and others have shed much light upon the species-question that the relation between a species and its subdivisions stands out far more clearly than in Darwin's time. Of especial importance is the fundamental difference shown by de Vries* between unfixed variants, whose forms depend upon the environment, and his elementary species (micospecies) which come true from seed under different outer circumstances—a distinction quite unsuspected by the early naturalists, as seen from the quotations already cited, so far as groups distinguished by quite trivial characters were concerned. In other words, if in the light of modern knowledge not only were the pre-Darwinian definition of Bentham to be accepted, but even that of the post-Darwinian Romanes, species after species would have to be rejected, variety after variety would be raised to specific rank, though others would be cast aside as mere fluctuations, and hundreds of trivial forms, apparently identical, as gauged by floristic methods, would have to be accepted as valid species, because they fulfilled perfectly the breeding test. Needless to say, the confusion would quickly become indescribable.

**SOME CONSIDERATIONS FROM THE FOREGOING REGARDING SPECIES AND VARIETIES**

What, then, should be done in order to make a flora serve its primary purpose of enabling any member of a plant-population to be readily recognized? First of all, most will agree that no drastic changes are advisable. Linnean aggregate species answer admirably, provided it be understood that such are groups of varieties connected, it may be, by intermediate hybrids, and that species of this kind do not exist in nature as true-breeding entities, while their limitations are a matter of individual taste and not of scientific fact.

What really do occur in nature are the individuals, and these, obviously, should be the starting-point of classification. Unfortunately, these individuals are not all biologically equal. Some reproduce their like, and groups of these are the true self-breeding entities—e.g., the microspecies, or biotypes. Others are of hybrid origin, and should, on the one hand, segregate in Mendelian fashion, or, on the other hand, be more or less intermediate in character between the parent species and come true from seed. Others, again, according to those believing either in natural selection or in neo-Lamarckism, should be neither microspecies nor hybrids, and would be expected in course of time to give rise sexually to individuals more or less distinct from their parents. Whether this last class really exists is, in the opinion of some, not scientifically established. It may quite well be that all "intermediates" which are not microspecies are merely hybrids between these latter or between the hybrids themselves. The hybrids of taxonomists are the result of crosses between one variety of a Linnean species and that of

---

*Die Mutationstheorie, Bd. 1, Lief 1, pp 32-41, Leipzig, 1901
another such species, and not crosses between forms of the same aggregate
hardly distinguishable from one another. But undoubtedly this latter class
of hybrids—i.e., crosses between microspecies—forms a considerable per-
centage in many plant-populations, and in no few instances these hybrids con-
stitute connecting-links between groups otherwise distinct; in fact, they
are then the so-called “intermediates.” The surprising individual differ-
ences between the members of a young colony of Leptospermum scoparium
which I recently studied in the neighbourhood of Wellington can be best
explained on the above supposition, which doubtless holds good for many
aggregate species. If this be true, the presence of “intermediates” loses
much of its reputed taxonomic significance. In critical cases herbarium
studies are obviously futile; no progress can be made by such means.
Experimental taxonomy, preceded by careful field observations, is alone
of moment, and should eventually decide all doubtful points. In this nothing
novel is suggested; the procedure would be merely a return to the methods
so wisely advocated by de Candolle and Sprengel in 1821.

The duty of the taxonomist is to arrange into groups the individuals
he is dealing with. This he can do by comparing large numbers of indi-
viduals and placing together such as appear to possess constant characters
in common. A number of such groups possessing definite characters
common to all, but each group having one or more characters peculiar to
itself, would form an aggregate species, the groups themselves being dealt
with as varieties; or, where the aggregate species is extremely comprehen-
sive, some authors may first unite certain of these varietal groups, accord-
ing to their relationship, as “subspecies.”* Such a plan as the above
is rarely followed. The species itself is first defined, perhaps from the
characters of a few individuals, or at times from one only. Later
on other individuals come into consideration, which, though probably
not exactly like those of the first defined group—they may, indeed,
be very different—appear closely allied, and they are referred to
the species, which accordingly is assumed to “vary.” Should the new
groups be fairly distinct from the original set of individuals, they may
receive varietal names, and the species will be said to be “variable”; or
if other more distinct groups are added as “varieties” it can be made
“highly variable,” or “protean,” in which case the final word has been
spoken, and further research is considered unnecessary.† This conception of

* The term “subspecies” has been but little used by writers dealing with the New
Zealand flora. Generally speaking, its use is confined to the primary subdivision of a
species of such magnitude that it becomes a moot point whether to treat it as a huge
aggregate or to split it up into smaller species. Where this latter course is not taken
the establishment of a “subspecies” is not infrequently in the nature of a compromise,
for the author, on the one hand, cannot make the aggregate of practical use in its
entirety, and, on the other hand, is averse to acknowledging that the divisions he
proposes are “good” species, so he adopts a group higher than a variety but rather
lower in rank than a species. This aspect of the question is well illustrated by Kirk’s
revised treatment of Hokia populnea in The Students’ Flora, pp. 71–72. Or an author, as
in the case of Butcher when dealing with Acacia Sanguisorbae, may establish subspecies
as convenient subdivisions, whose components, for practical purposes, are the varieties.
For the use of the syllable “sub” in taxonomic procedure, see Art. 12 of the Inter-
national Rules for Botanical Nomenclature.

† The opposite course can be taken and a “highly variable” species can be made
to “vary” comparatively feebly. The treatment of Rubus australis Forst. f in this
regard by Kirk (Students’ Flora, pp 125–26) and Cheeseman (Manual, p 125) as
compared with that of Hooker in the Handbook, p. 54, is an instructive case, Hooker
defining the aggregate together with three varieties which he states “are united by
every intermediate form.”
variability is indeed remarkable! "Variability" of this kind is not the work of nature, but of the taxonomist. In order to produce a variety something must vary, and that something must be more primitive than the variant. But in taxonomie practice "variation" has an altogether different meaning from its everyday use. The "type" of the species is assumed to be the primitive form, and the "varieties" modifications of this "type." Of course, this may sometimes be the case and the varieties truly such, but usually no one variety can be considered the parent of the others; all may possess an equal claim, or, quite likely, the actual ancestor may no longer exist. As seen from the definition of the terms "species" and "variety" by the pre-evolutionary writers, the species was, ipso facto, the primitive entity, and it was assumed actually to give rise to varieties, which, if raised from seed, would sooner or later revert to the specific form. Species formed by evolution may also give rise to "varieties," but in this case, if the varieties are microspecies, neither the most accurate field observations nor even breeding experiments can decide whether "species" or "variety" is the parent. The "mere variety," to use the frequent phrase, is thus of equal rank to the "type," and in nine cases out of ten it only escaped being constituted the "type" through its later discovery—that is, it is a floristic variety by accident!

The "type," another most misleading term, was, with good cause, declared by Hooker to be a "phantom" (i.e., p. xvi). One would naturally imagine a "type" to be the most widespread form of a species in its area of distribution, and in some instances this fortunately happens. Generally, however, the "type" is the form first described, which quite well may be the rarest form of the species. In such a case much commoner forms are later on described as "varieties." Now, these curious usages of the words "variety" and "type" can do but little harm so long as the student of a flora recognizes that they are terms which, though correct enough from the standpoint of special creation, no longer possess the meaning attached to them by the early taxonomists, but one which is quite technical. All the same, "variety" and "type" are established taxonomic terms which can hardly be abandoned, even were it biologically desirable to do so, still, so far as aggregate species are concerned, it seems much better to speak of them as "polyneophic" rather than as "variable."*

The term "variety" requires further discussion. In the case of aggregates all the varieties should be of equal value. Now, it can be readily conceived that, in process of time, all the varieties of an aggregate species except one may vanish from the face of the earth. This survivor, however, would at once change its rank and be deemed an invariable species. Also, it might not be closely related to any other species, yet the fact of its invariability would certainly place it not in the same category as the aggregates, but in that of the microspecies. According to this view, if the supposedly invariable Agathis australis Salisb. were considered the sole survivor of a number of closely related microspecies, it would biologically rank no higher than various unnamed true-breeding forms of Veronica luxifolia Benth., or the micro-

* Under this conception a "variety" is a microspecies—a stable entity—but a "variation" is an unstable form depending on its special environment. Moss, in the Cambridge British Flora, now in course of publication, calls such "variations" forms, and distinguishes them by names. For such fine distinctions, even if advisable, the time is not ripe so far as the New Zealand flora is concerned.
species segregated by Jordan from Draba verna L. (Erophila verna E. Meyer) in his classical experiments. The true biological species are, in fact, the microspecies, whereas the aggregates are merely collections of these and may be termed "taxonomic species." Included with the biological species are the supposed true-breeding species, on the supposition either that these were once parts of aggregates, or that they are true-breeding entities which have originated from some earlier species by a change of great magnitude,④ but are none the less biotypes. To the gardener and farmer, both of whom are deeply concerned with species, it is only the microspecies (biotypes) and the supposed invariable species, no matter their origin, which are of any moment.† And these alone answer the conception of a species as a true-breeding entity of both the pre-Darwinian and post-Darwinian biologists.

The paramount importance of varietal names stands out clearly enough. As noted early on in this paper, in all the floras of New Zealand up to the present time an aggregate species, if it has been split up at all, has usually been treated in two distinct ways. In the one case the specific name has been applied to one distinct variety, the so-called species; but in the other case each variety has been accorded a distinct name, and the specific name applied to the varieties taken together. In the latter case there is really a trinomial nomenclature, the species being but an idea. Moss has adopted the latter method in his Cambridge British Flora, and it seems to me the only biologically sound procedure. By the believer in species having originated through small changes each of the invariable species should be accorded, in addition to its specific name, a varietal name, since each is, in his opinion, the surviving variety of a former aggregate; but in practice this does not seem necessary, or even advisable. On the other hand, in the case of aggregates each variety, including the "type," should receive a distinguishing name. Of course, this procedure would lead to many new varietal names being given; but it would, of necessity, take place by slow degrees, while its value for extending a real

④ Latsy is of opinion, according to his highly suggestive theory of the origin of species by crossing (Proc. Linn. Soc., p. 73 et seq., 1914), that changes may be of extraordinary size. "So," he writes, "the introduction of the unknown factor x may have caused in some invertebrate the formation of the skeleton, and, if this is true, it is hopeless to look for a transition between an invertebrate and a vertebrate, as none such ever existed" (L., p. 81).

A recent paper by C. S. Haw (Sterility as the Result of Hybridization and the Condition of the Pollen in Rubus, Bot. Gaz., vol. 62, pp. 370-88, 1916) gives various examples of more or less fertile interspecific hybrids. He points out how Linnaean considered various hybrids good species, using hybridum for the specific name. The case of Viola, investigated by Brainerd, is also cited, where hybrids made between closely related but distinct species with characters intermediate between unlike characters of the present forms bred true from generation to generation (Hybridization in the Genus Viola, Rhodora, vol. 8, pp. 6 and 49, 1900).

Wills, as the result of studies in the distribution of the Ceylon flora, based on the comparative frequency of species (Phil. Trans. Roy. Soc., ser. B, vol. 206, p. 307 et seq., 1915), considers that all species arise by mutation, and that the new character may be either small or of great size. This botanist does not believe in the killing-out of intermediate forms (L., p. 330).

† Horticultural and agricultural common-sense is far in advance of taxonomic science, as evidenced by the gardener and farmer having definite names for their innumerable varieties of ornamental plants, vegetables, and cereals. A binomial or even a trinomial nomenclature is of little use in actual horticultural or agricultural practice.
insight into plant-distribution can hardly be overestimated.* Doubtless many varieties would themselves frequently be aggregates, so that it might be advisable in some instances to establish further subdivisions. But without experiment it is impossible to deal with the ultimate microspecies (biotypes), nor in an admittedly artificial classification is this necessary. Finally, unstable forms, dependent on some particular environment, should never receive a varietal name; therefore many such names, the result either of the pre-evolutionary views or of ignorance, would have to be abandoned, unless, as explained in the footnote on p. 76, they were treated as "forms."

At the present time, in New Zealand floristic botany, as elsewhere, the tendency of authors is to split up the aggregates, and, in addition, to accept as species newly discovered plants which a few years ago would have been referred to existing species and hardly have received varietal rank. This method possesses the merit of convenience only, insomuch as it is easier to use a binomial than a trinomial. Biologically it is a retrograde step, for the aggregate emphasizes the intimate relationship of its component groups, as well as being an important biological conception for phytogeography. If the fact were generally recognized that such taxonomic varieties as are here suggested are far nearer being biological units than are species, and that their accurate delimiting is a matter of quite equal or even greater skill than the establishing of "new" species, and certainly reounds as much to the credit of the describer, probably fewer of the latter and more of the former would be published. But it will be a long time, if ever, before the student of a limited area can look at plant-classification with the same eyes as the general systematist. These words of Hooker written in 1853 still reflect the attitude of many: "There are local observers... who take the exclusion of plants accidentally introduced into the flora of their neighbourhood, and the reduction of supposed local types to varieties of better-known and wider-spread plants, as little short of an insult to their understandings and a slight upon the natural history of their village or island, and suppose that because the systematist cannot see with their eyes he therefore takes a less true interest in what he observes" (Introductory Essay to the Flora Novae-Zelandiae, p. xvii).

Bateson in no uncertain language declares his belief in intensely critical taxonomic work, and in the inadequacy of undivided aggregate species, in these words: "Between Jordan with his 200-odd species for Erophila (Draba)† and Grenier and Godron with one there is no hesitation possible. Jordan's view, as he again and again declares with vehemence, is at least a view of natural facts, whereas the collective (aggregate) species is a mere abstraction, convenient indeed for librarians and beginners, but an madious

* There is a good deal to be said in favour of constituting "subspecies" (looking on this term as virtually equivalent to species) in widely distributed aggregate species which occur in areas far distant from one another. Skottsberg has proposed (and rightly, in my opinion) that the aggregate Sophora tetragona should be maintained, and that the various groups of that species in New Zealand, Lord Howe Island, Easter Island, Juan Fernandez, and south Chile should be dealt with as subspecies or varieties (Plant World, vol. 18, p 134, 1915). In New Zealand the groups of Sophora are so distinct both in form and life-histories that the term "subspecies" is more applicable to them than "variety." Were it not for phytogeographical reasons they should certainly be considered species.

† The words in parentheses are added by me.
misrepresentation of natural truth, perhaps more than any other the source of the plausible fallacies regarding evolution that have so long obstructed progress.”*

Before publishing either a species or a variety it seems best to be in no hurry. Few dream of testing the stability of characters in a plant by cultivation, and fewer of extending this to methods of pedigree-culture. Taxonomy is assuredly not a matter of the herbarium merely; real progress depends upon intensive studies in the field and cultural experiments in the garden. It is surely better to apply a temporary “nomen nudum” to a supposed new species or variety than to rush into print and bestow a name based upon insufficient material and with no knowledge of the plant other than that which a few scraps of dried material afford. A temporary name for a plant may be a necessity in the case of ecological studies, but for a permanent one wrongly bestowed, which must enter into synonymy, there is generally no excuse, since it can serve no conceivable purpose. There is ample room for taxonomic research in the New Zealand flora apart from the hunt for “something new.” There are few aggregates which do not demand a most searching examination, and variety after variety requires delimiting and naming. But this work cannot be carried out merely by an examination of dried material. Examples of the species must be brought together from every part of its area of distribution. Material of all kinds, both dried and living, must be accumulated, and the latter cultivated under various conditions. Above all, actual field-knowledge is an essential. Research of the above character could employ many investigators, while specialization would be fundamental. For instance, a searching examination of the little-known aggregate Veronica pinguisfolia† carried out on the above lines would require several years’ close study. In the case of certain trees and shrubs, and also in that of herbs difficult to cultivate, the experimental method might be too prolonged or almost impossible to apply, but, on the other hand, there are dozens of species easy to cultivate and raise from seed. Experimental taxonomy should become in course of time a most important branch of botany. At present, however, it is hampered by the methods of the university, where the garden plays so small a part and the laboratory rules; by the taxonomic traditions of the past; by the lack of sympathy between taxonomy and ecology, genetics, and horticulture; and by the length of time that must elapse before results can be published.

* Problems of Genetics, Yale University Press, p 250, 1913
† There is hardly a more “variable” species in the flora, and yet no varietes have as yet been described. But in gardens where veronicas are extensively cultivated there are frequently several absolutely different plants which under the circumstances must all be accorded the name V pinguisfolia—a proceeding which does not increase the gardener’s respect for botany. Doubtless in their natural habitats these forms might be connected by intermediates, but constantly raised from cuttings in the garden they appear invariable. Cheeseman, e.g. writing of V Buchanans (Manual, p 527), states, “Larger forms approach V pinguisfolia so closely that it is difficult to draw a line of demarcation between the two species. My var major might be referred to either.”