friends and pupils among the most passionate leaders in this revolt."

Professor Rogers found many channels in which to suppress his feelings of loyalty and patriotism, and the newspapers of the time record the fact that only a week or two after the outbreak of hostilities when he was called upon to speak at the Thursday Club on some matter pertaining to science he "very gracefully declined to discuss the topic proposed, and then made a stirring appeal to the Club in favor of providing a regiment of our brave volunteers with knapsacks." This appeal was seconded by the Hon. Edward Everett, the President of the Club, and in a few minutes a thousand dollars were subscribed.

Innumerable examples showing this intense spirit might be quoted, not alone from letters to his brother in Glasgow, but from those to many prominent Englishmen whose friendship he enjoyed. During the darker days, in spite of his feeble health, he made patriotic addresses on Boston Common and on September 26, 1862, he referred to the Emancipation Proclamation of President Lincoln in the following thrilling words, a part of a letter to his brother Henry: "The great event since my last letter, the greatest event beyond comparison of the war, is the late proclamation of the President, declaring the slaves of all rebellious States after January next to be *forever free*. On the 22d of September this momentous voice was uttered. On that day—in a sublimer sense than ever before—*the sun crossed the line*."

Under date of July 5, 1863, he describes the celebration of the 'Fourth' in Boston. Dr. Holmes gave the oration in the 'great theatre to an audience packed to the dome,' and the enthusiasm was great, all hearts being absorbed in one feeling of patriotism.

As evidence, however, that his interest in the progress of science was by no means dormant during those potentous days, he adds: "What kept me in the city, however, was my interest in the exhibition of the electric light, which the Council, at the instance of George Hale, encouraged by me, decided to make one feature of the evening celebration, as a substitute for part of the usual fireworks." He describes in interesting detail how Ritchie accomplished a superb success by putting a battery of 250 cells on the top of the State House dome, from which a 'flood of light' was thrown upon over 100,000 pedestrians who thronged the streets.

But surely enough has been quoted and enough said to give some notion of the extreme interest of these volumes, not only to scientific men, but to all intelligent people who admire exalted character and lofty sentiment. America has produced no finer type of man than was exhibited in William Barton Rogers, who showed that it was not impossible for one who was primarily devoted to learning and original research in pure science to be at the same time a lovable companion, an eloquent lecturer and a man of affairs whose influence upon his contemporaries resulted in the creation of new institutions and the remodeling of old. It is even now too early to recognize in full measure the value of his life to the people, to whose best interests he showed a rare fidelity.

T. C. MENDENHALL.

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*THE MERTON RULES.**

THESE are the Code of Nomenclature "at present in force for regulating all work done in the study of Microlepidoptera at Merton," and "the object of these rules is to insure absolute obedience to the Law of Priority." In so far as this Code is pecu-

*Rules for regulating Nomenclature with a view to secure a strict application of the Law of Priority in Entomological work. Compiled by Lord Walsingham an [and] John Hartley Durrant. Longmans, Green & Co.: London, New York and Bombay. 2 Nov., 1896. 8vo, pp. 18.

liarly or exclusively adapted to the exigencies of entomological work it is of course not within the purview of ornithology; but its most avowed object of insisting upon the Law of Priority, its whole tenor, and most of its fifty-one canons, are no more pertinent to one branch of zoology than to any other. Lord Walsingham and his secretary thus submit a set of rules to the consideration of all zoologists, and no apology is needed for examining them with special reference to the A. O. U. Code.

Naturalists are in substantial agreement upon such a large majority of the propositions which bear upon the 'highly-contentious' subject of Nomenclature that we should expect any code drawn up by competent workers to be good in the main. Such is emphatically the case in the present instance. If, therefore, we seem to dwell upon the exceptions we take, to an extent disproportionate with the commendation we give, it will be understood that the latter goes without saying. Probably four-fifths of these rules will receive general unqualified assent; it is mainly regarding the remainder that we have here to do. Most of these may be reasonably questioned, and some of them are likely to be regarded as highly objectionable.

We think that the authors make a strong point in the introduction, but it is made so quietly that its full force may not be recognized at first sight. This is where (p. 4), referring to other codes, the authors say they "are not aware that in any of these rules the actual work and intention of an author has been guarded to the same extent as the names which he has given to his conceptions," and then add: "It has been one of our objects to define a method by which the recognition of antecedent work can be consistently secured, regarding this also as no less a moral obligation capable of being met by the same rules which apply to mere names."

This raises a large question, concerning which we are heartily on the side of the authors. We have too often heard it said that, as we cannot enter into the inner consciousness of another, especially if he have lived in other times and been long dead, we have no concern with his spirit, purpose or intents, but only with his acts, and that consequently we must go by what he actually did, without regard to what we may think he intended to do. We believe this to be bad—unjust and dangerous; Lord Walsingham has the right of it in introducing something of a moral or ethical principle, difficult as such may be of application in all cases. An author's intent or purpose can generally be fairly presumed or inferred from his writings; when such presumption or inference is reasonable it surely should not be disregarded, and the cases must be very few in which no meaning is discoverable.

Example: In founding his genus *Dendragapus* Mr. Elliot intended to separate grouse of the *obscurus* group from those of the *canadensis* type. That was the sole intent, purpose, scope and function of his generic name. What right, then, has any subsequent author to use *Dendragapus* for the sole purpose of uniting *obscurus* with *canadensis?* None; such a travesty of the generic name, such a perversion, or rather reversal, of Mr. Elliot's express purpose, is simply nomenclatural hocus-pocus, and as such it is puerile, unscientific and immoral.

There is another point in Lord Walsingham's introduction we must pause to applaud heartily. Though it be one of those which, as we say above, go without saying, it is particularly well said (p. 5): "The object of all rules should surely be to secure precision, uniformity, and finality: any sacrifice of these objects to considerations of mere convenience can only result in the creation of greater inconvenience at some future date. The inconveniences from which

workers in entomology now suffer would have been entirely avoided had the earlier authors studied and recognized the work of their predecessors, or adopted some such rules as are here set forth, beginning from the time of Linnæus. The earliest sinner in this respect was Fabricius, but Stephens in creating the necessity for Rule 48, has exhibited even greater ingenuity in his aberrations." Here, again, is an obvious ethical principle. We do owe something to posterity, notwithstanding the question which some wit once asked: "What has posterity done for us?" This debt is in the nature of a moral obligation not to consult our own present convenience at the expense of those who are to come after us. And if we may disregard the altruism involved, then self-respect and an enlightened selfishness should alike prompt us to the same end, merely as a matter of looking out for our own good repute.

A part of Lord Walsingham's 'Premiss' will be hailed with acclamation by almost every American zoologist. It is as follows:

"Zoology became an intelligent science when it was recognized that every species should possess a special name and every genus a generic name. This system of nomenclature was first enunciated by Linnæus in the 'Systema Naturæ' (edition X.), 1758; and as we owe the conception of the special and generic name to Linnæus, we are bound to commence our nomenclature from the year 1758, when he published his epoch-marking work."

Shade of Strickland! Requiescat in pace! We do not propose to echo an anthem to this requiem, nor even to argue the point; for we could say nothing that has not been said fully and perfectly well in the A. O. U. Code itself in support of this reasonable proposition—one of self-evident logical necessity; and if what is recorded there does not budge our British friends, nothing will move them from the isolation of their insularity. We know that most of our respected colleagues on the other side of the herring pond still stand on the rock of offense, whence to denounce with objurgation those who do not subscribe to the B. A. Code. Possibly we undertook to split that rock with the butt end of our wedge; probably Lord Walsingham may prove to have insinuated the thin edge from his own coign of vantage. He may not be the first among English naturalists to favor the heresy of 'Linnæus at '58;' but he is certainly one of the strongest, and much may be confidently anticipated from the force of his example. We remember once discussing with him in person the 'American idea' of Trinomials. We may have been persuasive, though we failed to be conclusive, in our presentation of that case to his liberal and progressive judgment; but the stand he has now taken against the extreme conservatism of his countrymen leads us to the confident hope of his enlightenment even upon those 'dark sayings' of Trinomialism. If the scales could fall from the eyes of such a one as Saul of Tarsus, a Paul of London, Cambridge or Merton may not be a zoological impossibility in the course of natural evolution.

Passing by most of the Merton rules as self-evident, or as admitted by the concensus of naturalists, or else as peculiar to microlepidopterists*, we note some few

* A valued entomological friend of high standing, who has not authorized the use of his name in this connection, writes to us regarding some of these: "I think that most entomologists would take exception to the group of rules beginning with No. 19 and ending with No. 25, except that No. 23, since it begins with the words 'it is advisable,' is acceptable. Similarly, under No. 33, *e. g.*, Zeller's correction is not admissible. Under No. 38, the third paragraph, indicating that the type is the sum of the co-types. Mr. Oldfield Thomas's terms under No. 39 have not come into general use, though they are sensible enough. Under No. 41: No one accepts the idea that the type of the genus is the sum of the species as under 2 and 3."

others for special approval or the reverse.

Rule 18. "If it be held that the generic and special names may not be tautonymic, the law of priority will determine whether the special or the generic name should be changed, *e. g.*, *Cossus cossus*, L." The proviso in this case nullifies the rule for us, because we do not forbid tautonymity. Our practice is bad—obviously so, on the score of literary propriety; it is to be strongly discouraged; but it seems an unabatable nuisance, which most naturalists will put up with perforce, and it has one redeeming feature—we know absolutely what every tautonym means.

Rule 19, bringing up 'three classes' of invalid names, is likely to remain a bone of contention in one-third of its scope. We all agree regarding homonymous names, that they are absolutely to be rejected; so, of course, regarding synonymous names. But not so regarding homophonous words, for surely no one of us would reject *Seiurus* because its sound when spoken is identical with that of *Sciurus*.

Rules 20 and 21, hanging on the foregoing, are open to difference of good sound opinion, possibly because, for one reason, it may not be always clear what words come under these provisions. It is desirable, but probably impossible, to have rigid rules here. No rule can possibly be more rigid or stable than the sum of the cases to which it applies; but the cases intended to be covered by Rules 20 and 21 are so shifty and mobile that no seive can be devised with meshes fine enough to catch them all. We confess ourselves puzzled here; we cannot offer better rules than Lord Walsingham has, yet we doubt their sufficiency. It seems to be a case where common sense, tact and expertness may work better than any formality. Let us agree, as most, if not all, ornithologists would, that *Telea* invalidates *Teleia*, and *Pandemos* invalidates *Pandemis;* does *Ucetia* invalidate *Eusesia?* Personally, we should say that would depend upon the etymology in the case. If *Ucetia* be merely a bad way of spelling *Eusesia*, the etymon in the two cases being identical, we should say they were the same word, not available for two names, for all that they happen to be spelled so differently. We would, therefore, spell to the best of our ability both names as one, and use the right spelling or not according to the provisions of some other rule regarding homonyms. But if *Ucetia* and *Eusesia* be of different etymons, only accidentally homophonous, we should regard them as distinct words, neither of which would invalidate the other. Yet we know that others might take exactly the opposite view and argue strongly in its favor. As we said in substance, this whole class of cases has thus far proven refractory to, or elusive of, any rule naturalists have had the wit or ingenuity to devise.

Rule 22. "A name wrongly written is invalid if, on legitimate correction, it becomes homonymous or homophonous with a valid name; *e. g.*, *Grapholitha*, Hb., invalidates *Grapholita*, Tr." The A. O. U. Code would have simply to quash this rule, because it forbids all correction of names, 'legitimate' or illegitimate. We think it is the very worst blot on our Code, which has done more to bring that work into disrepute than all our other weak or bad points put together. It puts a premium on ignorance or carelessness to rule, as we do in Canon XL., that a name shall endure exactly as it originally appeared in print, no matter how mangled it was, unless a *typographical* error be evident. Why be so fierce with the poor compositor, and let the guilty author go scot free? We need not become formal impurists for fear of purism. Personally, we decline to bind ourselves to misspelt words forever, for no better reason than that some zoologists, too ignorant or

too slovenly to spell them properly, sinned in the beginning. We are opposed to 'original sin,' whether as a theological dogma or a canon of nomenclature. On this subject we cite from a private letter lately addressed to us by one of the most learned and distinguished of American philologists, Professor C. P. G. Scott:

"I think you are quite right, as a scholar, in your disapproval of the mechanical rule which, as a member of a committee constrained to compromise, you passively sanctioned in 1886. It seems to me that these verbal uncertainties will *never* end, no matter what committees may recommend or do. Therefore, since the only purpose of compromise is to end uncertainties and discrepancies, it would be better for your committee to revise the code of nomenclature with reference to etymologic principles which *can* be ascertained and stated, and to consider all material deviations, intended or unintended, old or new, as if misprints to be corrected in subsequent works. Etymologic principles are a good deal more stable and visible than is commonly supposed; but you know by long observation that not every zoologist (particularly in France) is sound in his tackling of Greek and Latin."

Our zeal for 'spelling reform' might not now lead us to the length it did, for example, when we emended Richardson's genus *Aplodontia* into *Haplodon*, only to find, to our dismay, that in its new guise the name was homonymous with several others of prior date, and therefore inadmissible for the genus of mammals. We would treat such a case as incorrigible, and let it go at that, without regard to its etymology. It seems to us that tact, discretion and common sense, applied to each individual case, is likely to work better than any rigid rule which could be devised to cover all cases. We are not such rigid purists as to sacrifice the Law of Priority to purism. In fact, we would not go to the length Lord Walsingham does in '33 *e. g.* (2),' where he 'corrects' *cretidactylus* into *gypsodactylus*. If both these names mean 'chalk-toe,' as we suppose, the substitution of gypsum for chalk, to prevent a Græco-Latin hybrid, seems hardly required. That is mere purism, only less unobjectionable than the systematic impurism which the A. O. U. Code would force upon us. A certain 'sweet reasonableness' would seem to be the best prophylactic or preventive—better than extreme measures either way. Such heroic treatment is likely to become the mock heroism of opera bouffe. As Horace said, some years ago:

> "Est modus in rebus; sunt certi denique fines,
> Ultra citraque nequit consistere rectum;"

and as Professor B. G. Wilder lately remarked, in the course of his controversy with Wilhelm His:

"As with biologic generalizations, there are few philological rules without exceptions. Yet the reformer, especially if young and enthusiastic, either ignorant of history or undismayed thereby, 'too often imagines that a principle, if right, cannot be carried too far.'" (Barclay, 1803.)

Our A. O. U. committee may be neither very young nor over-enthusiastic; yet this is precisely what we have done to the excellent principle of priority—carried it too far, in attempting to impose verbal abortions upon nomenclature. The results sometimes better befit the nursery in their puerility than the halls of science. Take that miserable botch of a word *Leptotila* Swainson for a genus of pigeons. If the celebrated quinarian had printed *Leptoptila*, in proper form, his genus would have been invalidated by the prior *Leptoptilos* or *Leptoptilus* for a genus of storks, because by our rules a difference of termination in words etymologically identical does not prevent homonymity; but because Swainson or his printer did not mind his *p*'s—whatever he

may have done with his *q*'s—*Leptotila* becomes valid and tenable!* This is mere juggling with the letters of the alphabet; it is absurd, undignified, infantile. We should say, first spell *Leptoptila* correctly, according to its obvious formation, and then decide on other grounds whether or not it be in this form different enough from *Leptoptilus* or *Leptoptilos*. Literary abortion should not be viable in the language of science.

Rule 23 allows difference in termination of words to make formal difference in names, *e. g.*, *Sciaphilus* in Coleoptera and *Sciaphila* in Lepidoptera, on the ground that no confusion results. How it would be did these two fall in the same order of insects is not stated. We believe the practice of entomologists has generally been to allow even identical names to coexist in different orders, on the same ground. Probably the latter state of the case is inadmissible in any other branch of zoology; but on the question whether difference of termination or inflection, as indicating grammatical gender, shall suffice to distinguish names, much might be said either way. Our practice is against it. Yet there stand our *Picus* and *Pica*, and we may yet have to reconsider our present canon on this question. Certainly *Leptoptila* and *Leptoptilus* are better distinguished from each other than *Leptotila* would be from *Leptoptila*. One is a distinction with a real difference, viz., of gender; the other is a distinction with a bogus, spurious, bastard, abortive, illegitimate apology for a difference.

Rule 24. "A name which involves a false proposition is invalid and may be changed." One would think this a self-evident proposition in science—a truism, to adult minds, hardly requiring statutory provision. The reverse would be, or should be, unthinkable in science. Yet so far afield in following the *ignis fatuus* of verbal veneration for the fetish of 'priority' have some of our codifiers been led that they would not dare to correct an error of scientific fact for fear of disturbing the cerements of verbiage in which it was embalmed! Let some one describe an albino crow as *Corvus albus*; shall we go on calling black white to the end of the chapter? Let some one describe a broken-billed popinjay as *Picus semirostris*; shall *semirostris* be the name of any species which has a whole beak? Let Gmelin describe a Mexican woodpecker as *Picus cafer*; shall we declare every time we write the name that it is a South African bird? Yet the last is exactly what we do in the A. O. U. Check List, where *Colaptes cafer* stands in a catalogue of North American birds. It is *brutum fulmen* for us to declare that a name has no necessary meaning. Such declarations simply beat the air in a futile and fatuous manner. '*Cafer*' has no meaning for those who do not know what it means, but those who do know what it means can no more divest it of its reference to a South African locality than they can take away from *Colaptes* its implication of carpentering. Granted that we have plenty of nonsense-words in science—mere combinations of letters, *sensu carentes*; granted that we have to put up with them, and that they do very well, like Smith or Jones; that does not make *cafer* mean Mexican, or *albus* mean black. The proposition is false, in form and in fact; and falsity is foreign to science. Any attempt to save the Law of Priority at such hazard, or im-

* We are sorry to observe that the A. O. U. is not consistent with itself, even in wrong-doing. Thus: (1) *Leptotila* is held to be tenable, from lack of the *p*. Had it been *Leptoptila* it would have been untenable, from similarity to *Leptoptilos*. (2) *Fregetta* Bp. is held to be untenable, owing to a prior *Fregata* of Brisson, and *Cymodroma* is used instead. (3) But *Fregetta* is quite as different from *Fregata* as either *Leptotila* or *Leptoptila* is from either *Leptoptilus* or *Leptoptilos*; *i. e.*, there is no real difference in either case. The A. O. U. is wrong in one of these cases, necessarily—which one?

pose uniformity of nomenclature by the arbitrary authority of an individual or a committee who should go to such a length as to say either that words have no meaning or that we shall use words in wrong senses must fail, for the simple reason that common sense will not be coerced, and sensible writers will go on writing as sensibly as they know how.*

Rule 25 provides for expunging from nomenclature any name which is 'offensive (whether politically, morally, or by its irreverence).' This raises a question of great and probably insuperable difficulty; for we may at once ask, offensive to whom? In a certain sense, science is non-political, non-moral (a different thing from being immoral, of course), and non-reverent of anything but ascertained or ascertainable truth. In religion it is agnostic; in printed decencies it is usually found to side with decent people; in political lampooning it might display partisanship as a part of human nature, without great offence to propriety. It would seem to be a case which ordinary self-respect and regard for public opinion would suffice to regulate; yet we can hardly arbitrate it in its nomenclatural aspects. *Sivatherium* might be as offensive to a Hindu devotee of the cult whose god is invoked in the name of a fossil beast as any other blasphemy. Neither *Phallusia* nor *Ithyphallus* offends more than the translations of these terms would in a medical treatise, nor does *Clitoria* keep a manual of botany out of schools. While the drift and purport of Rule 25 are obvious and admirable, its enforcement to the extent of expunging any names but those quite outside the pale of public decency is probably impracticable. The way to deal with such things is to cut the perpetrator the first time he shows himself in society.

We have already protracted this review beyond usual limits, and must hasten to cloture. Several following rules bear upon correction of names, orthographically or zoologically. In the former regard they will be nugatory with those who hold to our Canon XL.; in the latter respect they will command the assent of some naturalists, but not of all. Rule 42 will, we imagine, be found decidedly objectionable, as will most of its corollaries; though some of the refinements regarding types may be specially serviceable in microlepidoptera, while less so or not so in zoology at large. In any event, Walsingham and Durrant have given us in this Code a notable contribution to the literature of the subject, which can be studied to advantage by every zoologist, perhaps by every botanist also. For ourselves, we are among a large number of naturalists who are fully convinced that the A. O. U. Code is, on the whole, by far the best one ever formulated. Our appreciation of its manifold good qualities and general utility leads us not unnaturally to set it up as a standard of excellence with which other codes are to be compared. That we are not blind to its defects is obvious from what has preceded; but it is our very conviction of its strength and worth which makes us feel free to express ourselves perhaps more forcibly regarding its blemishes than we should if we considered it a weak or tender thing that needed nursing. At the same time it were idle to consider our Code a faultless finality; no sensible man is going to be bound by it against his own convictions, and if naturalists are ever to be

*Dr. B. G. Wilder states, Journ. Comp. Neurology, vi., Dec., 1896, pub. Feb., 1897, p. 334: "No such attempt is known to me." Then he never knew the A. O. U. Code and Check List, which are an attempt to secure uniformity of nomenclature by the authority of a committee, "*quorum pars magna fui.*" But his criticism is excellent: "The very notion savors of ecclesiasticism rather than of science. At the most, individuals have set certain fashions, more or less commendable and permanent, while committees have made recommendations which even their own members may disregard when their information is increased or their views are modified."

blessed with such a thing as an infallible rule of faith and morals in matters of nomenclature that state of beatitude may be sought along the line of one of Lord Walsingham's suggestions: "All branches of zoological study should undoubtedly be represented on any committee entrusted with the task of drawing up rules for general guidance." Pending any final consummation, British ornithologists will no doubt continue to lean upon Strickland and the B. A. Code. American ornithologists, and most zoologists of this country, will stand by the A. O. U. Code; while doubtless the Merton Rules will be respected by most of those entomologists whose requirements are so ably met in this instance.

Elliott Coues.

As said above, the 'Merton Rules' agree in the main, or at least on most points of leading importance, with other recently promulgated Codes of Nomenclature; but they embrace many provisions, by no means all new, which are open to strenuous objection, on the ground that they seriously militate against the stability of names in zoology. Some of these have been pointed out in the foregoing review; others have been passed over leniently or quite unnoticed. On the other hand, some which we consider utterly objectionable have received approval.

Rule 5, for example, provides that "The same name may be used once only in the same grade, with the exception of special [specific] names, so long as they occur in different genera, and of subspecial [subspecific] names so long as they are subservient to different species." Since 'subspecial,' or subspecific, names are often applied to forms of uncertain status, and may be regarded either as species or subspecies by different writers, it is obvious that they should fall under the same rule as 'special,' or specific, names. Otherwise they are open to instability, according to whether the forms to which they may be applied may be regarded as 'species' or 'subspecies' by different authors.

Rule 19 divides 'invalid names, considered merely as words,' into three classes: (1) *Homonymous*, (2) *Homophonous*, and (3) *Synonymous*. 'Homophonous' words are 'words differently written, but indistinguishable in sound, applied to different conceptions.'

Rule 20 provides that "A name homophonous with a valid name is invalid, in accordance with the rule governing homonomy (Rule 5)." (See Rule 5, as quoted above.) This we regard as a pernicious innovation. Previous codes advise that in selecting new (especially generic) names those closely resembling previous names in orthography or sound be avoided; but this is the first instance, we believe, where they have been declared invalid. The objections to this rule are: (1) that scientific nomenclature belongs properly to written language, not to spoken language; (2) that whether a name is or is not too near in sound, when properly pronounced, to another earlier name must be largely a matter of opinion, respecting which authors must frequently disagree; this disagreement being necessarily a fruitful source of instability in names. It opens a loophole for the displacement of well-known names by new ones on the ground of personal opinion or preference, perhaps biased by the opportunities thus presented. In all languages there are almost innumerable homophonous words of radically different origin and meaning; why should they not be admissible in the language of science?

Rule 20 is thus in opposition to the intent of nearly all modern codes, which are designed to leave as little as possible open to the notoriously unsafe decision of personal judgment or option in matters of nomenclature.

According to Rule 21, which is an amplification of Rule 20, a name may be only 'so similar to a valid one as to be *almost homophonous*' to be rejected, which, of course, but emphasizes the objections already made to Rule 20.

It follows that Rule 22 is also objectionable (1) in that it provides that "a name wrongly written is invalid if, on legitimate correction, it becomes (either) homonymous or homophonous with a valid name;" and (2) in that it implies the right of emendation of names.

Yet Rule 23 permits, in the case of genera, the use of names which differ merely in 'a different sexual suffix,' as in cases like *Sciaphilus* and *Sciaphila*, in opposition to previous codes and the consensus of probably nine-tenths of present zoological writers. The case of *Picus* and *Pica* is obviously not parallel, inasmuch as these names have come down to us from pre-Linnæan classical writers, who employed them, not interchangeably, but as names for entirely distinct objects. The etymology of these words is admittedly unknown, but their use for centuries as distinct names, applied to totally unlike birds, seems sufficient reason for their use in modern systematic nomenclature, and for their adoption as distinct words, despite the accident of their similarity.*

Rule 24. "A name which involves a false proposition is invalid and may be changed." Note first the use of 'may' instead of *must*, leaving the enforcement of the rule optional, and thus opening a way to instability of names through mere personal opinion or preference. Note, secondly, that it opens the way to wholesale changes on trivial grounds. If a species of a certain genus has been named *minimus* or *major*, and a species is subsequently found that is smaller or larger, the names in question 'involve a false proposition.' And so in cases where names relate to color, as where a species is named *nigra*, or *purpurea* or *rubra*, and is not of the color implied; or in the case of species given geographical names that do not occur in the countries whose names they bear, or in other cases indicate portions of their range which are not characteristic of their distribution. For instance, a Mexican species may have been named *brasiliensis*, or a Peruvian species *cayennensis*, which do not occur respectively in Brazil or Cayenne. This class of cases, as everyone knows, is legion. To avoid the adoption of a rule necessitating such sweeping subversion of names we can well afford to endure the isolated case of a name like *cafer* for a North American woodpecker! *Ludovicianus*, as applied to one of our tanagers, now 'involves a false proposition,' since the species does not occur in Louisiana. There is, in fact, every shade of falseness in names, from a time-honored *Paradisea apoda* to such as involve a falsity so slight as to be of questionable importance in the mind of even the greatest stickler for nomenclatural veracity.

Rule 27 is the corollary of the principle laid down in the 'introduction' regarding the 'guarding' of the 'actual work and *intention* of an author,' quoted, and so emphatically commended in the foregoing review of the 'Merton Rules.' The Rule is: "The right of an author to correct antecedent work is undoubted, provided always that in making such corrections the *intentions* of his predecessors be respected, unless proved to be erroneous." This is an innovation fraught with the gravest possibilities for mischief. Heretofore it has been deemed to be the only safe course to take an author's work as he left it, without attempting to

*Since this was written this point has been touched upon in this journal (SCIENCE, N. S., Vol. V, No. 126, May 28, 1897, p. 847) by Dr. Stejneger, who says *Picus* and *Pica* "are distinct and separate Latin classical names for widely different birds, though the philological root of the two words is probably the same."

guess at what he intended to do, even if in rare cases we may be compelled to ignore an obvious intent. The trouble here is that when the bars are let down there is no limit to the license authors may take, for one reason or another. The case of *Dendragapus*, cited above as an illustration, shows to what strange conclusions such license may lead. It is purely a matter of opinion whether *obscurus* and *canadensis* are generically separable. For those who think they are not, the only course open is to treat them as members of one genus, and to take the oldest tenable name available for the group, in accordance with a fixed principle of nomenclature especially provided for such emergencies. The first tenable name happens in this case to be *Dendragapus*. If this is 'puerile, unscientific and immoral,' there is no help for it, even under the 'Merton Rules.' All it amounts to is a difference of opinion between ornithologists as to the value of the differences which distinguish the two species *obscurus* and *canadensis*.

Rule 30 provides, among other things, that "an orthographical correction may be made by emending a name wrongly formed." This is not an innovation, as the same provision is found in other codes. But it is diametrically opposed to Canon XL. of the A. O. U. Code, which provides that "the original orthography of a name must be rigidly preserved, unless a typographical error is evident." Sound and weighty reasons are given in the A. O. U. Code for its adoption; it is in accord with the practice and advice of many eminent authorities—some of whom lived before the A. O. U. Code was thought of, and were scholars as well as naturalists; and, while still repellant to some, it is obviously gaining ground, as there is an increasing number of writers who consider fixity of names as of higher importance than the correction of grammatical or philological imperfections in their construction. Not only is this view shared very generally by American mammalogists, but we have recently found that it has strenuous advocates among eminent specialists in other departments of zoology, notably in entomology.

When we consider that purists and classicists are often at loggerheads among themselves over the emendation of a name; that names are often 'emended' out of all resemblance to their original form and become, to all intents and purposes, new words; that, when less modified, the initial letter is often the part affected, and that through this change the name takes a new place in all indexes and in all alphabetic lists where it appears, resulting in an inconvenience of serious magnitude—it seems far the lesser of two evils to put up with here and there an orthographic abomination than to sacrifice stability of nomenclature to philological refinement. Carelessness or ignorance in the construction of names is to be deprecated and frowned upon, as it is in the remarks under the much abused Canon XL. of the A. O. U. Code, which, therefore, does not place 'a premium on ignorance or carelessness,' but simply chooses by far the lesser of two evils. It conforms to the whole spirit of the Code, which aims at stability in names, and the elimination of every element of instability that may arise from personal preferences in matters where opinions must inevitably differ to a greater or less extent. Even my fellow critic of the Merton Rules cannot agree with Lord Walsingham as to the extent emendation is allowable, as in the above-cited case of changing *cretidactylus* to *gypsodatylus*.

As to the case of *Leptotila*, dwelt on at length above, if 'tales out of school' were admissible, the inside history of its adoption by the committee would afford an amusing commentary on some of the remarks above made. Suffice it to say that

as *Leptotila* was repeatedly used by Swainson, and thus with obvious intent, it could not be ruled out as an 'evident typograhical error' for *Leptoptila*, and so was accepted as simply a name, and therefore available under the A. O. U. maxim, "A name is only a name and has no necessary meaning;" or, to cite the B. A. Code of 1842, "In truth, it matters not in the least by what conventional sound we agree to designate an individual object, provided the sign to be employed be stamped with such an authority as will suffice to make it pass current." It is, therefore, entirely thrown out of the category of such cases as *Fregetta* and *Fregata*, discussed above.

It certainly is to be hoped that all sensible writers will go on writing as 'sensibly as they know how;' but in the above remarks on *cafer* and *Leptotila*—ostensibly anent the 'Merton Rules,' but really in ridicule of the A. O. U. Code—it is evident that not all of the 'puerility' is on the side of the supporters of Canon XL.

Nos. 34–37 of the Merton Rules call for no comment, being in essential conformity to current usage. We must dissent, however, from Rule 38 in so far as it relates to 'co-types,' this part being to the effect that when a species is 'described from more than one specimen, no single one being selected as the type,' the 'type' in this case is 'the sum of the co-types.' The position here taken seems so obviously unwarranted as to hardly merit discussion.

Rules 42–48, on the restriction of genera, are refinements of existing rules relating to this subject, treating the matter in detail on lines already for the most part generally approved.

Rule 49 provides a most cumbersome way of designating subspecies. Rules 50 and 51 relate respectively to the use of signs and methods of citation, the latter formulating practices already more or less in vogue.

As already said, the 'Merton Rules' are in the main in accord with other advanced modern rules and usages; the innovations, as noted above, are for the most part positively mischievous, from the standpoint of fixity in names; the adoption of the tenth edition of Linnæus's 'Systema Naturæ' we regard as the one especially commendable feature of this new code, only so, however, on account of its geographical origin, since all recently promulgated Codes take this date as the starting point for the law of priority.

J. A. ALLEN.

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*THE ORIGIN OF GREEN RIVER.*

IN his Current Notes on Physiography in No. 121 (April 23d) of this JOURNAL, Professor Davis, under the heading: 'Is Green River antecedent to the Uinta Mountains?' remarks that this question is not closed, as had been assumed by Mr. J. D. Irving in his paper on 'the Brown's Park beds of Utah,' and further that it does not appear clear from the latter's statements whether he considers it to be a superposed river, as maintained by me, or antecedent, as stated by Powell. He very pertinently remarks that it is remarkable, considering how frequently the Green is referred to as an antecedent river, that so little attention is given to the difficulties that such origin involves. Long before the appearance of the two textbooks he quotes (Tarr and Scott), LeConte and Geikie had each referred to it as antecedent and illustrating the slow uplift of mountain ranges, in apparent unconsciousness that any other view is possible. Suess, on the other hand, in his exceedingly careful review (Antlitz der Erde, I., p. 736) of the structure of this region, adopted my view without any reference to that of Powell.

In Powell's original publication (Exploration of the Colorado river of the West, p. 153) he makes no mention of the struc-