
17 Celebrating 250 Dynamic Years of Nomenclatural Debates

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By promulgating today these *Règles internationales*, the International Congresses of Zoology do not have the pretension to have accomplished a definitive work. Exactly as the rules that sufficed in Linné’s time could not meet our needs, the code that seems to us to appropriately answer our current concerns will be regarded as inadequate by our successors. Science moves forward: it asks new questions, for which new solutions must be found. (Preface to the first *Règles internationales de la nomenclature zoologique*, Blanchard in ICZN 1905:11, tr.)

INTRODUCTION

Biological nomenclature is fundamentally important to all biological sciences and beyond, because it ensures that one taxon is designated by only one name and that one name refers to only one taxon. This is what Darwin understood when he accepted to be one of the eight members of the British Association committee that in 1842 devised the first code of zoological nomenclature (Strickland 1843). Biological nomenclature, which governs the application of taxon names across the Tree of Life, is one of the pillars of the language of taxonomy and, therefore, more broadly, all biological sciences—other important pillars of taxonomy are found in the philosophy of classification, especially set theory and logic, and, naturally, evolutionary theory. Whenever we refer to the common housefly as *Musca domestica* and not as 10-7-26-081-052-0325761, as was once proposed, we follow nomenclatural rules.

Most biologists would probably acknowledge that nomenclature helps guarantee clarity of communication about taxa, at least in principle. However, nomenclature is poorly understood. For instance, name changes, which taxonomists regard as a sign of progress, are a common source of frustration for many users of names (e.g., Blackwelder 1948; Barnett 1986; Erzinclioglu and Unwin 1986; Feldmann 1986; Tubbs 1986; Dubois 1998). Also, nomenclature is often perceived as complicated, perhaps too complicated, and this perception is not unfounded. In fact, it can be

so complicated that even taxonomists deal with it reluctantly. It requires a great deal of time and patience to be acquainted with all the minute details of the entire literature published on a certain taxon as well as other necessary information (e.g., type material), and fewer and fewer of us can afford the luxury of being scholars. Also, solving nomenclatural issues can be quite tricky, and not everyone is familiar with nomenclatural rules: for instance, how many taxonomists could define allotype, cotype, genotype, hapantotype, holotype, lectotype, neotype, paralectotype, paratype, syntype, and topotype?

Also, most biologists probably perceive our codes of nomenclature as completely static, as if they were written in stone. This perception may result from the idea that since nomenclatural rules aim at promoting stability of taxon names, they ought to be as stable as possible. However, this perception is unfounded. As surprising as it might seem, nomenclature has always been and still currently is dynamic. In fact, one of the fathers of the *International Code of Zoological Nomenclature* (ICZN), Raphaël Blanchard (first president of the International Commission on Zoological Nomenclature from 1895 to 1919), made it clear in the Preface to the first edition of the *Règles Internationales de Nomenclature Zoologique*, that there is no reason that nomenclatural rules should remain unchanged over time (and many changes have actually been introduced in the codes throughout the years):

By promulgating today these *Règles internationales*, the International Congresses of Zoology do not have the pretension to have accomplished a definitive work. Exactly as the rules that sufficed in Linne's time could not meet our needs, the code that seems to us to appropriately answer our current concerns will be regarded as inadequate by our successors. Science moves forward: it asks new questions, for which new solutions must be found. (Blanchard in ICZN 1905:11, tr.)

Beyond changes introduced in the codes, the main reason that nomenclature is dynamic is that it is characterized by a long history of intense debates, some of which led to major changes in nomenclatural practices. For instance, the starting point of zoological nomenclature, 1758, celebrated in this volume, has been actively discussed. As everyone knows, the starting point in today's zoological nomenclature is the 10th edition of Linné's (1758) *Systema Naturae*, "deemed to have been published on 1 January 1758." (Article 3; ICZN 1999). Animal taxon names published in the 10th edition of *Systema Naturae* have precedence over names published in or after 1758, with the exception of the names published in Clerck's (1757) *Aranei Suecici* [Spiders of Sweden] also "deemed to have been published on 1 January 1758." What fewer people know, however, is that it took zoologists 50 years to decide that the 10th edition of the *Systema Naturae* should be the starting point for zoological nomenclature. Many alternative starting dates (e.g., 1700, 1722, 1735, 1751, 1760, 1766, 1767, 1871) were proposed between 1842, when the British Association committee on zoological nomenclature devised the Strickland Code (in which the starting point is not 1758), and 1892, when the Second International Congress of Zoology (Moscow) finally adopted the 10th edition of the *Systema Naturae* as a starting point for zoological nomenclature. Naturally, some authors also thought that no starting date was needed.

One of the goals of the present contribution is to show that celebrating 250 years of Linnaean nomenclature means celebrating 250 dynamic years of debate. Nomenclatural concepts and rules with which we are familiar today have developed through a slow historical process, and their adoption was in most cases preceded by warm discussions in which opposite views were actively defended. The history of any major nomenclatural issue (principle of priority, typification, grammar and spelling, gender agreement, ranks, suffixes, etc.) illustrates that nomenclature has not always been what it currently is.

However, not all nomenclatural issues can be reviewed here. Therefore, to illustrate that nomenclature has always been dynamic, the present study focuses on: (1) the development of the Principle of Priority throughout the 19th century, from its first definition by de Candolle (1813) up to its application in the first *International Rules of Zoological Nomenclature* (ICZN 1905) and the implementation of a new article altering a strict application of priority under specific conditions in the first

edition of the Code (ICZN 1961); and (2) the debates that took place in the 1960s, as a consequence of the emergence of numerical taxonomy and the early use of computers. The debates about priority are analyzed here in detail because priority is certainly one of the most fundamental concepts in nomenclature. Also, the history of the Principle of Priority is particularly interesting because disagreements over its application led to the coexistence of several codes of zoological nomenclature in the second half of the 19th century. The 1960s were a period of great nomenclatural creativity in which some of the most radical ideas were proposed, such as a system of numerclature in which numbers would replace names.

The two historical accounts presented in detail here show that debates have always been one of the main factors of progress in nomenclature, both from a practical and theoretical point of view. Indeed, it is because established nomenclatural practices have continually been challenged that new rules and recommendations have been introduced. Naturally, not all new proposals ended up as part of an actual rule or recommendation. However, regardless of whether a challenging idea had any impact on actual nomenclatural rules, all new ideas and debates have positively impacted our understanding of nomenclatural concepts, by uncovering new issues to address or simply stimulating discussion. For instance, theoretical work on the philosophy and logic of the concept of type specimen (Heise and Starr 1968) was published after Oldroyd (1966) suggested that type specimens be abandoned.

Another goal of this chapter is to discuss today's nomenclatural debates within a long-term historical context, by using as a general approach the lessons we learn from the history of nomenclature. History of nomenclature provides us with two main lessons. First, nomenclature has always remained dynamic: as a set of rules or recommendations, nomenclature has changed; also, established practices have always been challenged and debated. However, this dynamism takes place within a slow and long-term time frame: again, no fewer than 50 years were needed for zoologists to make a final decision on the starting point for zoological nomenclature. So, in nomenclature, things require time to happen (or not to happen, for that matter). Second, all new ideas and proposals, even those that seemed "pure folly," have participated in increasing our understanding of nomenclatural concepts. History thus provides us with a new, interesting approach to our current debates. Indeed, nomenclature is undergoing serious discussion, partly thanks to the emergence of the PhyloCode (Cantino and de Queiroz 2007 and references therein), which has generated some of the most heated debates in the field of systematics since the 1990s. Discussions have also been generated by supporters of the current rank-based zoological code (e.g., Dubois 2006, 2008), especially with the possible unification of the three rank-based codes into a BioCode (e.g., Greuter et al. 1996, 1998; Hawksworth 1997).

In the early history of the International Commission on Zoological Nomenclature, Blanchard thought that new nomenclatural questions might be discovered and need to be addressed in the future. New questions may come from new theories or methodologies: numerical taxonomy generated serious nomenclatural debates in the 1960s; phylogenetic nomenclature is regarded by its founders (de Queiroz and Gauthier 1990, 1992, 1994; for a complete bibliography, see Cantino and de Queiroz 2007) as an extension of Hennig's (1966) Phylogenetic Systematics. In any case, after the publication and official implementation of the PhyloCode, two codes will then coexist (ICZN Code and PhyloCode) for zoologists. Possible scenarios for the long-term future of this coexistence are discussed here and placed in a broader historical context.

A HISTORY OF THE DEVELOPMENT OF THE PRINCIPLE OF PRIORITY

EARLY NOMENCLATURE: FROM LINNAEUS'S APHORISMS TO DE CANDOLLE'S *THÉORIE ÉLÉMENTAIRE*

Linnaeus's Aphorisms

The development of Linnaeus's nomenclature is not presented in detail here. However, a few important points that deeply impacted the development of nomenclature during the 19th century are

briefly mentioned, especially because several Linnaean dates (e.g., 1735, 1751, 1758, 1766, and 1767) were discussed as possible starting points for zoological nomenclature.

Linnaeus's (1751) aphorisms from the *Philosophia botanica* served as the basis for nomenclatural practices up to the 1840s in zoology, when the Strickland Code was devised (Strickland 1843), and up to the 1860s in botany, when de Candolle (1867) published the first botanical rules (Nicolson 1991). Agassiz (1842–1846) still cited them as an authority in his *Nomenclator Zoologicus*.

Linnaeus developed his nomenclatural aphorisms in several books, starting with the *Critica botanica* (1737), which constituted a development of the Aphorisms #210–324 from the *Fundamenta botanica* (1736). The *Critica botanica* is divided into four parts. The first, entitled *Nomina generica* (Aphorisms #210–255), deals with the formation of generic names. The second part, *Nomina specifica* (#256–305), does not deal with our current specific names (or epithets), but with the diagnostic phrases then used to express the *differentiae* among species; in fact, this section is entitled “*Differentiae*” in the *Fundamenta botanica*. The third part, *Nomina variantia* (#306–317), deals with varietal names. Finally, the fourth part, *Nomina synonyma* (#318–324) deals with synonyms, i.e., different generic, specific (as *differentiae*), or varietal names given to the same plant.

A close examination of the fourth part is particularly interesting because it tells us how Linnaeus dealt with synonyms. In that regard, it clearly demonstrates that, as pointed out by several authors (e.g., Lewis 1875; Dall 1878; Nicolson 1991), the Principle of PArriority as we understand it today was absent from Linnaeus's Aphorisms. Linnaeus's main interest was to find the *best* name for a plant: “If it is decided that none of the synonyms is really suitable for the plant, then necessity compels us to make up a new one.” (Linnaeus 1737:259; tr. Hort 1938:209) In the first three sections, Linnaeus provided recommendations on how to form good generic, specific (again, as diagnoses), and varietal names. In any case, the notion that the oldest synonym should have precedence over more recent ones did not exist in Linnaeus's *Critica botanica* (1737); nor did it exist in his cornerstone *Philosophia botanica* (1751), in which binominal nomenclature was described.

In 1751, Linnaeus described binominal nomenclature as the association of a generic name and a uninominal “trivial” name. The “specific” names then still referred to the polynominal *differentiae*, or specific differences; it is only later that Linnaeus referred to the trivial names as “specific” names, and Lamarck (1778:lxxxiv), for instance, insisted on the fact that trivial names should be more properly called specific names. However, specific names (or epithets) were commonly referred to as “trivial” names throughout the 19th century (e.g., Wallace 1874:259). Although Linnaeus associated generic and trivial names in binominals before 1751 (Stearns 1959), such as in his *Pan Suecicus* (Linnaeus 1749), he consistently adopted binominal nomenclature for the first time in *Species plantarum* (1753) for plants, and then in the 10th edition of the *Systema Naturae* (1758) for animals and minerals. By combining generic and trivial names, Linnaeus's revolutionary idea was to separate two roles of names, the designation and the diagnosis, which had remained mixed in the polynominal names in phrases until then (e.g., Stearns 1959).

In Aphorism #257 of the *Philosophia Botanica*, Linnaeus (1751) clearly defines a trivial name as a single word (*vocabulo uno*). However, more importantly, he also indicates that trivial names do not follow any laws and can be selected freely (*vocabulo libere undequaque defunto*). This largely explains why Linnaeus changed some specific (trivial) names between the 10th and the 12th edition of the *Systema Naturae*, which generated so many debates in the 19th century as to which edition should be used as a starting point in zoological nomenclature. Also, as pointed out by Dall (1878:14), binominal nomenclature remained an accessory matter for Linnaeus, who mainly focused on *differentiae* (the polynominal specific names), which he regarded as more informative, being more complete than the trivial names. This is particularly obvious in Aphorism #257, where Linnaeus distinctly claims that trivial names are left aside from his aphorisms because specific differences are his main concern.

This leads to what is probably the most important aspect of Linnaeus's aphorisms, i.e., whether they were meant to be strict rules or just recommendations. Linnaeus only occasionally referred

to his aphorisms as rules, such as at the end of the preface (no pagination) of the *Critica botanica*, where he claims that those rules (*regulas*) apply in the animal and mineral kingdoms as well as in the vegetable kingdom. However, as clearly illustrated by his treatment of synonyms, Linnaeus did not accept any objective rule for selecting a name among several synonyms; authors have to choose the *best* name, and even create a new name if no good name is available. Thus, most of Linnaeus's aphorisms explain how to form or choose a good name and in practice most of them are recommendations instead of firm rules. When Verrill (1869:92) complained about the fact that authors have ignored the “most essential laws” proposed by Linnaeus and “made sacred by the usage of the best naturalists of the past century,” he missed the fact that, actually, Linnaeus's aphorisms were for the most part subjective and, in fact, directly allowed flexibility.

Since Linnaeus allowed name changes, there was no strict application of “precedence” in his aphorisms. This is an important point that deeply impacted nomenclatural debates throughout the 19th century. Indeed, following Linnaeus's model, up until the 1830s, naturalists freely replaced existing names by new—and supposedly better—names. Some authors, however, complained about those endless and unjustified name changes. In fact, one of the main purposes of the Strickland Code (1843) was to help end, or at least restrict, name changes by adopting the Principle of Priority. Then, starting in the 1840s, the question became whether priority should apply without or with exceptions.

Early Reactions to Linnaeus's Nomenclature

Although binominal nomenclature was widely used by most naturalists soon after Linnaeus proposed it, a few individuals refused to adopt it. Albrecht von Haller continued to use names in phrases even in his late works (e.g., Haller 1768). De Candolle (1813:224–225, tr.) mentioned that Haller's students were using the numbers that Haller had given to his plants instead of his phrases but that “everyone agreed that a specific name was better than an arbitrary and insignificant number.” The same kind of argument was used in a different context by authors opposed to the numerical system of nomenclature proposed in the 1960s. Stearns (1959:12) mentions that Linnaeus and his students were using a similar system before 1753; during an excursion, in 1748, species were designated as “*Galium* 119,” the latter number referring to an entry in Linnaeus's (1745) *Flora Suecica*.

According to De Candolle (1813:225, tr.), several naturalists, in particular Buffon, thought that species names should be “independent from classification” because species are too often placed in different genera by different authors. Generally speaking, however, Buffon was not interested in classification, nor was he interested in names. Buffon thought that all methods and systems of classification are arbitrary because intermediary forms prevent classifying organisms in distinct groups (Roger 1993). Also, in an article on the description of animals, Buffon (1753:114–115) argued that one should know organisms completely before one gives them a name, and that describing organisms is more fundamental and necessary than naming species and groups.

Michel Adanson, in his *Histoire du Sénégal* (1757:xvi–xix), adopted binominal nomenclature, but was strongly opposed to the use of meaningful specific names because, according to him, such names usually become erroneous once the species is better known. In fact, Adanson (1757:xvi) insisted that we should make sure that names are not built from any recognizable etymological roots. Thus, new names created by Adanson had no classic etymology, such as *Yetus* Adanson (1757:cxiii, 44), which refers to a seashell. This approach would have been a radical way to avoid the endless debates that took place in the 19th century on whether Latin grammar and orthography should be strictly enforced. Interestingly, Adanson seemed to regret unjustified name changes, an issue that caused much ink to flow in the 19th century: “We must conserve the old [names], especially those that seem good, and that have been adopted by the masters of the art” (1757:xv, tr.).

Finally, Bernard de Jussieu was another famous Parisian naturalist who refused to adopt binominal nomenclature, although he let his nephew Antoine-Laurent de Jussieu, and André Thouin, use it in *Jardin du Roi* (Dayrat 2003).

Nomenclature in Lamarck's *Flore Française*

Lamarck (1778:lxxxii–lxxxvii) gave a few general remarks on nomenclature in the *Discours préliminaire* of his *Flore française*. First, Lamarck (1778:lxxxii) complained about name changes, which make nomenclature the most difficult part of botany. Lamarck also provided a few recommendations on how to form appropriate names. Generic names should not be precise; for instance, *Potentilla* should be preferred to *Quinquefolium* because some species do not have compound leaves with five leaflets. Specific names (epithets) should be as precise as possible and help give a distinctive idea of the species referred to, such as *Menianthus trifolia*. Finally, misleading names should be avoided, such as in *Euphorbia spinosa* (which bears no spines). However, Lamarck did not say a word on what to do if a name is not appropriate. Should it be replaced, or kept? As we shall see, it took several decades before zoologists could agree on the action to be taken.

Fabricius's *Philosophia botanica*

Fabricius (1778:101–121) provided 46 aphorisms of nomenclature in his section on *Nomina* of his *Philosophia entomologica*, which were largely inspired by Linnaeus's (1751) aphorisms. Thirteen canons deal with *Nomina trivialia*, i.e., our specific names. In canon #45, Fabricius (1778:121) asserts that “Trivial names ought never to be changed without the most urgent necessity (*Nomina trivialia nunquam absque summa urgente necessitate mutanda sunt*).” This canon, which seems close to the concept of priority, was quoted by Strickland (1835:36) in the beginning of one of his first articles on nomenclature.

Bergeret's *Phytonomatotechnie universelle*

Jean-Pierre Bergeret's system of nomenclature, exposed in his *Phytonomatotechnie universelle* (Bergeret 1783–1785), now a rare book (Dayrat 2003), is radically different from anything proposed before and even after him. Bergeret's main idea was to build species names based on characters found in each species (each character state being coded by a particular letter, and the characters following each other in a precise way). Although Bergeret's system was quite logical, it yielded unusual “names” with nearly unpronounceable sounds, such as “LUPXYGVEAHQEZ,” for our *Eranthis hyemalis* (Linné) Salisbury. Bergeret's method was never adopted. As Rozier (1783:224, tr.) pointed out in his review of Bergeret's *Phytonomatotechnie*: “The most well-made methods are sometimes very awkward in their application.” Nonetheless, Bergeret's method remains an original attempt at making nomenclature more logical.

Du Petit-Thouars' Innovative (But Never Adopted) Nomenclature

Although Aubert du Petit-Thouars started presenting his studies before the members of the Institut de France as early as 1806, and even published some of them, the publication of his major work on orchids was delayed. A report evaluating his manuscript in 1806 indicated that it still was a draft and that du Petit-Thouars needed a great deal of time to complete it (du Petit-Thouars 1817:153–156). The referees who wrote this report also noted his unusual nomenclature (Desfontaines, Richard, Bosc in du Petit-Thouars 1817:156, tr.): “We find more disadvantages than advantages in adopting such a nomenclature, which, by the way, would turn science upside down.”

Du Petit-Thouars' system of nomenclature was described in detail in his *Histoire particulière des plantes Orchidées* (1822). His goal was to build a system of nomenclature that would be more logical, based on a model of chemical nomenclature defended by French chemists (du Petit-Thouars 1817:168). His fundamental idea was to use the same suffix for each generic name that would belong to the same family. For the orchids, the suffix *-orchis* (which he also spells *-orkis*) was used to build names such as *Dryorchis*, *Amphorchis*, *Satorchis*, *Habenorchis*, and *Stellorchis*. Some of du Petit-Thouars' new names replaced existing names (e.g., *Habenorchis* replaced *Habenaria* Willdenow). Du Petit-Thouars also built species uninomials based on part of the specific name and part of the generic name. For instance, on Plate 4 of his *Histoire particulière des plantes Orchidées*, one reads

three names: *Amphorchis* (the generic name), *Amphorchis calcarata* (the Linnaean binomial), and *Calcaramphis* (his newly invented uninominal species name), which supposedly helped retrieve or name more efficiently the species illustrated. Naturalists were not interested in the logic of du Petit-Thouars' new system. They saw only its disadvantages: "This ingenious method presents almost all the disadvantages of that of Bergeret." (De Candolle 1813:226, tr.)

De Candolle's (1813) First Definition of the Principle of Priority

Augustin-Pyramus de Candolle is a central author in nomenclatural history because he provided one of the first definitions of the principle of "priority" in his *Théorie élémentaire de la botanique* (1813), a cornerstone in the history of systematics (it also is in this book that de Candolle coined and defined "*taxonomie*," as the *théorie des classifications*, i.e., classification theory). Augustin-Pyramus de Candolle was the father of Alphonse-Pyramus de Candolle, one of the fathers of the code of botanical nomenclature.

De Candolle (1813:227) mentioned "*onomatologie*," a term then more or less synonymous with "dictionary" (it was often used in titles of medical dictionaries), as a synonym of nomenclature. De Candolle also gave Bergeret's "*phytonomatotechnie*," which literally refers to a "technique to form plant names," as a synonym of nomenclature. This suggests that, according to de Candolle, nomenclature had two meanings: (1) a list of names and (2) the formation of names, or the act of forming names. Today, nomenclature often also refers to the rules that govern the use of names. However, for de Candolle, "nomenclature" does not seem to cover the rules of nomenclature, to which he refers as the rules of nomenclature (*règles de la nomenclature*). So, when de Candolle (1813:227, tr.) claimed that "the goal of nomenclature of natural history is to be universal, common to scientists of all nations," he primarily meant that names should be universal and stable; by extension, the rules should also be common.

De Candolle (1813:227–228) mentioned three primary principles: (1) names must be in Latin; (2) names must follow basic grammar (e.g., names mixing Greek and Latin roots can be replaced); (3) the first name given to a species must remain unchanged, unless it is already in use for another species or contradicts some essential rules of nomenclature. De Candolle (1813:228–241) mainly focused on the formation of generic names (*noms des genres*); if based on a character, the latter cannot be in contradiction with any of the species contained in the genus; mythological names can be of some help if one is really in despair; the name of a scientist can be used, but only for those who contributed to science; anagrams or names made of letters randomly selected (as in Adanson's work) should be avoided. According to de Candolle (1813:241–246), finding a specific name (*nom d'espèce*) is much easier. A specific name could be changed only in cases where it was in complete contradiction with the plant itself: the name *Lunaria annua* should be changed into *Lunaria biennis*. Finally, de Candolle (1813:247–250) gave a few recommendations on family and variety names. All those rules were abundantly discussed by naturalists throughout the 19th century—in particular, should the "indelicate" names (mixing Greek and Latin) be changed?

More importantly, de Candolle (1813:250–251, tr.) defined for the first time the term priority (*priority*): "One must always adopt the oldest name, except in the following cases..." Naturalists did not stop asking throughout the entire 19th century whether priority should apply with or without exceptions. De Candolle considered three main exceptions: a name that is completely inappropriate (such as in *Lunaria annua*); a name that is not binominal or in Latin; a name already in use (in animals or plants). Also, de Candolle insisted on the fact that when a species is transferred from one genus to another the specific name should not be changed, and that a plant name should be published and accompanied with a description (catalogues of names are excluded). As we shall see, Strickland's (1835, 1837a) priority rules were similar to those of de Candolle.

Although de Candolle (1813) provided a detailed analysis of the principle and application of priority, the latter was already in use in that period, both as a term and as a concept. For instance, Spinola (1811:145, tr.) asserted: "As for nomenclature, priority is my law. It also is that of M. Latreille and French entomologists." Lewis (1875:ii), strongly opposed to a strict application of priority argued:

Although the principle had been started some twenty years before, I believe it is the fact that until the British Association Rules of 1842, “priority” to intents and purposes remained a theory.

It certainly is true that replacing existing names that were not “good enough” was a common practice among naturalists in the first part of the 19th century. However, it is not true that priority remained just a theory during that period, in which several clear references to priority exist (e.g., Spinola 1811:145; Swainson 1821:pl. 42; De Candolle 1822:143, 1829:19–20; Bennett 1830:243–244). Brongniart (1827:80) also regretted that the law of priority did not apply more strictly in mineralogical nomenclature.

Dejean (1825:x) pointed out a conflict between two opposite trends in nomenclature: (1) the maintenance of names widely used, although more recent than older but forgotten names (which Dejean supported); (2) a strict application of priority and the use of the oldest name regardless of the situation (which Latreille supported, according to Dejean). Debates between proponents of “priority” and proponents of “convenience” remained stormy throughout the 19th century. It is in this context that Strickland originally dedicated himself to devising rules for zoological nomenclature.

TOWARD THE STRICKLAND CODE

Strickland’s (1837) Rules

At the initiative of Hugh Strickland, it was resolved in 1842 by the British Association for the Advancement of Science that a committee be appointed to consider rules for zoological nomenclature. The history of this committee (its members, its meetings, etc.) is well documented (Sclater 1878; Verrill 1869; Melville 1995). Rules were discussed and adopted at a Manchester meeting in June 1842, and published the following year (Strickland 1843; Sclater 1878). Strickland, who deserves most of the credit for those rules, started working on nomenclatural rules in the 1830s.

In 1835, Strickland published a brief article in which he argued that the practice of replacing or altering generic and specific names without any compelling reason was detrimental to science:

Surely, then, the evil of changing a name, which has once become current among naturalists, is much greater than any advantage supposed to result from substituting a term which is ‘more appropriate’” (Strickland 1835:38).

Indeed, naturalists then commonly replaced an existing name by another, knowing that both names referred to exactly the same entity. So, those replacement names were different from our synonyms, created in most cases by authors who ignore the existence of a former name (or disagree on species limits). Strickland (1835:39) referred to those replacement names as “aliases.” Strickland thought that, in order to prevent the arbitrary replacement of names from worsening

It would (...) be highly desirable if an authorized body could be constituted, to frame a code of laws for naturalists, instead of the present anarchical state of things in which every one does that which is right in his own eyes.

As Spinola (1811), de Candolle (1813), and others, Strickland (1835) adopted the Principle of Priority as his primary law of nomenclature, but accepted exceptions similar to those proposed by de Candolle.

Another issue that Strickland wanted to address was the fact that, in addition and in relation to the existence of “aliases,” different names could refer to the same entity in different countries, which Westwood (1837:169), a member of the 1842 British Association committee, illustrated by several examples, such as: “The giant beetles, *Hercules*, *Actaeon*, etc., with which Linnaeus commences the insect tribes, are named *Scarabaeus* in France, *Geotrupes* in Germany, *Dynastes* in England.” Westwood (1837) also linked the application of the Principle of Priority of generic names

to type species, something that became heavily debated in the second part of the century, especially in the 1870s.

In 1837, Strickland published some Rules for Zoological Nomenclature, which have “little pretension to originality, but ... are selected from the writings of several naturalists, especially from the birds of Mr. Swainson, many of whose aphorisms are adopted here” (Strickland 1837a: 173). Indeed, the previous year, Swainson published a 19-page book chapter, “On the nomenclature and description of birds,” in which he proposed rules distributed in 24 paragraphs: “The necessity of nomenclature being regulated by fixed laws has been advocated by Linnaeus, Fabricius, and all the best systematic writers, both in zoology and botany” (Swainson 1836:228). Strickland’s (1837a) first proposed rules were similar to the rules officially adopted by the British Association, especially regarding the law of priority, which would always apply except in the cases mentioned by de Candolle (1813). However, Strickland (1837a) did not mention any starting point for zoological nomenclature.

Ogilby and Strickland Debate (1837–1838)

Naturally, reactions to Strickland’s (1837a) rules were soon published. In particular, a debate took place between Strickland and Ogilby (a future member of the British Association committee). Strickland (1837b:605) regretted that Ogilby did not adopt a “very convenient rule, now generally adopted by naturalists, that the name of a *family* ... should be compounded of the name of the most typical or best known genus contained in it, with the termination *idae* or *adae*.” Ogilby (1838a:150) reprobated the use of the suffix *-idae* for family names, which was not included in Strickland’s (1837a) original set of rules, as well as the rest of Strickland’s (1837a) “arbitrary, dogmatical, unfounded and unnecessary rules,” such as the fact that names should be taken either from the Latin or Greek languages because it is “opposed to the genius and spirit of all languages, ancient and modern, and directly contradicted by the practice of the Greeks and Romans themselves” (Ogilby 1838a:153). This issue remained debated for many years. Naturally, Strickland (1838a:198) replied and tried to “do [his] utmost to preserve a temperate tone in the discussion.” Strickland (1838a:200) explained better what he intended to achieve when submitting rules to naturalists “not as an *act* for their guidance, but as a *bill* for their approval.” Strickland also proposed that naturalists

form a congress, or in humbler phrase, a committee of naturalists from all parts of the scientific world, to draw up a code of zoological laws, not indeed to be like those of the Medes and Persians, yet to be adhered to for the sake of order and convenience. (1838a:199)

This constituted one of the first mentions of the necessity of an international body of experts for nomenclatural rules.

In the context of this debate, Ogilby (1838b:281) and Strickland (1838b:330) exchanged ideas on the definition of type species or type genus. They both thought that “type” was synonymous with “example.” Ogilby (1838b:281) mentioned that “if the word type [is] merely synonymous with example, [he sees] no objection to it.” Strickland reassured him that “by the type of a genus [he means] that species which is usually selected as an *example* of the genus.” However, a few questions came next that were all abundantly debated for several decades. How do we determine which species (for the type of a genus) or genus (for the type of a family) “afford the best sample of the characters” (Strickland 1838b:330) on which the genus or family is based? Also, how can one determine which species should be the type when no type was designated originally?

Strickland (1838c) and Ogilby (1838c) exchanged a few more thoughts. Their conclusions are memorable. Ogilby (1838c:493) called for less dogmatism in nomenclature:

If it tend to open the eyes of zoological legislators to their own fallibility, and to make them a little more moderate in their pretensions and a little less dogmatical and inaccurate in their statements, the controversy will have accomplished a very great *desideratum*, and fulfilled my intentions.

Strickland (1838c:555) wished to add “a few “more last words” on this exhausted subject,” which, we know now, may never be exhausted. That first debate ended constructively: both gentlemen participated in the British Association committee on rules for zoological nomenclature.

The Strickland Code (1843)

The Strickland Code was first published in 1843 in the *Report of the British Association*, as well as in the *Philosophical Magazine*, and the *Annals of Natural History*. It was published again with a few revisions in 1865, under Jardine’s supervision. Both the original set of rules and Jardine’s Report were re-published by Sclater (1878), along with a list of all naturalists to whom Strickland had originally sent a draft of the rules for comments. It is not the object of this chapter to analyze all the rules of the Strickland Code. However, the treatment of priority, as well as rules related to it, are commented on.

The law of priority, the fundamental basis of the entire code, was defined as follows:

The name originally given by the founder of a group or the describer of a species should be permanently retained, to the exclusion of all subsequent synonyms (with the exceptions about to be noticed). (Strickland 1843 in Sclater 1878:6)

Although priority had been adopted as a guiding principle by some naturalists for at least 30 years, no starting point had been proposed so far. In his personal draft, Strickland had already indicated that the starting point should be the *Systema Naturae*, but had not indicated which edition (Jardine 1866 in Sclater 1878:23). The Committee decided that the starting point was the 12th edition: “We ought not to attempt to carry back the principle of priority beyond the date of the 12th edition of the *Systema Naturae*.” (Strickland 1843 in Sclater 1878:23) The main reason was that the 12th edition was the last edition published under Linnaeus’s supervision. A precise date was not provided in the Strickland Code, at least in 1843; in the 12th edition, the volume on the Animal Kingdom is split between a Part one (1766) and a Part two (1767).

In 1865, the Committee in charge of revising the Strickland Code confirmed the choice of the 12th edition after “much deliberation” (Jardine 1866 in Sclater 1878:23), and gave the date 1766 for a starting point, which, strictly speaking, applies only to the first half of the animal kingdom. An alternative was the 10th edition, in which Linnaeus (1758) applied consistently the binominal nomenclature to all animals for the first time. However, the 12th edition was favored because Linnaeus changed quite a few names between the 10th and 12th editions of the *Systema Naturae*; selecting the 12th edition guaranteed that Linnaeus’s last and supposedly preferred names would have precedence over former names that he discarded. The starting point of zoological nomenclature, which clearly is central in the application of the principle of priority, remained discussed extensively until the end of the 19th century, when a decision was finally made to select the 10th edition of the *Systema Naturae*.

Even though the British Association committee selected the 12th edition as a starting point, we owe to Strickland and the rest of the Committee to have pointed out the critical importance of a starting point for the application of priority. The need for a starting point probably became more pressing in the early 1840s. As de Selys-Longchamps (1842:iv, tr.) acknowledged:

Recognizing the right of priority seems the only way to understand each other and prevent that zoology soon become chaos, a true Babel as long as it is fixed at 1760, the period [*époque*] of the establishment of the binary nomenclature by Linné and of the publication of Brisson’s book for all the genera not adopted by Linné.

In 1842, two starting points had already been proposed: 1760 and the 12th edition of the *Systema Naturae*. ... It was just a start.

The Strickland Code is divided in two parts. The first includes “rules for rectifying the present nomenclature” and deals with names that have already been created. It contains only 14 articles (rules), most of which deal with the application of the Principle of Priority. The second part includes “recommendations [not rules] for improving the nomenclature in future” and deals with the formation of new generic and specific names.

Several articles of the first part of the Strickland Code set the stage for typification within genera. In particular, the Strickland Code provides ways of determining which is the type species in case no typical species was designated originally, especially when a genus is split into several genera. Articles related to the application of the law of priority mainly concern generic names. The notion of type specimen (for species) is not in the Code. A few acceptable exceptions to the law of priority are provided that are similar to the exceptions proposed by de Candolle (1813). For instance, under the Strickland Code, “a name whose meaning is glaringly false may be changed.” (Strickland in Sclater 1878:10)

Article #10, which has remained misunderstood and debated, merits commenting on:

A name should be changed which has before been proposed for some other genus in zoology or botany, or for some other species in the same genus, when still retained for such genus or species. (Strickland in Sclater 1878:10)

The last part of the article (“when still retained for such genus or species”) indicates that Article #10 applies only when names are valid, but not for synonyms. In other words, a name does not have to be replaced (by a new name or a junior synonym) if the homonym is a synonym. In the current Code (ICZN 1999), names have to be replaced even if senior homonyms are synonymous. Also, note that in Article #10 both zoological and botanical names have to be considered, which was rejected by most future codes.

Article #12, which deals with another exception to priority, has also been widely debated: “A name which has never been clearly defined in some published work should be changed for the earliest name by which the object shall have been so defined.” (Strickland in Sclater 1878:11) Although the Strickland Code provides a few guidelines about what constitutes a definition and a publication, many authors have followed their own interpretation, generating confusion in the application of Article #12.

Recommendations of the second part of the Strickland Code intend to render zoological nomenclature “palatable to the scholar and the man of taste.” They are not rules because there is nothing that can be done “if authors insist on infringing the rules of good taste by introducing into the science words of ... inelegant or unclassical character in future.” (Strickland in Sclater 1878:13) Also, those recommendations are not retroactive because they would “undermine the invaluable principle of priority.” However, some authors thought that “rules of good taste” should be enforced, and used that as a reason to replace many existing names, creating confusion and debates.

Within the long list of names that, according to the British Association committee should be avoided or at least used judiciously, one finds: geographical names (because *canadensis* may refer to a species distributed in Mexico); barbarous names (not from Greek or Latin); mythological or historical names because they most often can be regarded as “unmeaning and in bad taste;” names derived from persons “with no scientific reputation;” names of “harsh and inelegant pronunciation” (such as *Enaliolimnosaurus crocodilocephaloides*); hybrid names with parts taken from two different languages, which are “great deformities in nomenclature;” and, “nonsense names,” such as names made of letters randomly composed.

The fact that the Strickland Code devoted many paragraphs to the “rules of good taste” in name formation was clearly a heritage from concerns of naturalists from the late 18th and early 19th centuries, such as Linnaeus (1751) and de Candolle (1813). After several decades of intense debates, it was finally resolved that most kinds of names banished from nomenclature in the Strickland Code (especially the so-called “barbarous” names) could be admitted (ICZN 1905).

Note finally that, perhaps because of the presence of Ogilby in the British Association committee, the use of the suffixes –idæ for family names and –inæ for subfamily names is only a recommendation, not a rule. It also is recommended that all specific names be written with a small initial (including those based on persons), but that recommendation is removed from the second edition of the Strickland Code (1865).

The British Association committee was not international. All members were British, such as C. Darwin, R. Owen, W. Ogilby, and J.O. Westwood. However, Strickland thoughtfully sent a copy of the draft of the rules to many British and foreign naturalists and societies during the preparation of the Code (Sclater 1878). At least 180 individual naturalists, as well as 35 societies, journals, or libraries, were contacted. Unfortunately, it is not known how many comments were sent back. Strickland contacted all of the most famous naturalists of his time, such as Agassiz (Switzerland); Audubon, Gould, and Gray (United States); Blainville, Deshayes, Duméril, d’Orbigny, Milne-Edwards, and Valenciennes (France); Bronn, von Buch, Ehrenberg, Pfeiffer, and Rüppell (Germany); Fries and Lovén (Sweden); and Parlatore (Italy). Strickland’s efforts for reaching the international community were remarkable and visionary.

Impact of the Strickland Code on Nomenclature

The Strickland Code was translated into French in the *Revue Zoologique* (Guérin-Méneville, 1843) and the *Bibliothèque universelle de Genève* (Anonymous 1844). In both cases, however, the starting date (12th edition of the *Systema Naturae*) was not mentioned. Given that most readers probably did not go back to the original English version, such incomplete translations participated in generating confusion among naturalists. This seems to suggest that it took a long time for zoologists to realize how central a fixed starting point is in order to build a stable universal nomenclature. This is also supported by the fact that the Kiesenwetter Code (1858) simply starts at Linnaeus, with no specific date. In 1845, the Strickland Code was adopted by the Association of American Geologists and Naturalists with only one minor suggestion (no need to recommend writing all specific names with a small initial). It was translated into Italian by Charles Lucien Bonaparte for the fourth meeting of Italian scientists, in Padova (Anonymous 1843; Minelli 2008). As expected, one of the main questions that was discussed in Padova was the application of priority. Interestingly, however, it was also proposed that both zoological and botanical rules be joined (Minelli 2008). The Strickland Code does not appear to have been translated into German.

The Strickland Code did not seem to solve nomenclatural issues, as acknowledged in the report introducing the revised version (Jardine 1866 in Sclater 1878:23):

Whether it is from the Rules and recommendations not being sufficiently well known, or from an idea that no one has any right to interfere with or make rules for others, many gentlemen appear to cast them away, and do not recognize them at all, while others accept or reject just what pleases themselves; in consequence many very objectionable names have been given, and a very base coinage and spurious combinations have been going on.

This also was the opinion of Crotch (1870:59):

Notwithstanding the “Rules for Zoological Nomenclature” sanctioned by the authority of the British Association, it would not seem that any perceptible improvement has taken place. (...) The laws of priority are of course assented to tacitly by all, but not applied.

Finally, Lewis (1875:v) even accused the Strickland Code to have generated confusion:

The legislators of 1842 had made the discovery that the names employed here [in the United Kingdom] were different from those employed elsewhere [especially continental Europe], and they enacted a rule

to cure the evil. ... We discovered between three and four years ago that the bare rule of priority (as construed now) has let in practices which promote and do not dissipate confusion.

In the mid 19th century, each naturalist held his own independent opinion on how the principle of priority should apply. By promoting priority so strongly, the Strickland Code forced naturalists to express their opinion about nomenclature. Disagreements arose slowly but, inevitably, a second code of zoological nomenclature was adopted in Germany, fifteen years after the Strickland Code.

THE KIESENWETTER CODE (1858)

Kiesenwetter proposed some nomenclatural rules at the German Congress of Entomology (Dresden, May 1858), attended by 45 entomologists (41 from Germany, 2 from Spain, 1 from Norway, and 1 from Prague, Bohemia). Adopted after some discussion, those rules were published in German in the *Berliner entomologische Zeitschrift* (Kiesenwetter 1858) and then translated into French (e.g., Mulsant 1860; Fauvel 1869). According to Fauvel (1869:91), Kiesenwetter's rules were "unanimously adopted by the most commendable authorities," at least among entomologists. Kiesenwetter meant to design rules of entomological nomenclature, not zoological nomenclature, and those rules constitute one of several specialized codes produced in the second half of the 19th century. In fact, several authors then thought that codes could apply to specialized groups instead of all animals (Edwards 1873). As surprising as it might seem, the Strickland Code was not mentioned by Kiesenwetter, suggesting that Kiesenwetter and other (German) entomologists refused to concede any authority to the British Association and did not think that the existence of several codes would be a source of any problem in nomenclature.

The Dresden rules are similar to the Manchester rules in many regards, such as: Latin is the language of nomenclature; the rules for existing names are distinct from the (non-retrospective) rules for newly created names; names should be considered only when accompanied with a diagnosis, a description, or a figure that can enable species re-identification. There are also notable differences, such as: the Dresden rules include only rules, no recommendations; descriptions and diagnoses must be published in a Romance or Germanic language, and a diagnosis in Latin is mandatory (Rule #11); the use of the suffix *-idae* for family names is a rule, not a recommendation (Rule #3); all spelling that contradicts Latin orthography or grammar must be corrected (Rule #6), whereas in the Strickland Code (Rule #14) the original spelling of a name is retained as long as it has been amended; the starting point for nomenclature simply is "Linnaeus," with no specific date, suggesting that entomologists who met at Dresden did not understand the critical importance of a starting date; finally, inaccurate specific names (e.g., *albus* for a black animal) do not have to be changed, whereas, under the Strickland Code, the law of priority may not apply in such cases.

As in the Strickland Code, priority applies strictly, except in cases of homonymy: "When several names equally acceptable have been introduced to refer to the same object, the oldest one is the highest authority and must be preferred." (Kiesenwetter 1858:xviii) Several authors claimed that, in the Kiesenwetter Code, the choice of preserving the oldest name among several was not absolute and should also be guided based on "greater or less convenience" (Edwards 1873:26; Lewis 1875:vi; Melville 1995:10). However, Kiesenwetter allowed flexibility in the priority rule only in one particular case (Rule #15): "If it is not possible to establish which of several names is the oldest, one is free to choose that which is the most appropriate." (Kiesenwetter 1858:xix, tr.)

Finally, Kiesenwetter agreed with Strickland that, when a genus was being split, then its application should be governed by the type species. Ways to identify the type species are even provided: (1) the species designated as typical by the original author; (2) the species that presents the characters indicated by the original author; (3) the species that is the most remarkable or that is the most commonly found; (4) the first species described or listed by the author. As expected, the determination of type species has remained actively debated, which was partly due to the fact that the solutions

proposed were either arbitrary (first species mentioned) or subjective (species that presents most of the generic characters described by an author).

Interestingly, Kiesenwetter did not attach any particular importance to specimens from old authors, because it was thought that they were commonly mixed with non-original material (Rule #12):

Contrary to the opinion of many of today's entomologists, one cannot admit any right of priority for a description that can only be interpreted thanks to supposedly typical specimens, whether this is correct or not. ... For the old entomologists, one must leave aside species names for which no certain data are available. (Kiesenwetter 1858:xvii, tr.)

Naturally, authors disagreed on whether supposedly typical specimens should be used when discussing the application of names, as illustrated in the debate between Waterhouse (1862) and Schaum (1862).

DESCRIPTIONS VERSUS TYPE SPECIMENS: THE DEBATE BETWEEN WATERHOUSE AND SCHAUM (1862)

Although both the Strickland Code and the Kiesenwetter Code required that names be accompanied by a description in order to be "eligible" for priority, authors disagreed on criteria to determine whether a description was acceptable, as illustrated in the review of Waterhouse's (1858) *Catalogue of British Coleoptera* by Schaum (1862). Schaum (1862:323) complained about the fact that Waterhouse rejected well-known names in favor of older synonyms that were not accompanied by any proper description:

It is universally acknowledged that a species must have been described in order that priority can be claimed for its name; and no one thinks of assigning priority to a mere catalogue name, because we are unable thereby to recognize the species to which it has been given. This reason, however, compels us to put the query, *when* can it be said that a species has been described? Even the most fanatical advocate of the law of priority will not pretend that a species has been described, concerning which utterly false notices, or erroneous or unimportant indications, are given, which so completely fail in characterizing the species that no one is able to recognize it.

Also, Schaum (1862:324) thought that Waterhouse made the mistake of giving more importance to specimens than to written descriptions: "Mr Waterhouse did not ascertain these older names by the study of the books, but merely by the investigation of real or supposed typical specimens."

Waterhouse (1862) regretted that Schaum gave too much importance to descriptions (even incomplete) compared with type specimens. Although Waterhouse admitted that some type specimens might be mixed, he argued that it could still be most often possible to link a description (which Schaum regarded as incomplete) to a type specimen (which Waterhouse regarded as a key source to determine the meaning of a species name). Thus, Waterhouse (1862:338) concluded by arguing that:

When a described species can, *by any means*, be determined, so as to leave no reasonable doubt of the identification, it appears to me desirable that the name given to the species by the describer be adopted, provided that name be the earliest the species has received.

The expression "*by any means*," emphasized by Waterhouse, indicates that, according to him, it was possible to determine the meaning of a name using a description *and* type specimens, whereas Schaum considered that only written descriptions should be used.

This debate is highly interesting because it shows that people started thinking of type specimens while they were debating about priority, but did not necessarily understand that both issues were tightly linked, and that the application of the Principle of Priority could not be solved unless types were taken into consideration. It took several codes and several decades of debates before the link between priority and type specimens was fully understood (see below). Most taxonomists would agree today that using type specimens to determine the application of a species name is a rigorous approach.

A REVISED EDITION OF THE STRICKLAND CODE (1865)

Gray's Comments (1864) on the Strickland Code

In 1863, the Strickland Code was re-published in the *Edinburgh New Philosophical Journal*, and comments were requested by Jardine from whoever was interested. The comments published by Asa Gray, the famous American botanist, are particularly interesting.

Gray's (1864:278–279) comments on Rule #2 (“The binomial nomenclature, having originated with Linnaeus, the law of priority in respect of that nomenclature, is not to extend to the writings of antecedent authors”) are as follows:

The essential thing done by Linnaeus in the establishment of the binomial nomenclature was that he added the specific name to the generic. He also reformed genera and generic names; but he did not pretend to be the inventor or establisher of either, at least in Botany. This merit he assigns to Tournefort ... and he respected accordingly the genera of Tournefort, Plumier, etc. ... While, therefore, it is quite out of question to supersede established Linnaean names by Tournefortian, we think it only right that Tournefortian genera, adopted as such by Linnaeus, should continue to be cited as Tournefort. So, as did Linnaeus, we prefer to write *Jasminum*, Tourn., *Circaea*, Tourn., *Tamarindus* Tourn., etc.

Gray raised the question of the authorship in cases where Linnaeus himself accepted Tournefort's authorship: Gray did not mean that Tournefort's names should have priority over Linnaeus's names. However, several authors, especially Raphaël Blanchard, first president (1895–1919) of the International Commission on Zoological Nomenclature, argued that zoological nomenclature should go back to writers who coined genera in the early 18th century that were later used by Linnaeus.

On Rule #10 (“A name should be changed which has before been proposed for some other genus in zoology or botany, or for some other species in the same genus, when still retained for such genus or species.”), Gray (1864:279) writes:

We submit that this rule, however proper in its day, is now inapplicable. Endlicher, who in a few cases endeavored to apply it, will probably be the last general writer to change generic names in botany because they are established in zoology. It is quite enough if botanists, and perhaps more than can practically be effected if zoologists, will see that the same generic name is used but once in each respective kingdom of nature.

Note that this does not mean that Gray supported the existence of two distinct sets of rules.

Gray's (1864:279) comment on Rule #12 (“A name which has never been clearly defined in some published work should be changed for the earliest name by which the object shall have been so defined.”) seems to indicate an early difference between botanical and zoological practices, and perhaps partly explains why it was resolved in the revised version of the Strickland Code that Botany would not be introduced in the Code:

And as to names without characters, may not the affixing of a name to a sufficient specimen in distributed collections (a common way in botany) more surely identify the genus or species than might a brief published description!

This comment emphasizes the critical importance of specimens used by an author to create a new genus or species name (i.e., the type series or type species). As we already saw, some zoologists of that period gave more credit to the description than to specimens when deciding on the application of a name, as stated clearly in Kiesenwetter Rule #12.

The Minor Changes to the Revised Strickland Code (1865)

In 1860, it was resolved by the members of the British Association for the Advancement of Science that the surviving members of the committee appointed in 1842 meet again to discuss alteration and improvement potentially needed in the Stricklandian rules and recommendations. However, “from the difficulty of bringing such a committee together,” nothing happened until 1863, when it was decided that:

Sir W. Jardine [reporter], A.R. Wallace, J.E. Gray, Professor Babington, Dr Francis, Dr Sclater, C. Spence Bate, P.P. Carpenter, Dr J.D. Hooker [botanist], Professor Balfour, H.T. Stainton, J. Gwyn Jeffreys, A. Newton, Professor T.H. Huxley, Professor Allman, and G. Bentham [botanist], be a Committee, with power to add to their number, to report on the changes, if any, which they may consider it desirable to make in the Rules of Nomenclature drawn up at the instance of the Association. (Jardine 1866 in Sclater 1878:22)

The Code was then reprinted and zoologists were requested to send their comments to Jardine. At Bath (1864 meeting of the BAAS), the Committee did “little to complete further the code of Zoological Nomenclature.” (Jardine 1866 in Sclater 1878:22) However, a meeting took place in London (at Jeffreys’ house) with four members of the committee (Jeffreys, Wallace, Sclater, and Jardine) to review comments sent to Jardine, as well as opinions of the committee members (Babington sent his personal copy of the code with observations written on the margin). Finally, amendments to the code proposed by the committee were accepted, after discussion, at the Birmingham meeting of the BAAS (1865).

Overall, changes made were minor (Jardine 1866 in Sclater 1878:23–24). (1) It was claimed that Botany should not be introduced into the code, which certainly did not come as a surprise to naturalists since a split between zoology and botany already existed de facto, as illustrated in Gray’s (1864) comments on the 1842 version of the code. (2) It was recommended by the committee that the code should not be altered:

... the permanency of names and convenience of practical application being the two chief requisites in any code of rules for scientific nomenclature, it is not advisable to disturb by any material alterations the rules of zoological nomenclature.

Although committee members understandably wished to protect “their” code, several new codes were published in the second half of the 19th century. (3) The committee also was of the opinion

... after much deliberation, that the XIIth edition of the “*Systema Naturae*” is that to which the limit of time should apply, viz. 1766. But as the works of Artedi and Scopoli have already been extensively used by ichthyologists and entomologists, it is recommended that the names contained in or used from these authors should not be affected by this provision.

That the date 1766 was kept “after much deliberation” shows that the question of the starting date for zoological nomenclature was far from being settled. In fact, it remained actively debated for the next 30 years. (4) Rule #13 (“specific names, when adopted as generic, must be changed”) was altered so that it was actually the generic names that, in such cases, should be “thrown aside, not the old

specific name.” (5) The committee emphasized again the recommendation under which the use of a person’s name for a specific name should not be taken lightly. (6) Finally, the recommendation that specific names should always start with a small initial was removed (as suggested by the American Association of Geologists and Naturalists in 1845).

Surprisingly, Kiesenwetter’s (1858) entomological rules were not mentioned, although there is little doubt that the committee was aware of their existence. The reasons for this are unclear. In any case, this omission is not just a detail. It illustrates the state in which nomenclature was in that period. There was a lack of communication among national communities and, as a consequence, no common international efforts for discussing nomenclatural rules.

Ideas about critical issues, especially types and the starting date were still unsettled. Also, the application of priority was still poisoned by subjective opinions on so-called “indelicate” names. For instance, why should a specific name such as *tomcodus*, Latinized from the vernacular fish name Tom Cod, be rejected, as claimed by Jardine (in Sclater 1878:23)? Rejecting such a name would open a can of worms ... as the adoption (or creation) of a more recent name would violate the Principle of Priority for a subjective reason. Pascoe (1865) provided a good illustration of the negative and confusing impact of subjective opinions on the circumstances that should lead to a name change. Pascoe (1865:9543) argued that it is extremely difficult to decide objectively whether a name change is legitimate, such as in the case of Westwood replacing *Hyphaereon* into *Campylocnemis* because it was regarded as too close to *Hyperion*. As a result, Pascoe thought that name changes should be allowed only when spelling is strictly identical.

Generally speaking, it seems that naturalists did not have the time (or did not take the time) to debate enough about all those issues in the 1850s and 1860s. However, many debates took place in the 1870s that clarified many ideas and prepared the ground for the future codes proposed in the last quarter of the 19th century.

Verrill’s (1869) Comments on the Revised Edition of the Strickland Code

In 1869, Verrill commented on the 1842 Strickland Code as well as Jardine’s 1865 report on amendments to the code, in the *American Journal of Science and Arts*. Several of Verrill’s comments directly relate to the application of the priority rule.

First, the law of priority should be limited to the Xth edition of *Systema Naturae*, because it is “more logical.” (Verrill 1869:94) Verrill also emphasizes the importance of choosing a limit, whether it should be 1758 or 1767. Without being revolutionary (the Strickland Code indicated a date already), Verrill’s comments show that mentalities were slowly changing. Indeed, in the 1850s and early 1860s, authors would pay little attention to the starting date for zoological nomenclature:

If the Xth edition be taken as the limit, which seems to be the tendency among recent writers, especially in this country and in northern Europe, the date would be 1758. The second volume of the XIIth edition bears the date 1767. Disregard of this important and essential law has brought into conchology and some other branches of zoology an almost incredible amount of confusion within a few years, the indefinite names of Link, Klein, Brown, Columa and other ante-binomial and polynomial writers, having been revived and substituted for the well known names of Linnaeus and later authors. (Verrill 1869:97)

Second, application of Rule #4 (“The generic name should always be retained for that portion of the original genus which was considered typical by the author”) seems complicated:

This course has been systematically followed by some writers, and when carelessly done has often led to unfortunate and absurd results, especially when applied to the earlier writers, since it often happens that the actual position of the *first* species, in the restricted modern genera, cannot be determined with certainty. (Verrill 1869:98)

Verrill was right: several issues related to types had to be resolved before the law of priority could apply without generating confusion.

Third, Verrill (1869:101) thought that the application of Rule #10 (“A name should be changed which has before been proposed for some other genus in zoölogy or botany, or for some other species in the same genus, when still retained for such genus or species.”) is objectionable, at least for generic names, and agrees with Gray (1864) that “all appear to admit the necessity of allowing the same [generic] name to be used once in either kingdom [botany and zoology].” This opinion was adopted in most of the codes proposed in the last quarter of the 19th century.

Finally, Verrill agrees with Rule #11 (“A name may be changed when it implies a false proposition which is likely to propagate important errors.”) and cites abnormal structures caused by diseases or parasites as causes of errors:

Thus, *Echinus gibbosus* Val. proves to be identical with our *Euryechinus imbecillis*, but the gibbosity is caused only by a parasitic crab lodged in the anal region, and is not present in normal specimens. (Verrill 1869:102)

This rule, which goes back to the late 18th century, when authors thought that specific names should mean something and appropriately describe the species, remained endlessly challenged; determining names that could “likely propagate important errors” was clearly subjective and thus jeopardized the universality of the priority rule.

THORELL'S ON EUROPEAN SPIDERS

In his imposing monograph *On European Spiders*, Thorell (1869) defended several ideas related to the application of the law of priority, which differed in several aspects from the Strickland Code. First, based on the fact that Linnaeus first defined binominal nomenclature in his *Philosophia botanica*, Thorell argued that 1751 should be the starting point for zoological nomenclature (specific and generic names). One advantage was that binomials published in Clerck's (1757) *Aranei suecici*, in which binominal nomenclature was used consistently, could be considered. In fact, under our current Code (ICZN 1999), Clerck's *Aranei suecici* is deemed to have been published on January 1, 1758, and all names published in it have precedence over the spider species names published in the 10th edition of the *Systema Naturae*, which is the only exception accepted by the Code.

There were no rules, neither in the Strickland Code nor in the Kiesenwetter Code, according to which “indelicate” names should be rejected. Strickland thought that no rule could prevent people from “infringing the rules of good taste by introducing into the science words of the same inelegant or unclassical character in future.” (Strickland 1843 in Sclater 1878:13) In the Strickland Code, the “good taste” in forming new names was guided only by recommendations. However, in the 1860s, some zoologists clearly were of the opinion that “indelicate” names should be rejected (Jardine mentioned it in the Report on the revision of the Strickland Code, although no change was introduced in that regard in the Code). Thorell was opposed to keeping “indelicate” names:

It is rightly observed by the British Committee, that a name once published is the property of the science, and cannot therefore be revoked or altered, not even by the person who has composed it. Exceptions however exist ...: the Committee also admits, that there are names which ought unquestionably to be *discarded*, those namely, which in their signification are *absurd* or *false*. It would have been desirable that this sentence of reprobation had been extended also to certain classes of those names which the Committee only considers that naturalists ought in future to abstain from forming (“objectionable names”). Such are for instance mongrel names (compounded of two or more different languages) (...) and names manufactured by mutilating and mangling other names, e.g., *Cypsnagra* from *Cypselus* and *Tanagra*. To this class belong also the equally barbarous denominations that have arisen from the ridiculous practice of composing unmeaning generic names of arbitrarily combined letters, usually in the form of anagram: e.g., *Rocinela*, *Conilera*, *Cirolina*, *Anilocra*, formed from the letters in *Carolina*. We hope the time will come

when also such names as those just mentioned will be rejected, though this is not yet the case.
(Thorell 1869:12)

However, as pointed out by authors opposed to changing names regarded as “indelicate” or “barbarous” (e.g., Pascoe 1865), it is difficult to draw a line between “delicate” and “indelicate” names. Also, if such a rule were to apply retrospectively, it would greatly jeopardize the application of priority, as perfectly understood by Strickland in 1842.

Thorell (1869:11) discussed the potential need for replacement of nearly identical names, such as *Ariadne* and *Ariadna*. However, he did not regard as nearly identical names of different gender, such as *Euryopis* and *Euryopa*, which shows that his opinion was fairly arbitrary, or at least not easy to follow. Thorell (1869:14) also was in favor of a strict application of Latin grammar and orthography, and suggested that all names erroneously formed be corrected, whilst retaining authorship with the original author. Thorell (1869:11–12) disagreed that names should be replaced (or the next synonym used when available) if they had been created in botany or zoology. As Thorell rightly pointed out, Linnaeus, who wanted no name to be used more than once in any of the three kingdoms, was dealing with far fewer species. Finally, Thorell (1869:17) used our modern way of citing authorship (at least in zoology): no parentheses if the species is still placed in the original genus, and parentheses if the species is placed in a different genus. Wallace (1871:lix) thought that Thorell applied Latin grammar too strictly:

He is a strict purist, and alters the termination of every name he considers to be not classically constructed. He admits that there is often difference of opinion on these points, but does not seem to consider that the consequent confusion and instability of nomenclature is as great an evil as classical inaccuracy.

THE 1870s: A GREAT DECADE FOR NOMENCLATURE DEBATES

The literature on nomenclatural rules was abundant in the 1870s. Many authors expressed deep, serious worries about the situation in zoological nomenclature. In particular, many authors were of the opinion that the Strickland Code failed in bringing stability to nomenclature. Agassiz (1871:355) was quite clear about it:

The rules of nomenclature generally adopted are by no means satisfactory. The exceptions constantly taken to their application only increase the confusion, and the attempts made by the British Association to recommend a set of rules for the guidance of Naturalists have not been successful.

According to Edwards (1873:22), the Strickland Code “was not found to work altogether satisfactorily, and never did receive the general assent of Naturalists in their several departments.” In his *Catalog der Lepidopteren*, Staudinger (in Staudinger and Wocke, 1871:xi, tr.) asserted: “The diversity of laws that govern nomenclature and the confusion that characterizes it will end up generating a true chaos if we cannot establish fixed rules.” Other quotations could be cited.

The 1870s was a period of doubt. Important issues were still not solved, such as the starting date for zoological nomenclature, but one of the most serious discussions concerned the Principle of Priority. Although the vast majority of naturalists agreed that priority was a valuable and necessary principle, they also realized that its application could be subject to interpretation. The community began to split between those who remained loyal to priority and those who preferred to adopt a so-called “law of convenience.” The latter started growing after several authors, such as Kirby (1871) and Scudder (1872), restored many old but completely unknown names to replace well-known, commonly used names:

From 1861 to 1871 the tide went in the direction of restoring the earliest discoverable names. It is sufficient to mention the names of Gemminger and von Harold, the late Mr Crotch, Mr Scudder, and Mr Kirby to recall that the practice of “resurrection” resulted in the production of several volumes. (Lewis 1875:vii)

With the concept of priority’s being directly attacked, nomenclature lived one of its most difficult periods. The codes written in the last quarter of the 19th century cannot be understood if the debates that took place in the 1870s are not considered, as those codes had to address all the issues raised in the 1870s. Many of the authors who participated actively in those debates were entomologists, such as W.H. Edwards, W.A. Lewis, O. Staudinger, and J.L. LeConte. Interestingly, it is during that period of doubt that some quite radical solutions were proposed (though not adopted).

Kirby’s Controversial Synonymic Catalogue of Diurnal Lepidoptera

As soon as it was published, Kirby’s (1871a) *Synonymic catalogue of diurnal Lepidoptera* was profusely criticized. Kirby held radical views on generic nomenclature, which he presented before the members of the Entomological Society of London in 1868 (Kirby 1868) and published in the *Journal of the Linnean Society, Zoology* (Kirby 1870). Generally speaking, Kirby was in favor of simple nomenclatural rules that applied strictly, with no exceptions. For that reason, he rejected any exceptions to the rigid limit of 1767 for a starting date of zoological nomenclature (the revised Strickland Code accepted names published by Scopoli and Artedi before the XIIth edition of *Systema Naturae*). Later, however, Kirby (1871b) argued that 1758 should probably be selected. In the same spirit, he argued that when no type species was designated by original authors for generic names, then the first species listed should be considered typical.

Although Kirby (1870:494) genuinely intended to solve or at least improve the “indescribable confusion [that] has arisen in generic nomenclature,” several authors rejected what was his “revolution in generic nomenclature.” (Kirby 1868:xliv) Westwood indicated that he had proposed exactly the same rules several years earlier but had to change his mind because “such a rule and the application of it retrospectively would cause so much confusion that the remedy would be worse than the disease.” (in Kirby 1868:xliv)

Wallace (1871:lxiii) criticized heavily Kirby’s work in his presidential address to the Entomological Society of London: “The most novel, and, as many will think, the worst feature of the book is the entire revision of the generic nomenclature.” Wallace thought that Kirby had gone too far when applying some of the Strickland rules, and should have taken in consideration the limitations of those rules. For instance, under Rule #10, “A name should be changed which has before been proposed for some other genus in zoology or botany, or some other species in the same genus, *when still retained for such genus or species*,” but, according to Wallace (1871:xliv), Kirby failed to take into account the last point of Rule #10 and replaced names even when their homonyms were completely forgotten.

However, the most important disagreement between Wallace and Kirby was related to the use of Hübner’s names. In the 1870s, Hübner was regarded as an active “splitter” by the community of entomologists (Grote 1876:57). According to Wallace (1871:xliv), the consideration of generic names published in Hübner’s “obsolete and useless catalogue” yielded a “wholesale change” of names that was not warranted by the Strickland rules. In fact, the Strickland rules (as well as the Kiesenwetter rules) clearly required that names be “clearly defined in some published work” (Rule #12). According to Wallace, Hübner did not describe properly his generic names, which explains why his work was “set aside (...) by most European entomologists.” However, Kirby argued that the application of Hübner’s generic names could be determined through type species.

Kirby’s work shows that even when authors accepted the authority of the same rules (in this case the Strickland Code), they might still disagree on how to interpret those rules and apply them in practice, which probably explains why: “Notwithstanding the “Rules for Zoological Nomenclature” sanctioned by the authority of the British Association, it would not seem that any perceptible improvement has taken place” (Crotch 1870:59). For that reason, Kirby (1871b:42) claimed that “an

international congress of naturalists would be very desirable to reconsider and, if necessary, revise [the rules and recommendations of the Strickland code].”

Although Kirby’s approach to designating type species was rejected by the community, it forced naturalists to discuss the link between types and priority. In that regard, Kirby’s (1868, 1870, 1871a, 1871b) contributions impacted positively on nomenclature. For instance, Crotch (1870:59), regretting that “Mr. Kirby, unfortunately, merely [pointed] out certain inconsistencies without suggesting any remedies or consistent plan of action,” asserted:

A genus, as far as I understand it, for the purposes of nomenclature, consists of but one species—its type.... No genus can be considered defined until a type is indicated, for characters must vary with our knowledge in every case; but when the type of a genus is not indicated, I am not inclined to cut the knot by the simple process of taking the first species, but to trace the genus historically until it has a type given to it.

As the starting date, Crotch (1870) proposed 1735, for the 1st edition of the *Systema Naturae*.

Staudinger’s Bilingual *Catalog der Lepidopteren*

The *Catalog der Lepidopteren des Europäischen Faunengebiets* by Staudinger and Wocke (1871) was a bilingual work (German and French) that can also be referred to as the *Catalogue ou énumération méthodique des Lépidoptères qui habitent sur le territoire de la faune européenne*. Staudinger wrote the Macrolepidoptera section, as well as the Introduction (in which nomenclature is discussed), and Wocke wrote the Microlepidoptera section. Although Staudinger accepted the XIIth edition of the *Systema Naturae* as a starting date for zoological nomenclature, he thought that 1758 (Xth edition) would make more sense, an idea shared by Scudder (1873). Staudinger was opposed to changing names for grammatical or orthographic reasons, as indelicate names do not hurt communication among naturalists. As an illustration of the confusion created by Latin orthographic improvement, Staudinger (in Staudinger and Wocke 1871:xiii) gave the example of *Nyctimera*, “improved” successively in *Nychtemera*, *Nyctymera*, *Nyctimera*, *Nychthemera*, and *Nyctimera*...

The 1871–1872 Issue of the *Entomologist’s Monthly Magazine*

In the 1870s, many nomenclatural debates were published in the *Entomologist’s Monthly Magazine*. In particular, the eighth volume is interesting because authors replied to each other, revealing their disagreements. Those articles are analyzed here chronologically.

Lewis (1871:1) opened the volume with harsh comments about nomenclature:

It is a dreadfully frivolous work, I venture to think, this routing out from libraries of doubtful and obscure descriptions, and the spending of precious years in nicely balancing considerations upon the priority of a name. To give the title of scientific discovery to such a process would be to apply rich gilding to a very cheap and common sort of ginger-bread.

Lewis (1871:2) was strongly in favor of abandoning a strict law of priority:

My proposal is that no name (whenever and wherever it may be discovered) be received henceforth, to the displacement of a universally recognized name; and this I humbly consider to be founded on strict common sense.

Lewis (1871:4) was so opposed to the ideal of accepting the oldest names in all cases that he even provocatively proposed June 1st, 1871 as a starting date for zoological nomenclature, so that all issues related to the priority of old names would immediately disappear.

McLachlan (1871:40), less radical than Lewis, argued that “resurrection men” could not be condemned because they resurrect names, but because, in some cases, “in their reverence for old names, [they] raise ghosts, ... names that should sink into oblivion, or rest quietly in the list of ‘species

indeterminatae. Again, the problem here seems to have been the criteria used to determine whether a name was published properly, i.e., in a way that could help future authors to re-identify the species being referred to. McLachlan (1871:41) also commented on Lewis's remark on naturalists spending some of their time dealing with synonymy:

It is expedient there should be no more crime, no more deceit, in the world; and, as a consequence, no more prisons, police, and lawyers [Lewis was a professional lawyer]. But the evils exist, and the other *necessary* evils are required to keep them in check. Synonymy exists, and its existence renders necessary the evil that entomologists must waste precious time in unraveling it. The suppression of both crime and synonymy by a *fiat* is utterly impossible. I couple the words, but the existence of synonymy is too often owing to what are actual *crimes* against science. I hold that, when an entomologist describes an insect as new, without using every endeavor that is humanly possible to discover whether it be not *already* described, he commits one of the greatest crimes against science.

Briggs (1871:94) brought up an interesting argument against the use of priority in cases where it would replace a common species name that all naturalists agreed upon:

I look upon the *accord* of entomologists as a "law" of itself, governing and paramount to both these special "laws"; where there is want of accord we have resource to one of them to obtain that accord, *but, where accord exists already, I say that we have no right to call either law into operation*; they are not wanted, and I look upon the present resurrectional movements as a tortuous application of a useful law.

Naturally, whether "indelicate" names should be replaced was still debated in 1872. Against authors who complained against "nonsense" names such as anagrams (Dunning 1872a), Sharp (1872:254) brought a quite new way of looking at scientific nomenclature:

Mr. Dunning thinks *Lycaena Minimus* abhorrent; but I think it can be only because of some curious classical prejudices that he so considers it. Scientific nomenclature should be of no particular language, its object being to supply a universal language, and it is to be of assistance for this purpose that we make use of Latin and Greek words (as being more generally known than others); but we must handle them according to the rules of universal grammar.

This differs from the views expressed in both the Strickland and Kiesenwetter codes. Dunning (1872b:294) replies, citing Thorell, that without respect of Latin orthography and grammar, nomenclature will "gradually assume an appearance absolutely disgusting to a person possessing even the slenderest classical attainments."

Lewis: A Professional Lawyer Commenting on Nomenclature

Lewis was one of the strongest opponents to a strict application of the law of priority. His writings on priority impacted deeply the community of naturalists of that period. Like Briggs (1871), he thought there was no need to call for priority of older names when naturalists already agreed on a name for a given species because "the law of priority is a means to an end, and the end in this case is accord or common agreement to a name.... The object of the law is the important thing; not the law, which is only machinery." (Lewis 1875:viii) According to Lewis, giving priority to an ancient name over a more recent name made no sense because, in most cases, older descriptions are unrecognizable:

Linné described 780 Lepidopterous insects, the number now known cannot be less than 30,000. Dozens of allied species all equally fit numbers of the old descriptions; and such descriptions are now necessarily of no value. (Lewis 1875:xii)

Other authors, especially Edwards (1873:24), made similar arguments against the law of priority.

This issue could have been partly solved by going back to type specimens. However, as most authors in that period, Lewis paid much more attention to written descriptions, necessarily incomplete, than to types. In fact, he agreed with Sharp, a strong defender of the strict priority rule, that “it is clear that [types] must not be exclusively or even strongly relied on,” (Lewis 1875:xxv) when one wishes to determine which species is referred to by a species name. This opinion was widely shared in that period:

Studying the types will tell me the palaeontologists of science [an expression not actually referring to palaeontologists, but, as a metaphor, for people interested in “dead” things in science]. But who does not know that? Those famous types are more or less spread out, sometimes exchanged, often destroyed, and, which is worse, replaced randomly. (Puton 1880:37)

Lewis (1875:xxxvii) proposed an interesting solution to the conflict between “convenience” and “priority:”

The objects are clear—(1) to exclude doubtful names; (2) to preserve accepted names. They are not identical, but both objects can and must be pursued together. It has been several times suggested that the enjoyment of universal acceptance for a period of years should give a name an indefeasible title to adoption. For a purpose which I have in view, I will fill in the number “thirty” and make the proposal read thus: No name for thirty years in universal acceptance should be displaced.

In this passage, Lewis clearly pointed to what we call now the *nomina dubia* (names of doubtful application) and conserved names (over oldest names). This approach was then quite revolutionary:

This would be the working of the limitation. No name could be produced now for the first time from any book bearing date 1842 or previously.... That stops the evil spreading henceforward. No author can then bring up a name from Old Style books, unless the name has been kept alive by quotation as the true name in some work since 1842. Here is a measure there is no difficulty in applying, and its operation is simple. (Lewis 1875:xxxvii)

However, Lewis’s method was not adopted, mainly because taxonomists discovered soon enough that going back to the type specimens is critical when determining the entity a name refers to, which cannot be answered if only written descriptions are considered.

Edwards, LeConte, Mead, and Others

Edwards (1873, 1876) largely agreed with Lewis, and was opposed to a strict application of priority: “The laws of priority are not inexorable, and such laws anywhere lead to absurdity and injustice.” (Edwards 1876:87) As far as the starting point is concerned, he adopted Thorell’s (1869) date, 1751, as long as authors consistently used binominal nomenclature. Interestingly, Edwards (1873:35) was in favor of developing a code for entomologists if no agreement could be found with other branches of natural history (although it does not seem that entomologists could agree with one another):

I have heard ... that Entomologists have no right to separate themselves from other naturalists, and make a special Code for their own sole guidance. To this I would reply, why not?... Why should not each branch adopt Rules to suit its own case? If Botany may be excluded from the operations of a Code, why not Entomology?

In fact, several specialized codes were published in the last 25 years of the 19th century.

LeConte (1873, 1874a,b,c) was opposed to strict priority, because it is often difficult to determine the meaning of old names, which he proposed to deal with using a special starting date for “doubtful cases,” at least in insects, which would be Olivier’s (1789–1811) *Insectes* of the *Encyclopédie Méthodique*.

It is in this period of doubt that Sharp (1873), in order to address the fact that species names often change when moved from one genus to another, suggested that binomials be never changed after their creation, regardless of whether species would be transferred to a new genus or not. Wallace (1874) vividly criticized this idea based on the fact that generic names provided naturalists with valuable information. Interestingly, fixed binomials were re-invented in a different context (Michener 1963).

Finally, Mead (1874:108) coined the expression “law of stability” for the idea of keeping all names already widely adopted by naturalists, even if they were not the oldest ones, an expression that he preferred to “law of convenience.”

Dall's Survey of Naturalists' Opinions on Nomenclature

Dall's (1878) Report was not a new code:

This report does not form a proposition to be acted on by the Section, or by the Association in a final manner at the present time. It is merely an attempt at a complete presentation of the subject, without which no well advised action by naturalists as a body can be expected or is to be desired. (Dall 1878:10)

Dall wished to survey and present the diversity of North American opinions regarding nomenclatural issues, and to provide naturalists with a review of nomenclatural rules and recommendations proposed in zoology as well as botany up to that time.

One of the most interesting elements of Dall's Report was the results of his survey (Dall 1878:16–19). In 1876, he sent a circular including 27 questions to all North American naturalists (mainly zoologists) who had published systematic work in the last five years. Only “yes” or “no” were possible answers, in order to avoid ambiguity. Forty-five naturalists (three from Canada and forty-two from the United States) replied, including many of the naturalists already mentioned in this chapter, such as Lewis, LeConte, Edwards, Scudder, and A. Gray (for a complete list, see Dall 1878:20–21). According to Cope (1879:518), names on the list of people who replied include “an unquestionable majority of the best working naturalists of the country.” Most questions received overwhelming agreement. The community, however, was divided over eight questions. Those disagreements, which are of great interest, are reviewed here.

Question I. No agreement could be found on the starting date: Xth edition (18 votes), XIIth edition (17), 1736 (1), 1753 (2 botanists), no answer (7). This confirms what is sensed from the literature of the 1870s.

Question IV. “If an author has not indicated his adoption of the binomial system by discarding all polynomial names in a given work, are any of his names therein entitled to recognition otherwise than in bibliography?” no (18), yes (18), doubtful (4), no answer (5). This is an important disagreement because it means that both generic and specific names from such works could become eligible for priority.

Question IX. “Is a name, when used in a generic sense, and otherwise properly constituted, subject to have its orthography changed by a subsequent author, on the ground that a proper construction from its classical roots would result in a different spelling?” no (21), yes (19) doubtful (8), no answer (2). Again, this confirms what is found in the literature from the 1870s; naturalists disagreed on whether names should be changed for grammatical or orthographic reasons. The most extreme opinions regarding that question certainly were expressed by Saint-Lager (1882, 1886), author of several contributions in which he suggested that all existing names be corrected by philologists and that, before creating any new name, naturalists should get approval of its correctness from a philologist (e.g., Saint-Lager 1886:54).

Question XVIII. “When a generic name has lapsed from sufficient cause into synonymy, should it be thenceforth entirely rejected from nomenclature? Or should it still be applicable

to any new and valid genus?” reject (19), accept (23), doubtful (1), no answer (2). Answers to this question suggest that many naturalists considered that synonyms could be permanently rejected, which is something that does not exist in the current Code (ICZN 1999).

Question XIX. “Should a name which has been once used in one subkingdom, and has lapsed into synonymy, be considered available for use in any other if not entirely rejected from nomenclature?” no (20), doubtful (1), yes (18), no answer (6). This question relates to the previous one.

Question XXI. “Is it advisable to fix a limit of time, beyond which a name which had been received without objection during that time shall be held to have become valid, and no longer liable to change from the resuscitation of obsolete or uncurrent but actually prior names?” no (28), doubtful (1), yes (13), no answer (3). This question comes directly from Lewis’s proposal (1875), which intended to mix both the law of priority and “convenience” or “stability” (by preventing well-established names from being replaced).

Question XXIII. Yes (30) to the following: “Should it be permitted to alter, or replace by other and different appellations, class, ordinal, and family names, which owing to the advance of Science and consequent fluctuation of their supposed limits have become uncharacteristic?” Yes (11) to the following: “Or should these also be rigidly subject to such rules of priority as might be determined on for generic or specific names?” No answer (4). Answers to this question suggest that a majority of authors (~75%) thought that supra-generic names should not be governed by a code of nomenclature, although about 25% thought that supra-generic names should be governed by rigid rules. The codes proposed so far had dealt only with genera and species (except for the use of the suffix *-idae* for family names). Scudder (1872) was one of the authors who pushed to apply the priority principle to all names, including higher groups.

Question XXVII. “Should a series of rules be recommended for adoption by the Association, would you be guided by these recommendations in cases where they might not agree with your own preferences?” yes (29), yes with reservations (15), no (1). Answers to this questions show that, in 1878, one third of North American zoologists did not seem ready to adopt common nomenclatural rules, whether they agreed on them or not. Obviously, opinions were strong.

Although his Report is not a code, Dall suggested that a starting date be adopted. He (1878:15) rightly pointed out that any starting point would be arbitrary (as clearly indicated in the current Code, see Article 3): “The system being of slow and intermittent growth, even with its originator, an arbitrary starting point is necessary.” In the Report, Dall (1878:41–44) proposed that specialists from different areas of expertise (entomologists, malacologists, etc.) get together and agree on an “epoch-making” work, one for each group, i.e., Clerck’s (1757) *Aranei Suecici* could be the epoch-maker for spiders, as suggested by Thorell (1869); the first edition of the *Systema Naturae* (Linné, 1735) could be the epoch-maker for bird generic names, etc. This being said, Dall (1878:44) also added that, in case specialists could not agree on a date (which clearly was a foreseeable possibility ...), the XIIth edition be selected as the starting point because: (1) “it has been twice recommended by the British Association” and (2) “the usage founded on the B.A. rules should be maintained if possible.” Dall’s proposal was not adopted; in fact, several other possible starting points were proposed in the last two decades of the 19th century.

1880s: MORE NEW CODES

Chaper’s (1881) Rules

Like Dall’s (1878) Report, Chaper’s (1881) *De la nomenclature des êtres organisés* was not a code, in the sense that it was not adopted as an official code.

The work that it [the Committee] is honored to submit to you is not a code with any new principle, nor does it have the pretension to impose it to anyone under whatever authority. It simply is a circular where are put together, coordinated, and for everyone's interest and benefit, the rules and usages sanctioned by common sense and an already long practice. (Chaper 1881:37, tr.)

Chaper's set of rules was adopted by the Société Zoologique de France simply to be presented at the International Congress of Geology, Bologna, along with Douvillé's rules. Chaper (1881:7) wished to make sure that zoologists influenced the code that palaeontologists intended to develop. However, Blanchard (1889) mentioned that, after Chaper's rules were adopted on June 14, 1881 by the Société Zoologique de France, they were followed in the *Bulletin* of the Society. More importantly, 1300 copies of Chaper's rules were sent to members of the Société, as well as to foreign naturalists and societies for comments. Blanchard (1889) used Chaper's rules as a basis for the rules that he presented at the first International Congress of Zoology.

Chaper's (1881) article was divided into two parts: (1) three pages with 17 nomenclatural rules, and (2) 30 pages with comments on those rules. Chaper wrote everything, in the name of a committee of eight members, including R. Blanchard, future first president of the International Commission on Zoological Nomenclature. Chaper's set of rules is remarkably concise and brief. In fact, it is so brief that one of the most needed items is missing, i.e., a starting point for zoological nomenclature. However, Chaper's rules were not supposed to serve as a code. For that reason, he discussed in detail the history of the binominal system in the Report, but did not express any preference for any date. However, Chaper argued that the genus concept was already present in several authors who preceded Linné, such as Tournefort, Lang, and Klein, suggesting that Chaper and the committee might have favored early dates as starting points. In fact, Blanchard (1889) selected 1700 as a starting point for generic names.

One detail in Chaper's (1881) rules is intriguing. Indeed, the first Article reads: (Chaper 1881:3, tr.): "The nomenclature adopted for all organized beings is binary and binominal [*binaire et binominale*]. It is essentially Latin. Each being is distinguished by a genus name followed by a species name." Chaper's ideas on the meaning of binary and binominal have remained unclear, especially because both adjectives were used interchangeably in the 19th century to refer to the Linnaean nomenclature. As Melville (1995:17) pointed out: "It is not clear why he used both adjectives when one would have sufficed." Blanchard (1889), whose rules were deeply inspired from Chaper's rules, also used both adjectives without giving a clear distinction between them.

Douvillé's Rules

Douvillé's (1882) rules were prepared by a committee of eight members (six palaeontologists and two mineralogists) in preparation for the International Geological Congress, Bologna (1881). Those rules are brief and add up to only two pages. As with Chaper's rules, they were not officially adopted. They were a working document. In fact, because Douvillé proposed identical rules for Botany and Zoology, two letters (Van Tieghem et al. 1882; De Candolle 1882) were sent to the president of the International Geological Congress to let him know that botanists already have a code and will not accept new rules. In the *Comptes-Rendus* of the Congress, it was mentioned:

Our last session was filled with a serious study of the rules to be followed in the species nomenclature; and we are entitled to hope that what has been started by palaeontology, will be able to continue with help from botanists and zoologists. (Capellini 1882:188, tr.)

There was no follow-up. However, two features of Douvillé's rules are worth noting: (1) No starting point is proposed because any name that followed the binominal nomenclature could be considered; (2) An illustration is required for a specific name to be considered.

The Code of the American Ornithologists' Union (1886)

In April 1885, a Code of nomenclature was officially adopted by the American Ornithologists' Union. It was prepared by a committee of five members appointed in 1883 by the Union in the context of the revision of the checklist of North-American birds. This 69-page Code, known as the "AOU Code," was published in 1886 (Coues et al. 1886). Although it was prepared by ornithologists, it was written to serve all zoologists. It included one important innovation, i.e., the introduction of rules governing trinomials, names referring to varieties (Coues et al. 1886:30–32), which were already widely used by ornithologists. As far as priority is concerned, the AOU Code adopted the 10th edition of *Systema Naturae* as the starting point (Coues et al. 1886:32). Also, importantly, the AOU Code rejected the idea that the *lex prioritatis* would not apply for names that had been in use for a certain period of time (25 and 30 years proposed by Dall and Lewis, respectively): Committee members thought that it was difficult in most cases to determine whether a name had been universally used:

Unless perfect agreement could be obtained,—and of this there is very little probability,—the proposed rule would tend to increase rather than lessen the confusion it would be the design to remove. (Coues et al. 1886:39).

Thus, it was adopted that: "The law of priority is to be rigidly enforced in respect to all generic, specific, and subspecific names." (Coues et al. 1886:40) In the AOU Code, the application of priority is more flexible with supra-generic names; it applies only if family or sub-family names are strictly synonymous (i.e., identical delineation).

The AOU Code was commented on, especially by Sharpe (1886), the member of the committee in charge of establishing an authoritative list of birds of the British Islands for the British Ornithologists' Union, the "sister" of the AOU. Sharpe (1886:169) rejected a strict application of the law of priority: "The A.O.U. list does not simplify existing nomenclature to begin with, and it is the great love of change, which has been so characteristic of recent ornithological work in America, which makes us skeptical as to whether even the authority of the A.O.U. "List" will be sufficient to prevent further modifications in this direction." However, Sharpe (1886:170) constructively suggested that both sister unions communicate to see "if a common ground of agreement cannot be arrived at." This agreement took place in the context of the International Commission on Zoological Nomenclature in the next decade.

A Different Voice: Preudhomme de Borre

In 1886, Preudhomme de Borre, president of the Société entomologique de Belgique, expressed interesting opinions on how to get rid of nomenclatural confusion. According to him, the biggest problem in entomology was not nomenclatural but found its roots in taxonomic practices:

The big ill of our period is not the catalogues; it is the isolated description of novelties [new species] which are discovered every day; it too often means the description of novelties which are not new. (Preudhomme de Borre 1886:xcviii, tr.)

According to Preudhomme de Borre, changes of generic or specific names should be allowed only in monographs:

It thus is, in spite of all obstacles, in the return to monographic works that must reside the hope of those who desire that the systematic inventory of living beings that we are dealing with could regain true order. (1886:cci, tr.)

Although his idea of restricting the right of changing names in monographs seems a bit extreme, it is undeniable that many synonyms and a great deal of nomenclatural confusion was and still is

due to the fact that authors sometimes create new species names too lightly. This proposal was never adopted. Preudhomme de Borre (in Milne-Edwards 1889:414) raised his voice at the First International Congress of Zoology held in Paris in 1889, where he argued—in vain—during the discussion of Blanchard's proposed rules that name changes should be allowed only in monographs.

TOWARD THE *RÈGLES INTERNATIONALES DE LA NOMENCLATURE ZOOLOGIQUE* (1905)

Discussion of Blanchard's Rules at the First International Congress of Zoology

As first president of the International Commission on Zoological Nomenclature (1895–1919), Raphaël Blanchard played a critical role in the development of the *Code of Zoological Nomenclature*. Since his participation in Chaper's 1881 committee, he remained interested and involved in nomenclature. In particular, he used the opportunity of the first Congrès International de Zoologie (Paris, 1889), to present before the participants a series of rules for approval by the Congress (Blanchard 1889:397–404). Blanchard's (1889) contribution to the *Compte-Rendu des séances du Congrès International de Zoologie* is an imposing document that contains: a long essay (Blanchard 1889:333–397) explaining each rule he proposes; the actual rules he proposes (Blanchard 1889:397–404); the discussion that takes place at the Congress (Milne-Edwards 1889:405–418); and the rules adopted by the Congress (Blanchard 1889:419–424). Several documents related to the discussion on zoological nomenclature are added in an appendix (Chaper 1889; De Candolle 1889; Horst et al. 1889; Hubrecht et al. 1889; Oberthür 1889; Saint-Lager 1889). Finally, a list of conventional abbreviations for authors' names adopted by the Congress closes the *Compte-Rendu*.

The rules adopted by the Congrès International de Zoologie (Blanchard 1889:419–424) were very similar to the rules presented by Blanchard at the Congress. The 13-page-long document describing the discussion of each of Blanchard's proposed rules is fascinating because it shows the detailed reactions of participants. Although more people were probably present in the room during the discussion, the following zoologists expressed their opinions and were thus recorded in the *Procès-verbaux*: Bedel, Blanchard, Chaper, Dautzenberg, Fischer, Girard, de Guerne, Kraatz, McLachlan, Milne-Edwards, Preudhomme de Borre, Riley, de Selys-Longchamps, Simon, Trimen, Trouessart, and Vaillant. At the beginning of the discussion (Milne-Edwards 1889:405), Riley and de Selys-Longchamps suggested that any rule adopted by the Congress should simply be regarded as some kind of advice, but not as a law that would have authority over zoologists; Milne-Edwards argued that the International Congress of Zoology was the ideal opportunity to agree on common rules. Most articles were adopted without discussion or with only minor modifications. As for the articles relating to priority (Blanchard's articles 42 to 53), only Article #42 (which defines priority) was adopted in 1889. Due to a lack of time, the discussion was interrupted by the president, which is a pity because Blanchard's Articles #43–44 used 1722 as starting point...

Indeed, Blanchard (1889:386, tr.) held strong opinions about priority, and most particularly about the starting point for nomenclature:

We too, are of the opinion not to apply priority beyond the 10th edition of the *Systema Naturae*, the first one in which Linné used binary nomenclature. However, we must express some reservation in favor of Tournefort, Lang, Klein, Clerck, and Adanson. The works of those authors are in conformity with the binary method; at least, they had an exact notion of the genus and they precisely delineated its limits: we cannot leave them aside without an outrageous injustice.... For us, the year 1700 is thus the most extreme date beyond which the "search of paternity" is not allowed. This date, however, does work only for Botany; zoological nomenclature only really starts in 1722, with Lang. Any generic, and specific name established since 1700 for plants and since 1722 for animals, in conformity with rules that precede, will hold priority and will have to be substituted to any more recent name, even admitted by Linné.

The discussion on priority took place in the second International Congress of Zoology, in Moscow (1892).

Blanchard had three more years to think about the starting point... In particular, he could think about the comments published in the Appendix of the *Compte-Rendu du Congrès*: Dutch entomologists and zoologists favored 1751 as a starting point (Horst et al. 1889; Hubrecht et al. 1889); Oberthür (1889) suggested that a figure be mandatory for a new species name to be considered, and he interestingly argued that there was a great need in fixing the use of the word *typus* or *type* in natural history; as usual, Saint-Lager (1889) provided endless linguistic remarks; de Candolle (1889) agreed with Blanchard that Tournefort was the founder of the genus concept, but did not comment on Blanchard's starting point; he also argued that abbreviating the authors' last names was a bad idea, in Botany as in Zoology.

The Second International Congress of Zoology (1892): 1758 It Will Be

Blanchard's work between the first and second Congress was remarkable. This 82-page *Deuxième Rapport sur la nomenclature des êtres organisés* (Blanchard, 1893) includes: (1) precisions on the rules adopted in the first Congress (Blanchard 1893:3–13), which mainly concern spelling issues; (2) the main body of the Report (Blanchard 1893:14–65), which complements Blanchard's (1889) first Report; (3) the rules newly proposed by Blanchard (1893:65–72); (4) the rules adopted by the Paris and Moscow congresses (Blanchard 1893:72–83). Note that all rules were discussed and voted upon again, including the rules already adopted in Paris in 1889, because Blanchard changed the wording of several rules and added new rules (e.g., rules were added for naming hybrids).

All the discussions about Blanchard's proposed rules are available in the *Procès-verbaux du Congrès* for the two sessions in which the discussions took place, with A. Maklakov and E. Chantre as presidents. One gets a good idea of what happened in Moscow by comparing the rules proposed by Blanchard, the rules adopted, and the discussions. In brief, Blanchard was exceptionally successful at convincing most participants in accepting his rules. Most rules were accepted with no or little discussion. Blanchard's rules #13 and #30 were rejected, but with no major implications. Some rules were reworded, but with no change of meaning. Although more people were probably present in the room during the discussions, the following zoologists expressed their opinions and were thus recorded in the *Procès-verbaux*: Blanchard, Brusina, Bunge, Chantre, de Guerne, Janet, Jentink, Kapnist, Maklakov, Milne-Edwards, Oschanine, Schlumberger, Studer, and Zograf.

In the body of the Report, Blanchard (1893:37–45) defended again the idea that 1722 should be used as a starting point for zoology because Tournefort, Lang, Klein, etc. adopted a binary nomenclature before Linné. Logically, his proposed rules suggested that 1722 be used as a starting point (Blanchard 1893:68–69, tr.):

[Art. 15] Binary nomenclature was founded by Tournefort, in 1700; Lang was the first to apply it in Zoology, in 1722; it only is in 1758 that Linné used it in the classification of animals.... [Art. 16] The year 1722 thus is the date up to which zoologists must go back to search for the most ancient generic or specific names. Any pre-Linnaean name must be adopted if it is in conformity with article 35 of the rules adopted by the Congress of 1889....

Here is what happened during the discussion (Chantre 1893:xxxvii, tr.):

The Congress approves the terms of the Article 16 and expressively admits the exactness of the historical considerations and criticisms which it summarizes. Nonetheless, after having heard the observations of Mr the Prof. A. Milne-Edwards, who insists on the disadvantages that would result from rejecting of a large number of names presently in use and replacing them by more ancient names, from pre-Linnaean authors, the Congress adopts the following article, instead of articles 15 and 16 of the Report.

The text thus adopted reads (Blanchard 1889:81, tr.):

[Art. 45] The 10th edition of the *Systema Naturae* (1758) is the starting point of zoological nomenclature. The year 1758 is thus the date up to which zoologists must go back to search for the most ancient generic and specific names, acknowledging that they are in conformity with the fundamental rules of nomenclature.

Obviously, 1758 had more chances than 1722 to win the crowd. In fact, Blanchard (1893:44) admitted that most zoologists were in favor of 1758. Also, ornithologists adopted the date 1758 as their starting point during a Congress at Budapest, and the German Committee on nomenclature as well as Sherborn (author of the imposing *Index Animalium*) also adopted 1758. The Second International Congress of Zoology, Moscow, was thus a turning point in the history of “priority”; for the first time, 1758 was officially adopted as an arbitrary, but nonetheless common, starting point by zoologists. Blanchard’s role was immense, and, at the end of the session in which his rules were discussed, the Congress warmly applauded and thanked him for his “painful, long, and fruitful work.” It may be important to emphasize that: (1) Blanchard was smart enough not to push for his agenda (1722 as a starting point); and (2) Blanchard’s proposed date was rejected through a quick but fair process. It is almost embarrassing to realize that 1758 was selected so easily given that, for about 50 years, the 12th edition of the *Systema Naturae* had (supposedly) been used as starting point.

Again Another Code: The Code of the Deutsche Zoologische Gesellschaft

In 1894, on the initiative of J.V. Carus, the Deutsche Zoologische Gesellschaft published a newly adopted Code, the *Regeln für die wissenschaftliche Benennung der Thiere* (Carus et al. 1894). As far as priority is concerned, this Code selected 1758 as the starting point. It also insisted on a strict use of Latin grammar and orthography, and required that all names misspelled when first created be changed subsequently. As always, this Code was commented on. In particular, it was discussed in March 1896 at a meeting of the Zoological Society of London (Allen 1896; Sclater 1896; Stebbing 1896). Stebbing (1896:254) summarized that discussion as follows: “No body of regulations on the subject of scientific nomenclature can possibly give universal satisfaction.” Indeed, Stebbing (1896:257) asked about the necessity to comply with Latin grammar and spelling:

From among numerous examples offered, *Oplophorus* may be cited, which is to be corrected into *Hoplophorus*. Can anything be more superfluous? The Latins themselves were uncertain whether H was a letter or only a breathing. They fluctuated between the spelling of Adria and Hadrian, of Hannibal and Annibal. Why should we, then, be more Roman than the Romans?

As expected, Sclater (1896) stood by the Strickland Code on all important differences from this new German Code, especially the starting point, which he still claimed should be the 12th edition of the *Systema Naturae* instead of the 10th edition.

The Third International Congress of Zoology: The Creation of the International Commission on Zoological Nomenclature

The rules adopted by the International Congress of Zoology at Moscow, in 1892, were not the only rules available in the 1890s. For instance, ornithologists and other zoologists could use the Code of the American Ornithologists’ Union (which introduced rules for variety names); the Code of the Deutsche Zoologische Gesellschaft also was available to zoologists. This confusion ended at the Third International Congress of Zoology (Leiden, 1895) when F.E. Schulze (1896) proposed before the Congress participants that a commission of five members be nominated to produce one single code that could be recommended in different countries and be written in three languages (French, English, German). Schultze’s proposal was immediately adopted and the following five members, all experts on nomenclature, were nominated: Blanchard (Paris), V. Carus (Leipzig), F.A. Jentink (Leiden), P.L. Sclater (London), and W. Stiles (Washington, DC). Naturally, divergent opinions were soon expressed. At a meeting of the Zoological Society of London held in 1896, some participants argued that international committees be appointed, “not to draw up a Code of rules, but to produce

an authoritative list of names –once and for all– about which no lawyer-like haggling should hereafter be permitted.” (Allen 1896:328)

The decision made in Leiden marked the birth of the International Commission on Zoological Nomenclature. Now, the destiny of the Code would remain in the hands of both the Commission and the Congress. It would still be ten long years before the first Code would be published, as the *Règles internationales de la nomenclature zoologique* (ICZN 1905). As for priority, it was resolved at the Berlin Meeting (1901) that it would apply strictly, with no exceptions, although heated discussions about that issue did not cease.

Toward a Limitation of Priority

In today’s Code (ICZN 1999), exceptions to the principle of priority exist, such as in the case of *nomina protecta*, i.e., junior synonyms or homonyms being maintained under certain conditions (Article 23.9.1). This compromise, as always in nomenclature, has been heavily discussed before and after implementation: “The history of the Statute of Limitation [i.e., for the limitation of the application of priority] is long and tortuous” (Mayr 1972:453).

Early on, it was realized by the International Congress of Zoology and the International Commission on Zoological Nomenclature that, in some specific cases, the application of some of the 1905 *Règles* might need to be suspended to avoid nomenclatural confusion and promote stability. It is the reason that, in 1913, the International Congress of Zoology (Monaco) conferred upon the ICZN the plenary powers to limit the application of the 1905 rules. One of the purposes of those plenary powers was that zoologists could submit requests to the Commission to protect a junior synonym over a senior name (implying a reversal of precedence). That is the reason that, when Chapman (1923) argued that a strict application of priority generated confusion in nomenclature by resurrecting earlier and forgotten names, ICZN commissioner Bather (1923:182) promptly replied:

Mr. Chapman’s letter overflows with good sense; but it has all been said before. His laments, however, will not have been entirely wasted if you will permit this consolatory reply—namely, that in the year 1913, at the International Congress of Zoologists in Monaco, an agreement was reached in the largely attended section on nomenclature and confirmed in plenary session, by which the International Commission on Zoological Nomenclature was given power, on certain conditions, to suspend the rules in those cases where their operation was contrary to the general convenience. The Commission has, on the request of various zoologists, already taken action in several cases.

The possibility of submitting requests for limiting the application of priority was not enough for some authors, who thought, as did some taxonomists from the 1870s, that the Principle of Priority should be abandoned in favor of a principle of “continuity” (i.e., junior synonyms that were widely used should be conserved over earlier names). In the first half of the 20th century, debates about priority continued among zoologists as well as botanists (e.g., Shear 1924; Mansfield 1939; Heikertinger 1940, 1943a, b, 1953; Richter 1942a, b; Friebel and Richter 1943; Smith 1945; Blackwelder 1948). Beyond the Principle of Priority, it was the notion of stability that was discussed, i.e., whether it was realistic to think that nomenclature could reach absolute and permanent stability. Against authors who argued that favoring usage over priority was the best way to build a stable nomenclature (e.g., Shear 1924; Heikertinger 1940, 1943a, b, 1953), others replied that in stability could not and should not be avoided:

Any proposal that rates stability ahead of the advancement of the science of systematics or the development of one of its myriad components is a backward step and one doomed to ultimate failure and discard. (Blackwelder 1948:309)

Toward the end of the 1940s, it appeared to some zoologists that the ICZN needed to find more efficient ways to deal with regrettable name changes related to a strict application of priority, leading to confusion:

A great deal of name-changing continued during the inter-war years, a process which, as ... is evident from the correspondence reaching the Commission [Francis Hemming was then its secretary] from many sources, has led to an over-growing demand, both from systematists and from workers in the applied biological fields, that more effective means should be found to secure stability in zoological nomenclature. The whole question was considered with great care both by the International Commission and also by the International Congresses of Zoology at their joint meeting held in Paris in July, 1948. (Hemming 1950:371)

Much work and discussion led to the addition of an article limiting the application of priority in the first edition of the *Code* (ICZN 1961).

A major step toward a limitation of priority was the recognition (not the adoption) of the “principle of conservation” (of junior synonyms over senior synonyms) at the 14th International Congress of Zoology, Copenhagen, in 1953. Discussions that took place at the Colloquium on Zoological Nomenclature, in Copenhagen, are mentioned in the detailed Report edited by Hemming (1953), then secretary to the International Commission: “After considerable discussion, the Colloquium agreed, by a majority, to recommend the inclusion in the forthcoming edition of the Règles, of a provision recognizing the Principle of Conservation to a limited extent.” (Hemming 1953:25) Several drafts for an article governing the application of the principle of conservation were then presented by: (1) E. Mayr, (2) C.L.Hubbs and W.I. Follett, (3) N.D. Ridley, and (4) W.I. Follett, E. Mayr, R.V. Melville, and R.L. Usinger. All those drafts are reproduced in Hemming’s report (1953:119–122). However, “it was unfortunately found impossible to secure agreement upon any of these drafts” (Hemming 1953:25) and the precise wording was left to the responsibility of the International Commission.

A Statute of Limitation of the law of priority was adopted at the 15th International Congress of Zoology, London, in 1958, and was first introduced as Article 23b in the first edition of the *Code* (ICZN 1961). Article 23b reads as follows:

A name that has remained unused as a senior synonym in the primary zoological literature for more than fifty years is to be considered a forgotten name (*nomen oblitum*). (i) After 1960, a zoologist who discovers such a name is to refer it to the Commission, to be placed on either the appropriate Official Index of Rejected Names, or, if such action better serves the stability and universality of nomenclature, on the appropriate Official List. (ii) A *nomen oblitum* is not to be used unless the Commission so directs. (iii) This provision does not preclude application to the Commission for the preservation of names, important in applied zoology, of which the period of general usage has been less than fifty years.

Article 23b was controversial and, naturally, immediately discussed: “An unexpected and highly controversial innovation of the 1961 Code of Zoological Nomenclature ... is article 23(b), the “*nomen oblitum*” rule.” (Smith and Williams 1962:11) While some authors thought that Article 23b would promote stability, others only saw chaos in it (Smith 1964). A new vote, which took place at the International Congress of Zoology in Washington DC, in 1963, confirmed the limitation of priority by a small majority (Mayr et al. 1971). The problematic original wording of Article 23b partly explains the negative reactions. The idea of limiting the application of priority has remained in the *Code*, but Article 23b was modified throughout the successive editions of the *Code* in order to address several issues (e.g., Mayr et al. 1971; Collette et al. 1972; Corliss 1972; Mayr 1972; Cornelius 1987; Ng 1991; Melville 1995): Article 23b (ICZN 1961) became Article 79 (ICZN 1985) and is now back to Article 23.9–12 (ICZN 1999).

Smith and Williams (1962) mentioned several issues in the original wording: (1) every case of *nomen oblitum* needed to be submitted to the Commission (it now is an automatic process); (2) the limitation of priority dealt only with synonyms, not homonyms (reversal of precedence applies to

both synonyms and homonyms in the current Code); (3) “primary zoological literature” was poorly defined (this concept no longer is in the Code, where clearer criteria of publication are now considered; see Article 23.9.1, ICZN 1999:28). Ng (1991), in favor of a strict application of priority, provided a detailed analysis about interesting cases: a senior synonym became a *nomen oblitum* because of application of Article 23b; later on, the junior synonym and its senior synonym were thought to refer to two distinct units; a new name had to be created because the senior synonym had become a *nomen oblitum* and could no longer be used. The current Article 23.12.2 addressed that issue:

A name which was rejected under the former Article 23b may, in absence of any other cause of invalidity, be used as valid if it is no longer considered to be synonym of another name, or if its synonyms are themselves invalid under the provisions of the Code. (ICZN 1999:30)

The basic principle of conservation according to which priority may not apply strictly (in order to preserve stability and if certain conditions are met) has survived for about 50 years, i.e., since it was first introduced in the Code (ICZN 1961). However, debates about stability in nomenclature have certainly not decreased since 1961 in the context of both the zoological and botanical codes (e.g., Erzinclioglu and Unwin 1986; Tubbs 1986; Feldmann 1986; Brummit 1987; Hawksworth 1988, 1991; Hawksworth and Greuter 1989; Ng 1991; Silva 1996; Dubois 1998). The question being: how stable can or should nomenclature be? Although nomenclatural changes at the species level clearly are regrettable, especially when exclusively due to nomenclatural rules, it is important to keep in mind that, at least at the species level, nomenclature cannot be perfectly stable. For instance, Brummit (1987:80) argued that the type method (typification) “has an inbuilt element of instability” that can hardly be avoided. Also, Dubois (1998:1) argued that:

Name changes due to the mere application of nomenclatural rules are much less numerous than those due to the progress of taxonomic research, and they would be even much less common if zoologists and editors paid more attention to the international rules of nomenclature.

This last comment by Dubois leads us to discuss another issue related to the application of nomenclatural rules and stability of names: the enormous amounts of time it takes to get acquainted with the entire literature on a given taxon. Taxonomic work is poorly rewarded, and nomenclatural work is even more poorly rewarded. What people often do not understand, however, is that sound taxonomic work requires detailed nomenclatural knowledge. Regardless of the kind of rules supported by the Code (absolute priority versus priority and possible conservation of junior names), people talking about “stability” of zoological nomenclature need to understand that: (1) dealing with all nomenclatural situations requires valuable expertise; (2) in order to reach that level of expertise, taxonomists need to spend a great deal of time working on basic nomenclatural data (lists of names, type availability, etc.); (3) more importantly, this kind of work by taxonomists should be highly respected and regarded as a direct part of their mission and duties. Indeed, one of the reasons that nomenclature is unstable is that the history of taxon names is highly complex in all taxa; we need people to be experts of that history, regardless of our rules.

Let There Be Types!

As surprising as it might seem, a historical review of the type concept and the terms related to typification, as well as their practical and philosophical implications, still needs to be written. In particular, it is unclear when type specimens started being used by zoologists (and botanists for that matter). Although detailed studies clearly are needed on this topic, it seems that it was only in the 1890s that zoologists realized the critical importance of type specimens in relation to the application of the Principle of Priority for species names: in fact, many papers suddenly addressed that

particular issue starting in 1890 (Bather 1897; Cook 1898, 1900, 1902; Marsh 1898; Merriam 1897; Thomas 1893, 1897; Schubert 1897). Blanchard's (1889, 1893) early rules included articles on type species (because of the abundant literature on this topic published in the second half of the 19th century) but did not include anything on type specimens. Because of the literature published in the late 1890s on type specimens, the first *International Rules* (ICZN 1905:39) provided a brief recommendation on type specimens in the appendix: "This diagnosis should state in what museum the type specimen has been deposited and should give the museum number of said specimen." Terms such as holotype, paratype, syntype, and neotype, became more widely used only later (e.g., Frizzell 1933; Schenk and McMasters 1936).

One important challenge at the beginning of the 20th century was to go through all the terms that had been proposed so far for types and select those appropriate for the Code. This was not an easy process; Frizzell (1933) inventoried no fewer than 233 terms available in the literature, most of which have been completely forgotten, such as: aedoeotypus, agriotype, chorotype, graphotype, hypotype, ideotype, and tautogenotype. This long list of terms shows that taxonomists, especially those interested in nomenclature, have always been profusely creative. More importantly, it also shows that the terminology related to types was abundantly discussed before terms were chosen for the Code (as always in nomenclature).

THE 1960S: ANOTHER GREAT DECADE FOR NOMENCLATURE DEBATE

THE EMERGENCE OF NUMERICAL TAXONOMY AND ITS IMPACT ON NOMENCLATURE

The 1960s is a fascinating period for nomenclatural history. Indeed, it saw the publication of the first edition of the Code (ICZN 1961) as well as some of the most radical changes ever proposed in nomenclature (most of which were published in *Systematic Zoology*), mainly as a consequence of the emergence of numerical taxonomy (Sneath 1961; Sokal and Sneath, 1963; Sokal et al. 1965; references therein) and early use of computers (Jahn 1961; Sokal and Sneath 1963, 1966). The 1961 Code was preceded by the *Copenhagen Decisions on Zoological Nomenclature* (Hemming 1953), which were abundantly discussed (e.g., Follett 1954, 1956; McMichael 1956; Bradley 1957). Naturally, the Code was also discussed (e.g., Smith 1962).

Ralph and Wood (1954) introduced some brief thoughts on the use of "punch cards" in taxonomy and the need for a "standardized biological taxonomy code" whenever species are referred to with punch cards from different laboratories. However, some of the most radical changes in nomenclature were proposed immediately after the 200th birthday of the 10th edition of the *Systema Naturae* was celebrated (e.g., Lindsey & Usinger 1959; Stearns 1959).

In the 1960s, the use of computers in taxonomy was referred to as "electronic data processing," or EDP, brilliantly summarized by Sokal and Sneath (1966:2):

Computers can help the taxonomist in many ways. They rapidly compute similarity coefficients among taxa. They are able to generate classifications based on representation of taxonomic structure by any of a number of definable principles and can present the output of such computations as dendrograms or other schemes by a variety of graphic output devices. Computers can store information about single species, such as characters, collections, and distribution records. This information can be searched at tremendous speeds and summaries or reports obtained on any group classified by any of a variety of criteria. Computers can store accession lists of museums and regularly update these in terms of loans and new accessions. They can prepare lists of materials available for study in a museum and search the lists of museum holdings for special material of interest to a given investigator.

"The Machines Will Win, Whether the Taxonomists Fight or Not"

Jahn's (1961) article on "Man versus Machine: a future problem in protozoan taxonomy" constituted one of the first upfront radical attempts at challenging traditional nomenclature. In this contribution,

Jahn (1961:179–180) argued that a computer-based, fully logical and numerical system of nomenclature should be developed. Some of Jahn's statements merit citation here, such as:

(1) We could save a considerable amount of time and a larger amount of confusion for investigators and for students by the use of numbers as well as names for all taxa, and even by the substitution of numbers for names in many technical discussions. (2) The most efficient use of computers would require a completely logical taxonomic system. (3) The development of a completely logical system will require some severe revisions of our present system. (4) Names, although still used, will become less important. (5) The laws of priority will decrease in importance along with the names, and might eventually be discarded. (7) All of these developments will irritate most taxonomists. (8) The taxonomists will rise to fight the machines. (10) The machines will win, whether the taxonomists fight or not.

In Jahn's system, *Plasmodium hexamerium* is given the number "1310101D" (i.e., 1 for the Phylum Protozoa, 3 for the Class Sporozoa, 101 for the family Plasmodiidae, etc.). Jahn strongly favored logic above all concepts in nomenclature, including priority:

It may be necessary to ignore and/or discard the rule of priority, certainly, at the least, emend it, before we can devise logical systems of taxonomy. The literature on the superorder Lobida is complex enough to be used as an outstanding example of the fact that a completely "legal" system is likely to be illogical and that a completely logical system is therefore likely to be "illegal"... (Bovee and Jahn 1965:238)

Numericlature: Should Numbers Replace Names?

Several authors commented on and expanded Jahn's proposal during the 1960s. Michener (1963) applied Jahn's nomenclatural proposal to numerical taxonomy. Michener's basic idea was that, since numerical taxonomy constituted a new, objective, experimental way of classifying organisms, it also represented a great opportunity to reform nomenclature. Michener tried to imagine what kind of nomenclature would be developed by scientists (whom he refers to as "exobiologists") if they were to find organisms to classify on another planet. Michener argued that, as radically new as it might seem, such a new system could be implemented by "earth-oriented taxonomists." Under Michener's numerical system of nomenclature, *Musca domestica* is referred to as "0325761," i.e., a "meaningless reference number" in order to "avoid Linnaeus's error of incorporating into the designation of the organism information about its classification which is subject to change with improved knowledge or changing ideas." (Michener 1963:166) However, classification can be represented using a numbering system similar to that proposed by Jahn (1961), with the addition of a dash to separate numbers though, so that *Musca domestica* can be referred to as "10-7-26-081-052-0325761."

Little (1964) slightly modified Michener's system. Little introduced the possibility of adding subgroups between parentheses, so that "-001(002)-" would mean the subfamily 002 within the family 001; he also introduced a standard of three numbers within each category so that classification is represented as "...010-007-..." instead of "...10-7-..." because, according to him, it would ease the development of such a system using computers.

Hull (1966:14) proposed what he calls "Phylogenetic Numericlature," i.e., a "system of identification, positional, and phyletic numbers for taxa that makes possible a significant relationship between numerical classification and phylogeny." Indeed, the numbering system of nomenclature or numericlature, developed by Michener, intended to represent phenetic similarities. Hull (1966) slightly modified Michener's system so that phyletic relationships could be represented.

Other authors, such as Rivas (1965), were in favor of implementing a numerical system to ease storage and retrieval of taxonomic information using computers, but such a system would only complement the current nomenclature, especially the binominal nomenclature, which should not be abandoned.

However, several authors (Crovello 1967; Parkes 1967; Randall and Scott 1967) expressed some doubts as to whether a numerical system was absolutely necessary to develop computer-based

storage and retrieval of taxonomic data. According to those authors, storage and retrieval of taxonomic data could also be developed efficiently using the current nomenclatural system: “Michener and others under-estimate the ability of machine systems to conform to user conventions” (Randall and Scott 1967:281).

A debate about the syntax of nomenclature also took place. Indeed, Hull (1968:474) argued that Randall and Scott (1967) had overlooked an important aspect of numericulture: “The main reason for introducing numericultural systems into taxonomy is to provide an explicitly formulated syntax adequate for the purposes of taxonomy.” Also, according to Hull (1966:473), “In order for words to form a language (whether natural or artificial) there must be rules of syntax. The syntactical rules of Linnaean nomenclature are minimal.” Randall and Scott (1969) claimed that Hull had failed to provide convincing arguments to demonstrate that Linnaean nomenclature was less syntactical than numericulture, to which Hull (1969:469) replied that “the syntax of nomenclature may be adequate for the purposes of electronic data processing, but it is inadequate for several other purposes.” One of the purposes better served by numericulture than nomenclature was, according to Hull (1969:469), the “relationship between classification and phylogeny.”

Yochelson (1966) agreed that computers could help increase data processing and argued that, therefore, the Code should be simplified, especially with respect to grammar and orthographic issues because it would greatly simplify the use of computers. Thus, according to Yochelson (1966), taxon names (whether they have been Latinized or not) should not be regarded as Latin names, but rather as symbols used for communication:

Adoption of the underlying philosophy that nomenclature is a language in its own right will clear away much debris. Does it really matter in the space age whether *-opsis* is masculine, feminine, or neuter? Is it a matter of biologic significance that specific names should be changed from *-a* to *-us*? Why not forget all “latinization” and its accompanying sterile scholarship, and get on with the study of biology (Yochelson 1966:89)?

The idea that nomenclature should free itself from Latin grammar was not new. Some authors had argued similar ideas in the 1870s. It is interesting to note, however, that this idea was defended by Yochelson in a new context, as a consequence of the debate on the use of computers in taxonomy. As expected, some people disagreed and immediately reacted. Tortonese (1967:278) argues that “if we retain Latin, we really ought to have it in grammatically proper form and not complain if some time is necessary to effect this.”

Dupraw (1965) pushed the application of numerical treatment a step further and got rid of taxonomic categories and nomenclature altogether in a system that he referred to as “Non-Linnean taxonomy”:

In the non-Linnean system, definition of conventional, hierarchical categories (species, genus, etc.) is eliminated; specimens are dealt with as individual points on a multivariate scatter diagram, which represents the closest two-dimensional approximation to the specimen distribution in a “full-dimensional” character hyperspace.” (Dupraw 1965:1)

In a controversial article entitled Efficiency in Taxonomy, Sokal and Sneath (1966) reviewed the recent literature on taxonomy and addressed some misunderstandings about numerical taxonomy. They also discussed nomenclatural issues. In particular, they argued that “any new nomenclatural system must clearly be EDP oriented... [and] should allow for automated information storage and retrieval.” (Sokal and Sneath 1966:10) As we saw, this was a central theme developed during the 1960s that originated from the recent availability of computers and punch cards. Sokal and Sneath (1966:10) also argued that “we should free ourselves from the notion that a name can fulfill a multiplicity of functions.” Sokal and Sneath (1966) supported Michener’s proposal under which unique registration numbers could replace species names. Also, they thought that supra-specific affinities would better be expressed by “classification numbers” because “the usefulness of the old genus

names has largely vanished.” (Sokal and Sneath 1966:11) Naturally, some authors replied to Sokal and Sneath. In particular, Kalkman (1966) argued that names are much more easily remembered than numbers and that, in the future of nomenclature, numbers should not replace names, in spite of the clearly beneficial use of computers for other purposes.

Oldroyd’s Views on the Future of Taxonomy

Oldroyd (1966) wrote a controversial article entitled *The Future of Taxonomic Entomology* to which several people replied vigorously. In this contribution, Oldroyd focused on necessary changes in the Code and the role of the International Commission on Zoological Nomenclature in exploring and devising changes. In particular, Oldroyd (1966:253) argued that “the rigid framework of a mandatory Code, and in particular its type system are incompatible with quantitative methods based on a long series of specimens,” and that, therefore, the Commission should “plan for a date, not too far ahead, after which all traditional taxonomy shall cease at the species-level and below.” According to Oldroyd, new practices of quantitative taxonomy (based on large numbers of individuals) should replace traditional taxonomy (based too often, according to Oldroyd, on too few specimens) because of the new development of powerful computer-based tools that help analyze large amounts of data when delineating species. Oldroyd (1966:254) claimed that the type system indirectly encourages taxonomists to describe new species based on too few specimens: “The greatest evil of the type-concept, however, is that it actively encourages the practice of describing monotypic units: species based upon single specimens, or genera based upon single species.”

Several authors reacted against the idea of abandoning types (e.g., Gurney 1967; Parkes 1967; Steyskal 1967) and reminded Oldroyd that holotypes, lectotypes, neotypes, and syntypes are just name-bearers. But Oldroyd (1967) insisted: “We have made a fetish of types, and going somewhere to ‘see the types’ has become a taxonomic pilgrimage. In fact, the type of *Felis leo*, if it still exists, is a sacred cow.” I have spent (and intend to continue to spend) quite some time tracking “sacred slugs and snails.” Oldroyd actually confused two distinct issues. The first issue is taxonomic, i.e., the number of individuals used by taxonomists to delineate species, and most taxonomists would agree that so-called “typological” approaches in which only one or few specimens are considered should be avoided as much as possible. The second issue is nomenclatural, i.e., the use of single (holotype, lectotype, neotype) or several (syntypes) specimens as reference points for a species name.

Heise and Starr’s Philosophy of Nomenifers

A thorough analysis of the concept of “type specimen” was provided by Heise and Starr (1968). In this article, the authors explored philosophical questions related to the use of type specimens, which they referred to as “nomenifers,” (literally “name-bearers”), a term first coined by Schopf (1960):

The name associated with a type specimen (a nomenifer) is a class name, not a proper name, which names the taxon of which the nomenifer is a member. The nomenifer is not merely a member of the taxon than is any other member, which is to say that the nomenifer does not have a privileged logical status. By convention, the nomenifer’s name (unlike that of any other member of the taxon) cannot change as long as there is a taxon with that name. The nomenifer serves epistemologically as an “official,” certain, or unquestionable (but not necessarily typical) instance of the taxon whose name it bears. (Heise and Starr 1968:467)

This illustrates well the idea that debates in nomenclature help generate better understanding of the theory and philosophy of nomenclature, as Heise and Starr’s analysis helped clarify the philosophy surrounding type specimens after Oldroyd argued against the use of type specimens.

UNINOMINAL SPECIES NAMES

An Early Challenge to Binominal Nomenclature: Amyot

Amyot seems to be the first author who described precisely a uninominal species nomenclature, to which he referred as “*méthode mononymique*.” He also courageously applied it throughout his entire monograph on the *Rhynchota* (Amyot wrote all Latin taxon names in italics), a group that includes hemipteran insects. According to Amyot, species names were too unstable and species nomenclature should become independent from classification:

This species is indicated in your collection under the most modern name; it really is the specific name that is inherited from Linné, Fabricius, etc., but the name of the genus has changed several times.... You have today’s generic name, but tomorrow will come a new author who will change it again, then after him other ones, and you will thus have to change the name of the genus endlessly. (Amyot 1848:4, tr.)

Under Amyot’s nomenclature, species were named using a single Latin word beginning with a capital letter, such as *Solenostethium*, *Irochrotus*, *Odontotarsus*, etc. However, those uninomials were not the specific names of the Linnaean binomials designating those species. To guarantee uniqueness and avoid homonymy (too many species would be called simply *nigra*, *rubra*, etc.), Amyot created new uninomials for species.

Amyot’s higher classification was also quite unusual. His classification was based on a hierarchy of groups subordinated to each other, but included very few of the well-known ranks. Amyot used the terms “*Ordre*” (Order) and “*Tribu*” (Tribe) for two ranks in his hierarchy, but most other ranks were indicated by new terms: the Section, Subsection, and Trisection levels were above the Order; just below the Order was the Tribe; below the Tribe were the Division, Subdivision, and Tridivision levels. Also, Amyot rejected the term “genus,” which he thought had nothing to do with its etymological root (*generare* in Latin, which means “to reproduce”). Thus, Amyot did not use generic names in his monograph.

Amyot’s system of classification was highly innovative: species could be placed within divisions, subdivisions, or tridivisions, depending on the level of subordination needed to classify a certain taxon. For instance, the second Tribe of the Order *Hemiptera* was called *Breviscuti* and included three Divisions: the 31 species of the Division I (*Supericornes*) were classified in tridivisions; the 77 species of the Division II (*Infericornes*) were placed in subdivisions because tridivisions were not needed; and finally, the two species of the last Division III (*Caecigenae*) were directly placed within the Division because no subdivisions or tridivisions were needed. This system is interesting; Amyot classified species within taxa of any categorical rank. It clearly simplified the classification and avoided having to deal with long lists of taxon names when a suborder or a superfamily includes only one species. Although Amyot used ranks, he used them very freely, in a way similar to a rank-free system. The drawback of Amyot’s innovative nomenclatural system was that, because he rejected genera as well as other taxonomic ranks, he created new names for many higher taxa in addition to all the new names he created for the species. Amyot’s system was never adopted, probably because of confusion due to the abundance of new names.

One of Michener’s Proposals for “Earthly Nomenclature”

One of the “possible reforms in earthly nomenclature” proposed by Michener (1963) was a uninominal system of nomenclature for species names. In doing so, Michener applied ideas defended by others before him. For instance, Michener cited Cain (1959):

It seems clear that with the great alteration in the status of the genus since the time of Linnaeus, an effectively uninominal system will come into use. This will then be free from that part of the present instability caused by generic changes, which are mostly matters of opinion, and from the inclusion of a particular way of classifying things in the names of things to be classified.... The necessity for avoiding

homonyms and synonyms will, of course, remain as before, since it is a character of all reference systems. (Cain 1959 in Michener 1963:164)

Michener also cited Berio (1953), according to whom a “complete freeing of nomenclature from systematics would provide the ideal means for stabilizing nomenclature, for in the main, names are changed for reasons of a systematic nature.” (Berio 1953 in Michener 1963:164) However, as mentioned above, the idea of using uninominals had been defended by earlier writers, especially by Amyot (1848), and, according to De Candolle (1813), by Buffon.

Michener’s uninominal method was simple: the combination between the generic and specific names should remain fixed, even if the species is transferred to a new genus, and the original date of specific name is added to the fixed combination: *Apis mellifera* thus becomes *Apis-mellifera*, 1758 or can be abbreviated simply as *A.-mellifera*. Michener (1964) applied his uninominal method to the tribe Paracolletini (Hymenoptera, Apoidea) and showed that a new classification of the 332 species listed in this tribe would require 288 new combinations and 16 homonyms, all of which could be avoided using a uninominal system. The idea of fixing binomials to avoid generic name changes was first proposed by Sharp (1873).

Naturally, authors expressed their disagreement with Michener’s innovative ideas on species nomenclature. Steyskal (1965:348) defended the current system because, after all, it was “already serving quite well,” which is an argument that new proposals commonly have to face. And Johnson (1970:234) also argued that the current system of nomenclature should be retained chiefly “*faute de mieux*.”

An Answer to Michener: Amadon’s “Suneg” Concept

Amadon’s (1966) paper certainly shows how creative taxonomists were in the 1960s. Amadon thought that Michener’s fixed binomials were too confusing because *Binghamiella-antipodes* 1853 would be a species in the genus *Callomelitta* 1853 (example from Michener 1964:187). However, Amadon agreed that species binomials were not stable enough. Thus Amadon (1966) proposed a new system, based on a new concept, the “suneg,” (“genus” spelled backward) and defined as follows:

A term identical with the type genus of a family, subfamily, or tribe, but to be printed in Roman, not italics, and to be used, except in strictly taxonomic publications, in lieu of generic names for all the species in any given family, subfamily, or tribe. (Amadon 1965: 55)

Amadon’s “suneg” names referred to taxa that are more or less similar to family names in the sense that they are much larger (more inclusive) than genera. Thus, using them as the first part of species names reduced the risk of name changes due to the transfers of species from one genus to another. Amadon (1966:56) illustrates his system by proposing the names *Alca impennis*, *Alca torda*, *Alca brevirostre*, and *Alca grylle atlantis*, for, respectively, *Pinguinus impennis*, *Alca torda*, and *Brachyramphus brevirostre*, and *Cepphus grylle atlantis*... There seems to be a simpler way to solve that issue, which is to use family names as the first part of species binomials whenever generic names cannot be determined.

Lanham’s (1965) Uninominal Form of Species Names

Lanham (1965) agreed with Michener that species names are not stable enough because species are too often transferred from one genus to another, but he proposed another uninominal method for species names. This method, which was presented and commented upon on several occasions (Dayrat et al. 2004; Dayrat 2005a; Dayrat and Gosliner 2005), is a form of species names allowed under the *PhyloCode* (Cantino and de Queiroz 2007), at least once species names are established under the authority of the appropriate rank-based code (Dayrat et al. 2008). It is based on the asso-

ciation of the specific name, the author's name, the original date, and the page number, such as in *montana* Cockerell, 1928:365.

Lanham (1965), as well as Berio (1953), Cain (1959), and Michener (1963, 1964), and even Amyot (1848) and Sharp (1873) in earlier times, all favored a species nomenclature that would exclusively serve a designatory purpose, i.e., unique identification of species, but would not be used for indicating supra-specific relationships. Lanham (1965:144) also argued that "consideration of supra-specific categories should be deleted from the Code." According to Lanham:

An arbitrary list of families, used with the understanding that it is only a rough approximation to evolutionary relationships, and used only as a filing system, would fill many requirements. Perhaps the currently used array of supraspecific categories would serve quite well to express phylogeny, with informal considerations of usage and usefulness, instead of the present pseudo-legal technicalities, serving to give a degree of stability. (1965:144)

Also, as Lanham rightly pointed out, uninomials are "well suited to computer method of storing information" because they do not change over time (except for changes related to the taxonomy).

Whitehead's Comments on Uninominal versus Binominal Systems

According to Whitehead (1972), the binominal system is the interface between low-level (species) and high-level (supra-specific) taxonomy. Whitehead (1972:222) was of the opinion that it is a great thing that binominal nomenclature "provides the means for expressing each new advance in taxonomic understanding," and that adopting a uninominal system would simply "remove the evolutionary content from the very point in the nomenclatural system where it has greatest significance, i.e., at species level." Another interesting idea introduced by Whitehead was that, regardless of whether the binominal system was abolished, the interface between low- and high-level taxonomy would still exist, and that it probably was better to face it (binominal system) rather than avoid it (uninominal system).

The adoption of a uninominal system has not been considered in the context of the current rank-based codes. However, this does not solve the issues mentioned by Michener, Lanham, and others, i.e., the fact that species names are poorly stable because species are regularly moved from one genus to another. On the one hand, a uninominal system would radically and logically solve that issue. On the other hand, many taxonomists undeniably wish to maintain an indication of supra-specific relationships within the species name. The question thus becomes: could the current system be modified so that both goals could be served? A possible answer is discussed next.

TODAY'S NOMENCLATURE DEBATES

THE EMERGENCE OF THE *PHYLOCODE*

The *PhyloCode* (Cantino and de Queiroz 2007) or *International Code of Phylogenetic Nomenclature (ICPN)*, provides rules for defining clade names through explicit reference to phylogeny. Phylogenetic nomenclature constitutes a new approach to nomenclature mainly because, in contrast to the rank-based codes (*ICZN, Code; International Code of Botanical Nomenclature, or ICBN; Bacteriological Code, or BC*), it is independent of mandatory ranks and types. The *PhyloCode* has been through seven drafts since 2000. The current version (4b) differs from previous drafts because it includes a new article on species names (Dayrat et al. 2008). The *PhyloCode* will be published in the near future, along with the companion volume, which will include a first series of definitions of clade names across the Tree of Life. The date of publication of the *PhyloCode* will be its official starting date too: i.e., systematists will be able to refer to clades and publish new clade names using the *PhyloCode* instead of a rank-based code.

After the publication of the *PhyloCode*, two codes will then coexist (the Code and *PhyloCode* for zoologists, *ICBN* and *PhyloCode* for botanists, *BC* and *PhyloCode* for bacteriologists). It is difficult to predict how the coexistence of two codes will actually affect the work of systematists, biologists, as well as other users of taxon names. In any case, there is no particular reason to think that taxonomy will suddenly become “chaotic” when the *PhyloCode* is officially implemented. In that regard, it is interesting to note that, in the second half of the 19th century, zoologists had several competing codes to choose from, each code promoting different rules (including major differences such as distinct starting points and distinct applications of the principle of priority), which ultimately yielded a constructive outcome, i.e., the creation of the International Commission on Zoological Nomenclature (1895) and the first *International Rules for Zoological Nomenclature* (ICZN 1905). The existence of more than one nomenclatural code can thus be regarded as a positive sign of the current dynamism in nomenclature and taxonomy, instead of as a source of chaos, although other authors might argue that taxonomists should have other concerns and priorities in this era of biodiversity crisis.

WHAT FUTURE FOR THE COEXISTING CODES?

It is difficult to predict how this coexistence will evolve and end in the long-term future mainly because we cannot know in advance what practices will be favored by taxonomists. Also, there are other factors that are even harder to predict and that will certainly play an important role, such as the sociological behavior of taxonomists (e.g., some influential systematists might start a “trend” of using—or rejecting—the *PhyloCode*). At this stage, we can draw at least five possible scenarios. (1) The rank-based codes will prevail, and the *PhyloCode* will disappear without influencing them. (2) The rank-based codes will prevail, and the *PhyloCode* will disappear but only after influencing them and causing nomenclatural changes. (3) The rank-based codes will be replaced by the *PhyloCode*. (4) The rank-based codes and the *PhyloCode* will continue to coexist, overlap, and remain independent. (5) The rank-based codes and the *PhyloCode* will merge and be reconciled into a single code of nomenclature (specificities of each rank-based code could be maintained, such as the treatment of type specimens).

Clearly, different people would vote or hope for different scenarios. The last scenario would have my preference as it would seem to be a good solution. A possible way of merging the *PhyloCode* and the Code (for zoologists) would be that the *PhyloCode* rules govern all clade names above the species level (with, e.g., no mandatory ranks and types), and the ICZN rules govern the application of species names (with, e.g., type specimens). This would bring together the best of each code. In fact, the latest version of the *PhyloCode* requires that new species names be created under the authority of the appropriate rank-based code, although it also provides rules and recommendations on how to cite species names once those are made available.

Given the usual pace at which nomenclature evolves, it is probably going to take many years before we can determine how the interactions between the *PhyloCode* and the rank-based codes are going to evolve and, perhaps, end. Again, in nomenclature, things take a great deal of time: It took zoologists not less than 50 years to choose a common starting point for zoological nomenclature; 16 years went by between the time the first International Congress of Zoology (Paris, 1889) started to discuss the necessity of common and international rules of zoological nomenclature and the time those rules were published (ICZN 1905).

Before possible interactions between the *PhyloCode* and the Code are discussed in more detail, it is necessary to summarize their similarities as well as their differences. An abundant literature can be found on that subject (Cantino and de Queiroz 2007 and references therein); only the major points are summarized here. The *PhyloCode* shares several similarities with the rank-based codes, including the *BioCode*. First, both systems share the same fundamental goals, i.e., providing unambiguous methods for applying names to taxa, selecting a single accepted name for a taxon among competing synonyms, and promoting nomenclatural stability. Also, both systems deal with taxon

names, *not* taxa, that is, they govern the application of names, not how taxa are delineated. Both systems use the principle of priority, or precedence, to determine the correct name of a taxon when synonyms exist. However, both systems have conservation mechanisms that allow a later established name to have precedence over an earlier name.

The *PhyloCode* also differs significantly from the rank-based codes. The phylogenetic system is independent of mandatory taxonomic ranks. In the phylogenetic system, “species” and “clade” are not ranks but different kinds of biological entities. Phylogenetic nomenclature uses phylogenetic definitions (node-based, branch-based, apomorphy-based) to determine the application of clade names, whereas the rank-based codes use mandatory ranks and types. Under the *PhyloCode*, only clades (and species) can be named, whereas the rank-based codes allow the creation of names for all taxa regardless of their phylogenetic status. The establishment of a name under the *PhyloCode* requires both publication and registration, which is not mandatory in the rank-based codes. Finally, the *PhyloCode* applies to the entire Tree of Life (as does the *BioCode*).

The methods of application of taxon names constitute a major incompatibility between the phylogenetic system and the rank-based system. At this stage, it is difficult to envision a code in which the application of names would be governed through both mandatory ranks and types (type species, genera, etc., not type specimens) and phylogenetic definitions. Ranks could potentially be used in the context of the *PhyloCode*, as indicated in *PhyloCode* Article #3: “This code does not prohibit, discourage, encourage, or require the use of taxonomic ranks.” However, the use of *mandatory* ranks is incompatible with the *PhyloCode*. The possible introduction of optional ranks into the *PhyloCode* has been poorly explored so far; new interesting proposals may be provided in the future.

THE SPECIAL CASE OF SPECIES NAMES

Beyond and despite incompatibilities, it is worth asking whether there are areas in which the *PhyloCode* and the rank-based codes could be reconciled. Few articles address that question (e.g., Kuntner and Agnarsson 2006). However, as surprising as it might seem, the form of species names is an area in which the rank-based codes could potentially benefit from the recent findings in phylogenetic nomenclature. An Article (#21) dealing with species was recently added to the *PhyloCode*, after more than ten years of debates. At the first meeting of the International Society for Phylogenetic Nomenclature (ISPN) in Paris, July 2004, it was decided that a species code separate but compatible with the code for clade names be drafted by Philip Cantino, Julia Clarke, Kevin de Queiroz, and me. This species code was supposed to incorporate an epithet-based form of species name similar to the form of species names first proposed by Lanham (Lanham 1965; Dayrat et al. 2004, 2008; Dayrat 2005a; Dayrat and Gosliner 2005). However, after two years of work on this species code, it was realized that species names could be better dealt with in a different way. Problems encountered in the development of a species code included: (1) the coexistence of two species nomenclatures; (2) the fact that new species names established under the *PhyloCode* would have to be regarded as available (Code) or validly published (*ICBN, BC*) under the rank-based codes; (3) some uncertainty on how to address infra-specific names; (4) typification inconsistencies among the rank-based codes; and (5) an absence of major benefits justifying the development of a new species code. The alternative solution, presented at the second meeting of the ISPN (Yale University, June 2006), led to the adoption of Article #21 in May 2007 and its introduction into the *PhyloCode* in September 2007.

The historical development that led to Article #21 as well as its content have already been presented and discussed (Dayrat et al. 2008). Only the main properties of Article #21 are summarized here. (1) It requires compliance with the corresponding rank-based codes for creating new species names and determining precedence among synonyms. (2) The two parts of Linnaean binomials are interpreted in a way that is consistent with phylogenetic nomenclature. In particular, the first word of a binomial is interpreted as a “prenomen” instead of a generic (ranked) name, once it is established under the appropriate rank-based code. (3) The use of a species prenominal (i.e., ranked generic name under the *International Code of Zoological Nomenclature*) is not mandatory once a

species name is established. A uninominal form of species name can then be used (based on the citation of the specific name or epithet as well as, at least, the author's name and the original date), and supra-generic names can be associated with the specific epithet (see below). In any case, if a prenominal is used, it is not tied to any rank under the *PhyloCode*. (4) Symbols are recommended to convey phylogenetic information about generic names (prenomina under the *PhyloCode*).

Article #21 recently added to the *PhyloCode* presents several advantages. (1) One description satisfies the requirements of both phylogenetic nomenclature and the rank-based codes: more importantly, the authority of rank-based codes is recognized for the creation of new species names. (2) The *PhyloCode* now is complete in addressing both clade and species names. (3) Species names (once established) can be used in a way that is consistent with the principles of phylogenetic nomenclature. (4) It does not disrupt established and critical taxonomic practices, especially the use of name-bearing type specimens (holotypes, syntypes, lectotypes, neotypes) for determining precedence among competing synonyms. (5) Finally, better communication of taxonomic (phylogenetic) knowledge is promoted because, for instance, species can be referred to clades that correspond to supra-generic taxa if no monophyletic taxon of generic rank can be found.

The latter point is a significant improvement in species nomenclature. I have been arguing for several years (since the 69th Annual Meeting of the American Malacological Society, Ann Arbor, Michigan, 2003) that the mandatory use of a generic name as the first part of Linnaean species binomials is problematic in cases where species cannot be assigned to any monophyletic taxon of generic rank (Dayrat et al. 2004, 2008; Dayrat 2005a; Dayrat and Gosliner 2005). This situation is particularly common in groups for which phylogenetic relationships are poorly known. That is the reason that, in 2005, a new species was described and named using a uninominal form of species names, "*aliciae* Dayrat, 2005." The specific name was combined with a family name instead of a generic name as the first part of a binomial, *Discodorididae aliciae* Dayrat 2005, because the only clade in which *aliciae* could be placed was the "family" Discodorididae. It was intentional that the binominal nomenclature promoted by the *International Code of Zoological Nomenclature* was not followed, although the genus name "*Discodoris*," which then referred to a metaphyletic taxon, was proposed as a possible generic name for those who would want to follow the strictly Linnaean species nomenclature. In fact, Berhens and Hermosillo (2005) cite that species as *Discodoris aliciae* Dayrat 2005.

The combination of supra-generic taxon names and specific names (or epithets) is allowed under the *PhyloCode*, once species are established. However, because the *PhyloCode* relies on the rank-based codes for the creation of new species names, it cannot allow the use of a supra-generic name instead of generic names in a newly created species binomial. So, in other words, the creation of a new name such as *Discodorididae aliciae* would not be allowed under the *PhyloCode*, because it is not allowed under the *International Code of Zoological Nomenclature*. But could the Code allow, when necessary, the use of a supra-generic name in lieu of a generic name as the first part of a species binomial?

TOWARD A MORE FLEXIBLE LINNAEAN BINOMINAL NOMENCLATURE?

In several publications from the 1960s (Michener 1963; Lanham 1965; Sokal and Sneath 1966) authors supported the use of a uninominal species nomenclature to solve a major issue in nomenclature, i.e., the instability of species names when species are moved from one genus to another. This instability is directly related to an issue already discussed by me, i.e., the fact that species cannot always be placed in a clade of generic rank because relationships are poorly known (Dayrat et al. 2004, 2008; Dayrat 2005a, submitted; Dayrat and Gosliner 2005). The major source of the problem here is that the use of a generic name as the first part of a Linnaean binomial is *mandatory*. This is too rigid and sometimes forces taxonomists to place species within genera based only on mere intuitions, although there is no phylogenetic evidence indicating that that given genus is monophyletic. Because intuitions differ, species are moved from one genus to another or, if they are not moved, are temporarily placed in genera that are not clades. In any case, the rigid mandatory use of a generic

name generates confusion, as it tends to pretend that we know about relationships when we actually do not know.

The fact that a uninominal form of species names has only seldom been used or supported (Dayrat 2005a; Angielczyk 2007; Béthoux 2008) suggests that taxonomists may still not be ready to accept a uninominal method of species names, mainly because most taxonomists wish to conserve an indication of supra-specific relationships within the species name. So, could the binominal system be modified so that it could be both more stable and indicate more accurately evolutionary relationships? The conflict between those two goals (stability and indication of supra-specific relationships) is largely due to the fact that the current binominal system is too rigid; the mandatory use of a name of genus rank as the first part of the Linnaean binomials is the main source of conflict.

As pointed out by Amadon (1966) in his critique of Michener's (1963) uninominal form of species names, Linnaeus's genera correspond to much broader taxa than our current genera. There is no need to adopt a new concept, such as Amadon's "suneg." When no clade of genus rank can be determined, the specific name just needs to be combined with a supra-generic name, such as a family name, such as in the name *Discodorididae aliciae* (Dayrat et al. 2004, 2008; Dayrat 2005a, submitted; Dayrat and Gosliner 2005).

The name *Discodorididae aliciae* is binominal. It does not comply with the current rules of nomenclature however, because Discodorididae is a family name and not a generic name under the *International Code of Zoological Nomenclature*. The name *Discodorididae aliciae* is phylogenetically rigorous because, although we know that *aliciae* belongs to the clade Discodorididae, we do not know which "genus" it belongs to. The species name *Discodorididae aliciae* thus conveys accurate phylogenetic information.

In order to allow species names such as *Discodorididae aliciae*, the Code would have to be modified. Binominal nomenclature would need to become more flexible: supra-generic names would have to be allowed as first parts of species binomials. It does not mean that all species names would have to be changed. This solution could be used only in cases where we positively know that using a generic name is misleading and confusing. This flexible binominal nomenclature would probably be more stable in the long term because species would not have to be moved from one taxon to another, unless significant progress is made about systematic relationships.

Some readers probably think that this proposal is heretical and marks a complete rejection of the Linnaean species nomenclature. Although it is clear that it would mean an important change in the current species nomenclature of the Code (and other rank-based codes), it is not certain that it would mean a betrayal of Linnaeus's own taxonomic use of the binominal nomenclature. Indeed, if Linnaeus had discovered *aliciae*, he would have placed it in his genus *Doris*, which then included all dorids (Discodorididae, Dorididae, Chromodorididae, etc.) and corresponds to at least a superfamily or a suborder under the Code. Thus, it could be argued that, although a name such as *Discodorididae aliciae* is not strictly Linnaean under the Code, it is Linnaean in spirit because Linnaeus used names of much broader taxa as first parts of species binomials.

In a way, it appears that our problems come from the fact that the binominal species nomenclature has been maintained as a strict nomenclatural form of species names without taking into account the changes in taxonomic practices, especially the fact that Linnaeus's broadly inclusive genera are taxonomically very different from our more restricted genera. Our current rank-based nomenclatural system includes dozens of ranks, whereas Linnaeus used very few ranks. In most cases, Linnaeus's genera would now be taxonomically ranked at least as families or superfamilies. The only thing left in common between our current binominal nomenclature and Linnaeus's binominal nomenclature is the combination of a generic name with a specific name. However, taxonomically speaking, our current binominal nomenclature and Linnaeus's binominal nomenclature are quite distinct. Therefore, introducing some changes in the binominal nomenclature so that it could convey taxonomic information more accurately is not seen here as such a radical break from Linnaeus's legacy.

Allowing the use of supra-generic taxon names as first parts of Linnaean binomials would generate new homonymy issues, as specific names not found together in different genera may be found

together in the same family. Possible solutions exist. For instance, in cases where homonyms would need to be avoided, former generic names could be cited as anchors. For newly created names, it could be recommended that, in the future, taxonomists choose specific names that would avoid potential homonymy with species placed in neighboring genera. Other ways to handle homonymy issues could certainly be proposed: e.g., the authorship could become part of the species name.

CONCLUSION

Studying the history of nomenclature reveals that established practices have always been challenged and new ideas have always been proposed. In that regard, the recent emergence of phylogenetic nomenclature and the debates it has generated (Cantino and de Queiroz 2007; references therein) are not a new phenomenon. Nomenclature went through periods of much more radical discussions. For instance, in the 1870s, the principle of priority, one of the most fundamental concepts in nomenclature (including in the *PhyloCode*), was directly attacked by many authors who suggested that a principle of “convenience” be adopted. In the 1960s, some authors suggested that a system of numerclature (numbers) replace nomenclature (names). For 250 years, nomenclature has remained dynamic: it has always generated intense debates and it has changed.

Changes in nomenclatural practices, however, are perceived only over long periods of time, which probably explains why many biologists view nomenclature as being static, permanent. That it took 50 years for zoologists to select 1758 as the starting point for zoological nomenclature is one of the best illustrations of this long-term development of nomenclature. Two main reasons might explain why changes are slow in nomenclature. First, it seems that taxonomists disagree more easily than they can agree, and some nomenclatural issues have always generated endless debates; as reviewed in this chapter, whether “barbarous” names (derived neither from Latin nor Greek) should be allowed was debated during the entire 19th century until 1905, when they were finally allowed in the first *Règles internationales de nomenclature zoologique* (ICZN 1905). Second, taxonomists tend to be quite protective of existing practices, and, in fact, radical proposals have rarely been adopted.

Quite a few examples of new nomenclatural proposals have never been adopted and did not cause any practical nomenclatural change because they did not become recommendations or rules in the codes (e.g., Bergeret 1783; du Petit Thouars 1822; Amyot 1848; Sharp 1873; Jahn 1861; Lewis 1875; Michener 1963; Oldryod 1966). Although all those innovative contributions sometimes seemed “pure folly” to some taxonomists, they have played a critical role. By challenging the established nomenclature, they kept taxonomists thinking about nomenclature and largely participated in increasing our understanding of nomenclatural theory and concepts.

So, for these reasons, I am of the opinion that not only is there nothing wrong with the fact that phylogenetic nomenclature challenges so deeply our established nomenclatural practices, but also that phylogenetic nomenclature will, in the long term, be beneficial to nomenclature, which will grow stronger regardless of how the coexistence of the Code and the *PhyloCode* (for zoologists) evolves in the future.

Although phylogenetic nomenclature shares some similarities with the historical debates reviewed here (development of priority in the 19th century and debates in the 1960s largely due to numerical nomenclature), it has some undeniable specificities. First, in contrast to most innovative proposals made in the past, it has been transformed into an actual set of rules and recommendations, the *PhyloCode*, which will soon be available to taxonomists. Also, it is important to mention the existence of the International Society of Phylogenetic Nomenclature, which represents an international body of experts and elects a Committee on Phylogenetic Nomenclature in charge of revising and addressing issues related to the *PhyloCode*.

The second specificity is that the coexistence of codes in the 19th century and the coexistence of codes that will soon take place (when the *PhyloCode* is published) are the results of two distinct processes. Taxonomists then had to deal with disagreements among *too many* codes in a period where no scientific society or group of zoologists could claim any authority over zoological nomenclature.

Several codes coexisted in the 19th century because, before the creation of the International Commission on Zoological Nomenclature in 1895, no agreement had been reached. The upcoming coexistence of two codes (ICZN and *PhyloCode* for zoologists) results from a different historical process precisely because there is an International Commission on Zoological Nomenclature. So, several codes are soon going to coexist because a new nomenclatural code has branched out or split from an existing one (actually, from three rank-based codes).

Although they are the result of two distinct historical processes, the coexistence of several codes in the 19th century and today's coexistence of codes (e.g., *International Code of Zoological Nomenclature* and *PhyloCode* for zoologists) are similar situations: taxonomists are free to use different nomenclatural rules, and thus may possibly use different taxon names. We know how the story ended in the 19th century: The community of zoologists took the opportunity of the International Congress of Zoology to design a common set of rules and recommendations. However, it is unknown how today's coexistence of codes will end. Ideally, there should be only one code, and the *International Code of Zoological Nomenclature* and the *PhyloCode* could be united into a single code by taking the best from each code: all the species-level rules and recommendations from the Code (e.g., type specimens), and the higher-level rules and recommendations from the *PhyloCode* (e.g., clade name definition); at least this is what I regard as "the best" of each code at the present time; different people certainly hold different opinions. It is nearly impossible to predict whether this will happen, because too many factors are uncertain.

Regardless of whether the *PhyloCode* and the current rank-based codes will be united in the future, the main objective of this chapter is to argue that there is no reason for taxonomists to think that nomenclatural rules and practices should remain unchanged. They have continually changed in the past, and there would be nothing wrong if nomenclature were to change again: "In the world, nothing is permanent and laws, as living beings, perpetually evolve. We believe, however, that the present Report and the rules proposed therein meet the current nomenclatural needs." (Blanchard 1889:334, tr.) Does our current nomenclature meet our needs? Is it adequate? If yes, there is no reason to change it. If not, we may need to change it, and there would be nothing wrong.

One aspect of the future of nomenclature that is particularly unclear is how, in practice, communication can be guaranteed between the committees in charge of editing the rank-based codes and the Committee on Phylogenetic Nomenclature. It is because incompatibilities do exist between the *PhyloCode* and the rank-based codes that we need to make sure that regular exchanges take place among the people who are the most actively involved in the development of nomenclature in order to avoid misunderstanding and try to build a constructive environment for the future of nomenclature. It would be regrettable for the future of nomenclature and taxonomy that those communities of experts would not communicate with each other. Such exchanges could take place at nomenclature workshops, which sooner or later will be needed.

Another important aspect of nomenclature that we should also keep in mind when we discuss potential changes of nomenclatural practices is that nomenclature is a means of facilitating, not an end result, of taxonomy. In fact, one can read on the front page of the American Ornithologists' Union Code (Coues et al. 1886): "Zoölogical nomenclature is a means, not an end, of zoölogical science." This implies that nomenclature should remain open to changes that facilitate the work of taxonomists. This is exactly why numerical taxonomists argued in the 1960s that nomenclature should be changed so that it could serve better the modern context of numerical taxonomy. This also is why it has been more recently argued in the context of the *PhyloCode* that nomenclature should be changed to better serve the modern context of phylogenetic systematics.

New nomenclatural questions may arise from new techniques too. Early writers (e.g., Strickland 1835, 1843) rightly argued that names should be eligible for priority only if the species designated by that name had been properly described. As shown here, what constituted a "good" description was actively discussed throughout the 19th century, in relation to the Principle of Priority, but, basically, descriptions should include as much information as possible so that species can be re-identified. However, standards of species description change over time because new techniques are available.

For instance, when Blanchard and the other members of the International Commission devised the first *International Rules of Zoological Nomenclature* in the late 19th century, the ability of comparing nucleotide sequences for taxonomic purposes did not exist. Should the Code recommend that DNA sequences of a few standard markers be included in species descriptions (Dayrat 2005b), as, in the 19th century, some authors thought that illustrations of specimens should be mandatory when creating a new name?

It seems that the future of nomenclature is going to be fascinating. It is difficult to predict how the coexistence of the rank-based codes and the *PhyloCode* will evolve in the long term. However, it will be interesting to follow how systematists will react to the official implementation of the *PhyloCode*. After the publication of the *PhyloCode*, it will be critical that members of the committees in charge of editing the rank-based codes and the Committee on Phylogenetic Nomenclature work constructively together to try to explore the possibility of unifying nomenclature. Taxonomists should not fear that changes may be needed in nomenclature, as long as those changes can help to better serve modern needs in systematics. Nomenclature has remained dynamic for the last 250 years, and will be dynamic for the next 250.

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