the exchange of ideas between the staffs of this school and that of your affiliated school in New York. Perhaps you may enjoy the distinction of seeing these two branches of medicine grow into one subject. Fortunately you do not have the responsibility of providing here a complete course in general medicine. Our schools at home are carrying a burdensome curriculum that is constantly growing. But think of the situation which pertains, for example, in the University of the Philippines. There the students must be given a thorough foundation not only in general medicine but they must also be prepared to meet the daily problems in protozoology, helminthology and entomology. After all, the ideal location for a school of general medicine is in the tropics. Such schools have not as yet attained the distinction of a genuinely international reputation. The opportunity for the time being is lying dormant.

We have sketched very lightly some of the more obvious ways in which the interests of tropical and general medicine are intermingled to form the growing structure of the medical sciences. We have illustrated this by a consideration of vitamins and their relationship to beriberi, scurvy, rickets, pellagra and to the physiology of nutrition and reproduction. You have before you now the important results which have just been achieved in the study of rickets by the commission from the Yale Medical School. The relationship of sprue and pernicious anemia commands special interest here in Porto Rico. The study of the streptococci in the tropics will aid in advancing our knowledge of scarlet fever. Among the spirochaetal diseases there is much opportunity for reflection. We have the treponema of syphilis and yaws and the leptospira of Weil's disease and yellow fever. In the field of biochemistry, progress has been made in the study of nephritis as it occurs in Bright's disease and in Asiatic cholera. Turning from medicine to hygiene, we find in many respects a common interest in principles, and the necessities of travel and commerce bring about a closer association in practice. In the experience of the individual these relationships can as a practical matter be but little more than points of contact between the medicine and hygiene of these distant zones. As we look more closely we find firm bonds of union between the medical problems of lands that lie always in the summer sunshine and those accustomed to perpetual fog.

Little did the physiologists dream that a fundamental discovery in nutrition would originate in the small island of Java. Nature has lavishly endowed this island of Porto Rico. Its stimulating freshness lends inspiration for work and for ideas. The richness of your chosen subject defends it against monotony. Here it will be easy and natural to follow the precept recommended by Professor Williams at the founding of the Sigma Xi. He said in part, "In kindling your torches we bid you light them at the brightest living altars of learning and not at the smouldering embers of dead issues." As the years slip by, many students will look back with satisfaction on the incentive received in this favored place.

It is a matter of importance to the scientific world that the people of Porto Rico have achieved a definite consciousness of their responsibilities in science. The leaders in the development of this island are not satisfied merely with commercial progress. The activities of your investigators have given Porto Rico a place of leadership in science in tropical America. This is an enviable position which in time will be challenged by your neighbors in friendly rivalry. But with the foundation of past achievement and with mature plans and preparations for the future, it is a leadership which Porto Rico is in a position to maintain.

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NOMINA CONSERVANDA

AN article in the November, 1926, issue of the Proceedings of the Entomological Society of Washington by Mr. W. L. McAtee, entitled "Nomina Conservanda from the Standpoint of the Taxonomist," shows such an astonishing failure to grasp the relations that exist between nomenclature and taxonomy that I can not permit it to pass without protest.

"Why do scientists," queries Mr. McAtee, "most of whom presumably are evolutionists, attempt to block development in taxonomy while constantly accepting change in other fields both within and without the domain of science?" The inference is that adherents of the idea of nomina conservanda must answer to the charge of being obstructionists. The answer is that they do not attempt to block development in taxonomy; no such charge could be made by one who understood the function of the rules of nomenclature.

Taxonomy concerns itself with the classification of organisms, and modern taxonomists accept the principle that classification must express, as nearly as may be, organic relationship. In a word, taxonomy must as closely approximate the phylogeny of organisms as the state of our knowledge makes possible. It is therefore a science, and, like all sciences, is dependent upon our knowledge of facts and our interpretation of the significance of the facts we know. It would be intolerable to have it codified or ruled upon by any group of individuals, however organized, for it is the bounden duty of every man of science to make known the facts of science as he perceives them,

and it is his inalienable right to interpret those facts according to his own best judgment.

The nomenclature of organisms is, on the other hand, a matter of language. It is a tool that the taxonomist must use, and use well, in common with all other zoologists, too. A central body can regulate it, and should do so, for only in so far as it is standardized and universal is it useful, and in so far as it is individual is it not a tool at all, not a language, but babel. Mr. McAtee may prefer to babel names that no one else understands and has the inalienable right to do so, but if I, on the other hand, wish to apply my names in a manner that has been standardized by reasonable central authority and therefore make them intelligible to others, Mr.McAtee may not imply that my course is less progressive or that I am, in so doing, obstructing scientific progress.

The rules of nomenclature never attempt to settle the status of organic groups. Neither they in general nor nomina conservanda in particular settle or rule upon matters of scientific fact nor the interpretation of those facts. Given one hundred individuals, Mr. McAtee may interpret them as one hundred species and one hundred genera—one hundred families if he likes and has the inalienable right to do, nor will any ruling of nomenclature or any proposition of nomina conservanda prevent it, any more than it would prevent me from considering, if I so chose, that they were all one species. The rules of nomenclature say that *if* one accepts a certain group of organisms as having the status of species, subspecies, genus or what not he shall apply to them such and such a name, and they also provide what name he shall use if he change that status or accept them as of another status, or if he dissociate them from a group with which they have been previously combined or combine them anew with others. About what course the taxonomist shall follow in all these matters the rules of nomenclature are silent, for it is none of their concern.

Just that fact is the reason why the hope that any rules of nomenclature could or should afford a permanent stability in all cases is futile. Such an expectation is based on ignorance. Even theoretically they can only attain a nomenclatorial stability in so far as taxonomy remains stable.

If I to-day call species z and y both members of the genus A-us, and to-morrow decide that they are not, no rule of nomenclature can nor should prevent prevent me from then calling the one A-us z and the other B-us y, a change of name corresponding to the change of taxonomic status. If to-day I call two individuals both species z and to-morrow I do not, no rule of nomenclature can nor should prevent my assigning a new name to one of them. If I assign ten genera to one family, and Mr. McAtee assign them to ten families, no rule of nomenclature nor no nomen conservandum can nor should prevent his act, nor all the changed family names under which those organisms would thereafter be ranked, but if he follow his course (or I mine) the rules may prescribe what names we must use.

All rules of nomenclature must provide for unlimited change, corresponding to changed taxonomic concepts, and they do. To this nomina conservanda are no exception.

A nomen conservandum does not attempt to set up a status quo, thereby dictating for all time that a name shall be used for a group of specified limits. It does not specify the limits of a group for any time; no rule of nomenclature does so. They are not concerned with limits; for limits are questions of fact, or of judgment—not arbitrable, belonging to taxonomy. Just because it can *not* deal with limits nomenclature can only deal with types. It can only define a genus as all those organisms which any given taxonomist accepts as congeneric with the type species. It proclaims, and only proclaims, that now and for all time all those species[1] which any given taxonomist considers as congeneric with a specified type species shall by him be called by a specified generic name. It equally provides that any taxonomist, not considering some of these as congeneric with the specified type, shall not use that generic name in combination with them.

Therefore, given the Genus A-us, type species z, the principle of nomina conservanda may provide at one and the same time that Mr. Blank, accepting species x as congeneric with z, shall use the combination A-us x, and that Mr. Brown, not accepting species x as congeneric with z, shall *not* use the combination A-us x; which is right and as it should be.

Mr. McAtee goes on to say, "Certainly there is no real value in preserving a familiar name unless it embodies a definite concept. Proponents of nomina conservanda assume that these names do embody such concepts, but this is a fallacy. In fact, the longer a name has been in use the more we may be assured that authors have applied[2] it to diverse organisms.

[1] So far the principle of nomina conservanda has only been applied to generic names. If extended to specific names, or to family or other group names the principle would be identical, except that no principle of nomina conservanda could tolerably be applied to a combination of generic and specific names, other than for the name of a genus and that of its type species, for that proposition would instantly involve the limits of genera, and therefore taxonomic decisions.

[2] Misapplied would be better. It is the duty and purpose of rules of nomenclature to clear up and prevent such misapplications. But Mr. McAtee may mean cases

If Mr. McAtee will substitute the modern concept of a taxonomic group as its type and all other organisms that any given zoologist accepts as properly belonging to the same group for the old idea that a group consists of all organisms that come within its original definition he will see that the limits of a group may be as variable as the number of taxonomists who study it, but that its *nucleus* must remain fixed. With that understanding the force of the quoted sentences and of those that follow withers.

Mr. McAtee continues with his confusion between taxonomy and nomenclature:

> "The definite concept idea is not retroactive . . . Furthermore the definite concept idea has no anticipatory value, for we can not be insured against future change. . . . Taxonomy is dynamic not static, and its development demands never-ceasing perfecting of analysis and definition. Setting up nomina conservanda is attempting to establish fixed entities in a field where change, where progress, necessarily has been the rule. It amounts to fixing limits to the search for knowledge. . . ."

If the name A-us is a valid name for a generic group, consisting of species z as type and others, and ten different authors have used ten different generic names for groups in which they included (as type) species z all since the original proposition of the name A-us, obviously the definite concept idea is not retroactive in the sense that it can alter the fact that they have done it, but it is retroactive in the sense that it can proclaim that from our standpoint these were misapplications not to be followed. And it is anticipatory in the sense that it can proclaim that for all future time that particular organism and any others that the future may include with it as congeneric, if any, can only be termed the genus A-us. Taxonomy *is* dynamic and not static, but we must have an intelligible language for it; and our nomenclatorial system provides for unlimited flexibility, change, progress. A nomen conservandum differs from any other name only by the fact that for especial reason, by common agreement we have decided that a particular name E-us shall apply to the type of a genus and its accepted congeners (accepted by any given worker—not by any pronouncement) instead of any other name as A-us, which the rules might perhaps otherwise validate. Its effect never is to limit a generic concept, but only to fix a nucleus and a name; its result is therefore to stabilize and standardize nomenclature, but not to limit the search for knowledge. There is no "sacrifice of scientific ideals of evolution in methods and of progress in knowledge." There can not be, for these things are not involved.

"Furthermore they [*i. e.*, nomina conservanda] can be established only by nullification of the fundamental principle of nomenclature, prority." Why is priority fundamental and how far? Only because it has proved a useful tool and only so far as it is a useful tool. "Priority" is a convention to be discarded just at the point where it begins to impede instead of helping.

The generic name Crabro has been universally used in literature for a common and well-known group of aculeate wasps, in accordance with a usage introduced by Fabricius in 1775. It has escaped all authors (until it was pointed out in 1919) that this name had been used by Geoffroy in 1762 for a well-known group of saw-flies, universally since his day called Cimbex. By the law of priority we are hereafter obliged to call the saw-flies Crabro instead of Cimbex, and to call the wasps by some other less familiar name. That is an exceedingly confusing and awkward thing to have to do. It is not helpful in the case of these names; it is an abomination. But the writer and others who feel with him believe that it is better to accept an abominable situation and make the best of it in an occasional instance like this than it is on his own authority or that of any individual to abrogate the law of priority, which common agreement and experience has shown to be a useful tool. If we can by common agreement through a representative body decide selectively in such special cases that it is more useful to abrogate the principle of priority (or any other convention) than to follow it, we are relieving an abominable situation, and proving ourselves masters of our tools instead of slaves to them. That process is what we call establishing nomina conservanda. It is not a bogey to be afraid of. It is an act of common sense. The danger is only when individuals attempt to establish them by their own unsupported acts; for by their very nature they are useful and tolerable only by common agreement and adoption.

The next argument enunciated by Mr. McAtee is that the actuating purpose of a taxonomist's work is to build himself a monument and that the institution of nomina conservanda threatens the names that the taxonomists originate and which are to be their monuments. I can not read that paragraph without growing angry. The taxonomist who works to build himself a monument had better turn to another field: if

of organisms once supposed to be identical, but which with the lapse of time and the growth of knowledge are now known to be diverse—or supposed to be. In such instances the misapplication would only become such after the diversity was known, and by one who accepted the diversity as a fact. Neither case invalidates the idea that nomina conservanda apply to a definite conception—namely a type and all other organisms accepted by any given zoologist as of the same group.

his reward does not come from the joy of discovering new truths and relations and helping others to discover them, his work is more likely to be an impediment to progress than a useful thing. Taxonomy has suffered too much in the past and fallen too far into disrepute, from the petty work of persons infected with the "mihi itch." Were it not a bibliographical necessity—or so considered—it would be *far* better to not cite the name of an author in conjunction with a scientific name and to forget who proposed it. At least the sooner it is understood, the better off we will be, that we do *not* include the name of the author as part of the formal name of an organism in order to give him "credit," but as a matter of bibliographic record. If it must come to a question of a monument to posterity, there are those who would prefer to leave taxonomic work that would win the approbation of specialists for its sound judgment of phylogenetic relationships, for its scholarliness and helpfulness, even though it never proposed a new name, than to have coined names for a thousand genera and species, each flaunting the describer's name like a waving ensign to dazzle the uninitiated, who may not know how easy and insignificant a thing it is to propose a new name or describe an avowedly new form.

"Taxonomists originate the names, work with them more than other scientists, and in all ways have greater interests in them, and rights over them." As a taxonomist I protest against any such point of view, or against Mr. McAtee thinking that he speaks for "entomological taxonomists almost to a man." The language of zoology is the common property of all zoologists. If the taxonomist allows himself to become so sunk and enmeshed in his own limited group that he can not see, or disregards the needs of the non-specialist in that group for an intelligible nomenclature of it, if he fails to meet the legitimate needs of the general zoologist, of the morphologist, of the ecologist, he may expect that the general workers will ride rather roughly shod over him, for they will not tolerate hampering of their progress in a field that should contribute only cooperation and facility.

The principle of nomina conservanda is sane, sound common sense, when properly applied. It permits us to use the rules of nomenclature up to the point where they serve a useful purpose and to abrogate them just at that point where their further employment would be an unquestioned detriment. The "plenary power" resolution of the Monaco Congress gave the International Commission power to suspend the rules in any given case where in its judgment the strict application of the rules will clearly result in greater confusion than uniformity. It is like the executive clemency principle, which recognizes that in individual cases greater injustice may be done by application of the law and its penalties than by their suspension. But unlike executive clemency it is not subject to political considerations or to individual motives. It must be the unanimous[3] act of an international board of experts—the only representative body of zoologists that exists. There have been very few cases in which this power has been used; and that the commission will be conservative in its future application may be taken for granted. No individual or other body has any recognized right to establish a single nomen conservandum. That this power now exists in the International Commission is a cause for congratulation, a progressive step; a sign that we are to be bound by convention and rules only to the point where they serve a useful purpose, and are not to allow ourselves to become their slaves.

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SCIENTIFIC EVENTS

A CODE OF ETHICS FOR SCIENTIFIC MEN

THE Committee on Social and Economic Welfare of Scientific Men, appointed at the Phoenix meeting of the Southwestern Division, American Association for the Advancement of Science, presented the following tentative code of ethics for discussion at the Santa Fe meeting of the division, April 13, 1927. The code was unanimously adopted.

(1) Assume an obligation to do honest work and to impartially present the same to the public, regardless of political, economic or religious prejudice, pressure or tradition.

(2) Exemplify in your conduct and work a courageous regard for the whole people, and not alone some powerful and influential fraction thereof with which you come in close personal contact.

(3) Recognize and assume a dual obligation (a) to do the best possible work in your field, (b) to promote the social and economic welfare of your colleagues and yourself.

(4) Promote the dignity of your profession; avoid malicious criticism of colleagues; cultivate a professional consciousness.

(5) Support laws to insure competency and high standards on the part of scientific-technical men in every field.

(6) Respect yourself and your profession; do not underbid your colleagues; insist that the laborer is worthy of his hire.

(7) Be slow to change jobs and institutions where such a change means a loss of project efficiency, but do not

[3] A two-thirds agreement results in final decision by a special committee appointed by the succeeding International Congress of Zoology.