

that ideal of physical form which the keen artistic sense of the ancient Greeks recognized as the perfection of corporeal symmetry. Wherever it is present in any degree, it is sure to be recognized. As Novalis says in one of his apothegms, "Beauty alone is visible."

SOME POINTS IN THE NOMENCLATURE-PRIORITY QUESTION.

BY LUCIEN M. UNDERWOOD.

THERE are some of our younger botanists who see no possible merit in the nomenclature-priority discussion. That this is the case is naturally due to the fact that neither their age nor training have been sufficient to enable them to obtain a general view of botany as a science in which the relations of plants to each other and to other living things form the crowning summit of achievement. When we say *relations*, we mean the word in its deepest and widest sense — morphologic, embryologic, physiologic, geographic, and chronologic.

To those whose work involves the weighing, sifting, and correlating of all the truth concerning some group of plants that has been found out by patient workers in times past and present, as well as that brought to light in their own comparative research, the necessity of some uniform, authoritative, and permanent system of nomenclature needs no argument. If some have acute inflammation of the morphologic nerve so that their attention is largely drawn away from the general wants of the system to the nursing of their peculiar member, they are worthy of our sympathy, but they must reduce their hypertrophy before they can expect the botanical world to regard their judgment as normal outside their special sphere.

While we thoroughly believe in Goethe's assertion that "species are the creation of text-books while Nature knows only individuals," we have not yet advanced sufficiently far to be able to discontinue the present method of grouping individuals into species and recognizing them by certain fixed names. This is a matter of convenience, and it is a present logical necessity. We believe, therefore, that the matter of nomenclature ought to be settled at once and permanently, and this we believe to be the opinion of all who look at systematic botany, not as a mere "battle of synonyms," but in its true position, representing as it does the ultimatum toward which every fact in the science tends, and into which the whole science will be ultimately crystallized. So far is this desirable that if a system can be agreed upon, it must and ought to be by the yielding of personal opinions to the will of the best and maturest judgment of the botanical world.

One phase of the question has not yet been sufficiently dwelt upon, and that is the one which involves the element of personal justice. There are some who say that there is no ethical side to the question, that it is a mere matter of expediency. If *justice* pertains to ethics then there is an ethical element in the problem. It has always been maintained that a man has the right to the product of his brain. If he invents a new mechanical contrivance he is awarded a patent. If he writes a book he is given a copyright. If he discovers a new principle or process in the natural world his name is inseparably connected with that principle. Otherwise why do we speak of the Bell telephone, of Marsh's test for arsenic, or of Newton's law of gravitation? The same is true of discoveries in botanical science, for we inseparably connect certain names with the earliest recogni-

tion of protoplasm, the announcement of its identity with sarcode, the discovery of fertilization by antherozoids, the continuity of protoplasm, and every other important addition to a knowledge of the plant world. In the same way the recognition of a natural group of plants, an order, a genus, or even a species is now regarded as of sufficient importance to be credited to the one who makes the discovery, not by any means on the ground of expediency (though it is doubtless in the highest degree expedient), but because of an innate feeling of justice due him who thus publishes the result of his work.

It is true that favored students or organizations may, for a time, regard themselves as the only rightly-appointed medium of description of species, but the multiplication of botanical centres, the specialization of workers, and the growing urbanity and cordiality in extending to specialists the privileges of public and private collections will all tend to prevent the growth of monopolies in a field which is not likely to become narrow enough for any to jostle offensively.

As a worker in one group of plants we present some questions that have suggested themselves in our work, drawing illustrations largely from the genera and species with which we are most interested, seeking not so much to offer dogmatic principles as to call to mind the feature of personal justice.

1. Shall there be an initial date in nomenclature?

What justice on the one hand, or advantage on the other, is there in accepting those of Micheli's genera that were adopted by Linnæus, and rejecting others equally valid that were not? What virtue did the great compiler add to an adopted name that should render it either sacred or immortal? We have *Anthoceros* and *Sphaerocarpus*, *Blasia*, *Riccia*, and *Lunularia*, all established by Micheli in 1729, and all accepted to-day without question, forsooth, because they have received the stamp of the immortal Linnæus, who could scarcely distinguish a hepatic from other Bryophytes. And yet Micheli, the founder of generic distinctions among Cryptogams, who knew and studied plants, adopted other generic names at the same time; these the great Linnæus did not accept because he could not get down to the study of plants and learn to distinguish genera among hepatics and other Cryptogams. Are we of this age so blinded that we must fall down and worship this popularizer of botany and accept his dictum as against that of a man whose shrewdness enabled him thus early to discriminate genera among Cryptogams?

But we must have a starting-point, some say. Why not then commence genera with the men who first originated them? Let us not award merit where merit is not due. Let us not assume for Linnæus a virtue that he did not possess. Micheli, Ruppius, and Dillenius were the originators of genera among hepatics. Why not recognize their genera that represent natural groups? If others are the progenitors of genera in other groups of plants, there is no reason why their work should not also stand, provided their names were not already preoccupied.

2. Shall names long used be laid aside when claimed for other plants on grounds of strict priority? Shall we recognize the principle of outlaw in nomenclature?

For example, *Marsilea* (Micheli, 1729) is a hepatic which since Raddi's time (1818) has been known as *Pellia*. *Marsilea* Linn. has since its establishment been used for a genus of quadrifoliate Pteridophytes. Shall the latter stand in the face of evident priority? While a compromise of this kind,

sacrificing an individual for the general good, if it could be agreed upon by an authoritative body, would be in the interests of both science and peace, it could not be accomplished without personal injustice.

Another case more complicated is that of *Asterella*. This genus was established by Palisot de Beauvais in 1810. Raddi independently established *Reboullia* in 1818. After many years European hepaticologists, with Lindberg at the head, discovered that the two genera were identical; so *Reboullia* yielded to *Asterella*. Meanwhile Nees von Esenbeck had established the genus *Fimbriaria* (1820). Latterly Lindberg took a second thought and regarded Beauvais's three-line description as more nearly representing *Fimbriaria* Nees. So this generic name, known for over half a century, is laid on the shelf and *Asterella*, which we have been using for a totally different plant, is put in its place. On this basis *Reboullia* Raddi was restored.

3. Shall "the first name under a genus" hold against a previous specific name?

Riccia reticulata (Gmelin, 1796) was erected into *Corsinia* by Raddi, in 1818, under the name of *Corsinia marchantioides*. Shall this name hold, or shall we write *Corsinia reticulata* (Gmelin) Dumort. (1874)?

We believe the latter more justly covers the case, although on the ground that Raddi's name had been long in use this might be a proper time to sacrifice an individual for the public good!

4. Shall varietal names have priority over established specific names?

Madame Libert described *Lejeunea calcarea* in 1822. It proved to be the same as had been described by Hooker in 1816, as *Jungermannia hamatifolia* β *echinata*. Taylor in 1846 wrote *Lejeunea echinata* Tayl., perhaps more for displaying the caudal appendage than for principle, but he has not been generally followed until latterly, when there is a tendency to revert to his name. Since varieties, especially among Cryptogams, are too often established on mere sports, forms, or other slight variations, and species are the units of classification, we believe that description as a species ought to be the ultimatum in matters of priority. If Madame Libert had recognized the identity with Hooker's variety, and had named it *Lejeunea echinata* in the first place no one would have quarrelled with her, for it would have been advantageous to preserve Hooker's name. Since she named it *L. calcarea* we believe this name should stand.

5. Can inappropriate names be cancelled on that ground alone?

In 1867 Alphonso Wood established a new lileaceous genus from California under the name of *Brevoortia*. Out of compliment to the little daughter of the stage-driver who first showed him the plant, he called it *Brevoortia Ida-Maia*. When Dr. Gray reviewed Wood's species a year later, we deem that he did a double injustice: (1) In hastily cancelling a genus which had not originated at Cambridge, and (2) in substituting a specific name on the ground that the one chosen was a compound. He thus obliterated all trace of Wood's discovery by writing *Brodiaea coccinea* Gray! The first injustice was partly atoned for by Dr. Watson who recognized Wood's genus as valid in his "Revision of the Liliaceae," but instead of writing Wood's name in accord with the principle of "the first name under a genus" he wrote *Brevoortia coccinea* Watson! It might be well to ask why *Ida-Maia* is any more objectionable than *Hart-Wrightii*, *Asagrayana*, *Donnell-Smithii*, or any other of the many compounds of our system.

To take another example, Berkeley established the genus *Cronisia*, closely related to *Corsinia*. Lindberg, not recognizing Dr. Gray's aphorism that "a neat anagram is not bad," cancelled *Cronisia* and substituted *Carringtonia* Lindberg.

We maintain that a name once established cannot be cancelled on the ground of offended personal taste even though it have the euphonious melody and the suspicious flavor of *Mariae-Wilsoni*!

6. How far has a later writer a right to correct names previously established?

We cite three instances:—

(1). In 1821 S. F. Gray established a large number of genera of British Hepaticae. To these he gave personal names *Kantius*, *Herbertus*, *Pallavicinius*, etc. These have been changed by Carrington to a feminine ending *Kantia*, *Herberta*, *Pallavicinia*, etc.

(2). Lindberg has adopted the plan of changing all personal names ending in *ianus*, *a*, *um* to *ii*; for instance, he writes *Jungermannia Helleri* for *J. Helleriana* as originally written by Nees.

(3). *Tricholea* Dumort. was corrected by Nees to *Trichocolae* to bring it into harmony with its derivation. Dumortier originally wrote it *Thricolea*.

Except in manifest errors of orthography, names should be let alone.

7. What credit should be given for generic and specific names?

(a) Shall we write the name of the author of the specific name in case there has been a transfer to a new genus, and if so in parentheses or not? (b) Shall we write the double combination of the first describer of the species in parentheses followed by the name of the author of the generic combination? (c) Shall we write the name of the one who made the transfer?

While we shall hail with joy the time when the bare binary shall be all that is necessary to identify a plant, we believe the following to represent in a specific instance the order in which the demands of personal justice as well as scientific convenience are most fully met:—

(1). *Metzgeria pubescens* (Schränk) Raddi.

(2). *Metzgeria pubescens* (Schränk).

(3). *Metzgeria pubescens* Schrank.

(4). *Metzgeria pubescens* Raddi.

To write *M. pubescens* Schrank, makes that writer say what he never thought of saying. To say *M. pubescens* Raddi, in accordance with the system long familiar to us by the use of Gray's Manual, is to unjustly transfer the credit of the species where it never rightly belonged, and appears to us the most faulty system of all.

The above questions should be settled by a commission after the example, if not the manner, of the American Ornithologists' Union, if individuals of strong personality can lay aside their peculiar idiosyncracies and unite in a system that will both meet the demands of justice and at the same time serve the highest interests of the science.

To this commission could be referred minor questions like that of "once a synonym always a synonym;" how close may generic names agree in orthography¹; what form of nomenclature is best for varieties, sub-species and "forms;" and the punctuation and capitalization of specific names. In nomenclature individuality ought to disappear and uniformity universally obtain.

DePauw University, Aug. 15.

¹ For example, should *Richardia* preclude *Riccardia*, or *Caesia*, *Cesia*?