

## SOME RECENT PUBLICATIONS AND THE NOMENCLATORIAL PRINCIPLES THEY REPRESENT.

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THE second edition of Mr. Heller's *Catalogue*<sup>1</sup> presents, as a piece of presswork, a great improvement over its predecessor. For thus materially improving the dress of his work the author is certainly to be commended. The book also contains a very large increase of species over the first edition; and for bringing together hundreds of recently published names, where they can be readily consulted, the compiler should have the gratitude of students of systematic botany. In a work of this sort, necessarily accomplished largely by compilation, monographic treatment of recent and doubtful species can not and should not be expected; but there can be no question that the compiler of a check list or catalogue owes to the public the product of the best light he has upon the species with which he deals. Mr. Heller's new *Catalogue*, especially, representing the so-called reform tendencies in American botany, should be judged primarily by the degree of adherence to or divergence from the principles which he has taken upon himself to exploit. This second edition, too, should be judged by the degree of readiness shown by its author to correct such obvious errors and inconsistencies in his preceding work as have been definitely called to his attention in print.

It is a question which is the point of greater significance to systematic botany—the hopeless tangle of nomenclatorial principles here exhibited, or the tendency, by no means new, to break through the traditional though necessarily vague barriers separating the minor categories to which plant-variations may be assigned, namely, the species, variety, and form. That the author of this *Catalogue*, and numerous other American botanists,

<sup>1</sup>Catalogue of North American plants north of Mexico, exclusive of the lower cryptogams. By A. A. HELLER. Second edition. Issued November 10, 1900.

like Jordan and others in Europe, have essentially abandoned these finer discriminations in classification, has been made perfectly evident in their past work, and now we are informed that "there is also a growing tendency to discard the use of varietal names, and to call all plants species which have characters enough to justify the use of a distinctive name."<sup>2</sup> To the critical student, who is familiar with growing plants and the causes which so often control their variations, such action as this cannot appeal; and, though the author of the *Catalogue* is not alone in his standpoint, there are still many students whose conceptions of plants and their relationships can be expressed only by the retention of categories which are subordinate in rank to the species.

However, even if, by putting essentially all variations within the genus upon a common level, the author chooses to obscure the minor degrees of relationship in plants, there should be no question of personal choice or opinion in judging the method so often adopted by him in order to increase the number of so-called species. In the introduction to the new *Catalogue* (as well as in the original edition) many new combinations of names are made; but, finding that space would not permit the publication there of all the changes he desired to make, the author has ventured a new private journal,<sup>3</sup> the first issue of which is occupied by an appendix to the nomenclatorial changes begun in the *Catalogue*. It seems that the author has, or did have on November 10, 1900, some conception of the unsatisfactory methods he was employing, for in apologizing for so proceeding he says: "The bare citation without discussion in most cases is *undesirable*, but *lack of time* forbids a more extended treatment of the different species under *consideration* [italics ours]."<sup>4</sup> Does anyone suppose that by careful botanists such an apology can be accepted as a pledge of sincere desire to advance botanical science; or can it be that

<sup>2</sup> HELLER, *l. c.* 3.

<sup>3</sup> *Muhlenbergia*, a Journal of Botany. Edited and Published by A. A. HELLER, Lancaster, Pa.

<sup>4</sup> HELLER, *l. c.* I: I.

the author conceives that *his time*, the "lack" of which alone permits him to launch so many *unconsidered* species, is of far greater importance than that of the scores of other botanists who must now spend weary hours trying to unravel the snarls he has produced? An example or two may make our point more clear. The names *Prenanthes Serpentaria* and *Nabalus Serpentarius* have been essentially interchangeable in American floras,<sup>5</sup> and the name in either case has been made to cover until recently two very different species. In the *Illustrated Flora*, however, Dr. Britton has revived Cassini's *Nabalus trifoliolatus* for a well-marked northern plant, and has left the name *N. Serpentarius* to cover (as it should) the thick-leaved species of more southern range with the "involucre more or less bristly-hispid." Torrey and Gray described *Nabalus Fraseri*, var. *barbatus*, with the "involucre (12-15-flowered) hirsute when young with long purplish hairs," and in the *Synoptical Flora*, Dr. Gray, writing at a time when the name *Prenanthes Serpentaria* covered the northern plant with usually glabrous involucre, published *P. Serpentaria*, var. *barbata*, with the remark that "occasionally a few of these setose hairs are found on the involucre of ordinary *P. Serpentaria*, and in this variety [*barbata*] some heads are almost destitute of them." Now the original Torrey and Gray specimen of this variety is in no way different from the species, *Nabalus Serpentarius*, as correctly interpreted by Dr. Britton. Nevertheless, we have in *Muhlenbergia* (1:8) the new combination *Nabulus barbatus* (T. & G.) Heller, although in the *Catalogue* both *N. Serpentarius* and *N. trifoliolatus* are listed. Again, *Ilex verticillata*, forma *chrysocarpa*, noted by Dr. Robinson in *Rhodora* (2:106), appears in the new *Catalogue* as "[var.] *chrysocarpa* Robinson." The original specimen in the Gray Herbarium has never been borrowed by the author of the *Catalogue*, and it is perhaps elevated by him to varietal rank through carelessness; but now that it is listed as a variety it will be interesting to see how soon it will be erected to a species by one who believes in calling "all plants species which have

<sup>5</sup>In this paper these names may be thus accepted without discussion as to their status.

characters enough to justify the use of a distinctive name," and who, like him, knows the plant only from the descriptive phrase "fruit bright yellow." These two cases are sufficient to show the character of work which can be done by one who believes in making all the new combinations possible, when he is handicapped by "lack of time" to consider his work, and who apparently holds it more important to launch a mass of ill-considered (and often to him unknown) species than to publish only the results of critical and scholarly consideration.

If in thus launching so many species (and occasional varieties) of which he can have little or no personal knowledge, the author were producing combinations consistent with the names in the remainder of his book, his reason would be obvious and to some extent justifiable. But only a slight examination of the names taken up is sufficient to show that he has had little conception of any clearly defined principle to govern his selection of names. Professedly the names in his work, like those in the Botanical Club *Check List* and in Britton and Brown's *Illustrated Flora*, are based upon the principle of strict priority; but the result, as shown here perhaps even more than in those works, gives us little assurance that the publications on such a basis are bringing us the uniformity which has been so loudly proclaimed and which every one would so gladly welcome.

In the first group of plants listed, for instance, the Pteridophyta, the names essentially as defined by Professor Underwood<sup>6</sup> are taken up. Professor Underwood is one of the few authors among the radical botanists who has squarely faced the strict priority question, and in his selection of generic types he has attempted to follow the logic of his course to the bitter end. Thus, as the type of the genus he takes the first species described under the generic name, so long as the same plant does not belong to some previously defined genus. In such a case he logically takes for the generic type the first species which is clear from all previous genera. However much one may differ from him as to the expediency of such a course, it is indeed a

<sup>6</sup> Our Native Ferns and their Allies. Ed. 6. 1900.

satisfaction to know that at least one follower of the Rochester Code is ready to show us the actual task and the enormous upsetting of names consequent upon a conscientious and logical working out of the principle of strict priority.

In Mr. Heller's *Catalogue* many of the names accepted are not those which can be used consistently by authors who are committed to the Rochester Code. When that code was proposed it was professedly with the purpose of establishing uniformity in our nomenclature. As an outgrowth of its adoption by some American botanists the Botanical Club *Check List* was issued, a list which aimed to give us the names which our northeastern plants must henceforth bear according to the rulings of strict priority principles. That publication gave us the first tangible result upon which to base our estimate of the workings of the code; and though by some thoughtful and conservative students the book and the principles represented by it were carefully discussed, by other botanists the publication was hailed as "the sign that the day of 'authority' as such is ended, and the day of 'law' has begun,"<sup>7</sup> and we were informed that "even the most obscure botanist is nowadays entitled to know why an old plant comes out under a new name . . . and that their [the compilers of the *Check List*] work is plain work, the plain and straightforward statement of facts."<sup>8</sup>

It is pertinent, then, for "the most obscure botanist" to ask about some of the names now (at least at the time of this writing) in vogue among those who champion the Rochester Code, and we may be permitted to inquire of those who have been instrumental in bringing about the present "uniformity" how they account for a few of the names in their pages. Reference has already been made to Professor Underwood's treatment of the ferns partially adopted by Mr. Heller in his *Catalogue*. In Britton and Brown's *Illustrated Flora*, published in 1896, 59 species of true ferns are recognized, and the names, we are told, are those authorized by the Rochester Code. But in Professor Underwood's latest treatment more than 25 per

<sup>7, 8</sup> BESSEY, C. E.: Am. Nat. 29: 350.

cent.<sup>9</sup> of those very species appear under different names—still the names authorized by the Rochester Code. For the benefit of those not familiar with these works, but who have an interest in seeing the working of the same rule in the hands of one of its strong advocates, the fifteen northeastern species which have recently appeared under new names are here enumerated:

Names in Britton and Brown's <i>Illustrated Flora</i> (1896), based upon the Rochester Code.	Names in Underwood's <i>Our Native Ferns and their Allies</i> (1900), based upon the Rochester Code.
<i>Onoclea Struthiopteris</i> (L.) Hoffm.	<i>Matteuccia struthiopteris</i> (L.) Todaro.
<i>Dicksonia punctilobula</i> (Michx.) A. Gray.	<i>Dennstaedtia punctilobula</i> (Michx.) Bernh.
<i>Cystopteris bulbifera</i> (L.) Bernh.	<i>Filix bulbifera</i> (L.) Underwood.
" <i>fragilis</i> (L.) Bernh.	" <i>fragilis</i> (L.) Underwood.
" <i>montana</i> (Lam.) Bernh.	" <i>montana</i> (Lam.) Underwood.
<i>Dryopteris Lonchitis</i> (L.) Kuntze.	<i>Polystichum lonchitis</i> (L.) Roth.
" <i>acrostichoides</i> (Michx.) Kuntze.	" <i>acrostichoides</i> (Michx.) Schott.
" <i>Braunii</i> (Spenner) Underwood.	" <i>Braunii</i> (Spenner) Lawson.
<i>Phegopteris Dryopteris Robertiana</i> (Hoffm.) Davenp.	<i>Phegopteris Robertiana</i> (Hoffm.) Underwood.
<i>Scolopendrium Scolopendrium</i> (L.) Karst.	<i>Phyllitis scolopendrium</i> (L.) Newm.
<i>Asplenium acrostichoides</i> Sw.	<i>Asplenium thelypteroides</i> Michx.
<i>Pteris aquilina</i> L.	<i>Pteridium aquilinum</i> (L.) Kuhn.
<i>Pellaea Stelleri</i> (S. G. Gmel.) Watt.	<i>Cryptogramma Stelleri</i> (Gm.) Prantl.
<i>Cheilanthes gracilis</i> (Fée) Mett.	<i>Cheilanthes Feei</i> Moore.
<i>Notholaena nivea dealbata</i> (Pursh) Davenp.	<i>Notholaena dealbata</i> (Pursh) Kunze.

That the names recently used by Professor Underwood are more truly consistent with the strict priority principles than many names in other groups listed by Mr. Heller has been already emphasized. But why, we would ask, are there so many unexplained inconsistencies in this new *Catalogue*, especially when the attention of followers of the Rochester Code has

<sup>9</sup> The true ferns alone are here considered, and the genus *Botrychium* is purposely omitted, since that genus has been subdivided by Professor Underwood to such an extent that comparative figures would have little definite significance.

been called to a number of such erroneous matters? A very typical example of the inconsistent method (or lack of method) employed in the *Catalogue* is shown in the treatment of the genera *Cheiranthus* (*Erysimum* of authors), *Erysimum* (*Sisymbrium* of authors), and *Sisymbrium* (*Nasturtium* and *Roripa*<sup>10</sup> of authors). Professor E. L. Greene<sup>11</sup> pointed out, in December 1896, the only logical and consistent course to be followed, according to strict priority principles, in the cases of *Cheiranthus* and *Erysimum*. Mr. Howell<sup>12</sup> has followed his lead, and in a recent article the same point<sup>13</sup> was further emphasized. There is, then, no reason why the authors of the second volume of the *Illustrated Flora*, published May 31, 1897, and of the *Appendix*, published June 20, 1898, should have been ignorant of Professor Greene's logical article. But why did they ignore his conclusions and use names in a sense absolutely inconsistent with the

<sup>10</sup> The so-called reformers persist in writing *Roripa* instead of the original form, *Rorippa*, and they say *Bicuculla* instead of the original form, *Bikukulla*, although the matter has been freely discussed in the past, and by this time they should be aware of the facts in the case. But, on the other hand, after using the name *Koniga*, they now take up the original *Konig*. If in one case they adopt the original spelling, why not in the others? Is this what they consider a consistent method, and does it appeal to them as "the plain and straightforward statement of facts?" We should like to inquire also about the name which, in the publications of the reformers, has recently taken the place of *Mikania* Willd. In the Botanical Club *Check List* we have a name attributed to Necker and spelled *Willoughbya*, with the footnote remark "*Willoughbaeya* in original;" but in the *Illustrated Flora* the name is spelled *Willughbaea*, and we are informed that the plant was "probably named in honor of Francis Willoughby, 1635-1672, English naturalist, but the name spelled by Necker as above [*Willughbaea*]." This spelling is, therefore, faithfully followed in Mr. Heller's *Catalogue*. Otto Kuntze enumerates in his *Revisio Generum Plantarum* some "incorrect ways of writing the name" as follows: "*Willugbaeya*, *Willoughbeia*, *Willughbeia*, *Willoughbeja*, *Willugbeia*, *Willughbeja*, *Villughbeia*." Poor Necker, himself, if he were living, would indeed be dazed, particularly as his name was unlike either of those definitely asserted by Dr. Britton to be correct, and since, on the contrary, the true and original form *Willugbaeya*, is the *first* form enumerated by Kuntze as "incorrect." From these facts it would seem that to some botanists whose work is controlled by "law" such divergence from the original spelling is of slight moment. If so, will they be good enough to make clear why *Mikania* is rejected for *Willugbaeya* and its variations (1790), when in 1789 Scopoli published *Willughbeja* (compare Otto Kuntze above), a genus of the *Apocynaceae*?

<sup>11</sup> *Pittonia* 3: 128.

<sup>12</sup> *Fl. N. W. Am.* 1: 38-56.

<sup>13</sup> ROBINSON, B. L.: *BOT. GAZ.* 25: 439-442.

spirit and rulings of their own Rochester Code, especially when, as we were informed in 1895 by one of the *Check List* committee, "the committee . . . . would still be grateful . . . . for useful suggestions on these matters, and that all communications of this kind would receive fair hearing and sober judgment."<sup>14</sup> There is, furthermore, no possible reason why the author of the *Catalogue* which suggested this discussion, should have been, in 1900, uninformed of the publications on the subject. In fact, perhaps unconscious of the thoroughly inconsistent course he was taking, he has followed one third of the suggestions made and has adopted for the conventional *Erysimum* of authors the name *Cheiranthus*; but he still clings to the names *Sisymbrium* and *Roripa* for genera to which they cannot be applied by conscientious followers of strict priority principles dating from 1753.

Another point in regard to generic names pointed out in one of the articles cited<sup>15</sup> is in the case of *Cerastium* and *Stellaria*. It was there clearly shown that when the first part of the Linnaean *Stellaria* was transferred by the reformers to *Alsine* (a course not entirely free from question), one species was still left in *Stellaria*, namely, *S. cerastioides* L. This plant is treated by modern authors as a *Cerastium*, and in the Botanical Club *Check List*, the *Illustrated Flora* and in Mr. Heller's new *Catalogue* it appears as *C. cerastioides* (L.) Britton. But in the *Species Plantarum* of Linnaeus *Stellaria* preceded *Cerastium*, and therefore the portion of *Stellaria* (*S. cerastioides*) left when the remainder was transferred to *Alsine* should, according to the strict priority principle, become the type of *Stellaria*, and the succeeding genus *Cerastium* should be absorbed by it. Why, then, after this matter was clearly pointed out in June 1898, does the author of the *Catalogue*, who does not hesitate to launch a lot of new combinations based upon plants of whose status he is much less certain, still keep up the name *Cerastium* in its traditional sense?

The familiar vine known to most of us as *Wisteria* is listed in the

<sup>14</sup> COVILLE, F. V.: BOT. GAZ. 20: 164.

<sup>15</sup> ROBINSON, B. L.: BOT. GAZ. 25: 444, 445.

*Catalogue* as *Kraunhia frutescens* (L.) Greene (though that name was first published by Rafinesque in 1808); but the recently described *Apios Priceana* Robinson is given without change of name. The author of the *Catalogue* must admit that he knew of the publication of the latter plant, else how could he include it in his *Catalogue*. But will he inform us how it happens that he has ignored the facts presented in the original discussion,<sup>16</sup> of that species? Was it not shown as clearly as could be desired by anyone that the names *Apios* Moench (1794) and *Kraunhia* Rafinesque (1808) were both antedated by *Bradlea* Adanson (1763)—a name applied to two Linnaean species of *Glycine*, *G. Apios* (*Apios tuberosa* Moench), and *G. frutescens* (*Wisteria frutescens* Poir.) now referred by the reformers to *Kraunhia*? And was it not made clear that by those who would follow the Rochester Code the name *Bradlea* must be taken up for *Apios*? How does the author of the *Catalogue*, who lists *Apios Priceana*, explain his failure to stand by the principles he claims to follow?

There are many other generic names accepted by the reform botanists and now adopted in this *Catalogue*, which, according to the rules to which they have committed themselves, have no better status than those pointed out. But the few cases already explained in the past and here again emphasized are sufficient to show him who cares to examine the original references that the member of the *Check List* Committee, who, in 1895, wrote that "all communications of this kind would receive fair hearing and sober judgment" could not have been speaking for all the members of the committee, nor indeed for many whose prolific writings have done more than anything else to stultify the rules of which they claim to be true advocates. That such absolute recklessness in the application of these rules is not satisfactory to all members of the *Check List* Committee is occasionally made apparent. Professor Underwood's position in regard to fern names has been remarked; and another of the committee has thus expressed himself: "Why are some of us so openly at war with our own rules? Certainly no rule relating to the

<sup>16</sup> ROBINSON, B. L.: BOT. GAZ. 25: 452.

observance of priority has been more generally recognized and deferred to than this, that a genus, as to its name at least, stands or falls with its type species; no rule is more indispensably necessary; and nothing but endless change and confusion can come of the neglect of it."<sup>17</sup>

Numerous inconsistencies as to the treatment of species as well as genera have been publicly pointed out; yet here, as in case of some other perfectly just and logical criticisms, the effort seems to have been wasted upon those who are bringing us "the day of 'law.'"

In a review<sup>18</sup> of the first edition of the *Catalogue* attention was called to some of these specific names. *Anoda lavaterioides* Medic., for instance, as there intimated, has a Linnaean synonym in *Sida cristata*, while *Arenaria sajanensis* Willd. is the same as the Linnaean *Stellaria biflora* (*Arenaria biflora* (L.) Watson, which is the name accepted by Dr. Britton). Yet in spite of these very clear cases which have been emphasized in print, the second edition of the *Catalogue* follows the first in giving *Anoda lavateroides* [*lavaterioides*] and *Arenaria Sajanensis*. By what "law" are these names reconciled with the Rochester Code, and why is the public criticism of their use by the so-called reformers so openly ignored?

Another point emphasized by the same reviewer, whose words apparently bore too much of "authority" to influence the author of the book criticized, was the abundance of "perfect and confessed synonyms" in the *Catalogue*, thus swelling its bulk, but decreasing by inverse proportion the confidence we can feel in it as the product of careful work. Several cases were cited (*Silene Cucubalus* and *S. vulgaris*, for example); but, as we have now learned to expect, the same misleading and unjustified duplication of names occurs in the new edition. When, however, the same species appears under different genera, as in case of *Aster nemoralis* Ait., we must confess the least bit of surprise. Professor Greene, in splitting the genus *Aster*, revived for part of it the Nuttallian genus *Eucephalus*. Among other species which

<sup>17</sup> GREENE, E. L.: *Pittonia* 3: 129.

<sup>18</sup> ROBINSON, B. L.: *Am. Nat.* 32: 460.

he proposed was *Eucephalus nemoralis*, based upon *Aster nemoralis* Ait., though in *Aster* he left the closely related and often indistinguishable *A. acuminatus* Michx. In the new *Catalogue* we find under *Aster*, *A. nemoralis* Ait., listed and numbered, while under *Eucephalus* we have *E. nemoralis* Greene, treated in the same handsome manner. The troublesome *Aster nemoralis* var. *Blakei* Porter, however, a plant which so mingles the characters of *Eucephalus nemoralis* Greene and *Aster acuminatus* Michx. as to embarrass even its own author, is wisely left with *Aster nemoralis*. Why, then, if *Eucephalus nemoralis* is identical with *Aster nemoralis*, does the author of the *Catalogue* list the variety of the latter only under *Aster*, when the species is treated as belonging to both genera?

Many of us were brought up to speak of *Alisma Plantago* L. and *Veronica Anagallis* L., but during the past decade the followers of the Rochester Code have adopted the fad of calling these plants *Alisma Plantago-aquatica* and *Veronica Anagallis-aquatica*. The use of such names has indeed afforded an interesting diversion and has kept us constantly tingling with expectant excitement as we have waited to see what other familiar names would appear in new and fantastic garb; but it must be confessed that a careful search in the volumes of *Species Plantarum*, where these names are said to occur, has failed to reveal them. Instead this is what is found: *Veronica Anagall.* ▽ and *Alisma Plantago* Δ. Thus it seems that Linnaeus did not write even *Anagallis* in full; and we should like to be informed on what authority (in the *Species Plantarum*) we know that Δ and ▽ are both mysterious ways of writing *aquatica*? And if a triangle is said to mean *aquatica* why do not the reformers append that adjective to their *Roripa Nasturtium*, for Linnaeus wrote *Sisymbrium Nasturtium*; or does the position of the triangle in relation to the name give it a new meaning? Here is a great opportunity for someone to hunt up all the triangles in the *Species Plantarum* and thus give us a new lot of specific names. But, seriously, we may ask why, in the new edition of the *Catalogue* (as in the old), this modern fad was followed?

The triangles, like some other symbols occasionally used by Linnaeus, presumably descended from earlier authors, and if they are taken to mean *aquatica*, and that adjective is written as a portion of the plantname, we are simply reverting to the pre-Linnaean method of polynomial (or at least trinomial) names, and the whole system of binomials is weakened. The date 1753 has been generally accepted as the limit back of which we are not to go for names; and if Linnaeus himself did not use the name *Alisma Plantago-aquatica* or *Veronica Anagallis-aquatica*, are we justified in going back to some earlier author for such names? Right here is a very dangerous tendency in the usage of the reformers. If they will thus admit an occasional pre-Linnaean name which was not used in the first edition of the *Species Plantarum*, what assurance do they give us that their strict priority rule with a time-limit definitely set at 1753 may not at any time be made elastic enough to protect any whimsical exception its advocates choose to set up?

One of the members of the *Check List* Committee, speaking of the citation of the original author of a combination, has informed us that "it is no longer a question of credit, but a question of practical utility."<sup>19</sup> Surely this is the ideal for botanical nomenclature which every serious student will commend; and we may well put to ourselves the question, is "practical utility" in view or does it seem very near actual attainment, when we find the members of the committee which set out to give us a uniform system of names "at war with" their own rules? Has the "day of 'law'" really begun when those with whom a great trust has been placed juggle with it as with a toy, now following this principle, now that, and ignoring at their own wills such candid criticisms of their methods as show the inconsistencies in their work? Is the "day of 'authority' as such" indeed ended when, after one of their own associates on the committee has publicly reprimanded them and has pointed out the only course for one who would live up to the principles he has espoused, the supporters of the Rochester Code continue to

<sup>19</sup> WARD, L. F.: Bull. Torr. Bot. Club 22: 325.

employ names inconsistent with the principles there emphasized?

A member of the committee already quoted has said, in defending the principles of the Rochester Code (principles which as abstract principles need little defense), "if matters are to be left to the individual judgment of publishing botanists, there will be no comparing the confusion that is in store for us with that which we have had in the past."<sup>20</sup> Where in the past (as embodied in the recent editions of Gray's *Manual*, the book selected by this author for his comparisons and generalizations), will he find 25 per cent. of the names changed, as has occurred within four years in the case of our ferns, and that after the names were said to be established on strict priority principles?

The same author in speaking of the Rochester Code has written further: "Those who oppose this movement, if there be any (and I have no doubt there are) who *really see that it might be the last time* that serious changes would have to be made in botanical names, would seem to do so purely from a personal disinclination to incur the annoyance of accustoming themselves to a new set of names. It must be admitted that *this motive is not as high as we might hope botanists generally to be actuated by* [italics ours]." <sup>21</sup> An associate of this writer on the committee has expressed "the hope that Dr. Robinson and the few who think with him on this subject will *lay aside personal prejudices and join the remaining nine tenths of our botanists . . . .* in a nomenclature based on scientific needs and a scientific method [italics ours]." <sup>22</sup> Both of these authors wrote in 1895, when the *Check List* was a comparatively new topic for discussion. Can it be that now, in view of the facts here presented, they still believe that the *Check List* really represented "the last time that serious changes would have to be made in botanical names," or that the loose and indiscriminating methods employed by many who are now active exponents of the Rochester Code are bringing us any nearer that "last time?"

<sup>20</sup> WARD, L. F.: *ibid.*, 316.

<sup>21</sup> WARD, L. F.: *ibid.*, 319.

<sup>22</sup> COVILLE, F. V.: *BOT. GAZ.*, 20:167.

In formulating a system of nomenclature we should keep constantly in view the "question of practical utility." If this fundamental point is neglected, what woeful confusion must be encountered by everyone who attempts to use plant names! Already matters have reached such a state, that few followers of the Rochester Code can say offhand what many common plants should be called. The well-known species described by Ventenat as *Dalea purpurea*, then by Michaux as *Petalostemum violaceum*, but generally known of late as *Petalostemon violaceus*, has been treated as follows during the past decade. Otto Kuntze, in his *Revisio Generum Plantarum*, called it *Kuhnistera violacea*, ascribing the name to Aiton, who, however, wrote *Kuhnistera violacea*. This latter name is taken up by Kellerman and Werner who ascribe it to Otto Kuntze although (according to Steudel's *Nomenclator*, ed. 2, 1:851, a well-known work) the name originated with Aiton. In the *Metaspermae of the Minnesota Valley* the plant is called *Kuhnistera purpurea* (Vent.) MacMillan; but it has recently been published as *Petalostemon purpureum* (Vent.) Rydberg, and in Mr. Heller's new *Catalogue* it is listed essentially under this name (as *P. purpureus*). After these Jekyll-and-Hyde-like changes it is certainly reassuring to see Dr. Jekyll getting the upper hand, and to find in the latest writings of some of the reformers the long established name *Petalostemum* (on) reappearing. But do these names used by various reformers represent uniformity? Even if an occasional systematic botanist can keep track of the changes in names, how about the morphologist, the histologist, the physiologist, the pathologist, the paleontologist, the ecologist, to say nothing of the horticulturist, the pharmacist, and the everyday student of plants? Should not all these followers of pure or applied botany be considered in our interpretation of the "question of practical utility?" And what can they hope for in a system of names which shows no more stability than the one under discussion?

To the student whose work is in other fields than systematic botany, the present lack of uniformity in plant names is necessarily most perplexing. But to the systematist, who sees more

closely the constant haggling over names, the situation is quite as puzzling. The Rochester Code was formulated ostensibly to establish uniformity in our names. Its followers have worked vigorously to comply with its rulings. From time to time their attention has been publicly called to fundamentally weak spots in its wording. Again they have been asked to explain certain of their names seemingly inconsistent with their principles. Yet these criticisms have generally been ignored. Instead of strengthening the weak spots in their rules and correcting self-evident mistakes in their names, the reformers have faithfully clung to the discredited gods they had already set up. These statements are not extravagant nor vague generalizations. They are simple conclusions drawn from the facts presented in this discussion, and from others very apparent upon many recent pages. Is this the best the Rochester Code can do? Is this what we are to call "uniformity?"

If we are really desirous of obtaining stability in our nomenclature, and if at the same time the "question of practical utility" is to be considered, our clearest course cannot be by the Rochester Code, especially as followed by its originators. We shall, however, find a comparatively clear and practical method by adopting in our selection of generic names the Berlin rule; and in our selection of specific names, the so-called Kew rule of retaining the first specific name used under the accepted genus. In this way we are able to retain a very large proportion of the long-established and best-known combinations, without the necessity of wading (often blindly) through the mazes of obscure and poorly indexed literature. And, what is better, after comparatively slight alteration of the long-established names, we can feel that in only very rare cases must we abandon those known to practically all botanists. If, like Professor Ward, we all feel that "it is no longer a question of credit, but a question of practical utility," is not this simpler course worth testing?



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