clearly made in the first report of the House Judiciary Committee in 1836, when it said: "The sum given to the United States by Mr. Smithson's will is no wise and never can become part of their revenue. They can not claim or take it for their own benefit. They can only take it as trustees to apply to the charitable purpose for which it was intended by the donor."

It is because the institution still administers for the government seven of the public bureaus which it created that many people suppose this private research establishment to be a part of the government. The importance of keeping the Smithsonian—in so far as it is an institution for the "increase and diffusion of knowledge"—a private organization, was early brought out by Joseph Henry. He said: "That the institution is not a national establishment, in the sense in which institutions dependent on the government for support are so, must be evident when it is recollected that the money was not absolutely given to the United States, but intrusted to it for a special object, namely, the establishment of an institution for the benefit of men, to bear the name of the donor, and, consequently, to reflect upon his memory the honor of all the good which may be accomplished by means of the bequest. The operations of the Smithsonian Institution ought, therefore, to be mingled as little as possible with those of the government, and its funds should be applied exclusively and faithfully to the increase and diffusion of knowledge among men." That this opinion is a sound one, gentlemen, we believe the Smithsonian's achievements prove. It is obvious that the freedom from political exigencies which has permitted the institution to play so great a part is due primarily to the private nature of its funds.

Gentlemen, there seems something fateful in the timeliness of James Smithson's bequest to the United States. It came to meet an unexampled opportunity. Here in 1846 was a vast untouched continent, enclosing, in a single geographical and political unit, a prolific plant and animal life ready under the most favorable conditions to reveal their secrets to botanists and zoologists; a continent peopled by a primitive race, illustrating the mode of life and habits of thought of prehistoric man, and offering a useful key to the lost story of man's climb upward. At the same time, in the hands of an energetic people were the mechanical tools—particularly steam transportation—capable of developing this new continent. Such a setting and such men to deal with it offered possibilities for the increase of knowledge such as perhaps the world had never seen before. The danger was that the men would remain blind to those possibilities and waste the setting for practical ends before those of its secrets which were perishable should be gleaned. It was a crucial moment in the history of knowledge. What was needed was some powerful inspiring force, actuated by the highest ideal of knowledge for its own sake, which would be conscious of the possibilities and which would devote its energies to making the most of them. That force the liberality of an Englishman helped to supply, and the self-sacrificing idealism of American men of science—Joseph Henry and his associates—directed. The debt of America and of science to the Smithsonian Institution is great.

Joseph Henry had the vision to understand clearly what Smithson meant his foundation to be, and the energy and character to make it that. The Smithsonian has now come to a time when without the support of the nation, it can no longer continue to be what Henry made it. And yet the need for just such an institution as it has been is no less than the need was eighty years ago. In some respects the unique opportunities are even greater. This institution is not the product of a moment; eighty years of the toil of great men have gone into its making. There is that about it which can not be replaced.

The regents have felt it their duty to reveal to a leading group of representative American citizens what it is, and does, and to advise with them what its future shall be. For that reason they have invited you here. They wish you to see the broad and comprehensive scope of the institution, competing or interfering with nobody, cooperating with all, reaching the basic problems of mankind and of the time, with a view to furnishing the information through which alone they can be solved. They wish you to see what the future possibilities of the institution are, and if you think them worthy of realization, to advise us as to how we may go about achieving it.

Around this hall are arranged exhibits of the researches and publications of the Smithsonian, with especial emphasis on how they should and could most profitably be extended. The scientists in charge are at hand to answer your questions. May we invite your careful attention to them?

WILLIAM HOWARD TAFT

CHANCELLOR OF THE SMITHSONIAN INSTITUTION

UNDERLYING FACTORS IN THE CONFUSION IN ZOOLOGICAL NOMENCLATURE WITH A DEFINITE PRACTICAL SUGGESTION FOR THE FUTURE[1]

SERVICE of thirty-one years as member (twenty-nine of these as secretary) of the International Commis-

[1] Address of the retiring president of the American Society of Parasitologists, Philadelphia, December 29, 1926.

sion on Zoological Nomenclature has given me opportunity to study the principles of and practices in the subject rather carefully. On this basis I invite attention to certain practical aspects of the problems which should, I am persuaded, be of interest.

Raphael Blanchard once defined nomenclature as "the grammar of science"; this is the best definition of it that has come to my attention. In one of my less serious moods I once described zoological nomenclature as "that portion of zoology which puts us to sleep in the day time and keeps us awake at night; the combined nightmare, bugbear, bête noir and Katzenjammer of zoologists"; I believe you will agree with me that this is a fairly accurate generic diagnosis.

Individual zoologists have a nomenclatorial vocabulary which varies greatly in extent. Some have estimated their systematic vocabulary at about two hundred names, others at about five hundred or six hundred, still others at about one thousand to five thousand. These individual estimates are very low as compared with the estimates of the vocabulary of the entire profession, which runs into hundreds of thousands, and as fundamental in any consideration of the problems, it appears reasonable to hold in view the important principle that it is the vocabulary of the profession—not of the individual—which should govern our principles and practices. If you or I base our nomenclature solely on the names of the parasites with which we deal and overlook the importance of the names of the hosts which harbor these parasites, we soon reach a status of theoretical and practical confusion.

For instance, if, under existing conditions, one of us reports for "*Simia* species" an infection transmissible to man and of possible importance to the health and life of human beings, the rest of us do not know whether the systematic conceptions of mammalogy revert to an early status or, if they are more modern as is to be assumed, which of three genera of *Primates* we must consider in our efforts to protect human life. If one of us reports for "*Cercopithecus* species" an infection of public health importance, we do not know whether reference is made to a disease in Africa or in South America.

The first point I wish to make is that in citing our hosts it is important to cite them as definitely and as correctly as possible, otherwise our records are ambiguous both theoretically and practically; and if we govern our vocabulary purely from our subjective standpoint as applied to the parasites, instead of from the broader objective consideration as applied to the entire zoological profession, we follow a policy which is difficult of justification.

The second point I would emphasize is the tremendous economic loss, in cost of time, effort, study, paper and printer's ink, which results from the subjective rather than the objective use of a technical vocabulary. A few prominent examples may be of interest.

The economic loss to science attributable to the easily preventable confusion in the nomenclature of the protozoa (*Plasmodiidae*) involved in the malarias of man and of birds runs into the thousands of dollars, a sum of money which could have been used to much greater advantage.

The economic loss in protozoology due to easily preventable confusion in the nomenclature of the parasite (*Endamoeba histolytica*) of amoebic dysentery is probably not less than that involved in the nomenclature of the malarias.

The literature involved in the easily preventable confusion in the generic nomenclature of the common bedbug, *Cimex lectularius,* is so extensive that at a conservative estimate it required a total of sixty days' intensive study by several zoologists to work up the case for the International Commission. As one commissioner expressed it, the time spent on this case would have been sufficient to convict three murderers.

The money loss represented in time, paper and ink, involved in the controversy on Huebner's (1806) Tentamen, would supply some museum with a fairly representative collection of butterflies.

The ultimate easily preventable economic loss in mammalogy involved in the complex, interrelated cases of the names of the chimpanzee, the orang-utan and the Barbary ape would represent a very substantial contribution to the support of a college department of zoology for one or two years.

This type of preventable economic loss, in a field of science dependent upon endowment, public appropriations and private funds, is a practical question which we owe to our profession to consider seriously if we maintain that we are rational and practical human beings as well as students of science.

If the zoological profession continues to permit so much of its funds to be wasted, how can we expect practical business men and legislators to continue their confidence in the ability of the profession to administer trust funds? Had you ever thought of nomenclature from this point of view?

Clearly it behooves us to consider seriously the factors which have resulted in these and similar losses and to inhibit them in the future.

The most important factors involved are relatively few, namely, *four* involving principles and *one* involving practice, and can be easily formulated. Permit me to enumerate these five factors as I see them.

I. *Genotypes:* So far as my thirty-one years' intensive study of nomenclature can be taken as basis for conclusions, I am persuaded that a failure on the part of authors clearly to designate, at time of original publication, the type species of their new genera, together with a failure of later authors to follow the principle of early designation of genotypes, is the greatest single formal source of easily preventable confusion in the entire field of zoological nomenclature.

An author who proposes a new generic name without definite designation of the type species can justly, though kindly, be compared with a naval architect who builds a ship but provides no rudder with which to steer it; the genus becomes a nomenclatorial derelict in the sea of zoological literature, constantly colliding with other genera, usually of similar rudderless construction.

The addition of two words to the original generic description, namely, "type *x - us*," and the observance of well-established rules of navigation would remove this very important factor in confusion. How superlatively easy this would be.

There seems to be an impression that the principle of genotypes is a conception of relatively modern nomenclature. Permit me to invite attention to the fact that Linnaeus in 1751, when he proposed the binary-binomial system of nomenclature, formulated the rules on which the system was based and that he laid the foundation of the principle of genotypes in paragraph 246.[2]

As our science advanced, Latreille in 1804–1810 definitely designated many genotypes in arthropods. Latreille's policy was followed by a number of authors (Curtis, Westwood, etc.), but numerous other authors overlooked or ignored these important contributions to this practical technique. Various sets of rules enlarged the technique, and finally the International Commission studied the subject in detail and digested and formulated (in Article 30) the methods of procedure. Contrary to the view entertained by some of our colleagues, the Boston (1907) proposition by the commission was not a new idea made retroactive, but a codification and harmonization of principles and practices already adopted by various authors.

II. *The Law of Priority:* Linnaeus in his original rules of 1751 clearly enunciated the law of priority in paragraph 242.[3] The impression that this law of priority is a relatively modern conception and that its retroactive application has caused the existing confusion is based on erroneous premises. The confusion, as respects this law, is due to the fact that so many authors adopted the Linnaean system of nomenclature (namely, biological grammar), without applying the rules on which the system was based.

The law of priority has produced so much controversy that you will doubtless permit me to present the conception of it which my experience has given to me. As I see it, theoretically and practically, we face two alternatives: First, let every zoologist adopt any technical name he wishes; or second, let us all agree to follow the Linnaean law of priority of 1751.

The first alternative is subjective and makes for confusion; the second is objective and makes for uniformity in all objective cases; but of course it does not give finality to cases of subjective conceptions of taxonomy, for these conceptions are subject to revision on basis of additional data or additional subjective interpretations.

Those of us who elect to follow the objective alternative of priority, sinking, for the general good of science, our subjective preferences, certainly have a right to conclude that any of our colleagues, who elect to follow the subjective alternative and to use any names they prefer, would find it difficult to justify themselves in criticizing those of us who prefer the second alternative; for they are hardly in a position to deny to us a right of choice in governing our policy when they reserve to themselves the right of choice in governing their procedure.

Consistent with my support of the law of priority, I recognize the fact that we should make the rules our servants rather than make ourselves the slaves of rules. Thus I approve of giving to the International Commission, as the Monaco Congress did in 1913, plenary power to suspend the law of priority as well as the other rules "when in the judgment of the Commission the application of the rules will produce greater confusion than uniformity." We have practical as well as academic problems to solve; we face conditions in addition to theories.

In general I would evaluate a failure to apply the law of priority as the second most important formal factor in nomenclatorial confusion.

III. *Homonyms:* As third formal factor in easily preventable confusion I am inclined to cite homonyms. An author publishes as new a technical name without consulting the various nomenclators (Agassiz, Marschall, Scudder, Waterhouse, Sherborn, etc.) to see whether it has already been used by some other author for some other systematic unit in such a way as to invalidate its proposition for the new systematic unit. There are literally hundreds upon hundreds of cases of this kind. Self-understood, they result in confusion. For instance, suppose a reference is made simply to *"Hamadryas"*; does it concern a lepidopteron, a mollusk, a reptile or a primate?

[2] Paragraph 246: "Si genus receptum, secundum jus naturae & artis, in plura dirimi debet, tum nomen antea commune manebit vulgatissimae & officinali plantae."

[3] Paragraph 242: "Nomen genericum antiquum antiquo generi convenit."

As an instance in point: One of my colleagues recently had occasion to consider five generic names which he wished to publish as new; as his library did not contain the standard nomenclators, he sent the names to Washington and asked whether they were available. Half an hour's examination of the nomenclators showed that three of the five names were preoccupied.

Surely every author who publishes a name as new owes it to the profession to inform himself whether the name is unavailable (because preoccupied), or available. This procedure takes perhaps five to sixty minutes, but it is well worth while, for it prevents later confusion and changes in names.

If any of you desire to endow a practical economic proposition in zoology, costing about $1,800 or $2,000 per year, you might provide that salary for the appointment of a person at some museum with extensive library facilities, to whom prospective new generic names can be confidentially submitted for consideration as to availability in order to prevent the publication of additional homonyms.

IV. *Synonyms:* Synonyms are often viewed as due to carelessness of authors, and not infrequently writers are subjected to severe criticism on this account. Permit me to analyze the subject briefly. Synonyms are either (A) objective or (B) subjective.

(A) Objective (*i.e.,* absolute) synonyms are due to three causes. Many objective synonyms are (α) the direct and inevitable result of our advance in anatomical knowledge which leads to a division of taxonymic units. Others are due to (β) deliberate, necessary and justified, or (γ) unnecessary and unjustified renaming of systematic units.

(α). As examples of objective synonyms due to advance in anatomical knowledge may be cited three changes of the name *Taenia lata* Linn., 1758, to *Bothriocephalus latus* 1819, *Dibothriocephalus latus* 1899 and the necessary change on basis of the law of priority to *Diphyllobothrium latum* 1910. (γ). The eight changes of the name of this species (or of one of its subjective synonyms) to *Taenia acephala* 1772, *T. capitata* 1772, *T. grysea* 1766, *T. hominis* 1782, *T. inermis* 1803, and *T. membranacea* 1782, and to *Halysis lata* 1803, and *Dibothrium latum* 1850, were unnecessary and unwarranted. All eight of these names are objective synonyms of earlier names.

(B) Subjective synonyms are on a totally different basis. For instance, with *Diphyllobothrium latum* are now synonymized, more or less definitely, at least:

(δ) six subjective synonyms (*balticus* 1866, *cristatus* 1873, *dorpatensis* 1886, *latissimus* 1886, *tenella* 1781, and *vulgaris* 1758) proposed for species which were considered distinct from *lata*. The name *vulgaris* 1758 has several unnecessary objective synonyms already cited under (γ). While the eight objective synonyms cited under (γ) can not be justified by any code of nomenclature published from 1753 to 1926, opinion may differ in regard to the point whether the authors were justified in publishing the six subjective synonyms cited under (δ). My viewpoint is that in case of *reasonable* doubt as to the systematic status of an animal it is always wise to publish it under a new name. This view, which will doubtless be considered unusual or even extreme by many authors, is based on the following premises:

(1) Views as to generic and specific values are exceedingly subjective. They vary from generation to generation of authors and even from year to year by one and the same author. You may recognize a genus or species as new, but I may not agree with you; time may prove that your view is correct.

(2) If time shows that an allegedly new genus or new species is identical with an earlier genus or species, no special harm is done, for a name is easily sunken in synonymy.

(3) If, however, life cycles or anatomical details of two distinct species become confused in literature, the potentialities for prolonged confusion are obvious, for it is later difficult to unscramble the confused data, which perhaps have already found their way into text-books.

(4) One of the most perplexing, most confusing and most controversial problems in nomenclature is presented by generic names based upon a misdetermined species.

Admitting that there are arguments on both sides of the question, experience persuades me that the conservative policy is to name as new species or subspecies all specimens in regard to which there is a *reasonable* doubt as to their identity with an already described species *and for which this doubt can be expressed in a differential key.* The sarcastic references often made to so-called "splitters" in taxonomy are not based upon a judicial consideration of the confusion, nomenclatorial and other, occasionally produced by authors often nicknamed "lumpers." A middle ground between the "splitters" and the "lumpers" is to recognize subgenera and subspecies in case of doubt and during transitional periods of the classification of a given group. This is clearly brought out in well-known cases like *Amoeba, Endamoeba, Culex, Anopheles, Papio,* etc.

This last deduction leads to a few words regarding subgenera and subspecies. Authors who argue that these taxonomic units are not justified theoretically, in the binominal system, are in error in their premises. As for the practical consideration, on the other hand, do we not take ourselves too seriously in our differences of view as to the limits of a genus or a species? Even our definitions and conceptions of a genus may vary widely; hence they are subjective. A genus (as I conceive it) is a taxonomic complex of specimens grouped for the moment (according to our subjective and never absolutely perfect knowledge) around a genotype. The point that you and I may not group the same species or the same specimens around that genotype is not to be taken much more seriously, or much more capable of final objective decision between us, than is the fact why you prefer one and I prefer two lumps of sugar in a cup of coffee. If, however, we both work on the genotype basis, we inhibit confusion; and this confusion is reduced if we agree not to consider our own views as final, especially during the evolutionary and transitional stage of a new classification. This confusion is reduced still further if, instead of insisting on establishing or on rejecting full generic or specific status for a subjectively constructed group, it be conceded that both sides have grounds for the subjective conceptions, and then if we compromise by reducing the contested genus (or species) to a subgenus (or subspecies) until time proves which of us has the better grounds for his subjective views. How much more understandable the present literature on the *Culicidae* would be if this course had been followed!

Permit another illustration in point: One of the most bitter polemics ever published in zoological literature was based on the question whether certain specimens, let us call them genus *X*, species *tweedle-dee,* were conspecific with or distinct from *X. tweedle-dum.* Pages of printer's ink were issued over this point, which to the average zoologist did not amount to a proverbial "hill of beans," despite the fact that from the clinical and public health viewpoints an important problem was present. The author who claimed that *tweedle-dee* and *tweedle-dum* were specifically identical was one of my best personal friends, and he seriously assured me that he never published a statement unless he was absolutely certain of his point and that, therefore, I could safely accept his view as final. Time has proved, however, that *X. tweedle-dee* is anatomically distinct from *X. tweedle-dum,* in harmony with the clinical, biological and geographical findings. Had my good friend adopted two subspecies, namely, *X. tweedle-dum tweedle-dee* and *X. tweedle-dum tweedle-dum,* considerable time, paper, personal feeling and printer's ink would have been saved. The moral is: In case of *reasonable* doubt, recognize distinct genera and species or compromise by adopting provisionally distinct subgenera and subspecies.

The title of my paper promises you a "definite practical suggestion for the future." This promise involves the important factor of practice, as distinguished from the four factors of principles to which reference was made. The suggestion is so elementary, so easy of application, so superlatively practical that you may be surprised—possibly amused—at its enunciation. I wanted to describe it as "a common sense proposition"; but recognizing that "common sense" is subjective I will not classify it in this manner. It is based on a consideration of the question: What is the chief cause back of and underlying confusion in zoological nomenclature? Or, what is the one ultimate element which explains the chaos of words with which we have to deal? Or, what is the great explanation, *par excellence,* of the origin of the dreary, long-winded, uninteresting, somnific (rather than anesthetic), occasionally paraneurotic, nomenclatorial discussions which put us to sleep in the daytime and keep us awake at night? In technical phraseology, what is the etiology of the *average* nomenclatorial neurosis? (Note, please, I did not say psychosis.)

My answer is that it has been, from 1758 to 1926, the exception—not the rule—that pupils who study zoology have been taught the grammar of the technical language they are called upon to hear, read, write and speak.

Pardon, please, a personal experience: I studied zoology and botany in five well-known universities and zoological stations, in four countries, under nineteen well-known teachers. Then I returned home (proud of my degrees with zoology as "major") and became chief of a government division of zoology. A few weeks after taking oath of office, I was in conference with that inspiring and charming mind, Dr. C. Hart Merriam. Dr. Merriam happened to refer to the Linnaean rules of 1751, to the B. A. Rules, the Dall Rules and to the A. O. U. Code. I had no idea what he was talking about, but I refrained from differing with him in his deductions. Without exposing my ignorance, I visited the library to obtain these "things"—whatever-they-were-or-might-be. Then for the first time, after studying zoology six years, I learned to my surprise—what not one of my text-books and not one of nineteen teachers in biology had ever taught me—that the science in which I was specializing had rules of grammar, namely, rules of nomenclature, to govern the technical language I was hearing, reading, speaking and writing. If my dear friend, Dr. Merriam, ever hears of this confession, it will be the first intimation he has ever had that in the last analysis he is personally responsible for the rôle I have been playing for so many years as *Capra hircus* (synonym secretary) of the International Commission.

With my own personal experience in mind, I have quietly inquired of my zoological colleagues whether in their college courses they had been taught the principles and practices of nomenclature. The results of this inquiry have been exceedingly interesting. A few of the younger generation have stated that as students they had had instruction in the subject or at least were told of the existence of the rules. But quite generally the reply has been that in their college and university courses both the older and the younger generations had never heard of the subject during their student days. If deductions be based on the general literature of zoology, from 1758 to 1926, the conclusion can not be escaped that a majority of the authors have been blissfully innocent of the rules of zoological grammar and that, therefore, it is not to them but to their instructors that we owe our present residual confusion in nomenclature.

The practical question arises: How much grammar should be taught to pupils?

On the hypothesis that the teacher understands his subject, I would give the following general estimates:

(1) A candidate for the degree of bachelor, with any field of biology as "minor," can be taught in one hour all the theory of nomenclature he is likely to need, namely, the existence of rules and of nomenclators, the principles of family, subfamily, generic, and specific names, and the reciprocal relations of botanical and zoological nomenclature. See Articles (of the International Rules[4]) nos. 1, 2, 3, 4, 8, 13, 14, 17, 19, 22, 26, 32.

(2) Premedical students and candidates for the degree of bachelor, with any field of biology as "major," should have one additional hour instruction in the principles of nomenclature to meet their needs; especially, the various nomenclators, the restricted circumstances under which certain names are to be changed, the rules of synonyms and homonyms and the law of priority. See Articles, 5, 6, 7, 9, 10, 11, 12, 18, 20, 21, 23, 24, 25, 27, 28, 29, 31, 34, 35 and 36.

(3) A candidate for the degree of master, with any field of biology as "major," requires still another hour instruction for his nomenclature, including the principles of genotype selection. *Cf.* Articles 15, 16 and 30.

(4) A candidate for the degree of doctor, with any field of biology as "major," requires three hours additional (total four hours) theoretical instruction for his nomenclature, including a study of "cases," as for instance, the "Opinions" issued by the commission.

This short course of instruction will give to students a theoretical background which will enable them to "play the game fairly" with the profession, to walk in the straight and narrow path, and to avoid rather than create additional confusion. But if they wish to unscramble the scrambled nomenclatorial eggs, practice and experience are just as necessary in nomenclature as in music, art, baseball, football, golf, bridge or (if you prefer) poker.

Picture, if you will, a chemist who would endeavor to write a chemical thesis without understanding those delightfully lucid and highly exciting hieroglyphics known as formulae, with which papers in chemistry are adorned (in place of the classical, learned, and awe-inspiring Latin names in zoological literature!). I hope the comparison is clear.

In *conclusion,* unless and until the principles and practices of zoological nomenclature (namely, the grammar of our science) are taught to embryonic zoologists undergoing cleavage and development of the mental elements of their professional ectodermal layer, confusion will continue; teach the fundamentals of nomenclature to students and *pari passu* the confusion will decrease. And as we reflect on the problems which confront our profession during the next one hundred years, let us recall that there are hundreds of thousands, possibly millions, of genera and species still to be given technical baptismal certificates. The practical question is, are they to be named or misnamed? If they are properly named, we apply the principles of economy (*i.e.,* good housekeeping) to our subject; if they are misnamed, we adopt confusion, extravagance and wastefulness as professional zoological principles.

C. W. STILES

U. S. PUBLIC HEALTH SERVICE

[4] International Rules of Zoological Nomenclature <Proceedings of the Biological Society of Washington, July 30, 1926, Vol. 39, pp. 75–104.

LEONCE PIERRE MANOUVRIER

ONE of the world's leading anthropologists, Léonce Pierre Manouvrier, died at his home in Paris on January 18, 1927, in the seventy-seventh year of his age. He is survived by his widow and one son.

Manouvrier was born at Guéret, Creuse, on June 28, 1850, and received his degree of M.D., with the distinction of *Lauréat,* from the Faculty of Medicine, Paris, in 1881. He began his professional career as an assistant to the noted anthropologist, Paul Broca, in the Broca Laboratory. After Broca's death, Manouvrier succeeded to the directorship of the laboratory which then became one of the laboratories of the *Ecole des Hautes Etudes.* This laboratory under Professor Manouvrier continued to be a center to which students and specialists from all over the world came. At the time of his death, Manouvrier also held two other positions, namely, director of the physiological laboratory of the Collège de France and professor