# Zoological Nomenclature : International Rules and others.

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Individuals and societies are sometimes accused of running counter to the laws of nature. In popular speech and writing this behaviour is regarded as immoral and worthy of punishment. Beyond doubt, the love of freedom or misliking of restraint exercises over some minds so powerful a fascination that they would, if they could, crumple up the laws of nature with exquisite zest. But that particular piece of mischief is out of man's reach. We have to be content with breaking domestic, æsthetic, linguistic, social, ethical, ecclesiastical, statutory, and international law. This protean mass, unlike the immutable laws of nature, is ever varying with time, place, and circumstance. Neither Medes nor Persians have contrived to make it in any one particular fixed and unalterable, so that we find in the course of history falsehood, theft, murder, parricide, not only practised, but justified and delighted in, as well as unselfishness, purity, truth, and filial affection.

On this occasion we are principally concerned with the linguistic department, but there is an international aspect of the question of no small importance, and there are some æsthetic and ethical points of view which are worthy of notice.

Within the last half-year four papers have appeared directly dealing with the subject, and written by persons whose position, opportunities, and acknowledged eminence must reasonably give them a commanding influence among their fellow zoologists. In order of appearance these papers are : first, "A Draft of Rules for Zoological Nomenclature, as basis for a revision of the International Rules of the International Nomenclature Commission," proposed by F. C. von Maehrenthal in Berlin (published in Braun's 'Zoologische Annalen,' vol. i. p. 89, Sept. 1904, Königsberg-i-Pr.); second, "Some Changes in Crustacean Nomenclature," by Mary J. Rathbun (in the 'Proceedings of the Biological Society of Washington,' vol. xvii. p. 169, December 1904); third, "International Rules of Zoological Nomenclature," by Professor Raphael Blanchard, Professor von Maehrenthal,

and Dr. C. W. Stiles, the introduction, in French, by Professor Blanchard, being dated Berne, August 1904, but the whole paper containing the rules themselves, in French, English, and German, bearing as date of publication, Paris, 1905. Lastly, there is a leaflet by Professor F. E. Schulze, dated Feb. 2, 1905, reporting to the Academy of Berlin the progress of 'Das Tierreich' *, and especially emphasizing the efforts made by the editorial staff of that vast undertaking to secure the utmost possible unity among zoologists on this much-discussed subject of nomenclature.

That these distinguished naturalists should turn aside from their own special studies and occupations, concerned with things and facts and the deeper mysteries of nature, to spend much time and anxious thought in the endeavour to legislate about names and questions of spelling, should raise a presumption that the subject is in itself not wholly unimportant. In the ordinary business of life, in order that men may meet one another by appointment, in order that letters and parcels may reach their intended destinations, we all appreciate and use the facilities afforded by railway guides and postal directories. We all know the confusion caused by having in the same kingdom a dozen towns or villages called Walton, a dozen George Streets in the same city, two John Smiths in the same terrace; the inconvenience that arises when a long row of houses is re-numbered; the risk of confounding Vienne and Vienna, Tonbridge in the United States with Tonbridge in Kent; the difficulty of identifying Mechlin with Malines, Trèves with Tries, Hafnia with Copenhagen, or Constantinople with Stamboul. In common life, however, the troubles that arise from these causes pinch us but rarely. In systematic zoology it is different. Classification has to deal with thousands and ten-thousands of species, every one of which requires a distinctive designation. In making this assertion I readily admit that you cannot get all human beings to agree on any proposition whatever; but probably almost all zoologists do think it desirable that every species of animal should have a designation not shared by any other species of animal, a designation valid for it and it alone in Tokio and St. Petersburg, in Paris and Berlin, in Washington and London, in Naples and Madrid, in Valparaiso and Melbourne—in short,

* In this report the spelling of the name is changed without explanation to 'Das Thierreich.'

throughout the whole scientifically cultured world, without regard to race, political boundaries, or vernacular speech.

Then the question arises, how is this result to be attained? In his instructive, unassuming, and conciliatory preface to the trilingual international code, Professor Blanchard traces the history of the attempts that have been made to solve the problem. "Nomenclature," he says, "is the grammar of the natural sciences; it was defined for the first time by Linnæus in the 'Philosophia botanica' in 1751, and applied to begin with to the vegetable kingdom, not being introduced methodically into the animal kingdom by the celebrated Swedish naturalist until 1758." Since then, during the last sixty years, advisory rules or codes have been issued by Associations and Societies in different countries and with various aims. Some of these have not striven to control the whole field, but only special parts of it, as palæontology, ornithology, entomology. It is easy to understand that, when large departments of human learning are considered separately, regulations admirably fitted for one might not be equally applicable or convenient for them all. But the naturalist who begins his scientific life with the study of birds and butterflies can never be sure that either of those fascinating subjects will permanently secure his devotion. His affections may rove away in quite other directions, making him a student now of Protozoa and now of Primates, or onewhile a worshipper of earthworms and presently an authority upon whales. At any rate, whatever may be the varying requirements of individuals, it is the interest of the whole commonwealth of naturalists to have universal agreement as to the scientific names of the objects with which they are collectively concerned. Agreement is the principal thing, therefore get agreement. But to set the ships sailing from all quarters of the globe to capture this one position may not be easy. Each Agamemnon may have to sacrifice some darling Iphigenia if the whole fleet is ever to reach the point proposed.

The International Congress of Zoology, which held its first meeting in Paris in 1889, and has since then at triennial intervals met in Moscow, Leyden, Cambridge, Berlin, and Berne, from the very first took up this subject with the earnestness which it deserves, and is still handling it with commendable vigour and discretion. If this Congress is to be the legislative body for the future and our ultimate court of appeal on the

matter before us, it is desirable that we should clearly understand the source of its authority, its methods of arriving at its decisions, and therein especially the rights and opportunities reserved for appellants under its truly imperial jurisdiction. There is an old opinion that "General Councils may not be gathered together without the commandment and will of Princes," and, further, that when they be gathered together they are still liable to make mistakes. Among the men who have taken a leading part in the International Congress of Zoology there are many who might be rightly claimed as princes of science, and none, I expect, who would wish to make any claim to being infallible. So far, then, the authority, if in a large measure self-constituted, has an origin and status with which English-speaking people are not likely to find very much fault. On the whole we think more of what is well done than of what is logically done. Linnæus himself was a prince only by the divine right of genius, and his system of nomenclature won acceptance, not by act of parliament, but by virtue of sweet reasonableness. In dealing with the Linnean system, it would have been a courteous act, I think, to have invited the various Linnean Societies spread over the globe, and especially the Linnean Society of London, to take a prominent part. As things have turned out, for reasons not very easy to comprehend, Great Britain has been left in its favourite insular position, without any practical voice in the latest proposals. There is, happily, no need for any punctilious jealousy on this score, since it is still open to us to offer whatever criticisms and recommendations we please, with an excellent prospect of their commanding respectful attention.

It has long been agreed—and may we not say very wisely agreed?—that zoology and botany should be independent in respect to generic names, so that a name will not be invalidated in the one because it happens to have been earlier used in the other. The double use of course is merely permitted, not commended or recommended. The zoologist is not encouraged to found a new genus *Rosa* for a camel or a skunk, when he has good authority for believing that they would smell as sweet by any other name. But apart from, or even including, this one article of generic designation, would it not be convenient that zoology and botany should have rules of nomenclature in common, and should use the same symbols for identical purposes?

Nowhere can this question be more appropriately asked than before our Society, which not only in theory but in practice deals impartially with both these great branches of science. We turn from one to the other in the course of a single evening with facile versatility. Sometimes the two find common ground of report and argument. Not seldom one sheds interesting side-lights upon the other. Often by question and answer students of one draw forth from students of the other information and suggestions of value to both.

Professor Blanchard makes a frank and honourable appeal in behalf of the Commission over which he presides. "It is the right," he declares, "almost we might say the duty, of every zoologist to lay before us the difficulties which occur to him. The Commission is not a tribunal issuing absolute decrees, but a committee of philanthropic persons who have made a special study of the principles of nomenclature and have practical experience of the difficulties involved in their application. It examines impartially questions brought before it, seeking the most judicious solution of each problem in conformity with the standing rules, and submitting its answers with the reasons on which they are founded in a report to the International Congress, which then frames its decision in the light of full information." In spirit and expression nothing could be more to the purpose, and there is ground for thinking that the members of the Commission have made the most zealous endeavours to accomplish the impossible task of satisfying everyone. But there is a pregnant phrase in a recent biography of a statesman by a statesman, that "Agreement in principle is of little avail, without driving-force enough for practice" *. To secure this driving-force for practice in regard to the present subject seems to be far from a simple task. These comet-like zoological congresses, that make their dazzling brief appearance once in three years at different points of the scientific firmament, produce a very faint impression on naturalists who happen to be without inclination, means, health, or leisure for travelling, and on those who have no spare guineas to spend on miscellaneous Transactions. The several papers from Berlin and Paris, from Königsberg and Washington, brought under your notice as groundwork for this evening's discussion, may have been widely

* Morley's 'Life of Gladstone,' vol. ii. p. 398.

and liberally distributed. But that distribution is temporary, and almost of necessity limited to the persons who in a sense least need it—that is, to the persons known to be interested in the subject, who would therefore be almost sure to make themselves acquainted with essential items of its literature. Many will remember what happened with the Stricklandian Rules under the auspices of the British Association. They were left without any definite stamp of the issuing authority. They were allowed to go out of print. There was never any effort made in England, so far as I am aware, to impress upon beginners in zoology that any rules existed by which they might conveniently be guided. Editors in France were just as remiss. At least in one conspicuous instance they allowed a writer to load science with barbarous names as well as almost equally strange descriptions.

The Stricklandian Rules adopted the 12th edition of the 'Systema Naturæ,' which began its publication in 1766, as the starting-point for modern zoological nomenclature. The International Rules accept the 10th edition of the 'Systema,' and January 1st, 1758, as epoch-making for the same purpose. Might it not be better, even now, to fix the beginning of the new era in 1751? This would put the dividing-line in the exact middle of the eighteenth century. It would give the 'Philosophia botanica' its due acknowledgment as the leader in a great reform. It would bring into line at least one important work on zoology, Clerck's 'Aranei Suecici,' in which the binomial usage was followed prior to 1758. This last consideration is by no means trivial, for it seems inexcusably ungenerous and improper to set up a standard of nomenclature, and then to invalidate names used in accord with that standard, only because they were published before an arbitrary date. I urge this in spite of a small personal interest which I have in upholding the year 1758, because that is the year in which Borlase published 'The Natural History of Cornwall.' More than once I have maintained that *Astacus* is the proper generic name for the common English lobster. Now Borlase at page 274 of the work just mentioned, after speaking of what he calls the Long Oyster (the *Locusta marina* of Aldrovandi), distinguishes from it "the lobster, or *Astacus verus*, much superior in delicacy of food to the former, and in such plenty on the coasts of Cornwall, that Well-boats come to load, and

carry them to London and elsewhere." From this account there can be no doubt of the species intended. Accordingly the Cornish naturalist, as if with a prophetic eye to future controversy, at the earliest available moment here distinctively proclaims *Astacus* to be the genus of the common lobster, writing as he does in and concerning a county in which the rival claimant, the river crayfish, neither then nor now was ever known to occur. This is only an *argumentum ad hominem*. But it should be conclusive with those who think that, when any large loosely defined genus is eventually broken up into several genera, the original name is bound to go with that species which was first mentioned separately as a member of the genus.

A genus may be founded for a single species, and from that species, as long as the genus stands, it can never be separated. But a genus may be founded for a dozen species, no two of which in process of time are allowed to stand under the same generic name. Then the nice Sadducean question arises, which of the twelve has a right to the name of the original genus, once enjoyed by them all in common? A species indicated by the author as typical has the best claim. A species indicated by him as doubtful has no claim at all. But in old obscurely defined genera these helps are rarely at our command. We must then have recourse to Article 30 of the International code, which provides that, "If the original type of a genus was not indicated, the author who first subdivides the genus may apply the name of the original genus to such restricted genus or subgenus as may be judged advisable, and such assignment is not subject to subsequent change." To this rule are appended certain cautions and useful recommendations. But neither the precision of the rule nor Dr. von Maehrenthal's elaborate comment seems to meet all the problems which ingenuity and research have recently evolved. One might innocently suppose that the author who first subdivides a genus is the author who first subdivides it, and that no more need be said. But in so supposing one is likely to find oneself egregiously mistaken.

To make the matter intelligible, it will be necessary for me to tax your patience by bringing forward concrete examples. You will excuse my taking them from the branch of zoology with which I am most conversant. Do not think it unchivalrous

that on this point I challenge the opinions of a friend, an absentee, a lady. As a matter of fact, Miss Mary J. Rathbun, of the National Museum in the United States of America, if not the foremost living authority on the higher Crustacea, may be held to have in this department of knowledge no living and working superior, and probably no equal except in Major Alcock, a Fellow of our own Society. It is this very pre-eminence on her part that makes it a matter almost of urgency that we should come to an early understanding on the rules of nomenclature with a writer so accurate and copious, so full of knowledge and so deservedly influential as Miss Rathbun. Already no little entanglement has been introduced into synonymy by her acceptance of Latreille's Manual of the Arthropoda *, published in 1810, as a sort of bed-rock for generic subdivision. This book gave a conspectus of genera, many of them defined in the briefest and crudest manner, and concluded with a list in which, as a rule, the name of each genus was accompanied by that of a single species. In the view of Miss Rathbun, this catalogue sealed the fate of all those genera that were open to subdivision, although there was certainly and obviously no intention on Latreille's part to subdivide them. Supposing that he had intended to do so, is it to be conceded that an author may select the type of another man's genus without explaining why he selects it, or whether he has any reason for considering the rest of the species less typical than his chosen type? This matter has been argued elsewhere †. We may pass on to consider a still more startling step in the same direction, announced in the 'Proceedings of the Biological Society of Washington' for December 1904. Therein Miss Rathbun explains that she has become acquainted with Weber's 'Nomenclator entomologicus' ‡, published in 1795; that "under the Agonata or Crustacea, pp. 91–96, many of the genera first described in J. C. Fabricius's 'Supple-

* Considérations générales sur l'ordre naturel des animaux composant les classes des Crustacés, des Arachnides, et des Insectes; avec un tableau méthodique des leurs genres, disposés en familles. Paris, 1810.

† "The late lamented Latreille. A Study in Names." Natural Science, vol. xii. p. 239 (1898).

‡ Nomenclator entomologicus, secundum Entomologiam systematicam illustr. Fabricii, adjectis speciebus recens detectis et varietatibus conscriptus a Friderico Weber Chiloniensi. Chilonii et Hamburgii, 1795.

mentum Entomologiæ Systematicæ,' 1798, are enumerated, and as they are accompanied by lists of species most of which were previously known, the genera themselves must date from 1795 instead of 1798." She adds that "this has already been brought out by Sherborn in his 'Index Animalium,' 1902."

When Weber's book itself is examined, it seems to be the most extraordinary ground that ever was taken for throwing synonymy into needless confusion. In his preface he makes this statement:—"The discerning naturalist Daldorf will shortly publish a very important work on the Agonata. Meantime in this Nomenclator I have so named and denoted them, as the celebrated Fabricius will hereafter accept them. But more distinct characters of these genera will be set forth in the book presently to be published by Daldorf." Further on he says:—"In these Agonata you will find a quantity of new species. With these and others, which Fabricius, since the publication of his 'Entomologia Systematica,' has newly described, and will by and bye publish in the form of a supplement, he has been pleased to supply me, whereby the Nomenclator has been augmented with many new species." Weber's catalogue, it may be said, is confessedly the work of a busybody. He was allowed freely to examine the collections of his friends Fabricius and Daldorf. From them he borrowed provisional manuscript names of genera and species, and hastened to inform the world that such and such systematic and nominal changes were about to be adopted by his distinguished friends. Does this prediction, which in several instances was falsified by the event, attach any status or disability to those undefined generic names about which his false prophecies were made? In 1801, when Weber wrote on genera of insects which he had himself established *, he makes no claim or allusion to any genus of the Agonata. It would have been strange indeed, in a work which he dedicates to Fabricius with the most affectionate expressions, had he claimed genera which could only have been his by a scandalous theft from his much-eulogized friend. In the Index to the 'Entomologia Systematica,' published in 1796, there is a half contemptuous footnote-reference to the 'Nomenclator.' The Index itself mentions the new generic names about to be used in the 'Sup-

* 'Friderici Weberi Soc. phys. Ienens. adscr. Observationes entomologicæ, continentes novorum quæ condidit generum characteres, et nuper detectarum specierum descriptiones.' Kiliæ, MDCCCI.

plementum,' but merely as a civil invitation to other naturalists not to interfere with them—a very superfluous precaution if they had been already preoccupied by Weber. When the 'Supplementum' was published in 1798, it took no notice whatever of Weber's unauthorized programme. Miss Rathbun now wishes to re-introduce it as a dominant though very confounding force in carcinology. Are we to accept the ruling that a genus will be well founded if an author publishes the simple statement that another author proposes at some future time to use such and such a generic name for such and such previously-known species? By answering yes, you would, I conceive, put a weapon into the hands of idle, ignorant, mischievous persons who might soon make you regret the response.

But the rejection of Weber's catalogue as valueless still leaves open for consideration a point of some importance. It has, in fact, been hitherto the privilege of naturalists, in separating a species of which the distinctive characters are known, to establish a new genus for it by simply referring to the work in which those distinctive characters have been already published. They practically become the definition of the new genus, merely being raised from specific to generic value. But this privilege, more conducive to slovenliness in authors than to contentment in their students, is open to great abuse, should the new genus be created not for one or two species but for a considerable number. Would it not be well that the privilege should be strictly defined or cancelled—for the future?

In the last proposal stress is laid on the words "for the future." We cannot come to an agreement with posterity. We cannot bind our successors. But by equity towards the past we may win some title to equity from the future. Now, in the early Linnean time, as you know, one generic name often covered an enormous number of species. The genus *Cancer*, for example, included all the crabs and lobsters and shrimps and some other things, which are now dispersed over hundreds of genera in several orders and numerous families. When the necessary breaking up of an unwieldy genus began, it was a common practice, in endowing a particular species with separate generic rank, to adopt its specific name for the new genus and to bestow upon the species itself a new specific name. Thus the common shrimp, *Cancer crangon*, Linnæus, became *Crangon vulgaris*, Fabricius. When this was done, there was no rule against doing it. But

now the rule is set up that the old specific name must be restored, so that the species will be *Crangon crangon* (Linn.). Coupling this determination with new rules about subgenera and subspecies, it is apparently possible to have a creature called *Apus* (*Apus*) *apus apus*, which seems to me calculated to bring nomenclature into contempt. The equitable plan would be to accept the terminology which our scientific ancestors employed in *Crangon vulgaris* and the like, while ruling that *in future* specific names are to be left in their places and not transferred to a higher grade. This is not setting aside the essential law of priority, but upholding in the interest both of equity and euphony what our predecessors did, when they had a perfect right to do it, against *ex post facto* legislation.

Some points more easy to follow in print than in speech are relegated to an appendix. My main argument has been directed to enforcing upon your attention the overwhelming importance of agreement, the difficulties in the way of arriving at it, the desirability of keeping naturalists in touch with the best conclusions, and, finally, the claim which the subject of scientific nomenclature in its broadest aspects has upon the interest of this Society. None have a better right, none have a higher duty than ourselves to work for the improvement of the Linnean code till it wins the consent of naturalists in general as the best and most polished instrument of its kind for the advancement of science.

## Appendix on points of detail.

1. To signify that a specific name is combined with a generic name other than that with which it was originally published, might not botanists and zoologists agree to have a method of notation in common ?

2. To simplify synonymy, it is suggested that all new generic names of animals should be regarded as of the masculine gender. It is no essential part of natural history to discover that *Melicerta* is masculine, *Ino* feminine, *Callisoma* neuter; that *planus* and *plana* are adjectives, but *nanus* and *nana* substantives; or that you may say *longimana*, to signify long-handed, although *mana* in Latin means, not a hand, but a goddess or a sponge.

3. In regard to generic and specific names of more than two syllables, it would be a boon, at least to English-speaking

people, to have the proper pronunciation indicated by some accentual mark, as *Chenalópex*, *Callisóma*, *Rhizóstoma*. The use of the letter *k* in such names as *Ancístrodon* and *Cárcinus* is rather to be commended than deprecated. In the transcription of other Greek words the use of the letter *h* to represent the aspirate may be desired but should not be enforced, and the representation of the diphthongs *ει* and *ου* by the same letters in the Latin alphabet, if not enforced, should at least be legalised. A rule which leads to such a form as *Möbiusi*, and which rests apparently on a distinction between Latin down to the close of the eighteenth century and later Latin, carries its own condemnation.

4. The common practice of printing generic and specific names in italics is open to the objections that this character is less easy to read than Roman type and does not wear so well. It is suggested that some other distinctive type, which is not open to these objections, should be recommended.

5. A recommendation following article 36 of the International Rules implies that *caeruleus* and *coeruleus*, *silvestris* and *sylvestris*, *littoralis* and *litoralis*, *autumnalis* and *auctumnalis* may be held valid for pairs of species in the same genus. That would be very objectionable, seeing that these are only alternative spellings of the very same words, not at all comparable with such pairs as *fluvialis* and *fluviaticus*, *sinensis* and *sinicus*, *ceylonicus* and *zeylonicus*, words of the same meaning but perfectly distinct formation.

6. In reference to article 21 of the International Rules, the question arises whether for the future some rule might be formulated by which an author's claim to priority for new names of genera and species should rest, not unconditionally on date of publication, but on the date of reasonable publication. In 1885 Sarato published a new genus and species *Ligur Edwardsii* in 'Le Moniteur des Étrangers,' a weekly journal at Nice (see Senna, Bull. Soc. Ent. Ital. vol. xxxiv. p. 319, 1903). But, apart from scientific work published in an unsuitable medium, the rule in giving validity to a name published "in connection with an indication" seems vague and open to abuse by ignorant or even mischievous persons.