TOWARD NOMENCLATURAL STABILITY

by

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Now that the rules have been hashed and rehashed, their verbiage clarified, their holes plugged, their intrinsic contradictions at least partially resolved, the supreme question of nomenclature emerges all the more clearly: how can we stabilize specific names? For years now we have had to submit to the displacement of familiar and well-established names by prior names unearthed in obscure and forgotten works, often dealing primarily with horticulture, agriculture, or travel. There seems to be no future limit to the process. The problem confronts every botanist, every one who uses plants and products of plants, every forester, every horticulturist. It is not merely a question for taxonomists; indeed the professional taxonomist is less inconvenienced than others by the changing of names, for he is accustomed to dealing with lists of synonyms and with the necessary complex bibliography; but nevertheless the solution of the problem is the responsibility of the taxonomist, and one which he can no longer avoid. There were signs at the Paris Congress that those who were gathered in the Section of Nomenclature were more aware than ever before of the reality of the problem and of their own responsibility in it; the creation of the Special Committee on Stabilization is to be welcomed by every one.

The letter circulated by the Chairman of this committee, J. S. L. Gilmour, and published in Taxon (3: 243, 244. N 1954) presents in summary form the remedies that have been from time to time proposed for the mutability of specific names. This summary is taken as the basis of the comments which follow; which in turn lead up to our own proposal.

(a) The conservation of specific names. This would seem to be the ,,logical" method: to do with specific names as we have done with generic names and propose to do with familial names. Botanists unfamiliar with the problems of taxonomy have often come to us with the suggestion that all names be conserved once and for all. The argument that has again and again defeated the proposal is simply that it is impractical, that it would necessitate the activity of a vast force of clerical botanists. When we survey the history of conservation of generic names, we must admit the truth of this contention. The principle was adopted without serious misgivings in 1905 for application to the 81 genera “of more than a merely scientific interest, or that are very large” proposed by the German group. Everyone knows what has happened in the 50 years since then; and the end is not in sight, the list of conserved genera seemingly undergoing enlargement by geometrical progression with each successive congress. Whether it has been a good or a bad thing is perhaps a matter of opinion; but there is no question of the extensive bibliographic research that it has entailed; and there can be no question of the vast amount of such work that would be required if we were to embark on a similar process for specific names. Perhaps if it were practicable to limit the number of conserved names in some way, the principle could be accepted; its proponents have always insisted that it would be used for relatively few names, those of economically important plants. On the dangers of such arguments see below.

(b) The rejection of specific names. The proposal to create a list of specific names which cannot be used was made by the British botanists and ably defended by them at Paris. It avoids some of the difficulties of conservation. It would not be necessary to typify the names to be rejected, since (if we understand the proposal) they would be absolutely rejected; they could not be used thenceforth for any plant. Whether they had types or not would be irrelevant. However, the
difficulty of limiting the process would be the same. Gilmour wisely remarks
(Taxon 3: 243): “At Paris, some botanists feared that many hundreds of names
would have to be included in a list of Nom. Spec. Rej., while others thought that
the proposal had come too late, and that there were very few further changes to
be made! Clearly no progress can be made on the problem unless the approximate
number of actual cases likely to be involved can be assessed.” This seems to
envisage, on the part of the Chairman of the committee, no absolute limit to the
list of rejected names; it is our opinion that to make a proposal based on some
“approximate number” which was anticipated would be as delusive as the original
proposal of the German botanists for the (anticipated) conservation of about 100
genera (ten times that number have already been conserved).

We have always been assured that names to be included in lists of conservanda
or rejicienda would be those of “economic plants”; this would limit their number
at the same time as it would apply the stabilizing remedy where it was most needed.
But what is an “economic plant”? We are faced here with the same sort of
vagueness as we find in the proposal to reject names not “taken up” after a
certain date or period of years (see below). We can imagine the interminable
wrangles at successive congresses over the official meaning of the word “economic”.
Plants that until lately were of interest only to the systematist and the phyto-
geographer, such as Rauwolfia, are now used as sources of important drugs or
other products; there was even a time when Solanum tuberosum was grown
only as a botanical curiosity — that time lasted 200 years and more! It might
even be argued that a mat of moss or lichen, slowly transforming the components
of a rocky ledge to soil, is an “economic” plant, no less so than the subsequent
forest, or the crop which follows the clearing of the forest. The Actinomycetes of the
soil have recently been found of considerable economic importance — no one yet
knows how much; are they not “economic” plants?

Turning from the primrose path of theory into the stony road of reality, we
shall have to face the fact that the definition of “economic” will be a personal
matter and will vary with the interests of the persons concerned. The impossibility
of assigning a limit to the process of rejection, by a definition of “economic” or by
any other means that the present writers can propose, means that the list of rejected
names will be one of indefinite length, probably exceeding the length of the list of
conserved generic names in the proportion that the number of species exceeds the
number of genera in the plant world. And, in fact, it may be more realistic to
admit that “what is sauce for the goose is sauce for the gander”. If stabilization
of names is desirable for some one group of plants, no matter how narrowly
defined, then it is a good thing for all plants. That, after all, is the principal object
of a code of nomenclature, and has been at least since 1813.

Still another weakness in the method (stabilization by rejection) is that we are
dealing with synonyms based usually on different types (if on any) and whose
identity is therefore a matter for the judgment of individual taxonomists; not
the most stable foundation on which to construct an edifice of rules! In short
a name rejected as a synonym of a well-known name may prove, on further study,
aided perhaps by the discovery of a collection by the author, to be not synonymous
at all, but to apply to a taxon not recognized by others since the publication of the
original description. The name, being rejected, cannot be used, and a new name
must therefore be created; so the rejection of names may even lead to a multiplication
of names.

(c) The banning of certain books. Of this perhaps the less said the better.
It is a principle which we should be very reluctant to accept, with its aroma of
at least one fortunately extinct dictator. For a scientist to have to scan a list of
"banned books" to find out whether or not he should adopt certain names would be a strange pass indeed. Furthermore, it would be as difficult to gain the consent of all to the selection of the books as it would be for all to agree on what names should be conserved or rejected. And, again, it would not be wholly efficient as a remedy; for many books have yielded names which are now generally accepted — and can still yield additional names to those diligent in searching in obscure places. To ban such books would be to upset present nomenclature; not to ban them would be to fail in the expected stabilization.

(d) *The banning of names not taken up after a certain date.* It is surprising that such a proposal can be seriously made. What is meant by “not taken up”? This seems to be a term impossible of objective definition. Certainly one cannot be sure that a name has never been once used after the specified date; that is, without having read not only every botanical work published since that date but also every work on horticulture, forestry, economic botany, every newspaper, every magazine, in which a botanical name might conceivably have been used. And as soon as a name previously judged “not taken up” was discovered in a work published within the specified period — the stability which we thought we had attained would again vanish like a mirage!

(e) *The recognition in the rules of “economic names” in addition to the strictly correct “botanical names”.* But this does not even provide stability. It merely indicates the despair to which we have been driven. We draw up a set of rules for naming plants and at the same time admit that the names which satisfy the rules are not suitable for general use and so we agree to recognize others, not in accord with them. We take away with one hand what we give with the other!

If none of the proposed measures will actually bring stability of names, are we then to acquiesce in the present situation, to say, in effect: “These are the right names, which you should use; if you don’t like them, so much the worse for you”? There is, however, another kind of proposal which has been made but curiously omitted by Gilmour in his letter; to be sure, it was made (by one of us) without proper analysis and explanation, and seems to have been generally misunderstood, perhaps even by its author! In its former shape (Taxon 2: 213. N 1953) it probably deserved oblivion; the principle involved, however, seems sound to us; some analysis of the problem may help to reveal its bearing.

The prior names which occasion most of our difficulties are usually discovered in long-neglected or forgotten works. The descriptions which accompany the names are usually meager and *are seldom associated with cited specimens* which can now be found and studied. Interpretation of these ancient names is therefore often a matter of personal opinion, and liable to considerable fluctuation. We may assemble what seems to be a reasonable set of arguments in favour of our decision, but, lacking a physical type, we still cannot offer positive proof of identity. By adopting such a name we set the stage for possible confusion beyond the immediate change in name, for a subsequent author may offer as convincing a series of counter-arguments (to him more convincing) on the application of the name. We offer the following proposal as a means of avoiding such situations.

**New Article. No name, even if prior, may displace a hitherto accepted name unless it is associated with a holotype cited in the original place of publication.**

The obvious loophole in such a provision, if it should be adopted, is that some ancient names might be adequately typified and so would have the right to displace the accepted names. We are firmly of the opinion, however, that such cases would be extremely rare; that, if such a provision had existed in the past, we should have
been spared most of the present confusion caused by the name-changing which is imposed on us by the rules.

Ultimately this provision would be equivalent to the one proposed before, its progenitor: to reject all names not associated with types. The defeat of this proposal was undoubtedly due to the general fear that it meant a vast amount of clerical and bibliographic work in the near future, since it would be necessary to provide every accepted name with a type at once before we could write another flora or manual! The intent was, of course, to provide for rejection of untypified names as it became necessary or advisable; the proposal made above is, we hope, so worded as to make this plain. It is to be understood that the accepted name should be provided with either a lectotype or a neotype if it has no holotype.

The principle of priority was originally invoked only to stabilize names. Unfortunately (as one of us has shown elsewhere), it has had the opposite effect. We have subverted stability to bibliography. We have admitted this by subscribing to the conservation of genera, a troublesome and time-consuming device and one quite impracticable (as it seems to us) for specific names. But meanwhile we have acquired a "type method". This offers a means of stability not foreseen by our nomenclatural ancestors. We should use every effort to expound the type method (still not understood even by some systematists), to make clear that a type is not a "typical" or a "representative" specimen, not a means of identification, but a device to anchor a particular name. Names which we wish to stabilize can usually be anchored by means already at our disposal. Those which cause confusion and which are not so anchored should be allowed to drift — into the limbo of forgotten things. We have a type method — let us use it.

ONE PLANT NAME IN SEARCH OF AN AUTHOR

by
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When meeting in Paris last summer to add the final touch to the botanical code that was being amended, for the very last time, I am sure, we took up principle IV (Art. 5 of the Stockholm Code) which reads: "In the absence of a relevant rule, or where the consequences of rules are doubtful, established custom must be followed". A shrewd congressist asked for an example to illustrate this principle. The chairman, who had always to pretend that he was fully acquainted with the rules, sought an example, but in vain. Nor was the rapporteur, who, on the other hand, really knew the rules, more successful; and, likewise, the assistant-rapporteur, and all the honorable delegates who were already an integral part of the Brownian movement of the tobacco smoke.

Such a failure was easy to explain. Were we in position to indicate a case pertaining to this principle, we might have proposed a new rule to answer the question, and incorporate such in the code (as well as bring the appropriate modification to all the subsequent article numbers). Principle IV means substantially: "If between two congresses a taxonomist finds a problem not previously considered, we beg him not to search for a complicated solution and be quiet until the next meeting of the nomenclature section". As long as there will be taxonomists, with no special jail for those indulging in subversive taxonomical activity, such a principle is necessary.

1) With due apology to Pirandello for the plagiarism of one of his drama titles.