university has shown, a thousand times, that sound theory and correct practice are two sides of a shield. A theorist is one who sees, and the practical man must be in touch with theory if he is to see what it is that he does.

What the future development of the great universities is to be perhaps no one can foresee. But this much is certain. Every city which because of its size or wealth or position aims to be a center of enlightenment and a true world-capital must be the home of a great university. Here students and teachers will throng by the mere force of intellectual gravitation, and here service will abound from the mere host of opportunities. The city, not in its corporate capacity, but as a spiritual entity, will be the main support of the university, and the university, in turn, will be the chief servant of the city's higher life. True citizens will vie with each other in strengthening the university for scholarship and for service. In doing so they can say, with Horace, that they have builded themselves monuments more lasting than bronze and loftier than the pyramids reared by kings, monuments which neither flood nor storm nor the long flight of years can overthrow or destroy. Sir John de Balliol, doing a penance fixed by the Abbot of Durham; Walter de Merton, making over his minor house and estates to secure to others the advantages which he had not himself enjoyed; William of Wykeham, caring generously for New College and for Winchester school; John Harvard, leaving half his property and his library to the infant college by the Charles, and Elihu Yale, giving money and his books to the collegiate school in New Haven, have written their names on the roll of the immortals and have conferred untold benefits upon the human race. Who were their wealthy, powerful, and high-born contemporaries? Where are they in the grateful esteem of the generations that have come after them? What service have they made possible? What now avails their wealth, their power, their high birth? Balliol, Merton, Harvard, Yale are names known wherever the English language is spoken, and beyond. They signify high purpose, zeal for learning, opposition to philistinism and ignorance. They are closely interwoven with the social, the religious, the political, the literary history of our race. Where else are there monuments such as theirs?

Scholarship and service are the true university's ideal. The university of today is not the 'home of lost causes, and forsaken beliefs, and unpopular names, and impossible loyalties.' It keeps step with the march of progress, widens its sympathies with growing knowledge, and among a democratic people seeks only to instruct, to uplift and to serve, in order that the cause of religion and learning, and of human freedom and opportunity, may be continually advanced from century to century and from age to age.

TYPES AND SYNONYMS.

From the literary standpoint the existence of many names for the same or closely similar objects or ideas is thought to enrich language and to conduce to facility, elegance and accuracy of expression. In systematic biology, however, synonyms figure as superfluous designations which furnish no useful or welcome additions to the vocabulary of science; biological synonymy is a most burdensome legacy of ignorance and confusion, requiring constant revision and readjustment, and yielding no adequate returns for the labor which the naturalist must expend in historical or merely antiquarian research. Indeed, the study of systematic biology appears to be little more than a 'battle of the synonyms' when its most conspicuous

result is the replacement of long-used names by others whose tenure is guaranteed only by personal opinions and individual methods of literary and historical criticism.

It is true that several independent descriptions of the same animal or plant often furnish more complete and satisfactory knowledge than could have been expected from any one naturalist, but not even this consideration will reconcile us to the indefinite multiplication of names by those more anxious to announce discoveries than to contribute to the permanent progress of scientific knowledge. As in general literature, it may sometimes be permissible to coin new scientific terms to avoid the confusion likely to arise from the use of those of doubtful application, but the tendency for the last half century has been distinctly in the direction of a divorce of systematic from general literature by holding to the permanent use of old names in preference to the admission of new and improved designations, the substitution of which had previously been a very common practice.

INSTABILITY UNDER LITERARY METHODS.

In dealing with specific names both zoologists and botanists now recognize that nomenclatorial stability requires adherence to a definite law of priority, with a fixed initial date and other regulations necessary for securing uniform interpretation and eliminating the variable factor of individual opinion. The wisdom and utility of these laws are now generally considered obvious, although there were many objections at first, and even the great Bentham took the ground that as the names of plants consist of two parts, a law of priority could be applied only to 'correct combinations.' By a similar effort of casuistry an effective priority for genera is now held to be impossible by systematists who still work under the theory that we are not attempting to name the natural groups of plants and animals, but are merely attaching names to varying concepts and definitions, the applications of which are to be determined by a historical study of the various interpretations and arguments of previous students. Some counsel a strict adherence to the intention of the original author, while others are accustomed to accept the usage of subsequent writers, so that it not infrequently happens that a name is used for a group of species quite distinct from those at first placed under it. An instance of this kind is that of the royal palm,* where the failure to hold the names *Euterpe* and *Oreodoxa* to their original species has complicated the synonymy and distribution of at least six genera.

Such usage accords well with the literary vicissitudes of words and definitions, but it is obviously not likely to conduce to the precision and stability required in scientific terms. The method of elimination, under which interpretations of genera are limited by the original content of

* Already noted in SCIENCE, N. S., 12:479, and in *The Bulletin of the Torrey Botanical Club*, 28:549.

The name *Oreodoxa* was originated by Willdenow for two Venezuelan species, the first of which, *O. acuminata*, has been referred to the older genus *Euterpe* while the second, *O. praemorsa*, has been used by Wendland as the basis of his genus *Catoblastus*, the name *Oreodoxa* being transferred to still a third group, no species of which was known to the author of *Oreodoxa*. The extent of the carelessness induced by the method of concepts is further illustrated by the fact that the genus *Euterpe*, to which *Oreodoxa acuminata*, and numerous other American species have been referred by many eminent botanists, was not established for an American palm, but for an East Indian species described by Rumphius as a *Pinanga* in 1741, and renamed *Calyptrocalyx* by Blume in 1836. Gaertner's original use of the name *Euterpe* in 1788 was however also connected with seeds of still another old-world palm not yet identified.

species, was an important step in advance of general literary chaos, though still far from logical accord with the principles of evolution and synthetic classification. From the practical standpoint also it is seriously objectionable as being but a partial measure which perpetuates and legalizes, even if in somewhat limited form, the very confusion it was desired to end.

Under the Benthamian method or 'Kew Rule' a plant might have a different specific name in each of the several genera to which different systematists might refer it, and under the so-called 'method of elimination' a generic name may be applied to several entirely different groups of species, as a result of varying theories of classification. But however inadequate for bringing about uniformity and stability of nomenclatorial practice, these propositions are of interest as admissions of the desirability of a formulated procedure instead of unguided personal caprice.

It may be charged equally against these methods, as well as against the method of types, that the authors of the older genera did not expect their writings to be interpreted by such criteria, since all three propositions have resulted from the recognition of the fact that the tasks of systematic biology are very different from the anticipations of the eighteenth century naturalists. With the prospect of a few thousand genera to be dealt with, the matter of a few synonyms for each was not important, and each naturalist might hope for the general acceptance of his improved names and descriptions. But with strengthening indications that a million genera or more will be needed to present the complexities of organic nature, sentiments of literary liberty may well give way to measures promising the practical advantages of uniformity and stability.

Moreover, where carelessness and caprice have been the rule the application of any system must be expected to result in many changes from current usage. And if the followers of the system of elimination have not hesitated to set aside many names in universal use for others discovered only by antiquarian research and supported only by individual theories of historical and literary interpretations, how much less should they object to changes made in accordance with the requirements of a method which can end, instead of merely diminishing, the instability admitted by all to be a most serious hindrance to the progress of systematic biology?*

* While this paper has been waiting for the press the type question has broken out among the spiders, a group rendered nomenclatorially difficult because many of the older generic names were proposed in connection with numerous species. Mr. F. Pickard Cambridge concludes (*Annals Mag. Nat. Hist.* (7), VIII., p. 403, Nov., 1901), after a spirited discussion with the German arachnologist, Professor Dahl, that definite types are a necessity in generic nomenclature, and that the method of elimination will not yield stability either in theory or in practice.

"Now elimination pure and simple in its practical application almost invariably lands us in an absurdity. In this way, the species which the authors withdraw are usually those that are best known, with characters salient and well described, leaving in those less well known, with this result, that the last species left in is one which is not known, is badly described, and never likely to be identified with any certainty; and this miserable phantom is left us as the type of the genus."

As a means by which this objection may be partially avoided it is proposed that when a generic name has had a specific type assigned for it the question should not be reopened, but it becomes at once apparent that the determination of the fact of such assignment would itself be a question on which differences of opinion might be entertained, so that Mr. Cambridge is brought to the further suggestion that such a designation be accepted only when the word *typ, typus* or *type* is used, and would rule out *exemplum, exemple* and *example,* also, presumably, *original, original species, chef de file* and other linguistic and verbal differences of expression of the same idea. On the other hand, no notice is taken of the complications possible through the fact that the word type

NOMENCLATURE APART FROM CLASSIFICATION.

Another of the many sources of confusion attending literary methods of dealing with systematic writings appears in connection with the citation of authors of generic names. After the abandonment of the practice of renaming each newly adjusted concept it became customary to refer generic names, not to their original authors, but to those who had made the last or most improved emendation of the definition. With such excellent opportunities some biological highwaymen did not hesitate to appropriate for themselves the entire nomenclature of their specialties, and evidently thought that by introducing changes in the generic descriptions they would establish claims to permanent recognition. Adherence to the idea that a genus is a group of species, and not merely a concept, and that the generic name is to be attached to a species rather than to a definition, affords an effective remedy for all difficulties arising from emendations, *pro parte* references and similar complications. The generic name when firmly anchored to a type species is no longer affected by vicissitudes of opinion among systematists, and in an important practical sense the problems of nomenclature are made to stand apart from those of classification and expression.

Groups recognized as genera by some authors will not be so treated by others, but genera, however constituted, will uniformly bear the oldest name which was first applied to any of their component spe-

has been largely used, even in systematic writings, in a phylogenetic rather than in a nomenclatorial sense.

The relief afforded by this amendment is moreover very slight, as shown in a subsequent paper which undertakes the actual work of 'A Revision of the Genera of the Araneæ or Spiders with reference to their Type Species' (*ibid.*, 7, ser. IX., p. 5, Jan., 1902), and the essential instability of the process of securing types through elimination and 'implication' is recognized and frankly admitted.

"Of course an author has a perfect right to include any species he likes, and must face the consequences if the last species left in his group by subsequent withdrawals turns out to be congeneric with the type of some earlier genus, whereby he loses his name as a synonym. The process * * * leads to great confusion, for it may afterward be urged * * * that the species removed was not congeneric with the earlier genus * * * By this renewed claim * * * the equilibrium is upset all along the line, and down come a score perhaps of generic ninepins whose stability depended upon the validity of this first step. It is not possible of course to avoid this tragedy of the ninepins so well known to and so justly feared by everyone who has endeavored to fix genera upon solid ground * * * there is always the possibility that it may turn out that the two species were after all not identical, and down come several ninepins, and the whole position has to be reconsidered. We have thus to recognize and face this possibility. What we want to do however is to avoid as much as possible any steps of elimination which might court such a catastrophe."

As an example the genus *Neriene* is cited, which would become a synonym of *Linyphia* if, as some think, the last species, *N. marginata*, is congeneric with the type of that genus. Those who hold this view would however maintain that *Neriene cornuta* should serve as the type and would thus unseat the name *Dicyphus*, in which alternations 'other subsequent genera will be involved, and so on to distraction.'

That this condition is not chronic among systematists who defend the method of elimination is due to the fact that they use it with 'discretion,' as an eminent zoologist once informed me, and do not attempt any general or constant application of it to such a task as Mr. Cambridge has undertaken. Those who prefer their ninepins under the guise of nomenclature have but to hold fast to the beautifully absurd rule quoted with approval by both Messrs. Dahl and Cambridge.

"The first publication in which a genus is subdivided, whether justifiably or unjustifiably, whether in a conscious or unconscious manner, must, where no typical form was named, decide what portion of the original genus is to retain the original name."

cies, and a generic name, whenever and wherever used, will have a fixed point of attachment to nature. Progress in the science of systematic biology must still compel endless modifications of the supposed limits of genera, but the method of types affords a complete and ideal solution of all the attendant difficulties which can be correctly assigned to the province of nomenclature. By the simple expedient of treating a generic name as inseparably attached to its original species as its nomenclatorial type, the whole maze of definitions, history, casuistry, confusion and contention is resolved into definite elements capable of rational and permanent adjustment. Of two or more generic names established on the same species only the oldest should be used, no matter how much the original definitions may have differed, while genera founded on species belonging to distinct natural groups will never be the same, no matter how closely the definitions may have approximated.

HOMONYMS.

The adjustment of the claims of competing generic propositions by reference to types rather than to concepts has many practical advantages. It becomes, for example, more obvious than before that generic synonyms are of several kinds, the nomenclatorial standings of which are very different. The first recognition of such distinctions is to be found in the so-called 'law of homonyms,' to the effect that the same name should be used only once in the plant or animal series. It has been held by some systematists that a homonym or second use of the same name might hold where the first had for any reason miscarried, but the impossibility of establishing the fate of any particular name under the method of concepts and elimination has rendered it obviously unwise to risk the confusion attendant on a resurrection of the supposedly defunct older genus, and the rule or principle 'once a synonym always a synonym' is receiving general recognition. And yet this aphorism is very misleading, since all synonyms are not homonyms, and the restoration of other kinds of synonyms is a very common occurrence. 'Once a homonym always a homonym' or 'once a homonym always a synonym' would be a much more correct statement, though in these forms the idea becomes a mere truism.

TYPONYMS.

Another class of synonyms hopelessly invalid from the beginning is the typonym, a generic name based on a species which has already been used as the type of a genus. Even in dealing with a genus containing but a single species variety in definitions has often led systematists to continue the multiplication of names. Thus although Rostafinski found that the names *Strongylium fuliginoides, Dermodium inquinans* and *Lachnobolus cribrosus* had all been used for a single species of Myxomycetes, which he treated as representing a monotypic genus, he again redefined the same genus and rechristened it with a fourth name. The only possibility of resurrection for a typonym is in the event of the previous name being found to be a homonym, as in the present instance where *Strongylium* was preoccupied for a lichen, so that the correct name for the Rostafinskian genus *Amaurochætæ* appears to be *Dermodium*.*

* The binomial *Dermodium atrum* (A. & S.) would have been used by Rostafinski if the principle of priority had been observed, in spite of the fact that *Dermodium* is usually treated as a synonym of the unrelated genus *Lycogala*. Neither can the name *Lachnobolus* be used in the sense in which Rostafinski and subsequent writers have employed it, since it was originally established as monotypic and included only *L. cribrosus* as above. Fries had already in 1849 applied the name *Nassula* to the species which

The present use of the word typonym, though somewhat different, does not necessarily conflict with that in which it has been employed by Dr. Gill* for names founded on types instead of on descriptions, since under the method of types, which requires that all genera be connected with species, this distinction regarding typonyms is no longer important. Although objecting to the naming of genera on types and without diagnoses Dr. Gill well says: "Certainly it is more rational to use a typonym than to require a definition for show rather than use." As a matter of fact the great majority of the older generic definitions are of little use or taxonomic value except for historical purposes, and it is a great practical advantage to be able to gain an idea of a genus from specimens, figures or detailed descriptions of a type species instead of being limited to the reconstruction of concepts based, too often, on slight knowledge and careless record. Moreover, as all systematists know, it is quite possible for many of their number to write long acounts of genera without betraying any facts of diagnostic importance, a point to receive further attention below.

Rostafinski treated under *Lachnobolus globosus* in 1875. More recently Lister has carried the confusion a step further by relegating *L. globosus* back to *Arcyria* while retaining the name *Lachnobolus* for still a third generic group represented by *L. cincinans* Fries, for which no correct generic name exists.

Lister is also in error in citing Fries, 1835 (Fl. Scand., 356), as the original reference for the genus *Lachnobolus*, which was published ten years before (Sys. Orb. Veg., 1: 148), with *L. cribrosus* as the only species. Lister's suggestion (Mon. Mycetozoa, 112) that *Lachnobolus cribrosus* Fries may have been a confluent form of *Stemonitis splendens* does not furnish a justification for the use of the generic name in a different family. The genus called *Lachnobolus* by Lister, which differs from *Arcyria* in having the sporangia sessile and the wall persistent, must be renamed, and may be called *Arcyodes*, the type being *A. incarnata* (*Licea incarnata* Albertini & Schweinitz, Consp. Fung., 109, 1805).

* *Proc. A. A. A. S.*, 45: 155, 1896.

METONYMS.

Synonyms of the third class, which may be called metonyms, differ from typonyms in not being based on the same types as the older names with which they are held to be synonymous, and unlike homonyms and typonyms, they may often be restored to active use, even after long periods of retirement. Improvement in the systematic treatment of many groups has been extremely slow, and even periods of reaction are sometimes encountered. Some biologists are as far ahead as others are behind the times, and there have been numerous instances where taxonomic work of high quality has remained unheeded for many decades, or until general progress had reached the plane where the genius of its author could be appreciated. Strangely enough, some botanists who hold to liberty of literary and historical interpretation and deprecate legislation in the interest of uniformity, have given their support to the rather barbarous proposition that systematic study which is not accepted by somebody inside of fifty years becomes outlawed. The desire to wipe away old scores of casuistry and confusion can be readily understood, but that to do this it should be thought necessary to place a premium upon reaction and ignorance has brought the ultra-literary botanists within easy range, it would seem, of an appreciation of the absurdity of their own position.

The complications for which the 'Berlin Rule' of a fifty-year limit gives partial relief are much more thoroughly obviated under the method of types, and that without discriminating against conspicuous ability and advanced ideas, and without requiring the discoverers of rare plants and animals to see that their genera are re-

published every half century in order to prevent the loss of copyright privileges and scientific honors to an ungracious posterity.

HYPONYMS.

A generic hyponym is a name not used because inadequately published—that is, not printed in connection with a recognized species. Again will the consistently literary botanist insist that the earlier writers studied and described their genera quite apart from species, and that it is an empirical and revolutionary proposition which would set aside tradition and usage and insist upon the arbitrary requirement of a generic type. This, however, is but an obvious corollary of the taxonomic principle that genera should not be studied and named as concepts, but as groups of species. Moreover, the regulation which it has been sought to enforce under the method of concepts, that a generic name must be accepted which was accompanied by anything whatever in the way of description, is equally arbitrary and has a fatal lack of practical utility, since most of the older descriptions are utterly inadequate for diagnosis under modern classification. That the generic descriptions had come to be recognized as a mere formality which could even be entirely dispensed with, was well shown, quite apart from the method of types, by the selection of the 'Species Plantarum' of Linnæus as the initial work of reference for botanical nomenclature. This book contains no descriptions of genera, but it was very properly held that the genera could be much more satisfactorily inferred from the species than from the descriptions given in Linnæus' 'Genera Plantarum.'

Some naturalists who have appreciated the hollowness of the idea that a mere series of words must be taken as establishing a generic name in full nomenclatorial standing, are inclined to insist that genera must really be described so as to in some measure approximate modern ideas, even though this would require the abandonment of many well-known names of the large composite genera of the older authors. However logical this procedure may be, the general application of it could only result in increased confusion, since there is not the smallest probability of agreement among naturalists as to *how much* of a description is necessary to the diagnosis of any particular genus or other natural group.

The formal requirement of a description for a species has a far more logical justification. An identifiable species locates at once one point in a genus, but subsequent students may have no clue to an uncharacterized species. It is thus a matter of expediency as well as of right to reject specific names not accompanied by descriptions, though such a practice will lead to confusion unless it be applied only to actual *nomina nuda;* far too many changes and disagreements would appear if the question of the adequacy of specific descriptions were to be raised. Practical legislation must, of necessity, converge upon technical points, and the utility of any enactment depends upon reducing the number and making plain the location of these foci. Biologically a genus is generally a group of species, but nomenclatorially it may always be narrowed to a single species, and under a binomial system to a single binomial species, which must have nomenclatorial status before it can be made the basis of a nomenclatorially valid generic name.

The limitation of taxonomic recognition to generic names established in connection with identifiable binomial species would be a most useful regulation since it would dismiss to final oblivion a large number of still-born names which for a century or more were passed over by botanists, but

which the injudicious zeal of recent reformers has resurrected and attempted to galvanize to the life of modern taxonomy. The Rochester Rules were avowedly drawn to enact a law of priority under the binomial system of nomenclature, although, to give a definite point of departure, it was agreed to disregard the hundreds of binomials published before 1753. Every principle of logic and every practical consideration would have led us to expect the acceptance of the obvious corollary of that proposition—the rejection of the non-binomial literature published after that date. This simple distinction having been neglected, we have only the no longer logical but purely arbitrary 1753 rule to keep us from the older polynomial literature, to say nothing of the many pre-Linnæan books in which binomials were used. The process of restoring Adanson's names is only just begun in dealing with the botany of North America. There are pages and pages of the closely printed lists of the 'Familles' as yet not drawn upon by our antiquarian friends, but the zest with which they have delved in this débris only shows what would be their delight in first-class cemeteries like Micheli and Tournefort, if indeed they would remain content with these and not insist on pushing back to a more obscure antiquity. Seriously, however, the reinstatement of these Adansonian and similarly unattached and long-forgotten names is an utterly needless imposition contrary to the spirit in which the reform attempted at Rochester and Springfield was encouraged and supported by the botanical public.

CACONYMS.

This necessity of some provision for the more definite limitation of taxonomic literature on the sides of Latinity, brevity and binomiality can be made even more obvious by reference to a neglected contribution to the botany of Mexico, the work of Francisco Hernandez: 'De Historia Plantarum Novæ Hispaniæ.' This was published in Madrid in 1790 from manuscripts written in the sixteenth century. It records names for toward a thousand genera of Mexican plants and antedates a large part of the current systematic botany of that and the neighboring regions. The three quarto volumes contain a total of 1,611 pages, and are written in Latin throughout, with the adoption of the Aztec names which stand either alone or in the form of binomials. Thus the first chapter is headed: 'De Apitzalpatli crenata, seu de herba secta per ambitum fluxum alvi cohibente.' Then follow 'Apitzalpatli altera, Apitzalpatli Uyauhtepecensi, Apitzalpatli Tehoitztlac, Apitzalpatli Teuhaltzincensi,' and others equally unmanageable by the tongues of European peoples, though differing only in degree from Thlaspi, Gajati, Alhagi, Tsubaki, Tsjinkin, Hombak, Sinapi, Gansblum, Konig, Korosvel, Canschi, Malagu, Coddampulli, Mangostan, Japarandi, Celeri, Chocho, Mokos, Agialid, Tsususi, and hundreds of others which have not prevented the recent resurrection of the taxonomy of Adanson's 'Familles,' in spite of the fact that it had been almost universally ignored for upward of a century. An objection might be taken to the specific names of Hernandez because his work is not consistently bionomial, but the fact that he so frequently uses names of that form would seem to give his generic designations a better claim to recognition than those of the strictly monomial Adanson, or those of the numerous polynomial post-Linnæan writers like Haller. But however effective such reasoning might be if Apitzalpatli stood alone or with a few similar terms, the fiercest Adansonians may well quail before what Hernandez was able to transcribe after he had acquired more fluency in Aztec: Tlalaxixquilitl, Tlalte-

comaxochitl, Tlalacxouatl, Atonahuizpatli, Tlatlauhquichicomacatl, Yztacchacomacatl, Copalquahuitlpatlahoac, Tlahoelilocaquahuitl, Tzinacancuitlaquahuitl, Yztacpatlichichipiltic and Chichictlapalezquahuitl. Even Hernandez seems to have had a suspicion that some of these names were too long to meet with general popularity outside of Mexico, for in several instances he suggested more manageable abbreviations. Thus chapters are not infrequently headed like the following: 'De Chichictzompotonic, seu Tzompotonic amara'; 'De Cozticcoanenepilli, seu Coanenepilli lutea'; 'De Yztacquahxiotl, seu Quauhxiotl alba'; 'De Tecopalquahuitl, seu Copalli montana.'

But the second generic names, though shorter, are no more Latin than the first, and the practice of determining priority by position would prevent their being taken up in preference to the preceding unmodified designations.

From the standpoint of some taxonomists the forms of names appear of merely incidental importance, and the tendency of recent years has been toward the acceptance of the oldest designation, no matter how inappropriate, incorrect, barbarous or foolishly long it might be. Hybrids formed by the compounding of Greek with Latin roots, though a frequent cause of protest from biologists of classical training and sensibility, are really among the lesser difficulties, and a partial defense of them is to be found in the fact that, although the language of systematic biology is Latin, it has continued and extended the custom of the Romans in drawing freely from the richer and more convenient Greek vocabulary available for the formation of scientific terms. But there are practical as well as merely literary difficulties in connection with unreasonable names, and while some of these can be excluded on other grounds than those of form there will remain a not unimportant residue of the results of past, present and doubtless future ignorance and lawlessness, which it seems unnecessary to inflict as a permanent legacy to scientific posterity.

Whether in using hybrid and barbarous names we are following in the lines which Latin literature would have taken is, after all, of relatively little importance. Convenient names which can be understood readily and remembered easily are the object of our quest. Names like *Sebastianoschaueria* and *Reichembachanthus* may be etymologically correct, but they are certainly not convenient, and the same may be said of many impersonal compounds of ungainly length, such as *Archispirostreptus, Necrophlœophagus* and *Synthiloborhamphus*. Apparently to avoid the labor of finding an unused short name, some systematists seek safety in huge polysyllables which they feel sure that none of their predecessors can have had the hardihood to perpetrate. But that these absurd creations are strung out in accordance with the rules of Greek grammar is scarcely a sufficient reason why systematic biologists must remain at the mercy of nomenclatorial indolence and folly. The man who named his daughter Encyclopedia Britannica was rewarded for his pains by hearing the neighbors call her 'Tan,' and similar abbreviations are in many instances in order among scientific names.

To avoid the numerous complications and uncertainties attending the subject of caconyms it has been suggested that names be treated as arbitrary symbols outside language and literature, to be preserved in their original forms, typographic errors and all. For such the names of Adanson and Hernandez are but opportunities for the display of zeal in the cause of priority. Indeed, one phonetic outrage at a time is evidently not enough for those who think to serve science by compelling us to say

such things as *Symphoricarpus symphoricarpus, Taraxacum taraxacum, Hypopitis hypopitis, Opuntia opuntia, Zizyphus zizyphus, Cracca cracca, Sassafras sassafras, Benzoin benzoin* and other gibberish first advocated by the ornithologists who evidently proceeded on the analogy of *Coo coo, Caw caw, Quack quack* and other sounds familiar to them, and did not foresee the certain fate of the tropical planter who should find as the result of his botanical studies that his garden contained *Cajan cajan, Manihot manihot, Malvaviscus malvaviscus, Jambosa jambos, Ananas ananas, Karatas karatas, Guazuma guazuma, Lebbek lebbek* and *Lablab lablab,* to say nothing of the horrors he might encounter in the forest. And all this because Dr. Linnæus refused to accept numerous genera named by his predecessors, but used their generic names of species!

Those who believe that this historical complication compels the permanent use of duplicate binomials should begin practice with Chichictlapalezquahuitl, since the Juggernaut sect of devotees to priority has not hesitated to resurrect and even to make duplicates of equally barbarous, if less extensive, names from books much less scientific than Hernandez. If, as claimed by Mr. Pollard,* there is no middle ground between the correction of orthographic and typographic errors and the acceptance of all mistakes and barbarisms, a continuation of the present nomenclatorial tendencies will but prepare a welcome for the reformers who shall extend and complete the work of Professor Greene in the extirpation of incorrect, inconvenient and barbarous names, and the substitution of others justified by classical reference and usage; not primarily because such terms are Latin, nor because they are classical, but because it will have become apparent that adherence to a reasonably limited, never-changing vocabulary is the only safe basis for legislation in the interest of a convenient and stable nomenclature.

The hope that stability might be secured by the acceptance of incorrect, inconvenient, barbarous and nonsensical names is obviously vain, and it is rapidly becoming apparent that such concessions to ignorance, recklessness and freakishness carry with them more serious dangers than they avoid.* We could afford to have many differences of opinion and usage in the names of plants rather than accept taxonomic contributions like those of Hernandez and Adanson, and a stability which would bind us to such idols would be a doubtful blessing.

But notwithstanding its annoying complexity, the subject of caconyms has the redeeming feature that it can be treated quite apart from all other aspects of nomenclatorial reform, and as it is the side which touches nearest upon the field of general literature and individual opinion and taste, it is here that reliance upon usage or an agreement to disagree would be a benefit to systematic biology if it made possible the much-needed unanimity on the

* SCIENCE, N. S., XIV., p. 280, Aug. 23, 1901.

* The somewhat pharisaical complacency with which some of my zoological friends were inclined to view the Hernandez complication as a purely botanical difficulty is no longer appropriate in view of the recent delivery by a South American zoologist of a large brood of nomenclatorial monsters which, since they have come in the twentieth century, instead of in the sixteenth, show even more strikingly than those of Hernandez the necessity of nomenclatorial discrimination. Two protests have already appeared (*Osprey*, V., p. 142, Sept., 1901, by Professor Gill, and SCIENCE, N. S., XIV., p. 693, Nov. 1, 1901, by 'F. A. B.') but the authors of names like *Guilielmoscottia, Oldfieldthomasia, Edvardotrouessartia* and *Asmithwoodwardia,* are, of course, impervious to reason or to ridicule, and will be effectually deterred only by the refusal of systematists to recognize their multipedalian progeny as a legitimate part of biological taxonomy.

weightier principles and methods of taxonomic procedure. General legislation, to cover all normal instances, must be axiomatically rational, definite and simple if it is to be universally understood and approved, but there could be no serious objection to the reference of this semi-literary department of nomenclature to a permanent committee or academy, just as it has been found advantageous to have a board of specialists for officially determining the forms of geographic names. And it should also not be forgotten that if no direct provision for dealing with caconyms should prove possible, a large measure of relief from Adanson, Hernandez and other nomenclatorial incubi could still be obtained through closer adherence to the binomial requirement for genera as well as for species.

ESSENTIALS OF BIOLOGICAL NOMENCLATURE.

In the way of summary of the present and former discussions of the method of types* it may be repeated that the long-wished-for uniformity and stability could be secured by consistent adherence to a few simple and well-nigh axiomatic principles.

1. The primary object of formal nomenclature in systematic biology is to secure convenience, uniformity, and stability in the names of plants and animals.

2. Biological nomenclature should be treated as beginning with the general use of binomial Latin names for plants and animals.

3. A name must be used for the natural group to which it was first applied.

Moreover, if we begin from the practical end of the problem instead of viewing it merely from the literary standpoint, the formulation and application of these principles encounters far less serious complications than have attended the unstable method of elimination.

DESIGNATION OF TYPES.

1. The nomenclatorial type of a species is the specimen originally studied, named and described by the author of the specific name.

2. The type of a genus is the first species referred to it, and the generic name can be used only for species treated as congeneric with the type.

(*a*) The author may designate, however, some other species as type in the same paper in which the name is published.

(*b*) For a generic name adopted from a pre-Linnæan or a prebinomial writer the type species is selected without reference to the binomial system of nomenclature, but works older than Tournefort's 'Institutiones' (1700) should not be cited in botany.

CLASSIFICATION OF SYNONYMS.

Under the method of types names are rejected or treated as synonyms in biological taxonomy for the following reasons:

1. When preoccupied (homonyms).

(*a*) A generic name is preoccupied when it has been previously proposed for a different group of the same (plant or animal) series.

(*b*) A specific or subspecific name is preoccupied when it has been applied to a species or subspecies under the same generic name.

2. When there is an older valid name based on the same type (typonym).

3. When there is an older valid name based on another member of the same group (metonym).

4. When the natural group to which the name applies is undetermined (hyponym).

(*a*) A specific name is a hyponym when it has not been connected with a description identifiable by diagnostic characters or by reference to a type specimen, figure or locality.

(*b*) A generic name is a hyponym when it has not been associated with an identifiable binomial species.

5. When the form or signification of the name is inconvenient, incorrect or inappropriate (caconym), should a recognized method of dealing with these complications be formulated.

O. F. COOK.

WASHINGTON, D. C.,
February 10, 1902.

* *Bulletin of the Torrey Botanical Club*, 22: 431–434, October, 1895; SCIENCE, N. S., 8: 186–190, August 12, 1898; SCIENCE, N. S., 8: 513–516, October 14, 1898; *American Naturalist*, 33: 287–297, April, 1899; SCIENCE, N. S., 12: 475–481, September 28, 1900; SCIENCE, N. S., 13: 712–713, May 3, 1901.