Proposed Rules for the incorporation of nomina of higher-ranked zoological taxa in the *International Code of Zoological Nomenclature*. 1. Some general questions, concepts and terms of biological nomenclature

Alain DUBOIS
Muséum national d'Histoire naturelle, Département Systématique et Évolution,
USM 0602 Taxonomie-collections, Reptiles et Amphibiens, case postale 30,
25 rue Cuvier, F-75231 Paris cedex 05 (France)
adubois@mnhn.fr

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ABSTRACT
The scientific names (nomina) of higher-ranked taxa (above the superfamily) of animals are not regulated by the *International Code of Zoological Nomenclature*, but by "consensus" among workers. However, when there exists no real consensus, a frequent situation, some criteria must be used to establish which nomen should be considered valid for any given taxon. With the multiplication of taxa that follows the development of cladistic analyses, the implementation of such rules will become more and more necessary and important. To be acceptable by all zoologists worldwide, today and tomorrow, these rules should be independent from the philosophy of taxonomy adopted, but should allow unambiguous, automatic and universal allocation of a single nomen to each higher taxon, within the frame of any taxonomy, including "phylogenetic" ones. This first paper is devoted to the detailed discussion of general theoretical and terminological problems related with this question. It is here argued that it is misleading and dangerous to try and make nomenclature artificially "simple". The problems posed by the naming of millions of kinds of organisms, related through evolution and that have been studied for two and a half century under different approaches, are indeed complex: this complexity should be acknowledged, and the discipline in charge of this study should be recognized as a specific technical field, with its own methods, concepts and terms. Among various proposals made in this paper, it is suggested to definitely abandon the misleading term "type" in taxonomy and nomenclature, objective categories for the "usage" of nomina are defined for the first time, and a distinction is made between taxonomic "categories" and nomenclatural "ranks".

KEY WORDS
Zoological nomenclature,
definitions,
higher-ranked taxa,
types,
priority,
usage,
taxonomic category,
nomenclatural rank,
phylogenetic taxonomy,
terminology,
Rules.

RÉSUMÉ
Proposition de Règles pour l'incorporation des nomina de taxons de rangs supérieurs d'animaux dans le Code international de nomenclature zoologique. *1. Quelques questions générales, concepts et termes de la nomenclature biologique.* Les noms scientifiques (nomina) des taxons de rangs supérieurs (au-dessus de la super-famille) d'animaux ne sont pas régis par le *Code international de nomenclature zoologique*, mais par un « consensus » entre chercheurs. Cependant, lorsqu'il n'existe pas de vrai consensus, une situation fréquente, des critères doivent être employés pour établir quel nomen doit être considéré valide pour un taxon donné. Avec la multiplication des taxons qui résulte du développement des analyses cladistiques, la mise en vigueur de règles va devenir de plus en plus nécessaire et importante. Pour être acceptables par tous les zoologistes du monde, de nos jours et dans l'avenir, de telles règles devraient être indépendantes de la philosophie de la taxinomie adoptée, mais devraient permettre une attribution non-ambiguë, automatique et universelle d'un seul nomen à chaque taxon supérieur, dans le cadre de toute taxinomie, qu'elle soit « phylogénétique » ou non. Ce premier article est consacré à la discussion détaillée de divers problèmes théoriques et terminologiques relatifs à cette question. L'idée est défendue ici qu'il est trompeur et dangereux de tenter de rendre la nomenclature artificiellement « simple ». Les problèmes posés par la nomination de millions de sortes d'organismes, liés entre eux par l'évolution et qui ont été étudiés depuis deux siècles et demi selon différentes approches, sont indéniablement complexes : cette complexité doit être reconnue et assumée et la discipline chargée de cette étude doit être reconnue comme un domaine technique spécifique, avec ses propres méthodes, concepts et termes. Parmi diverses propositions avancées dans cet article, il est suggéré d'abandonner définitivement le terme mensonger de « type » en taxinomie et en nomenclature, des catégories objectives d'« usage » des nomina sont définies pour la première fois et une distinction est faite entre « catégories » taxinomiques et « rangs » nomenclaturaux.

MOTS CLÉS
Nomenclature zoologique,
définitions,
taxons de rangs supérieurs,
types,
priorité,
usage,
catégorie taxinomique,
rang nomenclatural,
taxinomie phylogénétique,
terminologie,
Règles.

PRINTING CONVENTIONS

In the text below, species-series and genus-series nomina (see Dubois 2000b) are printed, as usual, in lower case *italics*, whereas nomina of higher-ranked taxa are written in small capitals, with the following distinction: family-series nomina are in *ITALICS*, and class-series nomina in BOLD. The second paper of this series (Dubois in press) will give precise definitions of most technical terms used below, especially all new ones. The abbreviation "ICZN" refers to the International Commission on Zoological Nomenclature, "*BZN*" to its official organ, the *Bulletin of Zoological Nomenclature*, and the "*Code*" to the fourth edition, currently in force, of the *International Code of Zoological Nomenclature* (Anonymous 1999b). This latter book is here quoted as "Anonymous" for reasons explained in Dubois (2000b: 87), as it "does not state the name(s) of [its] author(s)", but is "attributed to a body (e.g., a committee or a commission)", thus following the definition of the term "*anonymous*" in the *Glossary* of the *Code* itself (p. 100).

PREAMBLE

"When the golden rule of *priority* is once laid aside, there seems to be no limit to these alterations; for, however appropriate a nomenclature may be, dabblers in science will always be found who will prefer terms of their own coinage to those which are already established".
Hugh Edward Strickland 1837: 128

"There are only two ways of naming plants and animals, either to give them their oldest names or to give them any names you please".
David Starr Jordan *in* Frederick V. Coville 1895: 163

"As has been said, there are two ways of naming plants, according to the system of priority, or according to the personal system, which is no system at all".
O. F. Cook 1895: 431

Among biological sciences, nomenclature is rather disliked. It is often considered as a boring, almost useless, intellectually poor but technically unnecessarily complex domain, that is not worthy to attract the attention of "great scientists" and should be left to "maniacs of names": "a non-science that is the province of those biologists with nothing better to do" (Knapp *et al.* 2004: 612). For a zoologist, to devote time and energy to this domain is clearly depreciating in the eyes of many, and is of little help in an academic career: rather, it can be used as evidence that a colleague is a mediocre archivist and not an active researcher. This social discrediting of the field is interesting and should attract the attention of historians of science and epistemologists. However, in contrast with this prejudice, a serious examination of the matter shows that: 1) nomenclature is crucial for all biological sciences, that cannot do without it: just like Molière's Monsieur Jourdain who was making prose without knowing it, *all* biologists use nomenclature, at least at a given time of their work, i.e. when they assign a name to the organisms they deal with; 2) the theory of nomenclature or *onymology* (Dubois 2000b) is an intellectually very complex and interesting field whose mastering requires a broad culture, strong methodological and conceptual rigour, and intellectual adaptability; 3) despite the "official" neglect or contempt for this activity in academic circles, thousands of pages have been devoted to this field over the last two centuries, as will be exemplified by the publications quoted in the present work. In the pages below, criticism of the "*Phylocode*" project will be developed. However, there is one point for which all biologists interested in nomenclatural matters can be grateful to the supporters of this project: it has rendered some of its letters patent of nobility to nomenclature in scientific publications. Hundreds of pages were published in the last decade, some in major periodicals, dealing with the presentation, defense and criticism of the "*Phylocode*". Nevertheless, despite this "renewal", the situation is not yet such as a free discussion develops in the international community about the "best system of nomenclature". In our society for which everything that is (or appears) "new" is considered better than the past, it is much easier to develop a new proposal, especially if it appears (slightly) "revolutionary", than defending an "ancient" system and simply stating that it works well: "there is no excitement in a story about retaining the 'old' way" (Nixon *et al.* 2003: 112). So if anything dealing with the "*Phylocode*", especially in support to this new system, is likely to be quickly published, the same is not true for papers placing themselves within the frame of the current *Code* and "just" proposing changes meant to improve the latter (see Note 1).

If this difficulty to publish some kinds of ideas can be understood from periodicals that are not specialised in taxonomy and nomenclature, it is less so for publications of the latter category, and especially for *BZN*, the official organ of ICZN. By the middle of the 20th century, this journal was largely open to contributions from all zoologists worldwide dealing with proposed changes to the *Code* or with specific nomenclatural problems. This is well exemplified by the huge volume 15 of this journal, published by Francis Hemming in 1957 and 1958, which only consists of free contributions to the discussion of various aspects of the *Code*, before some changes were introduced in its "first" edition (Anonymous 1961), when it replaced the last version of the previous *Règles internationales de la nomenclature zoolo-*

gique (Hemming 1953). An interesting point of this volume is that these discussions are not at all obsolete today. Many of these papers deal with questions that had already been discussed long before, in fact since the beginning of scientific classification, and that will no doubt be discussed again in the future, such as the rôle of name-bearing types in nomenclature, priority versus usage, or the relationships between nomenclature and taxonomy. This is easy to understand: whereas many of the hard discussions and debates that developed during the history of biology have finally come to an end because new data and/or theories made the older theories obsolete, nothing of this kind is ever to be expected with biological nomenclature, as nomenclature is not a scientific discipline, but a technique, a tool, and there is nothing like "truth" to be expected in this field. No nomenclatural system is better than another for being "closer to the truth", but different approaches to nomenclatural questions reflect different philosophical or practical choices. Actually, if they take the time to read the papers mentioned above, some of the recent contradictors on these matters will certainly be surprised to realise that much of what they can write now had already been written, sometimes more clearly or persuasively, in these works of the past. In particular, the opposition between two major "schools", one in favour of "usage" and *a posteriori* "consensus", and one in favour of universal *a priori* stringent rules like priority, is as old as the discipline of biological nomenclature (e.g., De Candolle 1813; Strickland *et al.* 1843; Blanchard 1889; Robinson 1895; Coville 1895; Ward 1895) and will no doubt continue to exist for decades. It is not justified, except on polemic grounds, to call the first one "radical" and the second one "reactionary" (Stace 1991) – when in fact the former is more aptly designated as "conservationist" (Melville 1958: 1248). It would be more exact to point out that this opposition is a much more general one, that has existed for centuries between "*de jure*" and "*de facto*", i.e. between Roman law, mostly based on *a priori* rules, and Anglo-Saxon law, relying largely on jurisprudences established *a posteriori*. It is unlikely that any of these two "schools" will disappear, as they correspond to basic philosophical differences that go much beyond taxonomy and nomenclature. Therefore, as the tenants of both attitudes have to collaborate within a single nomenclatural system, both camps are bound to make concessions and a compromise has to be found between both attitudes. Carrying the joke too far might result in a secession within the international community of zoologists, as was rightly pointed out by ICZN's president Richard Melville at a time when "conservationists" were dominant (1958: 1248): "Pursuing a defeated enemy to destruction may be good tactics, but it is bad strategy when both parties have to work together after the fight".

Unfortunately, in the recent decades ICZN has not followed the sound advice of its former president and has not repeated the experience of volume 15 of *BZN*. Most of the discussions preparatory to the last edition of the *Code* (Anonymous 1999b) were not held in a public and permanent medium such as *BZN*, where they can be consulted again decades later, but on an "internet forum", i.e. a completely labile support (see Dubois 2003b, 2004a), that furthermore at that time was accessible only to part of the international community of zoologists. Only a small selection of the contributions of the international community to this discussion on the last *Code* was published in *BZN*, although the total volume of texts received by the ICZN secretariat ("more than 800 pages of documents from some 500 people or groups", according to Minelli 2003: 650) was of an order of magnitude similar to that of volume 15 of *BZN* (1266 pages). A number of contributions were never published, and even then the published texts were authoritatively modified, mostly shortened, by the ICZN secretariat. Unfortunately, this is only one case among many, as in the recent decades a high number of applications, dealing with problems posed by specific cases of zoological nomenclature, were never published in *BZN* and these cases were never brought to the knowledge of the international community of zoologists, or had to be so through other periodicals where they are

more difficult to trace (see Note 2). Censorship of zoologists by the ICZN secretariat is all the more problematic that, unlike in the case of a paper in any other scientific field, there exists no other periodical specialised in the field, where rejected papers are likely to be published. *BZN* being the only journal in the world devoted to the discussion of theoretical and practical problems of zoological nomenclature, it is in a situation of *quasi-monopoly* for papers in this field. It should therefore be the responsibility of the editors of this bulletin to make sure that every opinion and problem presented by individual zoologists be brought to the knowledge of the international community through this journal. The only intervention of the ICZN secretariat should be to check that authors of the applications have a good knowledge of the *Code* and that the problems raised cannot be solved by a simple and normal use of the provisions of this official text. As long as this is not the case, zoologists who wish to submit new suggestions regarding zoological nomenclature to the international community will have to use other journals, as is the case here.

INTRODUCTION

Probably more than 10 million animal species live on our planet, of which less than 2 million have been described so far by biologists (Hammond 1992; Heywood & Watson 1995). In order to be able to communicate among us about these species, we need a taxonomic and nomenclatural system, that will act as a "convenient information storage and retrieval system" about taxa, their characters, evolution and relationships (Mayr 1981: 511).

Although connected, *taxonomy* and *nomenclature* are different disciplines. Taxonomy recognizes classificatory units or *taxa*, whereas nomenclature attaches a given scientific name or *nomen* (see Dubois 2000b) to each of these units: of course, "taxa must have nomina" (Dubois 1988b), but *a taxon is not a nomen*, and vice versa. A taxon is a *classificatory concept*, which can be considered either as a class (a group of organisms) or as an evolutionary individual (a clade), whereas a nomen is a *label*, by which a taxon can be unambiguously designated within the frame of a given taxonomy, and which serves as a universal tool for the storage and retrieval of the biological and non-biological information attached to this taxon. Taxonomy is a scientific discipline, whereas nomenclature is a technique. Both are partly related but also partly independent. Nomina can be given to "empty concepts", i.e. created without defining the taxa for which they were created (these are the "*nomina nuda*" of nomenclature). On the contrary, taxa can be recognized, defined or diagnosed but not named, and in the past other systems have been proposed to designate taxa, such as numbers (numericlature and related systems: Little 1964; Rivas 1965; Hull 1966; Johnson 1970; Heppell 1991), combinations of names, letters and numbers (Needham 1910) or artificial words resulting from a combination of "coded letters" (Bergeret 1783; Nicolson 1991; Knapp *et al.* 2004). The long-lasting use of a nomenclatural system for this purpose, however, is justified by the fact that in human language in general, and in scientific texts in particular, we usually communicate with words, not numbers, letters, abbreviations or other non-verbal systems. The current international rules for the allocation of nomina to taxa and for the automatic establishment of the valid nomen of any taxon within any taxonomy are those of the current international codes of zoological (Anonymous 1999b), botanical (Greuter *et al.* 2000), bacteriological (Lapage & Sneath 1992) and virological (Van Regenmortel *et al.* 2000) nomenclatures, which have force of law for all biologists and editors worldwide. The present work is a continuation of my previous efforts at contributing to a clarification of some basic concepts of biological nomenclature, which requires in some cases the establishment of a new terminology, a process that is part of *onymology*, i.e. the study of the concepts and theory of biological nomenclature (Dubois 2000b). This paper is more specifically centered on zoological nomenclature, currently regulated by the zoological

Code, although a number of discussions below have broader implications and also concern the nomenclature of other groups of organisms.

In zoology, the current taxonomic system allows to place all the known species unambiguously in a hierarchic arrangement of taxa (genus, family, order, class, etc.), and the nomenclatural system allows us to designate unambiguously any of these taxa by a single nomen, that should be agreed upon and used by all zoologists worldwide. Despite these general needs, however, the current zoological *Code* only recognizes and regulates the nomina of three "nominal groups", renamed "nominal-series" by Dubois (2000b) in order to avoid possible semantic confusions: the "*family-series*" (superfamily, family, subfamily, tribe, subtribe and additional intermediate taxa), the "*genus-series*" (genus, subgenus) and the "*species-series*" (species, subspecies, species-group, etc.). Nomina that may be given to forms or taxa below the subspecies (that can be called *variety-series* nomina) are ignored by the *Code*, as are nomina of taxa above the superfamily, i.e. *class-series* nomina (Dubois 2000b).

With the development of cladistic studies dealing with most groups of animals, new hypotheses of relationships between taxa are regularly proposed, and many of them are followed by new taxonomic proposals, that often include a multiplication of higher-ranked or "higher" taxa (see Note 3), especially within the frame of various systems of "phylogenetic taxonomy" (De Queiroz & Gauthier 1992). The units which such systems recognize are not taxa, in the traditional sense of the term, as they are defined by hypothesized cladistic relationships, not necessarily by characters. They were called *phylons* by Dubois (1991a) and more aptly *cladons* by Mayr (1995: 425), and the system ordering them was designated as *cladification* (Mayr 1997) or *cladonomy* (a term coined and published independently in the same year by Brummitt 1997 and Dubois 1997b).

The absence in the *Code* of rules for the nomination of taxa (or cladons) above the superfamily is a source of confusion and instability. Even when no new taxonomic proposals are involved, the absence of rules sometimes results in the same taxon being designated by different authors under different nomina, a situation that is certainly not satisfactory (Corliss 1983).

Recent amphibians are a good example of this situation, with the class being known either as **Amphibia** or as **Batrachia**, the subclass either as **Batrachia** or as **Lissamphibia**, and the three orders being designated by different authors under different nomina, the most common of which are **Gymnophiona** and **Apoda** for caecilians (worm-like amphibians), **Anura** and **Salientia** for frogs (tailless amphibians), and **Urodela** and **Caudata** for salamanders (tailed amphibians): whereas for the two former groups the first cited nomen is more and more often used by recent authors, uncertainty still strongly persists for the third group (salamanders). The unclear status of these nomina is well exemplified by the fact that in recent editions of the *Zoological Record* up to volume 139, the orders were designated as follows: "**Anura** (= **Salientia**)", "**Caudata** (= **Urodela**)" and "**Gymnophiona** (= **Apoda**)". It is also stressed by the fact that alternative nomina for the same taxon, e.g., **Urodela** and **Caudata**, or **Gymnophiona** and **Apoda**, are not rarely used either by different authors in different chapters of the same book, or, quite often, by the same author in different publications, or even in the title of *the same* publication (for more details, see Dubois in prep.). Such a situation may not seem very problematic for specialists of the zoological groups concerned, who all know these different nomina, but it may cause more confusion whenever non-specialists (either zoologists working on other animal groups or biologists or non-biologists involved in disciplines far from systematics) need to use the nomina of taxa to find information. Furthermore, with the growing computerisation of knowledge in databases, the risk is great that in the future some references and pieces of information may be lost or difficult to trace if the same taxon is designated by different nomina in different works.

Here I will discuss some general problems of zoological nomenclature, in particular dealing with higher taxa. In a second paper (Dubois in press),

I will propose general rules for this nomenclature, which may help to avoid the progressive development of nomenclatural chaos for higher nomina. In a third paper (Dubois in prep.), as an example of the use of the proposed rules I will present a detailed study of the status of the amphibian nomina just mentioned.

Let us first address the following question: what properties should the taxonomic and nomenclatural systems display to be most useful to taxonomists, other biologists and non-biologists users of nomina of taxa?

SOME DESIRABLE PROPERTIES OF TAXONOMIC AND NOMENCLATURAL SYSTEMS IN BIOLOGY

Taxonomic system

Biological classifications may be of various kinds according to the function we ask them to play (Gilmour 1951). Two major functions of such classifications (see e.g., Mayr 1982, 1997) are a practical one (i.e. providing a universal system of storage and retrieval of information) and an explanatory one (i.e. providing an evolutionary interpretation and explanation of the diversity of organisms). Ignoring the first of these functions to concentrate only on the second may seem appealing as a "purer" approach but is not doing a service to science and its innumerable users in all domains of human activity (Benton 2000). In contrast, emphasis on the first function is justified when classifications are understood as tools aiming at being useful to the whole scientific community as well as to non-scientific users, not designed for a "private usage" of taxonomists and phylogeneticists: "Classifications merely store information; they do not tell us why or how the information has the order it does" (Cracraft 1974: 79). "Classifications are not theories. Several authors of quite diverse views (...) have expressed the opinion that the best use of a formal classification is as an index to the analysis from which it is derived and the organisms that it includes. Because formal classifications can only imperfectly express a full phylogenetic analysis, classifications are of less significance to systematics than is progress in analytical methods. It follows that the classification that serves as the best index to the analysis that produced it, and the easiest to use, is the wisest choice" (Ashlock 1984: 44).

With these ideas in mind, Dubois (1991a: 63) stressed five properties that should be displayed by any system of classification of biological taxa (taxonomic system) to be theoretically satisfactory, acceptable by all biologists, workable and efficient. These properties, to which a sixth one, numbered (T4) below, can be added, are as follows:

(T1) *Unicity*: this means that *within* any hierarchical taxonomic system, there should be *a single taxonomic hierarchy* for all living beings, not several. This applies in particular to taxa that show some evolutionary or biological peculiarities, such as parthenogenetic or gynogenetic reproduction, or hybridogenetic meiosis: they should also be ascribed to the standard nomenclatural rank of species, even if attention should be pointed to their peculiarities in special ways, by referring them to particular taxonomic categories (Dubois & Günther 1982; Dubois 1991a). In this respect, the distinction between taxonomic category and nomenclatural rank (Dubois 2005a) is relevant. On the other hand, this does not imply at all that a single taxonomic system should be imposed "from outside" on all taxonomists, as suggested by some (e.g., Godfray 2002, rebutted e.g., by Knapp *et al.* 2002). "Reaching consensus on species concepts (...) would indeed be desirable, but many serious obstacles would need to be overcome before this could happen – not least of which is the failure of the species themselves to behave in a consistent and predictable way. Asking systematists to resolve the problem of species concepts is akin to asking ecologists to agree on which factors control the distribution of organisms on the landscape. That is to say, it is one of the central issues of the discipline and is not likely to be resolved definitively in the near future" (Smith 2004). "Taxonomy is a complex and dynamic science, not a simple (albeit technically difficult) collation of facts like the nucleotide sequences of

a genome survey. It is probably only at the most complex levels of genetics, in dynamic proteomics or developmental biology, that anything like the complexity of hypothesis-generation and testing involved in routine taxonomy and systematics is encountered" (Thiele & Yeates 2002). Furthermore, the rôle of specific and subspecific nomina in conservation biology poses particular problems which require formal recognition of taxa for this mere purpose (e.g., Zink 2004; Mace 2004). Emergence of a unified taxonomic system could possibly occur in the future, through three possible processes: free adherence of most taxonomists to one of the current taxonomic schools, partial synthesis between these different conceptions (Mayr 1981; Estabrook 1986; Rasnitsyn 1996; Knox 1998) or emergence of a new conception (Crowson 1970; Dubois 1988c). The situation is clearly not ripe for any of these three steps (Dubois 2003a), and for the time being the existence of different conceptions must be respected, as all the history of science shows that authoritarian imposition of an "official" theory or opinion on some scientists by other scientists, or by administrators or politicians, usually acts as a brake against the development of a scientific field rather than as a stimulating factor.

(T2) *Exhaustiveness*: the taxonomic system should be devised in such a way as to be able to accommodate *all living beings* ever to be found in the real world, not only some of them. This need for a global coverage of all living beings results in a need for taxonomic concepts to bear some determined relationship to *universally observable or demonstrable patterns and particularities* of the organisms of the real world, rather than being derived solely from some general *theory*, such as a theory of evolutionary process, or a theory of biological classification. A taxonomic system that would for some reason exclude from its field of competence some organisms (e.g., for being of hybrid origin, or for being teratological) would not be acceptable.

(T3) *Univocality*: the taxonomic system should be univocal, i.e. any given living being should *unambiguously* be ascribed a single place in the system. This place may be provisional, which has nothing surprising or shocking as any given taxonomy is not only the result of previous works, but also a *working hypothesis*, that may be confirmed or rejected by subsequent works: by itself, this *heuristic* function of taxonomy has always acted as a fruitful contribution to the development of our knowledge about organisms. It is much better to assign a given, albeit provisional, place to most taxa in a taxonomy, rather than to have a taxonomy with many "*incertae sedis*" taxa: although there are certainly very few "*certae sedis*" taxa in our classifications, this in no way impedes the latter from playing a useful rôle at every step of the history of biology (Kottelat 1997; Dubois & Ohler 1999; Dubois 1999b).

(T4) *Hierarchy*: in order to work as a system of storage and retrieval of information, a taxonomic system must have a hierarchical structure. Without such a structure, all taxa having the same "level" or "rank", scoring of millions of items would be necessary before finding the proper one. This is just like the difference between an alphabetical index (all words being listed similarly indented from the margin) and an alphabetical-thematic index (with successive indentations from the margin showing subordination of concepts). To be efficient and coherent, a hierarchy must be unique, each taxon at a lower level being referred to a single taxon at the next higher level. This can be rigorously defined in mathematical terms (e.g., Woodger 1952: 11) and was more simply called by Gould (2000: 66) "irrevocable branching without subsequent amalgamation", "continuous branching with no subsequent joining of branches" or "successive (and exceptionless) dichotomous branching": "two species in the same genus can't reside in different families, and two orders in the same class can't be placed in different phyla". More generally, this can be described as follows: "Taxa at the same rank are not included within one another; taxa at higher ranks are not contained within taxa of lower ranks" (Carpenter 2003: 84). Although apparently obvious, such a rule was not always strictly followed by early taxonomists (see e.g., the treatment of the genus *Proteus* in Laurenti 1768, discussed by Dubois 2004b) or even by

some more recent ones (Sigal 1966). A good taxonomic hierarchy is "an organizational model in which: 1) the highest level of organization consists of a single entity; 2) an entity at a lower level of organization is related to only one entity at the next higher level but can be related to more than one entity at the next lower level; and 3) entities at all lower levels are related by extension to the single entity at the highest level of organization" (Knox 1998: 7).

(T5) *Homogeneity*: there should be some *equivalence*, by some criteria, between various taxa ascribed to the same rank within the taxonomic hierarchy. Although discussed by various authors already (e.g., Hennig 1950, 1966; Van Valen 1973; Schaefer 1976; Dupuis 1979; Dubois 1988a, c; Avise & Johns 1999; Minelli 2000), this difficult problem has not yet been satisfactorily solved by the international community of taxonomists. This means that taxa ascribed to a given rank in different clades may not be equivalent in various biological terms (numbers of subordinate taxa; morphological, genetic or ecological diversity; speciation and extinction rates; genetic and evolutionary patterns, etc.; see e.g., the case of the taxa of birds as compared to those of other vertebrates, discussed in detail in Dubois 1988c). Consequently, comparisons that are sometimes made among lists or numbers of taxa in various works dealing with evolution, stratigraphy, biogeography or ecology, may be irrelevant and spurious, and must be considered very cautiously (Smith 1988; Sundberg & Pleijel 1994; Minelli 2000; Pleijel & Rouse 2003). This is not at all a good reason, however, to reject the hierarchical structure of the taxonomic system. Even if it could (and possibly will) be improved by making ranks more significant in biological terms, this structure already allows taxonomy to play the practical rôle of an indexation system of information, just like the hierarchical structure of keywords in an index or a database. In such systems, keywords at a given level are only roughly equivalent (in terms of information content or diversity, or of number of subordinate keywords) but are nevertheless quite useful to track down the information. A taxonomy without ranks would be most inconvenient in use and poorly informative.

(T6) *Robustness*: the taxonomic system should display at least *rather important* (but not complete) stability, so that every new discovery or hypothesis should not be liable to modify it partly or totally (but that some do). This stability should concern both the classificatory pattern and the nomenclature. The major consequence of this requirement is that very rigid systems of classification, in which any new fact, new taxon or new hypothesis of relationships (even when the change concerns only the phylogenetic position of a single species) must be followed by a complete change of the taxonomy (and consequently of the nomenclature), cannot be supported. A classificatory system should display at least some *inertia* or *flexibility*, in order to incorporate at least part of the new findings without taxonomic (and nomenclatural) changes. However, this does not mean that classification should remain the same forever: important new information must be acknowledged by a change in the taxonomic (and nomenclatural) patterns. As often in biology (see Mayr 1997), the best solution to this problem is in a good equilibrium between two extreme and opposite points of view. The need for relative stability of taxonomies would be most appropriately acknowledged by replacing the frequent request for *stability* by a request for *robustness*, which describes more adequately the kind of "stability" we need in zoological taxonomy and nomenclature (see also Dubois 1998). Robustness could be defined as "stability + change". Whereas "taxonomic stability" alone would mean that taxonomy must be definitively frozen and is not allowed to change, "taxonomic robustness" means that taxonomy must be flexible enough to be able to change, in order to take new information or ideas into account, but that it cannot do so "easily".

These six major criteria, to which a few others could be added, could form the basis for a relatively simple and lax "code of zoological taxonomy" which could be accepted and adopted by all zoologists worldwide, independently from their "taxonomic school", their ideas on organic evolu-

tion, and on the mutual relationships between phylogenetic hypotheses and classification. In contrast, it is strongly to be feared that any more stringent or "directive" taxonomic system would be shared only by a part of the international zoological community and likely to be strongly modified later, as our ideas and knowledge about biological and evolutionary processes evolve.

As a matter of fact, it is quite likely that future developments of biology will lead to further basic changes in our taxonomic thinking and practice (Jørgensen 2000). These may include the progressive taking into account of new knowledge concerning the evolution and functioning of genomes, in particular regarding various aspects of genetic regulation, which may ultimately lead to the development of a new "experimental taxonomy" (Crowson 1970; Dubois 1988c). Our growing knowledge on the way the genome works throws new light on long-known facts such as "character reversal" in some clades, that make sense if some parts of the genetic program, rather than being lost, may be kept "sleeping" for a while and "reactivated" later, as well exemplified by the frogs of the genus *Gastrotheca* (Del Pino & Escobar 1981; Wassersug & Duellman 1984). The currently dominant idea that all taxa are created by cladogenesis has no generality, first of all because viruses have no general cladistic relationships between them (so they cannot be properly "cladified", except at a very low level, but only classified), but also because evidence is now strong that, not only in the vegetal kingdom but also in animals, reticulate phylogenetic relationships and speciation by hybridization (even between species that are only distantly related, and at least not sister-species) are not uncommon: this applies to gynogenetic and hybridogenetic kleptons, parthenogenetic klonons, and allopolyploid species (e.g., Bogart & Wasserman 1972; Dubois 1977, 1991a; Dubois & Günther 1982; Dawley & Bogart 1989; Günther 1991). In particular, hybridogenetic kleptons or zygokleptons (Dubois 1991a) cannot be properly considered as clades, as hybridization occurs again at each generation, so that such taxa do not result from a single evolutionary event. In order to comply with the request that all taxonomies be cladonomies, these kinds of organisms should either be ignored or artificially included in the same taxa as the organisms from which they originated, which is not acceptable.

Finally, in the future, completely new facts may also have to be taken into account, such as the possibility of "lateral gene transfer" (LTG) between organisms (Mourant 1971; Ruffié 1982: 213-215; Kidwell 1993; Doolittle 1999, and references therein; Martin 1999; Nelson *et al.* 1999; Gould 2000; Kidwell & Lisch 2001; Oren 2004), which might go much further than the series of symbiotic events that underpinned the evolution of eucaryotes (Stevens 2002): although no evidence currently exists for this phenomenon in Metazoa, this cannot be theoretically ruled out, and actually phenomena of this kind might well account for some "strange" cases of shared characters between cladistically unrelated species occurring in the same region of the world, which are well known to field naturalists, and usually (although often "uncomfortably") "explained" by "ad hoc" concepts such as "convergence" or "mimetism". As Doolittle (1999: 2124) put it: "If 'chimerism' or 'lateral gene transfer' cannot be dismissed as trivial in extent or limited to special categories of genes, then no hierarchical universal classification can be taken as natural. Molecular phylogeneticists will have failed to find the 'true tree', not because their methods are inadequate or because they have chosen the wrong genes, but because the history of life cannot properly be represented as a tree". This would have drastic consequences on our conceptions of biological classification, exemplified by this suggestion: "While retaining useful names for recognized groups (Archaea and Bacteria), one could see these as taxonomic descriptors based directly on shared genes, but only based indirectly and unpredictably on shared ancestry" (Doolittle 1999: 2128).

All these possible future developments, and others that cannot be thought of today, suggest that taxonomy is not a discipline of the past, but may show major changes in the coming century: this implies that prudence is in order and that it

would not be a good idea to "freeze" taxonomic theory and practice according to our current thoughts.

NOMENCLATURAL SYSTEM

Similarly, it is possible to propose a set of properties that a formal and codified system of biological nomenclature should have in order to be coherent, efficient and acceptable by all taxonomists worldwide, whatever their "school of thought". These, of course, include the same six properties outlined above for the taxonomic system (as both systems should be compatible and "parallel"), but they also include additional properties, that can be requested from a set of rules and could not be so in a free scientific discipline. Eleven "principles" can be outlined for any biological nomenclatural system:

(N1) *Independence*: the nomenclatural system should be "theory-independent" (Lidén *et al.* 1997: 738), i.e. devised in such a way as not to infringe upon the independence of taxonomic thought and action. The function of a code of nomenclature is simply, and should be restricted to, providing clear rules allowing one to know the *valid* nomen of any given taxon *in any given taxonomy*. It should be possible to apply the same rules to all taxonomies and taxonomic systems, without having to discuss the theoretical foundations and practical aspects of the latter. As nicely written by Knapp *et al.* (2004: 620): "Keep the science out of nomenclature!".

(N2) *Exhaustiveness*: like the taxonomic system, the nomenclatural system should be devised in such a way as to be able to accomodate *all living beings* ever to be found in the real world, not only some of them. Here also, exclusion from its field of competence of some organisms (e.g., for being of hybrid origin, or for being teratological) would be unacceptable.

(N3) *Simplicity*: the system must have a coherence, an internal logic and an elegance that make it easily assimilated by anyone who makes a real effort to understand it. This means that *useless* complications should be avoided, but not necessary complexities that are imposed by the particularities of the matter themselves. An important source of complication of any nomenclatural system is the existence of numerous *exceptions* to the general rules. A good nomenclatural system must be in force in the vast majority of situations and must not include a forest of exceptions, for if the latter are too numerous they obscure the generality of the rules and their normative value.

(N4) *Unicity*: there should exist a single nomenclatural system for all organisms. This aim is currently not reached, as several distinct codes exist for different kinds of organisms (animals, plants, bacteria, viruses, and even cultivated plants). It is unlikely that these codes will ever be unified under a single "*Biocode*" (Hawksworth *et al.* 1994; Anonymous 1999b; Blackwell & Powell 1999): as the botanical and zoological codes have been in force for about one century, millions of nomina have been stabilized according to these respective codes, and homogenizing them would necessarily entail major changes in one group of organisms or in both, which would go against the need of nomenclatural robustness that will be discussed below. It is however necessary to stop at this point, and not to continue a process of subdivision of the codes that was illustrated for example in zoology by the introduction of a particular kind of "name-bearing types", the hapantotypes, for protistans (Anonymous 1999b). The risk would be to have a particular code for every group of organisms, i.e. for every community of "specialists", which would be a threat against the unity of biology.

(N5) *Universality*: to be usable everywhere in space and time, a nomenclatural code must be accepted by all biologists, whatever their country, language, etc. It must also be the same code for all taxonomists, whatever their conception of taxonomy. A universal code cannot be linked to a particular conception of taxonomy, as this would not respect the first principle of independence between taxonomy and nomenclature presented above.

(N6) *Univocality*: in a good nomenclatural system, allocation of a nomen to a taxon must be univocal, i.e. it should not allow ambiguity in either sense: *given a taxonomy*, a taxon must receive *one single nomen*, in all countries worlwide

and by all taxonomists, whatever their opinions or tastes. Of course, this requirement does not concern distinct taxonomies of the same group: each taxonomy implies its own nomenclature.

(N7) *Automaticity*: the rules that command the allocation of nomina to taxa must work in the most automatic possible way, without leaving place to arbitrary or bureaucratic decisions. The freedom of choice of taxonomists as concerns nomenclature must be restricted to the indispensable minimum, i.e. concretely to only two situations: at the *creation* of a nomen, and for every *nomenclatural act*, i.e. every *first-reviser action* whenever such an action is necessary. In all other cases, the decision should be automatic and should, in all rigour, arouse no emotion from the part of users. A good nomenclatural system should not only avoid arbitrary decisions, but also limit to the minimum the recourse to bureaucratic action through committees, commissions, courts or other boards, charged to take decisions whenever the rules leave some doubt in their application. The automaticity of the rules should ideally be such that two taxonomists living on opposite sides of the planet should be able, without communicating with each other, to reach the same nomenclatural conclusion whenever they adopt the same taxonomy. Any system requiring a frequent recourse to a bureaucratic authority would be too heavy and soon bound to become inefficient. Furthermore, the strong risk exists that it could place the taxonomists of different countries or having different opinions in an unbalanced situation, which leads us to the next point.

(N8) *Deontology*: the basic rules must be such that all taxonomists worldwide can use them and contribute equally to biological nomenclature. These rules should not favour any group of taxonomists against the others. In case of problems, the criteria allowing decision should be independent from country, language, taxonomic school, opinion, date, support used to publish the data, and also from other more "material" parameters such as the financial resources of researchers, laboratories or countries, their notoriety, their "international importance", the distribution or influence of their works, etc. If it were not the case, one would no doubt assist very rapidly to the leadership (not to say "dictatorship" or "tyranny", to use words quoted below) of some biologists, e.g., of some countries, on those of the rest of the world. This should be avoided by all means, not only for "ethical" reasons (although such reasons have no doubt their place in the policy of scientific research), but also for a much more practical one: not doing so would no doubt imply a risk of secession within the international community of biologists. If several nomenclatures became stabilized in different countries or groups of countries, this would imply major problems of communication between biologists worlwide, with risks of important consequences in various fields such as conservation biology, medical and agricultural sciences, not to mention basic biological research. In order to respect this deontological criterion, a very important feature of any nomenclatural system is that the initial *status* of a nomen (i.e. its nomenclatural availability and its allocation to a biological taxon) be *only* or *at least primarily* (in case of initial ambiguity) determined by the *original publication* in which this nomen was *first* proposed, and not by any subsequent, correct or incorrect, usage of this nomen.

(N9) *Hierarchy*: in order to allow taxonomy to play its role of system of storage and retrieval of information, a nomenclatural system must have a hierarchical structure. Nomina of different kinds should be given to taxa of different ranks. The way such an indexation system through nomina works is simple: even without being a specialist of the taxonomic group concerned, any biologist or non-biologist worldwide can find almost instantly, when reading nomina like "*Saga pedo* (Insecta, Orthoptera, Tettigoniidae)", "*Aubria masako* (Amphibia, Anura, Ranidae)" or "*Upupa epops* (Aves, Coraciiformes, Upupidae)", that the first one is a grasshopper, the second a frog and the third one a bird, and thus have immediate access to all the relevant literature concerning these taxa. This would not be possible, or much more difficult, under a uninominal and non-hierarchical nomenclatural system.

(N10) *Homogeneity*: in a good nomenclatural system, taxa of the same "nature", i.e. in a hierarchical taxonomic system, of same *rank* in the hierarchy, should be treated in the same way, i.e. they should bear nomina formed in a similar way and following the same rules. This allows taxa of same rank to be recognized as such by simple examination of the nomen. It is thus desirable that specific nomina cannot be mistaken for generic nomina, familial nomina for subfamilial nomina, etc. As discussed above, this does not mean that these taxa are necessarily "equivalent" in biological terms, but that they have an equivalent place in the hierarchical taxonomic system. A good example of this situation is provided by the species rank. There are various *concepts* of the *taxonomic category* of species (see e.g., Mayden 1997; Pleijel & Rouse 2000; Wheeler & Meier 2000). Many taxonomists claim that their concept is "the best" (Cracraft 2000) and should accommodate all forms of living organisms. However, this results from a strong influence of the reductionist mode of thinking on evolutionary biology. Given the diversity of evolutionary processes, there is no reason to think that all organisms should pertain in units of the same "kind". Systematics should try and account for real evolutionary phenomena, which requires fitting our taxonomic concepts to this reality, not the contrary. The correct attitude in this respect is probably to consider that, according to the biological group and its evolutionary history and particularities, different "kinds of units" should be recognized (Claridge *et al.* 1997). Thus, so-called agamospecies or *klonons*, or hybridogenetic or gynogenetic "species" or *kleptons*, have characteristics widely different from those of "bisexual Mendelian species", but this does not entail that they should not be ascribed the nomenclatural rank of species in a hierarchical taxonomy, provided their particular biological characteristics are pointed out, e.g., by the use of special marks like interpolation of "kn." or "kl." between the generic substantive and the specific epithet (Dubois 1991a). Contrary to the statements of Minelli (2000), this stresses the independence between taxonomy (which refers to biological concepts) and nomenclature (which only deals with nomina and their hierarchical arrangement). Quite independently from any biological discussion on their genetic structure and evolutionary particularities, kleptons, for example, *must* be named (Dubois 1988b) and given a place in the hierarchical system, in order to be unambiguously designated in various kinds of biological works or for more "trivial" purposes like placement in texts dealing with the management of biodiversity (official "species lists" for conservation biology, custom legislation, etc.).

(N11) *Robustness*: finally, it is desirable that nomina of taxa do not change if taxa do not change, or if they change only "slightly". Schuh (2003: 62) aptly wrote in this respect: "by their very nature, the codes are designed not so much to create a stable nomenclature but possibly better said, 'an orderly one which minimizes changes in names as they are applied to taxonomic concepts'". The problem is dual, as well outlined by Thiele & Yeates (2002): "The heart of the problem is the central duality and tension in taxonomic names. On the one hand, names are convenient shorthand representations of scientific hypotheses, and as such should be as volatile as hypotheses in any other field – proposed, used, modified and then perhaps discarded as evidence dictates. On the other hand, taxonomic names are keys to biological information, unique codes that unlock the library of knowledge about those organisms, and for this purpose they must be non-volatile or the links will be lost. A solution must adequately recognize these dual roles and decouple the system that allows maximum freedom of hypothesis-generation from the system that provides names for users". Here also, like for taxonomy above, the term *robustness* describes more adequately the kind of "stability" that we need in nomenclature. Complete stability of nomina is indispensable only in some very specific and quite rare cases, i.e. when it concerns nomina that are widely known and used in various fields of human knowledge and communication *outside the restricted domain of systematic research*. This situation only concerns a very small proportion of all the millions of nomina available in biological nomenclature. Beside these particu-

lar cases, which of course must be duly considered, the request for "stability" of nomina should not become a "religion", in the shadow of which the whole nomenclatural system can be weakened and subsequently dismantled. It is true that taxonomists need stability, but in reality what they need is *stability in the nomenclatural rules of allocation of nomina to taxa*, not stability of the nomina themselves. Supporting simply "nomenclatural stability" without further qualification would mean: 1) either that neither taxonomy nor nomenclature are allowed to change; or 2) that any change, even very minor, in taxonomy, must result in a nomenclatural change. In contrast, "nomenclatural robustness" means that taxonomy can evolve, but that a given change in taxonomy does not necessarily result in a nomenclatural change. More details on these points will be given below.

Confrontation of the 11 "principles" above with the actual *Code of Zoological Nomenclature* currently in force (Anonymous 1999b) shows that the latter follows most of these principles. There are a few exceptions however, and more so following some of the changes introduced in the last two editions of the *Code* (Anonymous 1985, 1999b), as compared to the previous ones (Anonymous 1961, 1964, 1974): this matter was discussed in more detail elsewhere (Dubois 1985, 1999a, 2000a), and will need more discussion in the future. For the purpose of the present work, however, the principles above will be taken as a basic guideline for the reflection on new, universal rules for the nomenclature of higher taxa in zoology. However, before going into the questions posed by higher nomenclature, a few more general theoretical questions concerning taxonomy and nomenclature must be discussed.

SOME BASIC CONCEPTS OF BIOLOGICAL TAXONOMY AND NOMENCLATURE

Intension and extension

Many of the discussions that have developed around biological taxonomy and nomenclature stem from a debate around the relationships between reality and language. The long-discussed question (see references and discussion in Keller *et al.* 2003) whether taxa are classes (i.e. concepts) or individuals (i.e. real "things") is largely irrelevant, because they are both. Doubtless, "things" do exist (or have existed) in the real world that, being the result of a real phylogenetic process, can qualify as "natural clades" or "individuals" (this term being taken in an ontological sense). The phylogeny of any group of organisms is a historical process that occurred only once and under a single pattern (real cladistic relationships). It is now widely acknowledged that a major aim of evolutionary systematics is to discover these cladistic relationships and to base biological taxonomy on them. Whether this aim is ultimately within the possible reach of science, using the (imperfect) tools that we have to reconstruct the past events of the history of life on earth, is open to question. What is indisputable however is that, *for the time being*, and probably for many decades yet, this aim is far from being reached. For the time being, virtually any cladistic analysis of any living group provides a cladogram that is slightly or largely different from the previous ones. Cladograms as we currently use them are *working hypotheses*, liable to be refuted and replaced by better ones, they are not final results of science. They are not *facts* or *things* of the real world, but scientific *concepts*. As Benton (2000: 634) put it: "phylogeny is real, classifications are not". Actually, classifications also are real, as creations of the human mind, but what is at stake is their relation to the actual history of life on earth, i.e. whether they reflect (or may, or will reflect) exactly the reality of evolution, or not.

Ghiselin (1966: 127) stated that: "Definitions only apply to words, not to the things to which the words correspond". However, the term "words" is not the end here, as a *word* refers to a *concept*, i.e. a tool used by man's mind to interpret reality. Although one does not define reality, one does define concepts, that are aiming at decipher and understand reality. Thus, the related statement of De Queiroz & Gauthier (1990:

308) that "definitions are given to words, including to names, not to the things that the names represent, that is, the taxa themselves", would apply to the (unknown) real clades of the real world, but not to taxa, even when based on cladograms: all taxa are working hypotheses that are *defined* by science, not *discovered*. Taxa defined on the basis of cladograms or cladons are *concepts*, they are not the "real clades of the real world". Cladons are hypotheses about clades, they are not the clades: "taxonomic definitions (or diagnoses, if you prefer) are hypotheses about entities in nature. Those hypotheses are subject to test and rejection or revision" (Schuh 2003: 70).

Although rather seldom mentioned in taxonomy, the terms *intension* and *extension*, of traditional use in philosophy, logics and didactics (Dupuis 1988: 87; Rey 1993: 461, 767, 1038), are quite adequate to describe the two ways of qualifying or characterizing taxa in biological classification (Buck & Hull 1966). Any concept can be qualified in two distinct ways: either by its *intension* or *comprehension*, that is "the set of all the properties or attributes that characterize this concept" (Papavero *et al.* 2001: 6); or by its *extension* or *circumscription*, that is "the set of all objects that satisfy the [intensional] definition of that concept" (Papavero *et al.* 2001: 6). These terms fully apply to all classificatory systems, including taxonomies: the units or classes recognized can be qualified and identified either by intension or by extension.

In any taxonomy, the intension or intensional definition of a taxon is a set of statements about its characteristics, or its supposed differences from other taxa, or its relationships with the latter. This definition may rely on different kinds of information according to the philosophy of taxonomy adopted: 1) it may be based on *characters*, the traditional practice in biological taxonomy, being then either a *description* (list of all known "major" and "relevant" characters of the taxon, whether proper to this taxon or common with other taxa), a *diagnosis* (list of all known characters that are supposed to distinguish this taxon from all other taxa) or an *apognosis* (Dubois 1997b) (list of known or supposed synapomorphies of its members that are considered to establish the fact that this taxon corresponds to a clade of its own); and 2) it may be based on known or hypothesized *relationships* between organisms. Among such relationships are those expressed in the topology of a cladogram, i.e. hypothetic cladistic relationships between organisms, that are used in the so-called *phylogenetic definitions* of taxa as advocated by De Queiroz & Gauthier (1992) and others. However these are not the only kinds of relationships used to define taxa, although this has not been much emphasized until now: another useful kind of information is provided by criteria like hybridizability (crossability), allopatry/parapatry/sympatry or competitive exclusion, which qualify as "relational taxonomic criteria" (Dubois 1988c) or more briefly *relacters* (Dubois 2004c). Ultimately however, even under the second kind of definition by relationships, we need to have information on characters to allocate organisms to taxa, except if one believes that this can be done simply by way of faith or intuition.

On the other hand, the extension or circumscription is the *content* of the taxon, i.e. a list of its *elements* or *members* (individual organisms and/or taxa), or, which is strictly equivalent, statements about its boundaries or limits, or even a list of its non-members (all organisms or taxa *excluded* from it). For more clarity in the discussion below, the first kind of qualification of a taxon, by a list of its included members, will be called *inclusive extension*, whereas the second kind, by a list of excluded elements (that are therefore referred to one or several other taxa), will be designated as *exclusive extension*.

Of course, in any consistent and rigorous classificatory system, both kinds of qualification are strictly equivalent, as a given intension corresponds to a given extension, and vice versa. However, concretely, in many cases, and especially in biological classification, they may give different results simply because some pieces of information concerning some organisms may be (and often are) lacking or inexact: whatever the criteria used to build up a biological classification (phenetic, cladistic, "phyletic" *sensu* Rasnitsyn

1996, or other), the result may be misleading as long as some taxa remain undiscovered or as some characters remain unknown or inaccurately ascertained. This is the reason why quite frequently some organisms or taxa are mistakenly referred to a taxon although their actual, but unknown, characters, in fact exclude them from the latter, and subsequent discovery of these problems is a major reason why taxonomies often have to change and cannot remain "stable" for ever.

Taxa, which are concepts, may be fully qualified or defined either by intension or by extension: they have both an intensional definition, and a content and limits. On the other hand nomina, which are labels and not concepts, cannot be fully defined either by intension or by extension, as will be discussed below. The discussion of De Queiroz & Gauthier (1990) about "character-based definitions" of nomina is irrelevant and stems from a typological concept of "types" (Lidén & Oxelman 1996) and a basic confusion between taxonomy and nomenclature (Benton 2000; Stuessy 2000, 2001): in recent taxonomy and nomenclature, such "intensional definitions" may apply to taxa but not to nomina, as according to the *Code* nomina are not *defined* by intension (characters or relationships) or extension, but *attached* to given members of the taxa (individuals or taxa) by *ostension* (Keller *et al.* 2003: 99). Such so-called "ostensive definitions" of nomina (De Queiroz & Cantino, 2001) are more aptly designated as a process of "christening" or "labelling" rather than "defining" nomina (Stuessy 2001; Kojima 2003): "In the Linnaean system the taxon names are not defined, but they are labels for taxon concepts that are defined or circumscribed based on the observation of characters" (Kojima 2003: 55). Blackwelder (1967: 166) used another useful comparison, stating that a type "provides an anchor for the name, indicating the point in the diversity to which the name is forever attached". Although "labelling by ostension" could be very formally designated as a definition through "partial extension" of the taxon, this is not a *complete* definition of the latter, as it provides no *boundary* for the taxon so "defined". At any rate, if "typification" is understood as a "definition", this can be only a definition of the nomen, not of the taxon: whether or not a given organism should be given a certain nomen depends not on the definition of the nomen but on that of the taxon it is referred to. All of this was already briefly and strikingly stated in the nice words of Luella Weresub quoted by Nicolson (1977: 569) and Jørgensen (2000: 779): "Taxa have circumscriptions but no types while names have types but no circumscriptions".

THE ZOOLOGICAL *CODE*, A THREE-STOREY NOMENCLATURAL SYSTEM

Biological nomenclature is not a system of *classification* but of *nomination* of taxa. Its rôle is not to define taxa, be it by extension or by intension, but to name them, i.e. to provide labels that will allow their universal and unambiguous designation. As argued above, the current *Code* is not using "character-based definitions of taxon names" (De Queiroz & Gauthier 1990: 308, 309). The *Code* indeed mentions the need to provide "character-based" definitions of *taxa*, but the status of *nomina* is not given by these definitions or by these characters. Characters as used in the *Code* are in no way useful either for the *allocation* of nomina to taxa or for the *validity* of nomina: their rôle is "simply", but importantly, to allow *availability* of nomina. For a better understanding of these questions, these three concepts must be clearly separated, which is not clearly done in the current *Code* and therefore deserves a special discussion.

The current nomenclatural system for zoological taxa is like a "three-storey" house, with three independent steps leading to the establishment of the valid nomen of a taxon, *availability-allocation-validity* (Av-Al-Va): 1) rules for the *nomenclatural availability* of nomina; 2) rules for the *taxonomic allocation* of nomina; and 3) rules for the *nomenclatural validity* of nomina. Each of these steps makes use of special *technical tools* to reach its aim. Let us consider these three steps separately.

1) *Nomenclatural availability* (Av) of nomina is determined by several criteria, the most notewor-

thy of which are *criteria of publication*, *criteria of formation of nomina* and *statement of diagnostic characters*. This step is the only one in the nomenclatural process where characters are mentioned. Why? Of course, mention of characters plays a rôle in the *taxonomic definition* of the *taxon* to which the new nomen is applied. But, in terms of nomenclature, its rôle is merely to avoid the introduction into biological nomenclature of nomina based on *hypothetical concepts* rather than on actual organisms, or of nomina based on unidentified material or undefined taxa (*nomina nuda*). The existence of *nomina nuda* implies that descriptions be attached to nomina but not that nomina have "intensional or extensional connotations" (Moore 1998: 562). What the *Code* asks is simply that a definition or diagnosis based on characters observed on actual specimens *be given*, but not that this definition *be exact* or *complete*. This is a very clever disposition of the *Code* indeed, because no definition or diagnosis of a taxon can ever be complete, and, even with the greatest precautions, it is difficult to completely avoid mistakes in this respect: the history of zoological nomenclature is full of examples of taxa that were first established on the basis of incomplete or inaccurate descriptions or definitions, but this in no way affects the availability, allocation or validity of the nomina that were then given to them. This question was discussed at more length elsewhere (Dubois & Ohler 1997).

2) According to the *Code* currently in force, *taxonomic allocation* (Al) of nomina does not depend in the least on the definitions, descriptions, diagnoses or apognoses of the specimens or of the taxa to which the nomina are applied, but on the combined use of three tools unique to biological nomenclature, those of so-called "nomenclatural types", of so-called "nominal taxa" and of *coordinate ranks*. In this nomenclatural system, a nomen is not attached to a *taxon* or to its *definition*, but to one or several *individual organism(s)*. These specimens, usually known as "types", or better as *onomatophores* (see below), establish an objective link

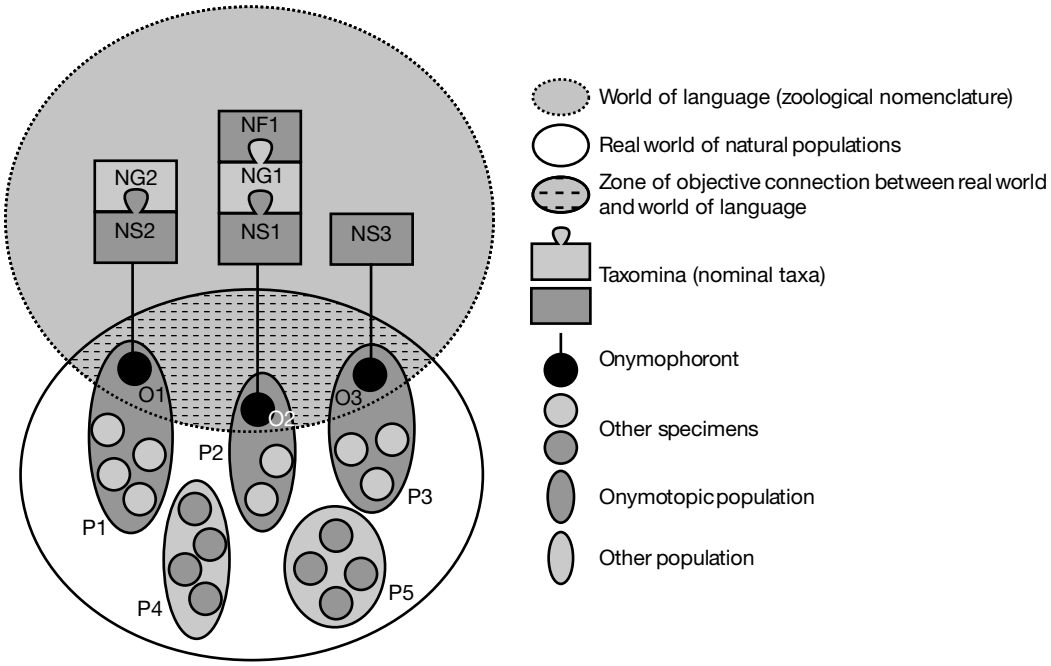

FIG. 1. — The rôle of onomatophores as an objective connection between the real world of organismal populations and the world of language (biological nomenclature). Abbreviations: **NF1**, nominal family 1; **NG1**, **NG2**, nominal genera 1 and 2; **NS1**-**NS3**, nominal species 1 to 3; **P1**-**P5**, natural populations 1 to 5; **O1**-**O3**, onomatophores 1 to 3 (modified from Dubois & Ohler 1997).

between the real world of organisms and the world of language (nomenclature) (Fig. 1; Dubois & Ohler 1997: 304). This permanent link between a real *specimen* (or a series of specimens) and a *nomen* is what the *Code* calls a "nominal taxon", a concept which was retermed *taxomen* (Dubois 2000b) to stress the fact that a taxomen is *not* a taxon, as it has no limits or defined contents. It is not equivalent to a definition of the taxon, as it does not refer to any character or relationships that would allow to establish whether a given organism should be referred to the taxon or not. The definition, limits and contents of the taxon may change without being followed by a change in the nomen, as long as the onomatophore remains allocated to it. In a way, it may be argued that the taxomen is qualified by its extension, i.e. either by its *original contents* or by *subsequently restricted contents* (e.g., after designation of a lectotype among syntypes or of a type species among several originally included species), but this extension only qualifies the *taxomen*, not the *taxon* to which it may apply. The allocation of a nomen to a given taxon *in a given taxonomy* does not rely on any kind of definition, but on the onomatophore of this nomen being *referred* to this taxon *in this taxonomy*: all these elements are liable to change as taxonomy evolves, whereas the taxomen remains a stable and permanent link between the onomatophore and the nomen. Finally, as will be discussed in more detail below, *within the frame of a given taxonomy* the hierarchical structure of the taxonomic and nomenclatural system allows the unambiguous allocation of a nomen to a taxon of a given rank whenever the onomatophore of a nomen is included in several hierarchically coordinate taxa.

3) Establishment of the *validity* (Va) of a nomen is the third and last step of the nomenclatural process briefly described here. In many cases, several nomina are available for a given taxon in a given taxonomy and might potentially be used to designate it, which would be a source of confusion and instability. Among these competing nomina termed *synonyms*, the *rule of priority* allows to establish *automatically* the valid one, usually without consideration of any other criteria (in some special but rare cases, i.e. to protect "well known" nomina, other criteria like "usage" may be called upon).

For reasons already discussed elsewhere (e.g., Dubois & Ohler 1997; Dubois 2000a, b), the major advantages of such an onomatophore-based nomenclatural system are that: 1) it allows a clear relationship between the world of organisms and the world of language; and 2) it is not linked to any given philosophy of taxonomy, which would be the case of an intension-based nomenclatural system. This independence between taxonomy and nomenclature respects the basic philosophical foundation of the *Code*, which is expressed as follows in its preamble: "The objects of the *Code* are to promote stability and universality in the scientific names of animals and to ensure that the name of each taxon is unique and distinct. All its provisions and recommendations are subservient to those ends and none restricts the freedom of taxonomic thought or actions" (Anonymous 1999b: 2). The need for absolute freedom of taxonomic thought and action was recognized long ago, as examplified by this statement of Strickland *et al.* (1843: 259): "So long as naturalists differ in the views which they are disposed to take of the natural affinities of animals there will always be diversities of classification, and the only way to arrive at the true system of nature is to allow perfect liberty to systematists in this respect". So the principle of a large (although not complete, which would be impossible) independence between taxonomy and nomenclature should be followed in any nomenclatural system, not only because it is the only one that respects the necessary freedom of thought, writing and action of all scientists – a major need for a harmonious and fruitful development of any scientific field –, but also, and perhaps mostly, because no one knows what will be the future of taxonomy. The philosophical bases of zoological taxonomy have dramatically changed during the last decades, with several periods where different paradigms were subsequently "dominant", and there is no reason to think that we have now reached the ultimate stage in this respect. As discussed above, taxonomy might

show basic developments and changes in the coming century, and it would be extremely risky and presumptuous to tie nomenclature to a particular conception of taxonomy. As noted by Moore (1998: 573), "the current system has supported artificial, natural, evolutionary, phenetic, and cladistic classifications" and it should not "be assumed that there will not be another theoretical shift some time in the future". The evolution of concepts and criteria in taxonomy should remain free and should not affect or be affected by nomenclatural rules.

Usage and stability

Two terms have had growing use in discussions about taxonomy and nomenclature in the recent decades: those of "usage" and of "stability". However these terms have rarely been seriously defined and discussed. The basic idea here is as follows: nomina (scientific names) are used as universal references by all biologists and by non-biologists to designate living organisms; in order to play this rôle unambiguously and universally, they should be stable and they should never change, or change only exceptionally. As a consequence, some authors (e.g., Cornelius 1987; Gould 1990; Savage 1990a, b, 1991; Bock 1994) have recently advocated the replacement of the basic principle, or "fundamental maxim" (Strickland *et al.* 1843: 262) of the *Code*, the Rule of Priority, by a so-called "rule of usage". Others have been even farther, suggesting to "stop taxonomists" and to set up "international standing commissions" excluding taxonomists to decide on stable "lists of official names" (Barnett 1989, 1991). This results in various problems, among which several can be stressed.

1) The poorly defined term "stability of nomina" covers at least six distinct meanings (and probably more), that are confused by many authors: (M1) genuine "taxonomic stability", i.e. stability of taxa; (M2) genuine "nomenclatural stability", i.e. stability of the nomina designating the taxa (in the absence of changes in the latter); (M3) stability in the nomenclatural rules, i.e. stability of the criteria allowing the identification of the valid nomen of a taxon in a given taxonomy; (M4) stability of the nomina given to "kinds of organisms"; (M5) stability of the nomina given to individual biological specimens; (M6) "metaphysical stability" of nomina as defined by Nixon & Carpenter (2000), i.e. stability of formal definition of cladons, irrespective of their contents (i.e. of the organisms referred to them).

Stability of the kind (M5) is of course totally impossible. Even if taxonomy, nomenclature and nomenclatural rules were absolutely clear and stable, there would remain numerous cases of initially (or subsequently) *wrongly identified* specimens, i.e. specimens that were allocated to a wrong taxon according to the taxonomic standards then in force. Their nomina will have to change whenever their wrong identification is corrected, and this may be viewed by some biologists of disciplines far from systematics as cases of "nomenclatural instability": a famous example in frogs was the discovery that specimens used in several laboraties and in various studies under the nomen *Xenopus muelleri* did in fact belong in the quite distinct, although related, species *Xenopus borealis* (Bisbee *et al.* 1977).

Item (M4) is the kind of stability that most biologists have in mind when they support "nomenclatural stability": it means that a given specimen correctly identified (according to the taxonomy then prevalent) as *Hyla arborea*, member of the family *Hylidae* and of the order Anura in 1882 will bear the same nomina in 1960 and in 2005. However, whether this will be the case or not can be due to different causes as it will depend on the three items (M1) to (M3).

Concerning (M1), as was stressed repeatedly (e.g., Hershkovitz 1958; Fosberg 1991; Dubois 1998; Schuh 2003), the first and major cause of instability of nomina is not related to nomenclature but to the evolution of the scientific discipline of taxonomy: new taxa are continuously being recognized, and existing taxa are continuously being re-analysed and re-defined, i.e. modified. In the example above, discovery that two distinct "sibling" species were hidden under the nomen *Hyla arborea* results in all specimens from a part of the original range now having to bear another nomen. The only way to avoid this kind of

"instability" would be to stop taxonomic research (Dubois 1998: 22), i.e. indeed to "stop taxonomists" (Barnett 1989). Changes in taxonomy include changes in the intensional definitions of taxa and changes in the contents of taxa: both may be linked, but they may also be independent, as a taxon may remain composed of the same organisms but with a different intensional definition, and on the contrary the discovery of new organisms (new species, genera, etc.) may modify largely the contents of a taxon but without modifying its intension: so the concept of "taxonomic instability" itself is a complex and multi-dimensional one.

As for the second source of instability of nomina, (M2), i.e. changes of nomina for purely nomenclatural reasons (e.g., discovery of a senior synonym or homonym of a nomen), despite a widely held belief, serious studies of concrete situations (e.g., Hołynski 1994; Dubois 1998) show that it plays a minor rôle at the scale of a zoological group, much less important than item (M1). Its rôle is bound to decrease as serious, professional taxonomic and nomenclatural revisions of groups are conducted, and to increase if taxonomy and nomenclature become more and more "endangered" within biological sciences, and are abandoned by professional zoologists to "amateurs" and poorly trained technicians.

Finally, item (M3) could be easily avoided if the *Code* was never modified by the introduction of new rules having a *retroactive* strength. Unfortunately such changes have regularly been introduced by ICZN into the rules, resulting in unnecessary nomenclatural changes. They can often have unexpected side-effects that may be more disturbating than the positive effects of the change in the rules. Botanists, who have experienced this more than zoologists, as changes are introduced in each edition of their code, i.e. every six years, emphasize this problem: "Fortunately it is now almost universally realised that any change in the provisions of the *Code*, no matter how small, is certain to have some nomenclatural implications that will not necessarily have been thought through by the proposers or even detailed in the Rapporteurs' comments. Whereas the provisions for conservation and rejection can mitigate some of these negative effects, it rarely makes sense to try to amend the *Code* to deal with highly specific situations. This does not mean that the *Code* cannot be modified or improved in certain areas, but it needs to be done with great care" (McNeill 2000: 715). Examples of unexpected side-effects brought by changes in subsequent editions of the zoological *Code* were pointed out for example by Van Valen (1963) and Dubois (1985). Changes in the Rules are particularly pernicious as non-systematists have no way to understand them, and this kind of changes is likely to have played a rôle in the negative appraisal of biological nomenclature by many biologists outside the field of systematics. Avoiding these cases of "instability" that are particularly perturbating for communication between systematists and other biologists and non-biologists is the kind of problems that depend on nomenclatural rules and that must be addressed by any nomenclatural code: "it is not reasonable to think of nomenclature as stable in an absolute sense. It is much preferable to think of the methods of applying names as being stable. This conclusion presupposes, of course, that names cannot be perfectly stable if the taxonomy to which they are attached is not stable" (Schuh 2003: 64).

As for "metaphysical stability" of nomina (M6), it will be discussed below.

2) In zoological taxonomy and nomenclature, "universal usage" is quite rare. In the most frequent situation, different usages exist in parallel, in different countries, among different taxonomic schools, or between different authors. Therefore, in these cases, "usage" cannot be unambiguously determined. As soon as different usages coexist, apart from the Rule of Priority there is virtually no indisputable way to decide which usage should be followed. Usage may change over time (at different epochs) and space (in different countries). Quantitative criteria are not philosophically acceptable here: in science, as soon as different opinions are expressed and supported, each one is entitled to respect and examination on its own merits, irrespective of the number of persons who share it. In the history of science, new theories or

unexpected discoveries are not always accepted by the whole scientific community at once and their supporters may remain a small minority for decades: fortunately, this does not imply that their opinion should be silenced. Even if quantitative criteria were to be used to define "predominant usage", it would be difficult to choose between the following "numbers": number of authors having used a nomen over a given period of time; number of books, periodicals, languages, countries, where this nomen was used; number of distributed copies of a publication using a nomen, etc. (see Note 4). Qualitative criteria are no more acceptable, as they would result in stating that some scientists, or some publications, or some countries, are more relevant, important or worthy of attention than others in science. Commenting the projects of "lists of protected names in current use" in botany, Cronquist (1991: 309) very aptly wrote: "how often must a name be used to be in current use? It may be fifty, or even a hundred years after a perfectly valid species is described and published, before anyone has occasion to refer to it in print again. (...) Will there be a crew of botanists available to search the revisionary and monographic literature for Names in Current Use? If not, are such works to be cast aside as meaningless? Any List of Names in Current Use is likely to contain anomalies. The same species may have different names in current use in different countries. Which country will prevail, and will the losers accept their loss? Different names on the List may be mutually incompatible because they reflect different taxonomic outlooks. Will not many botanists soon be tempted to ignore the Lists because the Lists are taxonomically obtuse? I think so, and probably I would be among them. Instead of fostering stability, such Lists would be likely to foster confusion".

3) "Stability" is not a scientific aim (Dubois 1998): "the goal of absolute nomenclatural stability is illusory and misguided" (Schuh 2003: 60). As Gaffney (1979a: 103), Dominguez & Wheeler (1997) and Benton (2000) put it, in taxonomy, "stability is ignorance". More than 25 years ago, Gaffney (1977: 23) already wrote: "Although stability is often considered an important quality of classifications, I believe that it is often a spurious and misleading indication of the attainment of phylogenetic 'truth'. All of our notions about phylogeny are hypotheses that could be wrong; they can never be proved correct. If a classification is to have wide-ranging biologic usefulness, it must be susceptible to change". Shortly after (Gaffney 1979a: 103), he commented again on this point and added: "In fact, temporal stability of classifications often reflects ignorance of relationships and lack of work. I hardly advocate change for its own sake, but the maintenance of names for discarded concepts seems useless and misleading". More recently, Bremer *et al.* (1990) wrote in forceful words: "Taxonomists should pursue their scientific venture and stop worrying about instability in classification. Taxonomy is not a service function for labelling organisms, but a science of its own, dealing with variation, relationships and phylogeny. Other biologists need to keep themselves informed, and should realize that removal of artificial groups and improvements in classification are desirable". The same authors aptly pointed out that "The problem with taxonomic instability is not one of science, but of information", and that "we need international database systems carrying continuously updated taxonomic information and providing easy access to synonymy and recent literature". Unlike any religious or other dogma, science, by its very nature, constantly modifies its (provisional) conclusions, and this is the basic reason for its permanent progress. Requiring a "freezing" of science would mean killing it.

Actually, the question of "stability" of taxa and nomina makes sense only for non-scientists. Therefore, discussions on stability should be strictly restricted to the very few taxa and nomina (probably much less than 1/10000 of the total) which happen to be well known and widely used by non-taxonomists. Thus, any statement that "protection" of a nomen because of "usage" and against priority should be considered is *potentially* acceptable *only* if based on evidence coming from *outside* the field of specialized publications dealing with taxonomy and related domains.

4) Naming a taxon is a three-dimensional process that involves the *nomen* of the taxon, the *extension* of the taxon (its contents and limits) and the *intension* of the taxon (its definition by characters or relationships). Thus, "nomenclatural stability" depends on the kind of link that exists between these three elements: it is a problem of relationships between taxonomy and nomenclature. Two major kinds of relationships can exist in this respect:

a) There is an absolute and definitive connection between nomen, extension and intension of the taxon: this means that every time either the contents or the intensional definition of the taxon change(s), the nomen must be abandoned and replaced by a new one. This kind of relationship between taxonomy and nomenclature may seem more "logical" and is praised by some non-biologists, in particular by some data processing and artificial intelligence specialists, as it allows unambiguous and definitive qualification of the nomen. But this stability is in fact a source of considerable instability, as taxonomy is always changing, and would require therefore a permanent increase in the number of nomina. This would not be only "cumbersome", it would also obscure the historical understanding of the taxonomy of any given taxon as given by its synonymy. This is the case today in the nomenclature of higher taxa, where no continuity exists between many nomina that are introduced in the literature as new taxa are recognized.

b) The other solution is to have a *certain* dissociation between nomen, extension and intension of the taxon. There are two possibilities then: either the nomen is linked to a certain *content* of the taxon, so that the intension may change but the nomen may remain the same; or the nomen is connected with a certain *intension* of the taxon, so that the content may change but the nomen may remain the same. The first situation is that in the rules of the present *Code*, whereas the second one is that advocated e.g., by the supporters of the "*Phylocode*" (see below).

The need for nomenclatural robustness, as defined above, requires to have criteria allowing nomina to *follow* in a certain way the evolution of taxonomic concepts without having constantly to be replaced by new nomina. Such criteria could be quantitative or qualitative. Quantitative criteria could be, for example, that whenever a taxon is split into two new taxa, its nomen remains attached to the taxon that contains the highest number of species, or that whenever two taxa are lumped together they take the nomen of the most speciose one. A very short reflection shows that any system of this kind would very soon result in problems of interpretation and decision, and would create a great risk of progressive *drift* of any nomen from the taxon for which it had been originally proposed to other ones that in the end may have nothing to do with this original concept. The solution to this problem is to use not quantitative but qualitative criteria. Those used in the *Code* are simple, universal and unambiguous. They are the concept of onomatophore, the concept of taxomen, the concept of priority and the rule of coordination. The *Code* uses these rules only for the nomina of the three nominal-series it recognizes. How such rules could be applied to the nomina of higher taxa is more problematic, and will need a special discussion (Dubois in press).

The discussion above shows that a seemingly simple formula like "taxonomic stability" or "nomenclatural stability" is a complex matter that needs careful study before peremptory statements can be formulated. Whereas it may be fashionable and easy to publish that "Taxonomic instability continues to irritate" (Crisp & Fogg 1988), few journals would agree to publish a paper stating that the current neglect (not to say quasi-destruction) of taxonomy by the international scientific community is more than "irritating", but a major error in the scientific policy of our times (Wheeler 2004). At any rate, "irritation" is not a solution to the many problems mentioned above, which need to be solved by taxonomists, not by arrogant outsiders of the field.

Opening the Pandora's box: the rôle of ICZN

The consequences of the recent movement in favor of "usage" vs priority in zoological nomenclature are on the whole largely negative, mostly because this "philosophy" results in strongly

undermining the legislative value of the *Code* in the eyes of all zoologists.

As a matter of fact, some authors recently suggested a more or less complete abandonment of the Rule of Priority as the major structuring principle of zoological nomenclature, stating that this rule amounts to a "tyranny of the past" (Savage 1990a, b, 1991). They apparently did not realise that their struggle against international Rules may eventually result in another, much more real, "tyranny", that of "bureaucracy" (Fosberg 1991), or, even worse, of lobbies and partisan groups having important financial resources, i.e. in the end, a tyranny of money on science. Thus doing, they opened a Pandora's box and played a major rôle in the recent challenging of the legislative function of the *Code* and in the development of proposals of alternative nomenclatural systems. Unfortunately, their analysis was partly shared by ICZN, which in the recent decades often adopted a lax attitude and repeatedly accepted to suspend the Rules in order to "conserve" so-called "usage". It did so even in cases where this "usage" concerned only a few "confidential" taxonomic publications. In so *validating* gross recent nomenclatural *errors*, especially when committed by "prominent zoologists", ICZN has failed to play properly its rôle of "Keeper of the Law" and has contributed to spread among the international community of zoologists the feeling that the *Code* is not a system of clear and stringent rules, but an impressionist set of "recommendations" and "suggestions" that zoologists are more or less free to follow or not (for more details on these points, see e.g., Kiriakoff 1962; Dubois *et al.* 1988; Ng 1991; Hołynski 1994; Dubois & Ohler 1997; Dubois 1999a, b, 2000a, b; and Note 5). This recent tendency of ICZN to ignore some basic rules of the *Code*, besides adding confusion and difficulties in the daily work of taxonomists, has resulted in clearly weakening the legislative value and strength of this text. In the eyes of many zoologists, the *Code* appears now as an obscure and obsolete text of little consequence, rather than a set of stringent Rules allowing automatic allocation of a nomen to a taxon within the frame of a given taxonomy, on the basis of priority alone, without interfering with other subjective criteria like the opinions or tastes of authors, or judgements on "respectability" of usages or authors. Another consequence has been that the editors of many periodicals (including "high-ranked" ones) and books now accept to publish papers containing major nomenclatural mistakes (see list of examples in Dubois 2003a), thus continuing to undermine the power of the *Code*, with strong negative long-term consequences regarding the unity of zoology as a science (Dubois 1999b).

Queirauthian nomenclature

Rather than improving the current *Code* (in particular in extending its use to higher nomina) and caring for its being more seriously applied in zoological publications, some recent authors, following De Queiroz & Gauthier (1990, 1994), suggested to abandon it and replace it by a brand new "*Phylocode*" (see Note 6). I propose to call such a nomenclatural system "Queirauthian", a term combining parts of the patronyms of both its original proponents. Even if in the future the "*Phylocode*" turns out to be quite different from their original proposal, this is quite similar to the situation regarding the so-called "Linnaean" nomenclature, which is now very different from that originally advocated by Linnaeus (Moore 2003). Strictly speaking, the *Code* currently in force is no less "Linnaean" than the "*Phylocode*", as most of its Rules were implemented much after the works of Linnaeus, whereas its basic logic is Aristotelian (see McNeill *in* Brummitt 1997: 723). It is particularly misleading to state e.g., that "the absence of a system of priority for suprafamiliar [*sic*, for suprafamilial] taxa" is a feature of "Linnaean taxonomy" (Laurin 1998: 8), for two reasons at least: 1) no current taxonomy can be considered to be "Linnaean", as, whatever the philosophy of taxonomy adopted, not one current author worldwide still relies on Linnaeus' philosophy in this respect; and 2) the *Code* is not a system of taxonomy but a system of nomenclature, which is quite different (see Note 7).

As a matter of fact, the Queirauthian proposals are based in part on misunderstandings of the

current *Code* and above all on a basic confusion between *nomenclature* and *taxonomy* (Dubois 1999b, 2000a; Benton 2000; Stuessy 2000, 2001). Although it is presented as a code of *nomenclature*, the "*Phylocode*" aims at playing in fact a double rôle, that of a *code of taxonomy* and that of a *code of nomenclature* formally linked to the former by rigorous rules. This led Stuessy (2001) to rename it "*Phylosystem*" instead of "*Phylocode*" (see Note 8). The basic philosophical foundation of this system is fully different from that of the current *Code*. The many negative comments on this proposal in the recent years (for references to several works that cover most of the question, see Wheeler 2004: 577) and the replies to these criticisms (e.g., Bryant & Cantino 2002) largely remind a dialogue of the deaf, as the same terms (e.g., "definition" or "reality") are often used with different meanings by the contradictors. The purpose of the present discussion is not to review all the problems posed by this project, but some of its aspects only, that are directly related to the matter of higher nomenclature. Four theoretical points will be tackled here: 1) Querauthian nomenclature is not a consistent onomatophore-based nomenclatural system; 2) it proposes "metaphysical stability" of nomina, but not genuine nomenclatural robustness; 3) it rejects the use of ranks for taxa; and 4) it does not care for the original taxonomic allocation of nomina, with their original authorship and date, and rejects priority as the basis of validity of nomina. Besides, *and even perhaps more importantly*, this project poses considerable practical problems, that also need to be dully considered.

Theoretical problems

1) Contrary to the original statements of De Queiroz & Gauthier (1990, 1994), *nomina* recognized by the current *Code* are not based on *intensional definitions* of taxa, but on the (complete or partial) *original contents* of the latter, and more precisely on their original *onomatophores*, through the use of the *taxomen*. Some supporters of the Querauthian system are in favour of using several onomatophores to define nomina in this system. Schander & Thollesson (1995: 266) proposed the term "*nominotypes*" for such multiple onomatophores, but this proposal was not adopted by their fellow workers. In the proposed "*Phylocode*", the status of nomina in biological taxonomy would be determined not by specimens but by "phylogenetic definitions" using "*specifiers*". Although specifiers *may* be specimens or taxa, they may also be characters (apomorphies, i.e. *concepts*), a reversal to the early days of taxonomy when "the method of concepts" or "conceptual types" was used instead of "the method of types" or "nomenclatural types" (Cook 1895, 1898a, b, 1900; Moore 1998: 564). Thus the "*Phylocode*" is not a consistent onomatophore-based nomenclatural system. In accepting that nomina can be qualified by characters, this system is, at least partly, a purely circular system inside language, dealing only with abstractions (intensions), whereas the major strength of the current *Code* is its connection with the world of real animals through onomatophores. Any intension-based nomenclatural system is bound to be linked to a philosophy of taxonomy, which is not the case in an onomatophore-based system, in which the reference for the allocation of nomina to taxa is not a conception of the taxa, but simply the inclusion of one or several specimens or taxa in the latter. This is the case in the present *Code* for family-, genus- and species-series nomina, and I propose to generalize this basic rule to class-series nomina (see Note 9). Lidén & Oxelman (1996: 183) were correct when they wrote: "Misunderstanding of the type method is probably behind the enthusiam for 'phylogenetic taxonomy' as reflected in recent discussions on the world wide web".

Abandonment of the onomatophore principle in the Querauthian system requires to link nomenclatural rules with a philosophy of taxonomy, namely "phylogenetic taxonomy" or cladonomy, which infringes upon taxonomic freedom (Forey 2002). It results in nomina defined within this system having no existence within any other possible system, so it will tend to act as a powerful tool to *impede evolution of the discipline of taxonomy*, by trying to impose a "definitive taxonomic system" whatever the future discoveries and evolutions of concepts in biological sciences. Actually, this

"freezing of ideas" may be, perhaps inconsciously in the minds of some, its major aim.

2) Proponents of the Querauthian system contend that it allows a better stability of nomina, but this is due to confusion between the various senses of the term "stability of nomina" mentioned above. Discussing this requires first to remind a seemingly trivial fact, i.e. that the purpose of biological nomenclature is *to name organisms* in order to be able to *designate* them in scientific and non-scientific language. It is not to name concepts, be them hypothesized clades (cladons *sensu* Mayr 1995) or "germ lines" (see in this respect: Echelle 1990a, b; Dubois 1991a: 70, note 3). The Querauthian system provides stability in the nomenclatural designation of *concepts* ("clades", a clade being defined as an ancestor and all of its descendants), a kind of stability aptly designated by Nixon & Carpenter (2000) as "metaphysical stability", but not stability in the nomenclature of *organisms*, i.e. stability of the (M4) kind above. The nomenclatural stability provided by the Querauthian system is *a priori* stability by definitions, but such a system is not falsifiable: "Predictivity is not tested. Particular clades, once names (created), are always true: these clades henceforth always exist. This is trivial: *every* higher level taxon name that has ever been proposed is verified in this sense" (Nixon & Carpenter 2000: 315).

Let us consider the example of the nomina *AGAMIDAE* and *CHAMAELEONIDAE* discussed by De Queiroz & Gauthier (1990, 1994) (Fig. 2). Under the *Code*, allocation of these nomina to familial taxa is established by the simple taxonomic *allocation* of the "type genera" of these nomina to the taxa, irrespective of the criteria used to define these taxa, and irrespective of the place of these genera in a cladogram. In contrast, in the Querauthian system, nomina are associated with "phylogenetic definitions", so that a nomen will designate the clade including all taxa stemming from the most common recent ancestor of all taxa included in the original taxon designated by this nomen. Despite the claim of De Queiroz & Gauthier (1990, 1994) that their system provides a better "nomenclatural stability" than the *Code*, this is simply not true: in both systems, nomina are unambiguously applied to a given taxon in a given taxonomy, but any taxonomic change entails a change in the nomina given to some organisms. In both systems, specimens of the genera *Agama* and *Chamaeleo* belong in two distinct cladons (or taxa) under the taxonomy of Figure 2A and in the same cladon (or taxon) under the taxonomy of Figure 2B: as a result, in both cases a new cladogram may results in new *contents* for the taxa designated by the same nomina, so that the same nomen (*AGAMIDAE* or *CHAMAELEONIDAE*) applies to different individuals. In Figure 2E, the nomen *CHAMAELEONIDAE* applies in part to specimens to which it does not apply in Figure 2A, but the exactly same situation occurs in Figure 2F regarding the nomen *AGAMIDAE*! The only difference in this respect between the two systems is the way in which nomina are attached to taxa, either through allocation of onomatophores to taxa or through their inclusion in the cladon defined as stemming from a most common ancestor, but in both cases there is no ambiguity in the nomination of given individuals within a given taxonomy. Nixon & Carpenter (2000) discussed other examples of similar situations.

According to the *Code*, a nomen potentially applies to all taxa including its onomatophore, and among several such nomina, the valid one is determined by priority: thus, the definition of the nomen is unambiguous, but it does not tell us which organisms should bear this nomen, as this is a matter of taxonomy. Similarly, in Querauthian nomenclature, a nomen applies unambiguously to a defined cladon, but this does not tell us which organisms are members of this cladon: this is to be established through a cladistic analysis resulting in a cladistic hypothesis. In both cases, the "definition" of the nomen remains the same whatever the taxonomy adopted, but any change in cladistic hypotheses may result in taxonomic and therefore nomenclatural changes. In the Querauthian system, just like in the *Code*, total nomenclatural stability would exist only if taxonomy was final and stable, but as long as taxonomies will evolve, nomina given to individual organisms will have to change.

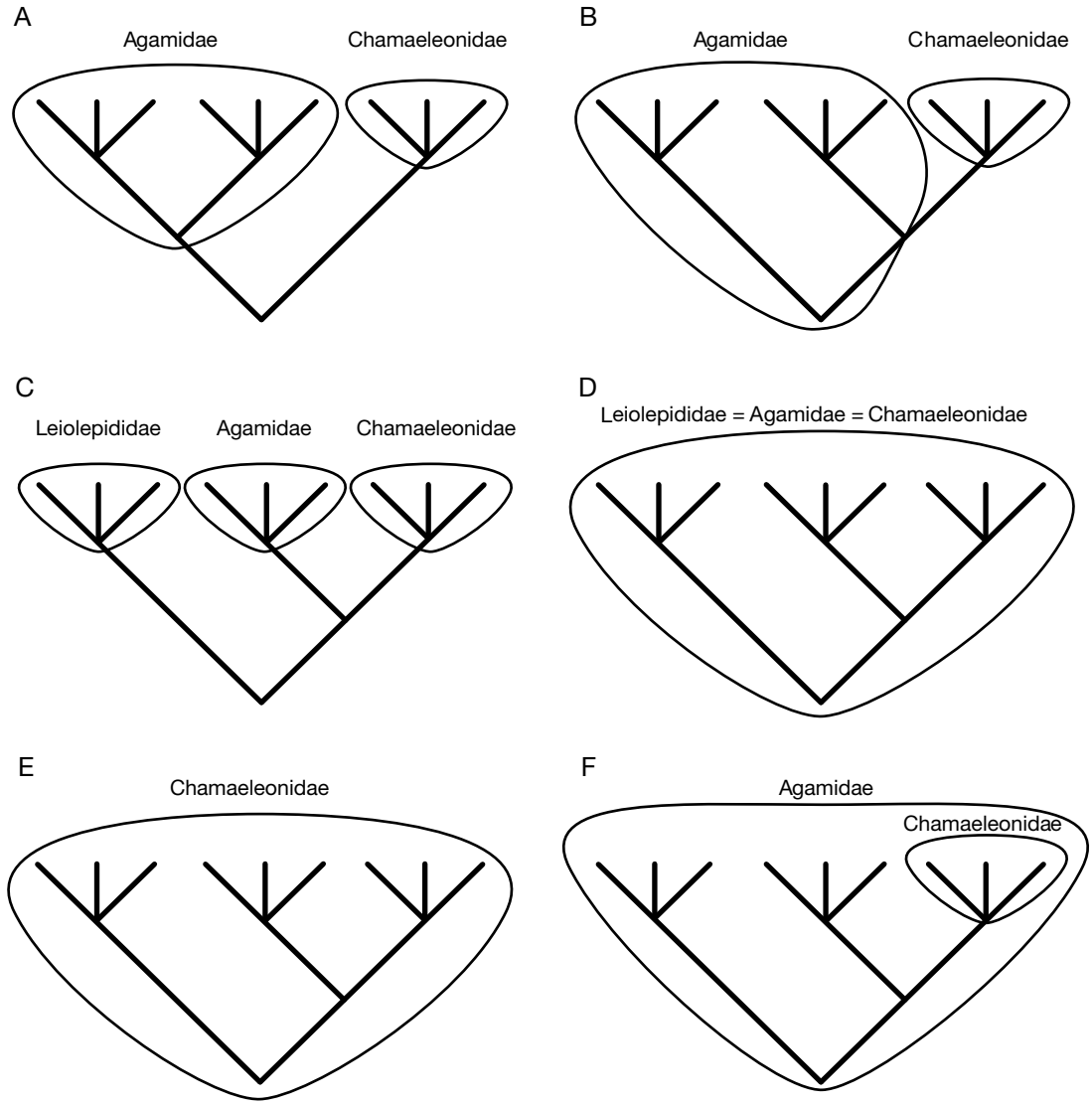

FIG. 2. — The "classical" example of the nomina *AGAMIDAE* and *CHAMAELEONIDAE* discussed by De Queiroz & Gauthier (1990, 1994). These nomina were first proposed for two families, i.e. implicitly two clades, the former including two groups or subfamilies (*AGAMINAE* and *LEIOLEPIDINAE*) (**A**). Under a revised hypothesis of relationships, the *AGAMIDAE* appear as paraphyletic relative to the *CHAMAELEONIDAE* (**B**). In any phylogenetic taxonomic system, paraphyletic groups cannot be recognized as taxa, which requires a new taxonomy, either with three taxa (**C**) or with a single taxon (**D**). According to the *Code*, there is no ambiguity for naming the taxa under both taxonomies, using the taxonomic allocation of the "type genera" of the familial nomina, the taxonomic hierarchy and priority among nomina: if three families are recognized, they will be called *AGAMIDAE*, *CHAMAELEONIDAE* and *LEIOLEPIDIDAE* (**C**), but if the three groups are placed in a single family, these three nomina are synonymous (**D**), and the nomen *CHAMAELEONIDAE* has priority (**E**). In the Querauthian system, nomina are associated with "phylogenetic definitions", and the nomen *AGAMIDAE* will designate the clade including all taxa stemming from the most common recent ancestor of all taxa included in the original *AGAMIDAE* (**A**), so that it will apply to the group including both the latter and the original *CHAMAELEONIDAE* (**F**).

This is in fact fully recognized by supporters of the Querauthian system, such as Bryant (1996: 174): "The clade referred to by the phylogenetic definition of a taxon name depends on 1) the reference phylogeny, 2) the meanings of taxon names referred to in the definition, and 3) whether the name is defined as a node-, stem-, or apomorphy-based taxon". Therefore the meaning of a nomen in this system depends on a reference phylogeny, i.e. the same nomen has different meanings under different taxonomic arrangements. This is exactly similar to the *Code* system, except that the latter is more flexible as it does not impose a *kind* of definition for taxa, which will make it compatible with any future taxonomic systems that may develop. De Queiroz (1997: 139) acknowledged this when he wrote: "neither phylogenetic nor Linnaean systems guarantee clarity, universality and stability in terms of hypotheses about the relationships and composition of taxa".

To put the same things differently, the Querauthian system defines *concepts*, but does not provide means to apply these concepts to real organisms. To be sure, the concept "clade composed of an ancestral species A and all its descendants" is fully unambiguous, but whether any given specimen belongs or not in this clade is and will remain ambiguous. The traditional way of attaching nomina to taxa (through inclusion of onomatophores in taxa) has more generality and practicability than the new proposed criterion (through ancestor-descendant relationships). All organisms can be allocated to taxa. In some cases this can be done through cladistic analysis, but in many others this is impossible, either for practical reasons, e.g., in the many groups whose majority of species are still undescribed and that first need basic descriptive and monographic revision works (Berry 2002; Janovec *et al.* 2003), or for more basic reasons, e.g., in cases of reticulate evolution or of "sexual parasitism" mentioned above. In the latter cases, individual organisms cannot be referred to cladons. To attach nomina to taxa, the criterion of *inclusion of onomatophore in taxon* has therefore more generality than that of *membership in a hypothetic clade* and is to be preferred on that account.

An additional problem regarding stability is posed by the fact that "specifiers" need not only be *members* of the taxon designated by the nomen they qualify (as is the case for onomatophores), but have also to be hypothesized *ancestors* of the cladon defined by the nomen: therefore their own ancestors or sister-taxa cannot be included in the taxon. If the specifier is not well chosen, the cladon so defined may exclude some taxa that have always be included in the taxon designated by this nomen, as remarked by Lidén *et al.* (1997: 737): "If by mistake or ignorance (and we are ignorant, that is why we pursue our research) an unhappy typification is made, we might – as fuller knowledge is gained – end up with having *Aves* (birds, traditionally a 'higher' taxon) subordinate to *Struthioniformes* (ostriches, traditionally a 'lower' taxon)". Given the high lability of many of our current cladistic hypotheses in the whole animal kingdom (illustrated by the permanent publication of new phylogenetic interpretations in many zoological groups), there is no need to be a soothsayer to predict that this situation is likely to appear very frequently. Of course, it will then be possible to emend the definition of ill-defined nomina (Bryant 1996: 186), but this will be a heavy procedure and there is no advantage in terms of stability of this new system over the traditional one.

3) Supporters of the Queirauthian system claim that taxonomic ranks should be abandoned as they are subjective and arbitrary, and do not warrant comparisons between taxa of same rank in different groups. The latter is indeed true (families and orders of birds are not equivalent by any criterion to families and orders of amphibians: see Dubois 1988a, c), but the same applies to many other scientific concepts and tools, which does not impede them from being useful. The hierarchy expressed by taxonomic-nomenclatural ranks certainly does not "exist in nature". "But what does it mean to ask if hierarchies really exist in nature? A hierarchy is not a natural object. It is an organizational model, and as such, it is a human conceptual construct. This organizational model of relationship, like others (e.g., networks, causal chains, vicious cycles, etc.) is an abstraction, but that does not make it any more imaginary than

numbers and mathematical relationships" (Knox 1998: 3). "Hierarchies are organizational models that are useful for keeping track of inter-level relationships among entities" (Knox 1998: 42). The fact that nomenclatural rank has been used by some biologists to make unwarranted comparisons as if taxa at the same hierarchical rank were somehow equivalent is not a good reason for rejecting ranks: "Even if some biologists misuse ranks, I do not think that the elimination of ranks is warranted. Just because some doctors misprescribe a particular drug, should other doctors be banned from prescribing it? Certainly not" (Moore 2003: 13). Just like in the case, discussed below, of the misunderstandings surrounding the use of the term "type" in biological nomenclature, the problem at stake here is mostly related to the current low level of training and knowledge of most biologists regarding the basic concepts, methods and achievements of taxonomy and nomenclature, a problem that should seriously be addressed but which has little to do with the function of ranks in taxonomy.

As reminded above, the major function of taxonomic ranks is not to provide biological or historical information on taxa, but to provide a useful organisation of the information offered by the taxonomy, according to the basic criteria used to build the latter. Taxonomic-nomenclatural hierarchies are "just" organizational models of taxonomic information, not "theories of relationships" or other theories. Thus, in purely phenetic taxonomies, the information provided by ranks is mostly related to the "degree of divergence" between taxa, whereas in phylogenetic taxonomies, hierarchical ranks provide information on the structure of the cladogram, and in evolutionary or phyletic taxonomies it provides information both on cladistic relationships and on "subsequent modification" (Knox 1998). But, more importantly, in both cases, the existence of this hierarchy, that acts as an indented index, facilitates the research of the information relative to any taxon in the system, which would be much more difficult and long if the nomina of all taxa were given as a mere list, just like in an non-indented, non-hierarchic alphabetical index. In most actual taxonomies, ranks afforded to taxa are not given automatically by any criterion but are chosen through a combination of criteria, including robustness of cladistic hypotheses (as long as a strong uncertainty remains regarding the relationships between clades, it is better to maintain the latter in the same taxon, in order to avoid frequent taxonomic and nomenclatural back-and-forth changes), degree of divergence, and taxonomic homogeneity *within* a given higher taxon. This is certainly not satisfactory from a purely theoretical point of view, but it allows taxonomy to play its major rôle of an indexation system for information, a rôle that a non-hierarchical system cannot play: "The context in which biological classifications must function is a hierarchical one. In this sense the Linnaean system has served biology effectively for nearly 250 years. Although some workers may have misconstrued the use of the 'family' category to mean that placement of a taxon at that level conveys some particular (essential) properties, most investigators using the system in this day and age are well aware that 'family' and all other categorical ranks are used to denote a scheme of nested hierarchical relationships, not to convey information on some additional aspect of reality over and above the characters by which the taxa are diagnosed" (Schuh 2003: 76). From a practical point of view, this hierarchy allows biologists of various disciplines to make useful comparisons, provided certain precautions are taken: ranks provide "a hierarchical series of partitions of biodiversity, however imperfect that might be. An ecologist can work with the generic and family diversity of insects in a field site, and compare this with other field sites. He or she knows that genera or family distinctions are arbitrary, but this matters less in comparative studies using the same taxa. (...) Were taxonomists to retreat from ranked taxa, they would be re-invented by ecologists, naturalists and horticulturalist, and we believe that it would be better done by taxonomists" (Godfray & Knapp 2004: 561).

As long as cladograms will evolve, no final taxonomy, and therefore no stable nomenclature, will exist, and the stability in the *definition of nomina*

provided by the Querauthian system will not result in stability in the nomina *given to individual organisms*. Even if a final "tree of life" was ever to be agreed upon by all biologists, a final taxonomic and nomenclatural stability would require that *all* dichotomies in this tree be taxonomically and nomenclaturally recognized. It is true that, with this "final cladogram" and with "phylogenetic definitions" of nomina, ranks would not be necessary to have an unambiguous allocation of nomina to individual organisms, but such a system using a "cornucopia of categories" (Colless 1977) would be so cumbersome and unpalatable that it would be as uninformative as mere chaos, as up to n -1 supraspecific taxa might be required to express taxonomically the cladistic relationships of all n species that ever existed (Szalay 1977: 363; Dupuis 1979: 45). This is why the "*Phylocode*" (Note 2.1.2) expressly states that "it is not necessary that all clades be named". Dupuis (1979: 46) already wrote in this respect: "Rien n'interdit en taxinomie cladiste ou autre de ne nommer, *par convention*, que les taxa opérationnellement utiles". How can these "operationally useful taxa" be recognized remains a mystery. Lee (1996: 188) worded the same idea differently: "Phylogenetic taxonomists are free to name only those clades they feel are important and robust enough to warrant formal recognition". But then, if it depends on the "feelings" of Querauthian cladonomists, the decision regarding which clades should be named is as arbitrary as in all other taxonomic and nomenclatural systems (Colless 1977; Forey 2001: 90).

In conclusion, the system of ranks as used in the *Code* provides a convenient and workable system of indexation of taxonomic information and it would simply be destructive and "masochist" for zoologists to abandon it.

4) In the project of "*Phylocode*", "the names of clades may be established through conversion of preexisting names or introduction of new names" (Rule 9.1). This heterogeneous rule is liable to cause considerable confusion in the future. The basic philosophy of Querauthian nomenclature is incompatible with that of the *Code*, and cannot be considered as a modification of the latter system. This drastic difference should be made clear to all zoologists and other users through use of different "kinds" of nomina, e.g., including special characteristics making them easily recognisable, as is the case in some other recently proposed nomenclatural systems (Kluge 2000; Papavero *et al.* 2001).

Furthermore, the tendency to consider authorship of a nomen as a kind of "ownership" and as more important than contributing to the knowledge of organisms, once aptly called *mihilism* (Bruun 1950), has never completely disappeared from zootaxonomy, but fight for the "*Phylocode*" may give a new vigour to this attitude. A possible way of reducing its importance would be to remove the names of the authors of nomina, but not their dates, from the *nominal-complexes* that designate zoological taxa (for detailed proposals in this respect, see Dubois 2000b: 62-65). As the "*Phylocode*" is a new system, its proponents could well have decided not to cite the authors of nomina in the nominal-complexes and to adopt such an "anonymous" form (but including date) for nomina of cladons, but they did not. According to the "*Phylocode*" supporters, the new meanings given to the same nomina will be acknowledged by crediting these nomina with new authors and dates, corresponding to the first publications where these nomina were used with Querauthian definitions. Such a practice is strikingly shocking for any honest taxonomist. Papers are now being published in which it is seriously discussed, e.g., whether the nomen TETRAPODA should be credited to Gaffney (1979b) or to Gauthier *et al.* (1989) (Laurin 1998, 2002; Lee 1999; Anderson 2002; Laurin & Anderson 2004), although this nomen was first used in zoological nomenclature by Fischer (1808)! Such an attitude certainly does not qualify as "courtesy" (Corliss 1972). For those who have a certain experience of study of early taxonomic works, this very much looks like a return to "prehistorical" times of zoological nomenclature: any author who would propose a new definition or content for a taxon would then feel entitled to use for it an existing nomen, but to credit himself with authorship of the nomen (in a new, "emended", sense), and to write "*mihi*"

or "*nobis*" after the nomen, instead of the name of the real author of that nomen. However, *a nomen is not a taxon*, and it is a common (and fully valid) practice in zoological nomenclature to change either the definition or the content of a taxon, but not its nomen and the author of the latter (see below for more details). Once available under the current *Code*, a nomen keeps its author and date whatever the vicissitudes of the taxonomic concept designated by this nomen, and the rule of priority relies on the date of the original publication of the nomen, not of any "redefinition" of the nomen. The status of the nomen regarding its allocation to a taxon within the frame of any given taxonomy is established once and for all either at the creation of the nomen or through a first-reviser action whenever necessary, and cannot be changed by subsequent authors. This is just to render unto Caesar the things that are Caesar's: nomenclature is not taxonomy, and the author of a nomen is not necessarily the author of the concept of the taxon designated today by this nomen. The idea of "borrowing" nomina from earlier authors and "converting" or "redefining" them is not acceptable. In the future, clarification will be necessary: although "freedom of taxonomic thought or actions" no doubt allows any taxonomist to follow a Querauthian philosophy of taxonomy and nomenclature, editors of scientific periodicals and books should not accept to publish works in which nomina established under the *Code* would be used under the "*Phylocode*" rules. For use under the latter rules, new nomina should be created, and the fact that these nomina are defined in a Querauthian way should be made clear to any reader by special marks.

Without such a provision, "converting" ancient nomina to the new system, may be a "strategic" way to introduce progressively and "surreptitiously" Querauthian nomenclature in zoological publications: the new nomenclature *looking similar* to the traditional one, many zoologists may not realise that such a drastic change has been brought in the way nomina are defined and used, and these nomina may become progressively "entrenched in zoological literature", thus eligible for "protection" on the basis of "usage". If Querauthian nomina are not clearly distinguished from nomina ruled by the *Code*, problems of homonymy between nomina following either rules will rapidly become a considerable source of confusion and error (Cantino 2000: 87; Blackwell 2002: 152). As Stevens (2002: 22) remarked: "If different groups of people apply the same name to different groups of organisms, or different names to the same group of organisms, it will be decidedly unsetting for society and perhaps damaging for our discipline". Greuter (2004: 24) put the same idea even more bluntly: "the *PhyloCode* operation might still be judged dispassionately if it had the decency to leave the current system of nomenclature alone; if it opted for using its own, independent set of names, perhaps in English if not in Esperanto, but anyway clearly distinct from the current scientific names of plants, animals and bacteria. The disturbing fact is that the *PhyloCode* chose to parasitise the extant system of names. In consequence, its adoption would inevitably lead to the dual use of the same name for distinct and often different concepts, thus undermining the principal function of biological nomenclature: to give access to biological information with a minimum of ambiguity. If, by misfortune, phylogenetic nomenclature should prove as successful as its promoters hope, this would result in the eventual collapse of the whole edifice of organismic nomenclature – without anything available to replace it". An excellent example of the kind of "parasitism" mentioned in this quotation is provided by the recent paper by Hillis & Wilcox (2005). It is therefore urgent that the international community of taxonomists, through the editorial rules and the action of the editorial boards of scientific periodicals and books, impose on Querauthian taxonomists the *obligation* to use special nomina that cannot be mistaken for scientific nomina as recognized by the *Code*. If this is viewed by some as a restriction of freedom of taxonomic thought and action, this should be compared with the following statement by Greuter & Nicolson (1993: 927): "For *Taxon* we will now certainly not accept any more papers

contrary to these principles, but we will instead urge authors to submit proposals as appropriate".

Which system should be used to distinguish Querauthian nomina from Linnaean nomina? Such a system should be simple, not cumbersome, clearly distinct and permanent, i.e. not likely to be omitted by some authors because of its uneasy use. Kron (1997) suggested using special endings for this purpose. However, the suffix -INA that she proposed is not well chosen, as the zoological *Code* prescribes its use for subtribes, so another ending, unused in traditional zoological nomenclature, should be substituted for it if her suggestion was retained. Cantino (2000: 87), followed by Pleijel & Rouse (2003: 170), suggested to add "a letter or symbol", e.g., [P], after nomina "governed by the *PhyloCode*", and [L] after nomina following the *Code*. This raises two distinct problems. First, the "*Phylocode*" being a new system, it is normal to require the use of a special sign to point to nomina following it, but not to ask for any change in the way "normal" nomina following the *Code* should be written: the latter should continue to be written in the same way as they have for about 250 years, i.e. without any special sign! In contrast, new nomina proposed within the new system should bear a distinctive mark. Adding a symbol like [P] after the nomen does not seem a good proposal: being disconnected from the nomen and not part of it, this symbol is likely to be omitted in many cases, either deliberately or not, especially if used in non-systematic publications.

I am proposing here another solution: to use for Querauthian nomina special printing conventions, similar to the use of italics for the nomina of lower taxa in the current practice of zoologists. I suggest placing the Querauthian nomina between less than... more than… signs, that are reminiscent of a frequent way of showing cladogeneses in cladograms, and writing these nomina in small capitals, e.g., <TETRAPODA>. Such a notation is distinct from all current notations of nomina in taxonomy. If Querauthian taxonomists insist on adding the name of the author of the valid nomen (under Querauthian criteria) in the nominal-complex, this name could follow directly the nomen, e.g., <TETRAPODA Gaffney, 1979>. Finally, if it was deemed useful to indicate the name of the author of the Linnaean nomen that has been "converted" into a Querauthian nomen, the name of this initial author should be clearly distinguished from the Querauthian author, which could be obtained by placing it between brackets, e.g., <RANULA [Peters, 1860] Hillis & Wilcox, 2005>.

In this discussion, an additional problem is posed by an important difference between the botanical and zoological nomenclatural codes. Whereas in zoology nomina of different ranks are coordinate within nominal-series, this is not the case in botany. So the statement of Cantino *et al.* (1997: 314) that, according to the "*Phylocode*", "Priority would operate within a clade, rather than within a rank", is irrelevant regarding zoological nomina. In zoology, priority does not operate within ranks but within nominal-series, so that taxa often keep their nomina even when they shift from one rank to another within a nominal-series. As pointed out by Stuessy (2001), and despite quibbles trying to show that this is not the case (De Queiroz & Cantino 2001), the Querauthian system is a two-ranked nomenclatural system, with a species-series and a higher-taxa-series. In the latter series, priority works among all nomina, just like in zoology among nomina of any nominal-series. In the Querauthian system the valid nomen of a taxon will still be fixed by priority, but this is not genuine priority as it only works among nomina "correctly" introduced within this system, which has already and will be a cause of endless discussions and instability, as well exemplified by the case of the nomen TETRAPODA (Laurin & Anderson 2004). In fact, the discussions about the definition of the *taxon* to which the nomen TETRAPODA should be applied are fully legitimate and could occur within any taxonomic system. But they should have no bearing on the definition and authorship of the *nomen* TETRAPODA. In order to avoid such discussions that can be solved only by arbitrary decision of a "board" or "court", i.e. by the way of "majority" or "power" among those few people who happen to be involved in the decision, the

status of nomina cannot be based on their history, on subsequent "redefinitions" or on "prevailing" or "dominant" usage: "The current usage of the term TETRAPODA (or the vernacular term tetrapod) is sufficiently variable to leave the question of the prevailing usage of this name unresolved, especially if we consider both intended and literal usage" (Laurin *in* Laurin & Anderson 2004: 77). The status of nomina must be based *only* on information provided in the *original* publication where they were *coined*, irrespective of subsequent avatars of the use of these nomina. This is the case already in zoology for all nomina regulated by the *Code*, and my proposals (Dubois in press) are meant at extending this practice also to class-series nomina. Once its status is established through such non-ambiguous rules, allocation of a nomen to a taxon in the frame of a given taxonomy is an automatic procedure which leaves no place to personal feelings, preferences, majority, power or authoritarism. This is the kind of nomenclatural rules zoological taxonomy needs.

The four points mentioned above are not the only ones that raise theoretical problems in the "*Phylocode*" project, but may be the most important theoretical flaws of this new system, especially regarding higher nomenclature. However, beside these basic philosophical problems, proper caution should be paid to more pragmatic, but not trivial, problems.

Practical problems

The current rules were not born at once, but are the results of a process that spread over several hundred years (Melville 1995) and which involved thousands of zoologists worldwide. This *Code* is not an abstract set of "ideal" rules that would have resulted from a purely "theoretical" approach to zoological nomenclature, but the result of a concrete confrontation, through a "trial-and-error process", of such possible "ideal" rules with the daily work of taxonomists. The *Code* is of course not perfect (is any juridical text perfect?), but its bases are quite sound (Ng 1991; Hołynski 1994; Dubois & Ohler 1997; Dubois 2000a, b) and it is indeed an excellent tool to achieve its aim, namely "the universality and stability of nomenclature". It should no doubt be improved, but this system has shown considerable flexibility and it can be used with any kind of taxonomic systems, including cladistic or "phylogenetic" ones. But, of course, for this, like for any human tool, its good use requires a good knowledge and understanding of its characteristics. Since its adoption by the international community of zoologists, the *Code* has had a major impact on publications in zoological systematics. Serious, consistent and permanent enforcement of the principles of onomatophore and of priority by thousands of zoologists working on all animal groups has allowed to establish the proper status of millions of nomina proposed since 1758 for animal taxa. The result has been a progressive stabilization of nomenclature of the vast majority of animal groups. The proportion of nomina that remain unallocated is very small indeed compared to the number of nomina the status of which is now clarified and stabilized. The Principle of Priority has proved in fact to be a very powerful, honest and automatic rule for the allocation of nomina to taxa and the *Code* based on this rule has proved so efficient that one may wonder "how to improve the excellent" (Hołynski 1994). As Benton (2000: 647) ironically wrote, "if it ain't broke, don't fix it". Its replacement by completely new rules would no doubt be followed by considerable confusion and loss of information, and cause many more new problems than those it would be supposed to solve (see e.g., Dubois 2000a, b). As mentioned in the preamble of the *Code*, the latter is not meant to interfere with taxonomic theories, concepts and practices, but simply to give *automatic rules* for the *unambiguous* allocation of a *single* nomen to a taxon *within the frame of a given taxonomy*. This is all and this should remain the only function of any nomenclatural code.

A practical but crucial question must be addressed here. We are not living in an abstract world of concepts and theories, but in a material and social world which imposes a certain number of contraints upon us. One of them is the highly insufficient number of taxonomists worldwide and the financial and other limitations for their work

over the whole planet. The *taxonomic impediment* is so strong, both quantitatively and qualitatively, that it is unlikely to be significantly reduced during the coming century. Before being able to build a reasonably reliable "Tree of Life", we need first a much more complete "Catalog of Life" on our planet than is currently available (Berry 2002). This would only be "sad and disappointing" if "we had time" and if we could think that this impediment will be progressively reduced in the next centuries, but the *biodiversity crisis* that is currently developing on earth, and that certainly qualifies our times as *the century of extinctions* (Dubois 2003a), throws a different light on these questions (Wilson 1985, 2000). The work of exploration of the biodiversity of our planet that will not have been done in the coming decades will be impossible to carry out later, because many species will have disappeared. This particular situation imposes on all taxonomists worldwide the need to realize that we are not living in the same world as one century ago and that strong acceleration of the taxonomic exploration of our planet is an urgent need and a scientific priority that requires a major re-examination of our research programs (May 2004; Wheeler 2004). This was forcefully stated by a few biologists in the recent years, e.g.: "There is really no excuse for perpetuating our level of ignorance about nematodes, fungi or mites and at the same time [having] no hope, in my opinion, of completing world catalogues before many of them disappear" (Raven 2004: 730). "Funds nominally allocated to taxonomy go largely to reconstruct molecular phylogenies, while thousands of species ate threatened by imminent extinction. (...) In the face of the biodiversity crisis, the need for urgency could be no greater. (...) Our generation is the first to fully comprehend the threat of the biodiversity crisis and the last with the opportunity to explore and document the species diversity of our planet. Time is rapidly running out. (...) The grand biological challenge of our age is to create a legacy of knowledge for a planet that is soon to be biologically decimated. To meet it, 21st-century taxonomists and museums must have the right tools" (Wheeler *et al.* 2004). Clearly, implementing the "*Phylocode*" cannot be one of these tools, but rather contribute to the taxonomic impediment by diverting funds and energies from the crucial work of taxonomic inventory of the planet. Furthermore, "in this urgent climate of seeking to inventory the world's biota (...), and requesting funds from the rest of society to do so, it would be highly counterproductive to simultaneously recommend whole-scale change of names of organisms for any reason" (Stuessy 1997: 117). The consequences of such a move could be deadly for the whole field of taxonomic research: "The wholesale abandonment of the Linnaean naming system and its replacement by a new and untried method would destroy the support base for the field and imperil its survival" (Godfray & Knapp 2004: 562) (see Note 10).

By and large, the international scientific community has not yet recognized the urgency and priority of accelerating collection and inventory of biodiversity before it is too late, and few voices are loud enough to draw efficiently attention on the current dramatic situation: despite nice words (e.g., Wheeler *et al.* 2004), few concrete decisions (in terms of large-scale research programs, allocation of budgets and working positions) are yet to be seen in most countries. But at the time where thousands of species are getting extinct under our eyes, it would be completely *foolish and irresponsible* for the community of taxonomists, or a significant part of it, to decide to devote a huge amount of effort, working time and funds to *redefining* the nomina of thousands of taxa in order to have a "better" nomenclatural system rather than working on the discovery and study of the millions of other taxa that we still never collected. As argued above, the current paradigm of "phylogenetic taxonomy" is unlikely to be the "last word forever" in this respect: for example if the LGT problem mentioned above came out as a major problem, taxonomists might have to develop a new paradigm of biological classification. As for today, even if it were true that Querauthian nomenclature is "better" (a statement that many taxonomists are not ready to endorse), "converting" the information stored in the existing nomenclatural system into the new system would be a much less urgent task than studying the still

unknown organisms of our planet. In an abstract world with unlimited human and financial resources, it might be considered "theoretically better" to replace the nomenclatural system by a "better one" *at each step* of the history of comparative biological sciences, i.e. every second or third decade, but this is not true in the real world. According to Wilson (2004), there are at present about 6000 working taxonomists worldwide, i.e. "a tiny slice of the biological community as a whole", and their discipline is "one of the weakest and most underfounded". In the last decade, the many discussions, meetings and papers devoted to the conception and promotion of Querauthian taxonomy, as well as to its rebuttal, have already diverted thousands of working hours (i.e. collectively, months or years) of competent taxonomists, who would have been better inspired to use this precious time (provided by their salaries) and their energy to collect, study and describe the biodiversity of our planet that is getting extinct at an accelerated rate in our indifference. It is more than time to stop this "pure folly" (Carpenter 2003): "The sooner the *Phylo Code* is buried the better for biology" (Greuter 2004: 24). For this to happen however, it may not be enough to witness "indifference from biological systematists in general" to these proposals (Lidén *et al.* 1997) and to "boycott meetings" (Wheeler 2004: 577) but "we need community involvement" against "a small group of people with very significant resources, and thus influence": "If the community does not become actively involved in fighting the PhyloCode, they will succeed, and in so doing, demolish much of the hard work that our predecessors have built" (Nixon *et al.* 2003: 119). It would be irresponsible for the majority of taxonomists to decide to embark on the "*Phylocode*" project in the coming years, and future generations, if they happen to be more clever than ours, may pass a severe judgement on such a choice if it was to be done (May 1988, 2004; Dubois 2003a).

Conclusion

To summarize the statements made above, three major problems are posed by the project of "*Phylocode*", which suggests that this project should not be supported by zoologists, despite the "favorable fanfare in the popular media" (Carpenter 2003: 79) that has been organized around this project: 1) transferring all the taxonomic information currently stored under the nomenclatural system of the *Code* to another system would not only be very time-, energy- and resources-consuming, at the expense of other more useful taxonomic works, but would also undoubtedly cause losses of information or introduce mistakes and confusions: the current nomenclatural system is not perfect, which is impossible, but it has been working for two and a half centuries, and continuity of this system should be maintained (of course, this system should be *improved* whenever needed and possible); 2) the "*Phylocode*" is not a consistent onomatophore-based nomenclatural system, i.e. it is a system that works partly only within language; in contrast, through onomatophores, the current *Code* maintains a permanent material connection between the world of language and the real world of specimens, and is highly to be preferred on that account; and 3) the "*Phylocode*" is both a code of nomenclature and a code of taxonomy, and as such it infringes upon taxonomic freedom, which is a major weakness of this system as compared to the current *Code* (see Note 11). For all these reasons, the proposals made below regarding the nomenclature of higher taxa are not based on a Querauthian philosophy of nomenclature, but on the same theoretical premisses as those of the current *Code* for the nomina of lower taxa.

The concept of "ambiguity" and the need for stability of nomina during the evolution of taxonomy

One of the reasons given by supporters of the Querauthian nomenclature for their proposed changes is that the new system would allow *unambiguous* and *definitive* allocation of a nomen to a taxon, whatever the changes in taxonomies. The question may therefore be asked whether it is acceptable to keep the same nomen for a taxon when the definition and content of the taxon have changed, which is the case under the current *Code*. Under a very rigid conception of nomen-

clature, this would be unacceptable and any change in the taxon's definition and content would require introduction of a new nomen. Such a request may sound appealing in particular to people having an approach to zoological nomenclature through computerized data bases and data processing: a given nomen would correspond only to a fixed definition and content of a taxon, and, as soon as one or both of these characteristics change, the nomen should change. However, this is not what biologists and laymen need. Biological nomenclature is not a "perfect system" designed only for taxonomists, but has the function to be useful to all biologists and even to society as a whole, far beyond biologists. The usefulness of biological nomenclature relies on its being a simple and handy system, not only for naming the taxa (an abbreviated way of referring to them, rather than by full description of all the data we have obtained about them), but also for tracing the taxonomic and other biological information about these taxa that may have accumulated through the history of biology. What is important is to be able to find rapidly, through the nomen, or through a list of nomina and references in a *logonymy* (see Dubois 2000b), all or most of the references to a given taxon and its close relatives, and thus to obtain, for those who wish to, not only information on what is currently understood under this nomen, but also on the history of the taxon's concept. Even if today what is understood under the nomen *Rana esculenta* is quite different from what was understood under the same nomen in 1758, 1897 or 1960 (see Dubois 1998), the continuity of use of the latter nomen through centuries allows any interested person, even if completely unaware of the history of the taxonomy of this group of frogs, to find rapidly (e.g., in a logonymy or by use of a database like the *Zoological Record*) all major references to understand this history and the current meaning of the nomen *Rana esculenta*. The same would not be true if any new conception of the taxonomy of this group of frogs had resulted in abandonment of this nomen or of its followers, on account of the fact that today's concept of *Rana esculenta* is very different from that of Linnaeus (1758): although indeed different, this concept is connected to the original one through the history of the nomen, and through the permanence of the (real or virtual) onomatophore on which this nomen was based.

More attention must be given to the meaning of the term "ambiguity". This term may be defined as "expression capable of more than one meaning" (Fowler & Fowler 1929: 35), i.e. as liability to be understood in more than one way. However, there are two kinds of unambiguity, that can be designated as *absolute* and *relative* unambiguity. In any classificatory system, i.e. a system that sorts elements or units into groups or classes, a fully unambiguous system would be one that would define a class so that not only its intension but also its extension is definitive and not liable to be modified: in other words, it would be a classificatory system based on *closed definitions*. In taxonomy, the discipline whose aim is to sort all living organisms into classes called taxa, it would be a system in which any taxon would be defined as including a closed and definitive list of organisms, any addition or subtraction of any single organism resulting in the definition of a new, distinct taxon. But this is not the case in any real taxonomy, whatever the philosophy of taxonomy adopted. The definition of the taxon may be unambiguous, but the content of the taxon as recognized by taxonomists will never be so until we have reached the End of Research: "The meaning of a word has nothing whatever to do with the practical problem of identifying the things which may happen to fit the definition" (Ghiselin 1966: 127).

Let us take an example outside taxonomy to make this clearer. Let's consider a human society in which the law would state that any child must bear the patronym of his/her biological father. This is a fully unambiguous definition of classes, according to which all humans bearing the same patronym share a common male ancestor. However, when one comes to reality, it is inescapable that in any such society some children bear a patronym although their biological father was not the "official" one. Although methods would exist (through genetic analysis) that would

potentially allow clarification of this point, they are unlikely ever to be applied: a man who has travelled much around the world may potentially have "left" children in many countries, so that it will be virtually impossible ever to establish the complete list of his descendants. So, a fully unambiguous definition of a class may not correspond to a fully unambiguous list of members of the class: the practical tool used in such a case may be that all children legally recognized by a man and bearing his patronym may be considered his descendants, but this is only an approximation of the "real" class (which no doubt exists in the real world but which may remain unknown forever).

All real taxonomies are approximations of the same kind, whatever the philosophy of classification adopted: they all rely on relative unambiguity, i.e. on *open definitions*. In any taxonomy, taxa are defined by characters or properties that may be fully unambiguous, but the characters or properties of some organisms may be unknown or wrong, so that their taxonomic allocation may change. In this respect, although the Querauthian proposal is to use unambiguous "phylogenetic definitions" of taxa, the taxonomic allocation of organisms to taxa remains in part ambiguous in this system, quite as much as in the "traditional" approach to taxonomy.

To play its rôle of information storage and retrieval system usefully, nomenclature must rely both on *strict and stable Rules* for the allocation of nomina to taxa and for the establishment of the valid nomen of each taxon, but also on a rather high *flexibility* in the relationship between taxon and nomen: the taxon must be allowed to change, both in its definition and content, without change in the nomen, which remains characterized by its permanent connection with the onomatophore through the taxomen. Under these rules, the question of the allocation of a nomen to a taxon cannot be considered "in general" or in an abstract way, but must be addressed *within the context of a given taxonomy*. If a nomen was unambiguously and definitively linked to a biological definition or a closed list of included individuals or taxa, every time the definition of the taxon changes, even slightly (a very frequent process in taxonomy), or every time a taxon, or even a single specimen, is added to the taxon or removed from it, this would require the establishment of a new nomen to account for this "new taxon": such a system would result in a permanent and endless increase in the number of nomina at the scale of all zoology, each nomen appearing in only a single or a few publications, without providing any system allowing us to trace the taxonomic history of a taxon through continuity of its nomen. Nomina could therefore not be used to establish historical connections between pieces of information added by subsequent authors, and zoological nomenclature would become a kind of game, a useless discipline, unable to play a rôle of information storage and retrieval. Actually, it is just such a system which is currently at work as concerns the nomenclature of higher taxa. The untold "rule" that prevails among many taxonomists is that, as nomina of higher taxa are not governed by the *Code*, zoologists are free to use any nomen for such taxa. They then often tend to consider that such nomina are unambiguously and definitively associated with the original content and/or definition of the taxa to which they were first applied, so that every time the concepts on taxonomy or phylogeny change, nomina have to change also. In the long run, such a tendency may not only unnecessary and dramatically increase the number of available higher nomina, but also, by suppressing all continuity, through the on-going use of the same nomina despite progressive changes in taxonomy, between subsequent taxonomies of a group, lead to a progressive chaos, where storage and retrieval of taxonomic information would become increasingly difficult.

Class-series nomina should therefore follow rules similar to those of the other nomina, that are regulated by the *Code*: after establishment of a higher taxon, the latter should be liable to be modified in its definition and content but not in its nomen, as long as the onomatophore remains included in the taxon. This is indeed what occurred very often in the past (see below for details and examples), and this should continue in the future: change (through addition of taxa) in the

content of a higher taxon should not require modification of its nomen, that remains in all cases attached to its onomatophore. The rules devised for higher nomenclature should allow *unambiguous allocation* of nomina to taxa *within the frame of a given taxonomy*, but not "in general", i.e. independently from the taxonomy adopted: in other words, nomenclature should be at the service of taxonomy, not the reverse.

NEW CONCEPTUAL AND TERMINOLOGICAL CLARIFICATIONS

A PROPOSED FAREWELL TO THE TERM "TYPE" IN BIOLOGICAL TAXONOMY AND NOMENCLATURE

Time may have come for seriously considering a progressive, but ultimately complete, abandonment of the term "type" traditionally used in biological taxonomy and nomenclature. As a matter of fact, this term is both ambiguous (as it designates both a taxonomic and a nomenclatural tool) and misleading (as it suggests that biological taxonomy is still typological). It is here argued that this term should be abandoned, and replaced by unambiguous and well defined terms.

Hypodigm and onomatophore

The term "type" is highly ambiguous as it has been used with various meanings in many fields of human activities. Even within evolutionary biology, it may designate either an evolutionary, a taxonomic or a nomenclatural concept. As an evolutionary concept, a type or "ground-plan" (see e.g., Danser 1950) "is a class whose members share a certain set of defining traits", and gaps are thought to exist between types (Levinton 1988: 17). In most of these acceptations, the term "type" designates a *concept*, i.e. an abstraction (see e.g., Bloch 1956: 68-69). In contrast, in biological systematics, "types" have long been used as reference *specimens* both for taxonomy and nomenclature. These were the specimens available to the author(s) who proposed a new specific or subpecific taxon and its nomen. In the early days of taxonomy, these specimens were believed to be "typical" of the taxon, i.e. to show all the relevant characters of the latter: any other specimen showing "the same" characters could then be considered as a "double" or a "duplicate" of the type(s), belonging in the same taxon, whereas any specimen showing "different" characters would have to be allocated to a distinct taxon. In such a typological approach to taxonomy, the types were mostly understood as specimens "bearing the characters" that allow recognition of the taxa in which they belong. However, with the development of population thinking and of the "new systematics", the idea that species were composed of identical (or almost so) specimens was replaced by a concept of species showing (sometimes important) internal polymorphism. This was the end of the typological or essentialistic concept of species and its replacement by an evolutionary or populational species concept (Mayr *et al.* 1953; Mayr 1969, 1981, 1982, 1995, 1997; Mayr & Ashlock 1991).

However, in parallel with the progressive consciousness that no specimen can be considered as "typical" or fully representative of a species, taxonomists also discovered that, beside their function as semaphoronts, "types" had a second and independent function, that of being a material, objective reference for the allocation of nomina to taxa. The idea was already clearly expressed at the end of the 19th century: "It is still protested by the surviving idealists that no single specimen can give an adequate idea of the species, and nobody claims that it can, but the desirability of a single definite nexus between nature and science is rapidly becoming patent to all" (Cook 1898a: 187). This led to the persistence of "types" in biological taxonomy, although mostly as a nomenclatural, instead of taxonomic, tool, i.e. as a "name-bearer" (Blackwelder 1967: 166). Unfortunately, the perpetuation of the use of the term "type" to designate this tool resulted in ambiguities. As well outlined, among others, by Rickett (1959: 24), "The word type is unfortunate". Its continued use is confusing for some taxonomists (e.g., Oldroyd 1966), but mostly for non-taxonomists, who sometimes consider that, because they are still using "types", taxonomists are still relying on a typological concept of species

(e.g., Ruffié 1982: 405-408). This is still a widespread belief, as illustrated, among many others, by the following statement under the pen of an editorialist of the journal *Science*: "Under the traditional system, a taxonomist (...) selects the most representative species to be the 'type' for each genus, then the most representative genus to be the type of the family and so forth. (...) as new specimens with similar characteristics are found, they are deemed part of a known species, or even a new genus based on how closely they resemble the type specimen" (Pennisi 2001: 2304). Disinformation or simple ignorance are widespread in this domain, as also shown by the formula "Linnaean type concept" used by some (Hansen 1961: 11), although Linnaeus himself did not refer to types.

To avoid definitively such misunderstanding, and to fully distinguish between the double (nomenclatural and taxonomic) function of "types", these two functions should be designated by different terms. A first clarification in this respect was due to Dennler (1939), followed by Simpson (1940, 1961), who both proposed the replacement of the term *type*, respectively by the terms *testimonium* or more briefly *test* (which is ambiguous), and *onomatophore* (which is clearly more appropriate). That this change was really needed is stressed by the fact that, a few years later and independently, Schopf (1960) made a similar proposal, introducing the new term *nomenifer*, which is a strict synonym of test and onomatophore. In contrast, Hennig (1950: 9; 1966: 6) introduced the term *semaphoront*, which means "bearing the signs, the characters" to designate a second and independent function of specimens, including "type specimens". All these terminological proposals show that the idea that "types" are "typical" and have the function to give the characters of a taxon is wrong and misleading, and should be extirpated from taxonomy and nomenclature.

The continued use, despite these judicious proposals, of the term "type" in the *Code*, probably played a non-negligible rôle in the fact that many authors, both within and also outside taxonomy (see e.g., Dubois 2000a), have had difficulties understanding the difference between the onymological tool of onomatophore and a pre-evolutionary, typological thinking in taxonomy. The time should now come for progressively considering going further in the direction indicated by the authors cited above. Of course, the specimens which are traditionally called "types" or "type specimens" play a central rôle in taxonomy and nomenclature, not because they would be "typical" in the essentialistic sense of the term, but because they constitute an objective, material reference to what the author who created a taxon and a nomen had in hand. This double rôle, both taxonomic and nomenclatural, of these specimens, should be accounted for by the use of two distinct terms, especially when dealing with microtaxonomy (Mayr & Ashlock 1991), i.e. taxonomy at the level of species and other lower taxa (subspecies, superspecies, etc.). The term *hypodigm* was introduced by Simpson (1940, 1961) to designate the set of specimens used by an author to establish and describe a new species-series *taxon*: this refers to a concept useful in *taxonomy*. On the other hand, the term *onomatophore* has only a *nomenclatural* usefulness, as it designates only the specimen(s) on which the *nomen* given to the taxon objectively relies. Hypodigm and onomatophore are not equivalent: in many cases, especially in recent taxonomy, the onomatophore consists only of a small part of the hypodigm, often being restricted to one of its specimens (the holotype or lectotype), or even being a specimen that was not part of the original hypodigm (the neotype). Rather than adopting the well defined term of onomatophore, which clearly refers to a technical tool specific to biological nomenclature, the *Code* employs the unpalatable formula "name-bearing type", which furthermore maintains the use of the term "type" in taxonomy and nomenclature. I suggest that in the future not only this formula be abandoned and replaced by onomatophore, but also that the technical terms designating the various kinds of onomatophores be changed.

Two major kinds of onomatophores

As will be exemplified below in this paper, it would be useful to dispose of a simple, unique

term to designate any of the various kinds of onomatophores that exist in biological nomenclature. This would avoid using unpalatable plurinominal designations such as "onomatophoric specimen", "onomatophoric genus" or "onomatophoric locality". Combination of various terms with the root "onomatophore" or "nomenifer" would also produce long, unpalatable terms which would have little chance to be adopted by the community of taxonomists. For the terms designating different kinds of onomatophores, I therefore propose to use shorter and simpler roots. To avoid the formation of heterogeneous terms, I propose to use two distinct roots to point to the function of onomatophore, i.e. to indicate that these are technical tools having the function of bearing a nomen. In some cases I simply propose to use the root *-phore* (from the Greek *-phoros*,"bearing", from the verb *phero*, "I bear"), but associated, just like in the term *semaphoront*, with the root *on ontos*, "being, individual", derived from the verb *eimi*, "I am": this means that such onomatophores are real organisms that exist in the world independently of science. In other cases I propose to use another, short, root, that of *nucleus*, "nucleus, core, stone" (from the Latin *nux*, "nut"). This term is used in various domains (astronomy, cell biology, sociology, history, etc.) to designate a "central part or thing round which others are collected, (…) beginning meant to receive additions" (Fowler & Fowler 1929: 776), which quite well corresponds to the status of onomatophores. In biology, a cell usually cannot live without its nucleus, and in biological nomenclature a nomen has no status (allocation to a taxon) without its onomatophore. This does not mean that the onomatophore is "typical", but that it is "vital", indispensable and irreplaceable.

I suggest to use these two different roots because, as discussed in more detail elsewhere (Dubois 2000b), two major kinds of onomatophores must first be distinguished: 1) some, called "collection types" by Farber (1976) and Moore (1998), are *specimens* (holotype, syntypes, lectotype or neotype), that are the objective reference by which the status of nomina of taxa of rank species or below is established; and 2) others, called "classification types" by Farber (1976) and Moore (1998), are *taxomina* (type species, type genus), that are the reference for the status of nomina of higher ranks (genus, family, etc.). Because of the hierarchical structure of the system, all taxomina refer ultimately to type-specimens, so that in this system all nomina are connected to the real world of living populations through the latter (Fig. 1). So, in order to distinguish these two major kinds of onomatophores, I propose: 1) for all type specimens, the term *onymophoront* (from the Greek terms *onymos*, "name", *-phoros*,"bearing", and *on ontos*, "being, individual"); and 2) for all onomatophores which are taxomina, the term *nucleomen* (plural *nucleomina*; from the Latin terms *nucleus* and *nomen*). In both these categories of onomatophores, subcategories can be distinguished.

Onymophoronts

For the four kinds of type specimens (onymophoronts) recognized by the *Code*, I propose the following terms: *holophoront* (instead of *holotype*; from the Greek *holos*, "whole, entire"), *symphoront* (instead of *syntype*; from the Greek *sun*, "with, together"), *lectophoront* (instead of *lectotype*; from the Greek *lektos*, "chosen", from the verb *lego*, "I choose") and *neophoront* (instead of *neotype*; from the Greek *neos*, "new"). After designation of a lectophoront among symphoronts, the remaining symphoronts become *exonymophoronts* (for *paralectotypes*) and lose their status of onomatophores (see e.g., Dubois & Ohler 1997: 309, fig. 2). I do not propose replacement terms for the terms *paratype*, *allotype*, *cotype* and *topotype*, which clearly do not designate onomatophores and should, in my opinion, not be regulated by any code of *nomenclature*, as they are in fact taxonomic, not nomenclatural, tools: such specimens are in fact just particular members of the hypodigm (for the first three terms) or "simple specimens" collected from the same locality as the hypodigm or part of the latter (for the last term). The suggestion of Fricke (1985) to use modified spellings in *-oid* (e.g., *paratypoid*, *allotypoid*) might be followed for those who find it useful to have special terms for these special members of the hypodigm: this proposal has the merit to point that such specimens

are not "real types", but the weakness to still use the root *typ-* in their designation. I also refrain from proposing a replacement term for the terms *hapantotype* of the zoological *Code*, or *epitype* of the botanical code, as I consider the concepts designated by these terms as belonging to typological thinking: onomatophores are not *semaphoronts* and do not have the function to give the *characters* of the taxon. In my opinion, all nomina, including those of protists, should rely on holophoronts, symphoronts, lectophoronts or neophoronts, and in case the latter do not allow unambiguous allocation of a nomen to a taxon, they should be *replaced* by adequate neophoronts, a process for which the *Code* should provide clear rules (this matter has not yet been properly addressed regarding the zoological *Code*).

An additional proposal meant to eradicate the use of the term "type" in biological taxonomy and nomenclature is the term *onymotope* (from the Greek *onymos*, "name", and *topos*, "place"), to designate the place of collection of the onymophoront(s), called *type-locality* in the *Code*, or sometimes also *terra typica* or *locus typicus* by some zoologists. Any specimen collected from the same locality, whether or not an original member of the hypodigm and/or of the onomatophore, can be designated as *onymotopic* (for *topotypic* in the *Code*). A "topotype" may thus be designated as an "onymotopic specimen".

Nucleomina

For taxa of nominal-series above the species-series, onomatophores are not specimens (onymophoronts) but taxomina (nucleomina). Nucleomina are of two kinds only: 1) a "*type species*" is a speciomen (i.e. a taxomen of rank species or below; see Note 12) that is the onomatophore of a nomen of the genus-series (i.e. of rank genus or subgenus); and 2) and a "*type genus*" is a generomen (i.e. a taxomen of rank genus) that is the onomatophore of a nomen of a rank above genus (i.e. of the family- or of the class-series). To avoid the formation of composite Latin-Greek terms, I propose the replacement terms *nucleospecies* for "type species" and *nucleogenus* for "type genus".

In the species-series of nomina, a taxomen can be created with several joint onomatophores, the symphoronts, and subsequent selection among them of a lectophoront restricts the onomatophore function to the latter. Similarly, in the genus-series of nomina, a taxomen can be created with several joint onomatophores, among which a subsequent choice allows to restrict the onomatophore function to one of them. I propose to call the initial joint onomatophores in such cases *prenucleomina* (from the Latin *prae*, in the sense of "before"), and more precisely *prenucleospecies* for the *originally included nominal species* of a new nominal genus or subgenus established with several included species, before subsequent designation among them of a single nucleospecies. In the family-series, the situation is simpler: although one could formally consider that all *originally included nominal genera* of a new family-series taxomen established with several included genera are its *prenucleogenera*, this almost never occurs concretely, as to be available a family-series nomen must be based on an available genus-series nomen, which is therefore its nucleogenus by "implicit etymological designation" (Dubois 1984b) from the very moment of its creation; however, as explained and discussed in more detail in Dubois (in press), the category of prenuclogenera is useful in a special situation, namely to establish the onomatophore of an unavailable family-series nomen not being based on an available genus-series nomen.

For reasons that will be explained in detail in Dubois (in press), in the class-series it is here proposed that the onomatophore of a classomen may be either a single nucleogenus or a permanent, indissoluble set of two or several nucleogenera, among which no subsequent restriction or designation can be made. In the latter case these can be termed *conucleogenera* (from the Latin *cum*, "with"), which means "nucleogenera together".

Additional proposals

Also in order to avoid using the root "type", I propose the new term *monophory* (from the Greek *monos*, "single, unique", and *-phoros*, "bearing") to designate the concept called *monotypy* in the *Code*.

Whenever a new taxomen was established based on more than one onomatophore (symphoronts for a new species, prenucleospecies for a new genus, conucleogenera for a new class, or even rarely prenucleogenera for a new family), I propose to designate this situation under the general term of *symphory* (from the Greek *sun*, "with, together"). In most cases, symphory is a transitional situation, as a lectotype often is designated among symphoronts or a nucleospecies among prenucleospecies, but in the class-series, for reasons explained in Dubois (in press), conucleogenera remain the onomatophore together. Finally, for the situation where a new taxomen was established without designated onomatophore, I propose the term *aphory* (from the Greek *a-* or *an-*, "without"): a taxon created with original aphory may later be provided an onomatophore in another publication ("subsequent type designation" under the terminology of the *Code*).

Finally, I propose to replace the term *nominotypical*, used in the *Code* to designate taxa hierarchically connected by the rule of coordination and having the same onomatophore, by the terms of *coordinate* (general) and of *subordinate* (hierarchically lower) and *superordinate* (hierarchically higher).

The terminological proposals made here will be summarised in Dubois (in press). The complete eradication of the term "type" in biological taxonomy and nomenclature would clarify the matters in two respects: 1) it would completely extirpate reference to essentialistic or typological terminology from biological taxonomy; and 2) it would suppress the ambiguity between the taxonomic function of the hypodigm and the onymological function of onomatophores. To be fully efficient, these terminological clarifications should first be adopted by individual taxonomists, and later incorporated into the existing codes of nomenclature.

Synotaxy and topotaxy

Synotaxy; morphotaxy; protaxon and apotaxon; ergotaxon

In order to facilitate the discussion that will follow (Dubois in press), I am also led to propose a few terminological novelties concerning the designation of various taxonomic and nomenclatural situations usually known under the general term of "synonymy". These proposals are complementary to those I already gave elsewhere (Dubois 2000b). For the time being, the term *synonymy* designates either a taxonomic or a nomenclatural situation, or both together. In order to distinguish these two situations, I propose the new term *synotaxy* (from the Greek terms *syn*, "with, together", and *taxis*, "action to put in order") to designate all cases of *different taxa having a partially or totally identical or common content*, and to restrict the use of the term *synonymy* (from the Greek terms *syn*, "with, together", and *onymos*, "name") to the designation of all cases of *different nomina applying to the same taxon*. These two kinds of relations between taxa and nomina are not necessarily superposed: two taxa can have the same content and the same nomen, the same content and different nomina, the same nomen and different contents, or different contents and nomina.

The basic distinction between the onymological tools of *nomen* and *taxomen* ("nominal taxon" in the *Code*) on one hand, and the taxonomic concept of *taxon* on the other, must be kept in mind (see Dubois 2000b). Concerning nomina, several distinctions can also be made (Dubois 2000b). Any nomen can exist under several *morphonyms*, i.e. several *spellings* or *ranks*. For each nomen, it is possible to distinguish the *protonym* (original morphonym of the nomen) from its *aponyms* (any subsequent, modified, morphonyms), which together constitute a group of *paronyms*.

Similarly, during the history of the taxonomy of a group, definitions and contents of taxa change frequently, so that any taxon can exist under several *morphotaxa* (from the Greek *morphos*, "form").

It may be useful and interesting to know which taxon was initially in the mind of the author of a new taxomen. I here propose to distinguish the *protaxon* (from the Greek *pro*, in the sense of "first", "primitive", "original"; see Note 13), i.e. the taxon with its original definition and content, from its *apotaxa* (from the Greek *apo*, "coming

from"), i.e. any subsequent, modified, definition and/or content of the taxon. The original taxonomy may be known as a *protaxonomy*, and the derived taxonomies as *apotaxonomies*. All the different morphotaxa considered by subsequent authors under the same nomen (i.e. referring to the same taxomen), i.e. the protaxon and all its apotaxa, constitute a "family" of related *parotaxa* (from the Greek *para*, "close to"; see Note 14) referring to the same taxomen. The different morphotaxa usually differ both by their intension and extension, except if some mistake was made by an author when defining a taxon or when listing its members or its non-members.

Among the apotaxa bearing a given nomen that may have been recognized in various taxonomies, some may today no longer be considered valid whereas others are currently recognized by some or all taxonomists. I propose to call *ergotaxon* (from the Greek *ergon*, "work, action") any taxon recognized as valid by a given author or in a given taxonomy. An ergotaxon may be either a protaxon or an apotaxon. Any taxonomy used by a given author, whether well stabilized or highly provisional ("working taxonomy" as defined by Dubois 1999b) can be qualified as an *ergotaxonomy*.

As will be shown below, apotaxa always share with their protaxa a minimal common extension, i.e. the original onomatophore, as exclusion of part or all the onomatophore results in fact in the creation of a new taxon. Therefore, all apotaxa are by definition obtained by addition into the taxon of new included specimens or taxa to the onomatophore. In this respect there are two major kinds of apotaxa. Some are obtained by mere addition of new specimens or taxa of low ranks, i.e. they do not result in a modification of the intension of the taxon. In contrast, some are obtained by addition of new immediately subordinate higher taxa (ametoendotaxa; see below) and in a modified definition or diagnosis of the taxon. For those of the first kind I propose the term *menapotaxon* (from the Greek *meno*, "I remain"), and for the second kind the term *auxapotaxon* (from the Greek *auxo*, "I increase"). Nomina used for such taxa can be either protonyms or aponyms, and may be designated as *menonyms* and *auxonyms*.

Topotaxy

Several kinds of synonymies and of synotaxies can be distinguished. Let us first consider the situation of *synonyms*, as here redefined. Two taxa can bear the same nomen for two different kinds of reasons (Dubois 2000b). *Nomenclatural* or *objective* synonyms, more shortly *isonyms* (Dubois 2000b), are nomina that are based on the same onomatophore, whereas *taxonomic* or *subjective* synonyms, or *doxisonyms* (Dubois 2000b), have different onomatophores. Isonyms will always remain so, whereas the relation between doxisonyms is liable to change as taxonomy evolves: two nomina that were once *considered subjective synonyms* may later be *considered to denote distinct taxa*, which can be more shortly stated by saying that they are then no longer considered synonyms but have become *xenonyms* (from the Greek *xenos*, "foreigner"). Doxisonyms and xenonyms are the two different kinds of *anisonyms* (from the Greek *a-* or *an-*, "without", and *isos*, "equal"), i.e. nomina based on different onomatophores.

Similarly, there exist several relationships between taxa regarding their contents (Fig. 3): I propose to call these relationships *topotaxy* (from the Greek *topos*, "place"). One can call *xenotaxa* two taxa whose contents are completely distinct, without any overlapping (i.e. in mathematical terms, two sets whose intersection is empty), and *synotaxa* two taxa whose contents are partially overlapping or are identical (i.e. in mathematical terms, two sets whose intersection is not empty). An example of *xenotaxy* is that of two taxa X1 and X2 composed respectively of the individuals (or taxa) A, B, C, D and E, and F, G, H, I and J. As for *synotaxy*, three major situations can be distinguished: 1) *isotaxy* (from the Greek *isos*, "equal"), i.e. absolute identity of contents of both *isotaxa*: e.g., both I1 and I2 and composed of A, B, C, D and E; 2) all other cases belong in the general category of *anisotaxy* (from the Greek *a-* or *an-*, "without", and *isos*, "equal"), with two subcategories: a) *peritaxy* (from the Greek *peri*, "around"), i.e. complete inclusion of one taxon (*endotaxon*, from the Greek *endon*, "inside of") within the other taxon (*angiotaxon*, from the Greek *aggeion*, "hull, capsule"): e.g., the angiotaxon A1 is composed of A, B, C, D and E,

and the endotaxon E1 is composed of B, C and D; and b) *gephyrotaxy* (from the Greek *gephyra*, "bridge"), i.e. partial overlap of the contents of both *gephyrotaxa*: e.g., the gephyrotaxon G1 is composed of A, B, C, D and E, and the gephyrotaxon G2 of C, D, E, F and G. These different kinds of relationships between taxa are diagrammatically shown in Figure 3. The following substantives and adjectives can be used to designate the relations between two taxa: *isotaxy/xic*, *anisotaxy/xic*, *peritaxy/xic*, *angiotaxy/xic*, *endotaxy/xic* and *gephyrotaxy/xic*. Thus, the convenient formula "two isotaxic nomina" is a shortened way of stating that these two nomina refer to exactly identical taxa or isotaxa (see Note 15). All these situations can be considered among the protaxa of any given protaxonomy as defined above. This can be designated as the relationships of *protopotaxy*: it is thus possible to distinguish *xenoprotaxa* from *synoprotaxa*, and among the latter category to distinguish *isoprotaxa*, *periprotaxa*, *angioprotaxa*, *endoprotaxa* and *gephyroprotaxa*. Similarly, relationships of *ergotopotaxy* exist between the ergotaxa of any ergotaxonomy: it is possible to distinguish *xenoergotaxa* from *synoergotaxa*, and among the latter category to distinguish *isoergotaxa*, *periergotaxa*, *angioergotaxa*, *endoergotaxa* and *gephyroergotaxa*.

In any hierarchical taxonomic system, all taxa occupy a given hierarchical rank. Within each of the three nominal-series recognized by the *Code*, a hierarchy of taxa includes: 1) *peritaxic* (coordinate) taxa, i.e. *endotaxic* (subordinate) and *angiotaxic* (superordinate) taxa, that are hierarchically related, and that may share the same nomen and onomatophore ("nominotypical" taxa) or not ("non-nominotypical" taxa); and 2) *xenotaxic* taxa, that always exclude mutually the onomatophore of each other.

Whenever a taxonomist proposes a new taxon, the latter occupies a given position in the hierarchy, and several different situations can be distinguished. Relative to any other taxon of this hierarchy, the new protaxon may be xenoprotaxic or periprotaxic. In the latter case, relative to the immediately superordinate taxon, the new taxon may either be alone or share its rank with other taxa. For more clarity in the following discussion, the various situations at stake may be designated by different terms. In some rare cases, a new taxon when first introduced is considered to be completely isolated, not closely related to any already recognized taxon, and it is the only taxon of a given rank subordinate to the taxon of

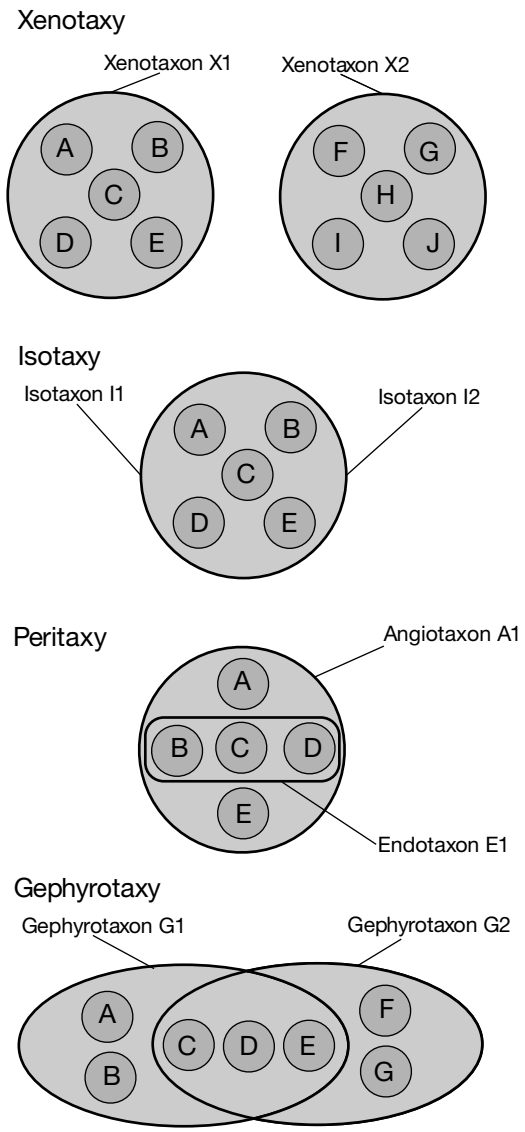

FIG. 3. — Diagrammatic illustration of the different kinds of relationships between taxa (A1, E1, G1, G2, I1, I2, X1, X2) concerning their contents or extensions. **A** to **J** are either *individuals* (specimens) in the case of species-series taxa, or *taxa* in the case of taxa of all other nominal-series. See text for explanation.

immediate higher rank: it then qualifies as an *eremoprotaxon* (from the Greek *eremos*, "desert, isolated"). In all other cases, the new taxon shares its rank with other xenotaxa immediately subordinate to the same higher taxon: it then qualifies as their *ametoxenoprotaxon* (from the Greek *ametos*, "immediate"). Two different situations may then be distinguished. In some cases, the new taxon is simply *added*, beside other already recognized taxa of the same rank, as a new subdivision of a taxon of immediately higher rank; this often requires some changes in the intensions and extensions of these already existing taxa, but not their creations. These taxa, as well as the newly created taxon, are ametoxenoprotaxa that were *not created together*. They can be designated as *adelphoprotaxa* (from the Greek *adelphos*, "brother"), and their nomina as *adelphonyms*. In the second situation, a previously existing taxon is divided by an author into two or more new taxa: the latter are *created together*, in the process of splitting and redefining an earlier taxon, and together they cover the whole extension of the latter as redefined. They can be called *didymoprotaxa* (from the Greek *didumos*, "double, twin"), and their nomina as *didymonyms*.

In contrast to ametoxenotaxa, *teletaxa* (singular *teletaxon*; from the Greek *tele*, "far from") are taxa that are more remotely related, i.e. taxa, whether of the same rank or not, that are not members of the same immediately superordinate higher taxon.

To put the same ideas more generally, in any hierarchical taxonomy, peritaxy exists at different levels: a class includes several orders, but also several families, several genera, etc. Of particular interest are the taxa of the *immediately higher* (superordinate) and *immediately lower* (subordinate) ranks. This kind of relationship can be designated as *ametoperitaxy* (from the Greek *ametos*, "immediate"): it includes *ametoangiotaxy* and *ametoendotaxy*. These categories can be recognized both in any protaxonomy (*ametoperiprotaxy*, *ametoangioprotaxy*, *ametoendoprotaxy*) and in any ergotaxonomy (*ametoperiergotaxy*, *ametoangioergotaxy*, *ametoendoergotaxy*). Furthermore, in any taxonomic hierarchy, the relationship between taxa of the same rank that are immediately subordinate to the same ametoangiotaxon can be designated as *ametoxenotaxy*, with two particular cases, *ametoxenoprotaxy* and *ametoxenoergotaxy*. In contrast, relationship between teletaxa can be designated as *teletaxy* (*teleprotaxy* and *telergotaxy*). All these terms that designate categories of *protopotaxy* and *ergotopotaxy* will be useful to designate in a concise and precise way the kinds of relationships that exist between taxa in any protaxonomy or ergotaxonomy.

A final clarification is needed concerning the relationship between endotaxy and isotaxy. Because of the hierarchical structure of taxonomies, two taxa may be isotaxic at one taxonomic level but endotaxic at another level. A taxon of a given hierarchical rank having several levels of hierarchically subordinate taxa may be isotaxic with another taxon of same rank including representatives (members) of *all* its ametoendotaxa (taxa of the immediately subordinate rank) but angiotaxic relative to this taxon at lower levels if the latter includes only *some* of its lower subordinate taxa, at some ranks of the hierarchy only. In the family-series, stating that a family is endotaxic to another means that it contains only a part of the tribes or genera of the latter (and no genus excluded from it). In the class-series, more possibilities are offered as a class is endotaxic to another if it contains only part of its families, tribes or genera, even if it is isotaxic to it at order level. Therefore it is possible to distinguish between *panisotaxic* taxa (from the Greek *pas*, "all, each"), that are isotaxic at all ranks, and *merisotaxic* taxa (from the Greek *meros*, "part") that are isotaxic only at the higher levels but peritaxic at lower hierarchical ranks.

The relations of xenotaxy and synotaxy here applied to nomina of the family- and class-series of nomina are also liable to be applied to nomina of the genus-, species- and variety-series, but this question won't be explored further in this paper. On the other hand, all the terms just introduced will be useful to describe the taxonomic and nomenclatural situations that are met with when one examines the evolution of higher zoological classifications, as will be exemplified for the amphibians (Dubois in press, in prep.).

Categories of usage

Some additional comments on the *concept of usage* in zoological nomenclature are also necessary. In recent decades, a number of zoologists have supported the idea that the basic principle of zoological nomenclature, that of priority, should be moderated by a "subsidiary principle", that of "usage". As well analyzed by Hołynski (1994), the transition from a simple system with a single structuring principle to a new system with two such principles is a source of terrible confusion and instability, and is certainly not to be viewed as progress (see also Dubois 2000a): "The co-existence in one code of diametrically opposed articles cannot possibly contribute to stability and universality of nomenclature" (Melville 1958: 1249). This is by far not a new question, as it was very clearly analysed more than one century ago by the most acute theoreticians of biological nomenclature, whose writings are still worth reading, such as those of O. F. Cook, who wrote for example: "Usage has never produced any general or permanent uniformity in manners, government, literature or science, and no reasons are apparent for supposing that it ever will. There could scarcely be a uniform logical system founded upon usage. The idea involves a contradiction in terms, and a plea for usage is, in effect, a plea for anarchy" (Cook 1898a: 189). Sixty years later, Tottenham (1958: 1080-1081) also rightly remarked: "While fully in agreement with the Principle of Conservation in itself, I deeply regret its introduction into the Code as a Rule. It is often directly contradictory to the Principle of Priority and therefore its introduction means a fundamental change of Rule. If the Rules themselves are subject to such fundamental change and are themselves unstable, how can the nomenclature which is based upon them be stable? Furthermore, its introduction as a Rule would mean that a large amount of zoological nomenclature would be based upon the errors of present day zoologists; this is wholly undesirable; accuracy is a hall-mark of good scientific work. It is sad to think that this system of basing much nomenclature on errors should be introduced into the Code this year when we are celebrating the bicentenary of Linnaeus who laid the foundations of our zoological nomenclature. It is still more regrettable that the Conservation Principle should be applied to names 'in general current use', for we today are the very zoologists who have the least excuse for errors in nomenclature having had the International Rules in force for a longer period than any of our predecessors. (...) Is the New Code designed to amplify the existing Rules to cover as many cases as possible, to make the rules clearer, to facilitate their application and so hasten law and order in nomenclature? Or is it designed to please as many conflicting personal preferences as possible and thus prolong differences of usage?".

Besides, basic problems exist regarding the real meaning of the term *usage*. Several recent authors have insisted that "usage" should be "protected" in many cases, even when it is not correct according to the rules of the *Code*, because nomina are not meant for the needs of taxonomists only but are also employed by many "non-systematists" (such as scientists working in various fields of biology, medicine, agronomy, etc., or even non-scientists such as members of administrations, governments, customs, conservation organizations, etc.). This is no doubt true, and agreed upon by most taxonomists. However, in a number of cases, the same authors argued for the conservation of an incorrect usage although the latter had only existed among systematists, i.e. taxonomists or phylogeneticists (see Note 16), which is either incoherent or dishonest. As a matter of fact, there is a real intellectual dishonesty in both stating that nomenclatural stability is necessary for non-systematists, users of taxonomies, but then to provide evidence for a "need of protecting usage" based on purely taxonomic or phylogenetic publications. In such a context, the formula "widely used" should be understood as meaning "widely used outside systematic literature", as was already pointed out: "When a name has been used only or almost only in taxonomic publications, the only biologists that are really acquainted with this name are taxonomists. If discovery of a senior 'overlooked' synonym occurs, all taxonomists can understand the reason why the junior name must be replaced by the

senior one, and no real disturbance of their work follows this change" (Dubois 1997a: 319). It might be added that, if some systematists are unable to understand this, then they should undertake some refresher courses or decide to change to another activity.

Another important comment regarding "usage" is that, if "universal usage" (by non-systematists) is a valid reason for asking for the protection of a nomen, the same is not true for a so-called "predominant usage". In all cultural domains including science, as soon as two different opinions exist, both have their philosophical justification and are entitled to consideration, irrespective of their numbers of supporters, of the institutional "importance" of the latter, of their financial situation or other similar criteria: no quantitative criterion is useful to decide which opinion is "more important" or "more significant" and therefore should be followed (actually, the history of science is full of examples where the majority was later shown to have been completely wrong).

Finally, consideration of "usage" as a reason for protecting a nomen should be restricted to publications that appeared *previous* to the public discussion of the case, because otherwise risks are great that "'current usage' be deliberately rigged or manipulated to support one's interpretation" (Dubois 1997a: 319). More generally, the risk of manipulation of usage (a risk that does not exist with priority) was identified long ago, e.g., by Hershkovitz (1958: 912) who very aptly wrote, in a public discussion on the "Principle of Conservation" (an earlier avatar of the "rule of predominant usage"): "According to the procedure suggested for use of the Principle of Conservation, a proposal for the rejection of a senior synonym may be made by one person. The proposal may be based, consciously or unconsciously, on prejudice. The pertinent bibliographic data may be deliberately or inadvertently one-sided and/or misleading. The proposer is not required to present personal credentials, his motives may not be openly questioned, his competence as a zoologist, if he is one, is not weighted. In the absence of a protest to a proposal because of ignorance, indifference, or inability on the part of an objecting specialist to meet the two-year deadline, the Commission, according to the suggested procedure, must reject the senior synonym and place the junior synonym on the appropriate *Official List* of Valid Names". These words read particularly prophetic in the light of a number of cases and decisions taken by ICZN in the recent years. Hershkovitz (1958: 913) went as far as concluding his paper by the following "Recommendation": "Any principle, rule or law that specifically provides for annulment or circumvention of the Law of Priority shall not be incorporated into the International Rules of Zoological Nomenclature".

All these clarifications allow the proposal of more precise definitions than those currently available for the different categories of usage of nomina. Clarity of the zoological *Code* would certainly benefit from adopting such well defined categories for the usage of nomina, instead of the vague concept of "predominant usage" introduced in the last edition of this book, which takes into account only nomina treated as *valid* but not *available*, so that a nomen duly listed as an invalid synonym in hundreds of publications since 1899 may now be considered a "*nomen oblitum*".

In what follows, "usage" of a nomen designates two different situations: 1) genuine usage of a nomen as the valid nomen for a taxon; and 2) explicit mention of this nomen (e.g., in a synonymy) as an available but invalid junior synonym or homonym of another nomen considered the valid one for the taxon; this category includes the cases of due consideration given to this nomen as a senior homonym of a junior nomen, which is therefore considered invalid. In the case 2, although the nomen is not used in a given taxonomy, it is considered available (and therefore cannot qualify as a "forgotten" nomen) but "inactive", a situation that may change whenever the taxonomy of the group changes (this corresponds to the rather common situation of "resurrection" of a doxisonym during the revision of a group); furthermore, in the special case of a nomen treated as a senior homonym making a junior homonym invalid, this nomen, although invalid, is fully "active" in zoological nomenclature.

After their creation, nomina can have four kinds of fate. 1) Some nomina are adopted universally as valid by all subsequent authors, either for the original taxon for which they had been coined, or for slightly or widely different taxa. Such nomina, used harmoniously by all authors, can be called *symphonyms* (from the Greek *symphonos*, "harmonious"). 2) Some nomina are adopted as valid by some of the subsequent authors, while some other authors use another nomen or other nomina for the same taxon or for related taxa. Independent of the respective numbers of uses of the competing nomina (see below), these can be called *diaphonyms* (from the Greek *diaphonos*, "discordant"). 3) Some nomina are regularly listed as available but invalid (for being junior synonyms or homonyms of valid nomina). These can be called *aphonyms* (from the Greek *aphonos*, "silent"). This category includes the nomina that are considered invalid but "active" as available senior homonyms that make their junior homonyms invalid. 4) Finally, some nomina are completely forgotten by all authors and never used as available nomina for a long time. Such nomina can be called *eneonyms* (from the Greek *eneos*, "dumb"). The category of "*nomina oblita*" as recognized by the *Code* is a strongly heterogeneous one, that mixes two widely different situations, that of aphonyms and that of eneonyms.

These four categories of nomina have general value for all zoological nomina, but here special attention will be given to their status and use in class-series nomenclature. The meaning and use of these categories in the three other nominal-series, and in particular relative to the definition of "usage" and "predominant usage" as recognized in the *Code* for these nomina, is a distinct matter that will not be addressed here.

Following some of the Rules of the *Code* for the nomina of other nominal series (e.g., Art. 11.7.2), I suggest that the deadline of 31 December 1899 be used to establish the status of class-series nomina relative to these four categories: to be considered a symphonym, a nomen must have been recognized as valid *for the taxon it denotes or for similar, synotaxic taxa* (see Note 17) in *all publications* and by *all authors* after 31 December 1899, whereas a class-series nomen that has *never* been used as a valid nomen (or an available senior homonym) by *any* author after that date must be considered an aphonym (if mentioned as available but invalid) or an eneonym (if never mentioned); all other nomina, that have been used as valid (or as available senior homonyms) after that date by only some of the authors and in only some of the publications (whatever the respective numbers or proportions of these different uses) are to be treated as diaphonyms. Therefore, any class-series nomen created after 31 December 1899 cannot be an aphonym or an eneonym, but is either a symphonym or a diaphonym.

As tackled above, the idea that a nomen should be protected because it has had a "common" (although not universal) usage should be qualified further. If some diaphonyms are to be protected in such cases because they are "well known to non-specialists", then the evidence for this statement should rely on "non-systematic" publications only. Some nomina, although known by specialists (taxonomists or phylogeneticists), are never, or very seldom, used in non-systematic literature, and the need of their "protection" cannot be supported for this reason, whereas this argument can be used to protect nomina that have been widely used outside systematics: the case of the nomina **Batrachia** and **Lissamphibia**, presented in detail in Dubois (2004b, in prep.), exemplifies this distinction well. In other cases, different nomina are used alternatively but almost as frequently by different authors (or even by the same author) to designate the same taxon, as exemplified by the case of the nomina **Urodela** and **Caudata**. To clarify these distinctions, I recognize several subcategories within the general category of diaphonym. I propose the term *stenodiaphonym* (from the Greek *stenos*, "narrow") for any nomen that has been used until now only, or almost only, within the narrow frame of systematic (i.e. taxonomic and/or phylogenetic) literature after 31 December 1899, as a valid nomen or as a senior homonym making a junior homonym invalid; therefore, such a nomen has *not* been *significantly* used as valid in non-systematic literature after 31 December

1899. In contrast, the term *eurydiaphonym* (from the Greek *eurus*, "wide") designates a nomen that has been *significantly* used as valid in non-systematic literature after 31 December 1899 for a given taxon. To qualify the term "significantly" further, I propose below some practical rules, which are offered for discussion. A further distinction can be made between two kinds of eurydiaphonyms: a *paneurydiaphonym* (from the Greek *pas*, "all, each") is a nomen that is *the only eurydiaphonym* to have been used as valid for the taxon it designates in numerous non-systematic publications, whereas a *schizeurydiaphonym* (from the Greek *skhizo*, "I split, I separate") designates an eurydiaphonym used as valid but *alternative* to another eurydiaphonym for the same taxon in numerous non-systematic publications. Of course, allocation of a nomen to one of the categories above is not necessarily definitive, as a stenodiaphonym can become an eurydiaphonym if it becomes regularly used in general literature.

Altogether, symphonyms and paneurydiaphonyms form a special category of nomina that have been applied by all or almost all authors in many works, including non-systematic ones, to the same taxon or to closely related ones, whereas no other nomen has been used significantly for the same taxon. Such nomina are therefore unambiguous and are well known not only to systematists, but also to other biologists and to laymen. In the class-series especially, where the only rule in force until now for the validity of nomina has been "consensus", such nomina no doubt merit protection and conservation, even if they had not been originally coined for the taxon that they now refer to in the eyes of many. I propose the term of *sozonym* (from the Greek *sozo*, "to keep, to protect") to designate any such nomen. This is a Greek-derived term equivalent to the Latin-derived expression *nomen conservandum* that is sometimes found in taxonomic literature, and that was introduced in the last edition of the *Code* under the form of *nomen protectum* (Anonymous 1999b: Article 23.9.2).

In contrast, schizeurydiaphonyms, stenodiaphonyms, aphonyms and eneonyms, which cannot be considered well known to non-systematists, or that may be known but are not universally adopted, can be grouped in a general category of *distagmonyms* (from the Greek *distagmos*, "doubt, uncertainty").

A last clarification should be added on this question of usage. As explained by Hołynski (1994: 14-16) in his excellent rebuttal of the ill-inspired paper of Gould (1990), the fact that a nomen has had widespread use and should be treated as a sozonym according to the proposals above, implies by no means a judgement on the "value" of this nomen or on the "quality" of its original publication. It only depends on the subsequent history of this nomen and of other nomina in the nomenclature of the zoological group concerned. In fact, in many cases a nomen has gained universal or general acceptance although it has been poorly introduced for a poorly defined or heterogeneous taxon, as well examplified by the nomen AMPHIBIA (Dubois 2004b). Protection of such a nomen is only justified by the need of nomenclatural stability for well known taxa, irrespective of the quality of the publications where their nomina were originally proposed.

TAXONOMIC CATEGORIES AND RANKS

In zoological taxonomy and nomenclature, the terms "category" and "rank" are often used as synonyms. For example, in the *Glossary* of the *Code*, the term *rank* is defined as "The level, for nomenclatural purposes, of a taxon in a taxonomic hierarchy", whereas the phrase *taxonomic hierarchy* is defined as "A system of classification based on a sequence of taxonomic categories ranked by their increasing levels of inclusiveness". As for the term *category*, no definition is provided in the English *Glossary*, but in the French *Glossaire* of the *Code* the following definition is provided: "Classe des taxons d'un certain rang, *p. ex.* l'espèce, la sous-famille etc.", which could be translated into: "Class of taxa of a certain rank, e.g., species, subfamily, etc.". Mayr & Ashlock (1991: 431) defined taxonomic category as follows: "Rank in a hierarchy of levels to which taxa are assigned, such as subspecies, species, and genus. A class whose members are all taxa assigned a given rank". The same authors provided the following definition of *ranking*:

"The placement of a taxon in the appropriate category in the hierarchy of categories" (Mayr & Ashlock 1991: 427). Many other authors who provided similar definitions could be mentioned.

It is here proposed to make a distinction between the two terms, by considering *category* as designating a taxonomic concept and *rank* as designating a nomenclatural concept. Whereas all ranks correspond to a category occupying the same position in the hierarchy (*ranked categories*), a few categories do not correspond to hierarchical ranks and could be called *unranked categories*. These are of two kinds.

First, they include the categories termed by Bernardi (1980) "taxonomic categories of evolutionary systematics". These are a number of categories recognized either between the rank genus and the rank species, or below the rank species, to point either to some *particular cases of species* (so-called "sibling species" or *dualspecies*; allopatric species or *prospecies*; incipient species; all surveyed by Bernardi 1980), or of *subspecies* (*grex*, *quasispecies*, *vicespecies* and *exerge*, all surveyed by Bernardi 1980; *morph* of Edwards 1954; *strong subspecies* and *weak subspecies* of Deuve 1994, 2004), or to taxa that do not have normal meiosis and reproduction, referable to categories such as "parthenogenetic species" (*klonon*), "gynogenetic species" (*gynoklepton*) or "hybridogenetic species" (*zygoklepton*) (Dubois 1991a). These are properly recognized as taxonomic categories, but they are not occupying a particular rank in the taxonomic-nomenclatural hierarchy. All these categories are referable to the species or subspecies rank. These "evolutionary categories" are useful to provide biological information on the "kind of taxa" that they designate, but no information on ranking. Thus, in the species-series, and also in the variety-series, such unranked categories can be used in parallel with the ranked categories that correspond to the nomenclatural ranks. At this level (species-subspecies-variety), a third, distinct, item that should be distinguished, is the recognition of special evolutionary or populational concepts, such as *deme* (Gilmour & Gregor 1939) or *cline* (Huxley 1939), that are neither taxonomic categories nor nomenclatural ranks.

Second, the category of *plesion* (Patterson & Rosen 1977: 160), used by some authors to designate fossil taxa that cannot properly be placed in the taxonomic hierarchy, is an unranked taxonomic category that has no nomenclatural rank counterpart. The same applies to the common use in all taxonomic literature of the phrase *incertae sedis* to designate taxa that cannot be properly ascribed a place in the taxonomic hierarchy of their group.

In most cases in zoological taxonomy, rank and category correspond strictly, thus being a single taxonomic-nomenclatural category-rank, but in the few cases just mentioned the two are dissociated: the taxonomic category does not directly correspond to a nomenclatural rank. For many purposes, including non-systematic usage (e.g., in legislation, customs, trade, etc.), taxonomic category is not an important information but nomenclatural rank is indispensable: to take just one example, in order to build up a strategy of conservation biology for a locality, country or continent, it is necessary to have lists of species-rank taxa, whatever the biological characteristics of these "species" (i.e. whether dualspecies, prospecies or kleptons). Thus the distinction between nomenclatural rank (of general use) and taxonomic category (of more specialised use in some cases) is relevant, and should be spread in all zoological taxonomy.

CONCLUSION

The present proposals are the result of a 20-year reflection, already published in part (Dubois 1984b, 2000b). I do not expect immediate understanding and agreement from all colleagues to these proposals: rather, I have no illusion about the fact that introduction of all these new terms will probably entail a strong resistance, if not aggressive comments, in the community of taxonomists, as nothing is more difficult than changing an "old tradition" in science, and especially in an old discipline. It may be easier to introduce completely new rules and terms, under a new philosophy, than trying to improve an

existing system. However, the latter attitude seems more justified in view of the fact that the current system of zoological taxonomy and nomenclature, although not perfect, has a number of merits, has been in existence for more than 250 years and that all the information stored in this system should be saved rather than thrown away; this does not mean that this information should remained unchanged, but in order for it to evolve and improve, it should remain available. Even if the process is slow, I do hope that in the long run the merits of completely abandoning the term "type" in biological taxonomy and nomenclature, and of using the other terms proposed above, will be progressively recognized. The next papers of this series (Dubois in press, in prep.) are meant to illustrate how the use of these terms can help in clarifying some complex nomenclatural situations that arise concerning the nomina of higher taxa because of the long absence of rules for these nomina, and, particularly, of the absence of a rule of coordination among them.

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NOTES

1. — One of my recent papers (Dubois 2000b), that contains a number of points worthy, in my opinion, to be discussed by zoologists, submitted in 1998 to a "major" journal, was rejected with the following comment: "I would hate to see nomenclature becoming even more complicated with lots of new technical terms, however logical. We should try to simplify the rules so that practising zoologists will follow them, and I think that this manuscript points in the wrong direction". In other words, it was rejected because the managing editor whom it had been sent to had an *opinion* different from mine regarding the way zoological nomenclature should evolve, and regarding the meaning of terms like "simple" and "complicated" (as in my opinion terminological clarity simplify matters that are obscure with imprecise terms). This is not acceptable, as widely different opinions regarding this controversial field have been defended, sometimes very bitterly, by different authors over the last two centuries: rejecting a paper in this field on account of differences in opinion (i.e. not of demonstrable flaws in the reasoning or of ignorance or misunderstanding of some nomenclatural rules) merits only one qualification: this is censorship. Silencing colleagues who disagree with one's opinions or ideas is certainly less difficult than having to refute their point of view and, above all, to *convince* other colleagues on the basis of arguments. Another possible explanation of this rejection could be the mere length of the manuscript, dealing with several questions at once, but this does not hold, as other much shorter papers, dealing with a single problem, were also rejected as not presenting some of the "fashionable" ideas that have today the favor of the "popular medias" (see e.g., list of references in Carpenter 2003: 79). This difficulty to simply make some ideas widely available is one of the reasons why the present work presents detailed approaches to several problems altogether. I am not prepared to spend months submitting manuscripts to journals before receiving negative replies simply because the referees have "opinions" different from mine regarding nomenclature.
2. — Hołynski's (1994) in my view very important contribution to the discussion on the last version of the *Code* had to be published in a Hungarian journal because it had been refused by *BZN*. The same is true of 14 of my own texts submitted to the ICZN secretariat for publication in *BZN*: six of them were finally published elsewhere (Dubois 1987c-e, 1989a, b, 1997a) and eight of them (Dubois unpublished-a-f; Bour & Dubois unpublished-a, b) are still unpublished. Interestingly, one of them (Dubois unpublished-a) was used by the ICZN secretary to write part of a paper published under his name (Tubbs 1992) and my text was never brought to the knowledge of the members of

ICZN before vote on this case (Anonymous 1993). Similarly, ICZN members voted on the "famous" *PETROPEDETINAE-PHRYNOBATRACHINAE* case (Anonymous 1999a) in the ignorance of my last manuscript on this case (Dubois unpublished-f) where I had shown that the previous published text about it (Frost & Savage 1995) contained gross factual mistakes in the presentation of the facts and demonstrably incomplete bibliographical coverage of the case. These examples are not given for "grumbling" but for pointing out a real problem of censorship of opinions and texts by ICZN, a problem that should attract the attention of zoologists worldwide. I know of several other cases of censorship that touched several other colleagues, who either decided to forget it (and not to care any more for ICZN when dealing with nomenclatural problems) or to discuss these problems "privately" with ICZN officers in order not to "create problems", but in my opinion it is not acceptable for zoologists to face such an attitude from officers that should serve the community of zoologists. Furthermore, most colleagues who ever published a text in *BZN* know how their text can be authoritatively shortened or modified before publication, not rarely in a sense quite different from that of the originally submitted manuscript. Indisputably, in the recent decades *BZN* has worked as the "private journal" of a small number of persons, not as the official journal of an international body supposedly at the service of zoology and zoologists worldwide.

3. — Strictly speaking, taxa of ranks above superfamily should be designated as "high-level taxa", "taxa of higher ranks" or "higher-ranked taxa", but for sake of brevity, because of the frequent use of this concept in this paper and followers, the shorter formula "higher taxa", frequently found in the literature, including in titles of papers (e.g., Chitwood 1958; Levine 1958; Ghiselin 1977; Lee 1999; Minelli 2000), will be employed here.

4. — With such a criterion, nomina like those used in the book of Fei *et al.* (1991), which was distributed in thousands of schools all over China, would certainly qualify much more for "protection" than nomina used in any book of which only a few hundred copies were printed and distributed only to specialists of the academic world in western countries. The words of Melville (1958: 1249) are also worth remembering here: "It is to my mind impossible to define numerically the amount of usage that a junior synonym must have enjoyed to qualify for conservation, or the amount of neglect that a senior synonym must have suffered to necessitate its rejection in a way that will cover both the household words of applied zoology (in the medical, agricultural, veterinary and geological fields) and the usage of highly restricted specialist fields; five usages in the latter may be as important to those concerned as five hundred in the former".

5. — In the recent years, in order to support its action against a strict respect of the Rules, the ICZN Secretariat has deliberately biased the international public discussion which should normally have developed freely in its *Bulletin*: it has done so both through active support to some interpretations of the cases and repeated censorship of adverse opinions submitted to *BZN* (see Note 2 above). A recent practice, quite foreign to the intellectual tradition of science but closer to political, economic or other social activities, has been the support brought by ICZN to clear "lobby" actions undertaken by some researchers in order for various colleagues to speak for "their" nomenclature, despite its clear invalidity according to the Rules (e.g., Jennings *et al.* 1994; Webb *et al.* 1994). That such suddenly numerous comments cannot have been "spontaneous" but were duly organized is made clear by the usual complete or almost complete absence of comments in similar cases in the same zoological group and at the same period, many votes of ICZN having been made in the absence of even a single published comment in *BZN*.

6. — Future historians of science will perhaps be interested to realize that, at the time of giving this to print (March 2005), I am unable to provide a bibliographic reference to a paper-published complete version of the proposed "*Phylocode*". A version dated 17 June 2004 and signed P. D. Cantino & K. De Queiroz is available on-line (http://www.ohiou.edu/phylocode), but as internet sites have no long-term permanency (see Dubois 2003b, 2004a), the text as it is today on this site may be unavailable to future readers. This will appear all the more strange that a number of papers have recently been published commenting on this "phantom" text. Discussing this unpublished text in publications is similar to discussing phantom unpublished manuscripts, quoting them as "personal communication", "in press", "submitted" or "in preparation" (Dubois 1999b).

7. — It is even more misleading to write that "there is much more consensus at the familiar [*sic,* for familial], generic and specific level [*sic,* for levels]" (Laurin 1998: 8). The notion of "consensus", here, is relevant only as concerns taxonomy, but not nomenclature. For nomina regulated by the *Code*, their validity is not fixed by "consensus" but by Rules, that should be considered stringent, not optional, by all zoologists. Needless to say, the recent tendency of ICZN to "validate" a so-called (although incomplete) "consensus", as opposed to the Rules, has certainly contributed to spread this idea that the nomina of taxa could be fixed by "consensus". This example is not the only one where an active supporter of the Querauthian nomenclatural system and opponent of the current *Code* showed ignorance of the latter. Here is another example: "the standard suffixes for the various Linnean ranks

do not exist in traditional zoological nomenclature. In zoological nomenclature, only taxon names traditionally assigned to the family level have a standard ending (-idae), and even here there are many exceptions" (Lee 1996: 189). The same author mentioned elsewhere the "rare instances where names have the same root but different endings (e.g., Iguanidae, Iguaninae, *Iguana*)" (Lee 1999: 365), without apparently realizing that such "rare instances" are in fact very common, because of the Rule of Coordination, a rule seldom, if ever, mentioned by Querauthian taxonomists and that imposes the existence of a nominotypical subordinate taxon in any taxon having subordinate taxa. The title of the last paper (on "stability of (…) taxa in (…) nomenclature") also well illustrates the basic confusion between taxa and nomina, i.e. between taxonomy and nomenclature, that characterizes many Querauthian militants: as far as nomenclature is concerned, one can only speak of stability of nomina, not of taxa, a matter that does not belong in nomenclature. That such gross factual mistakes can be quietly published in a well known refereed international journal of zoology gives support to the statement that "low quality taxonomic works, or works containing severe methodological taxonomic mistakes, can be and are regularly published in highly-ranked periodicals" (Dubois 2003a: S14).

8. — This confusion is plainly acknowledged by some Querauthian taxonomists, for example when they support "an alternative system, phylogenetic taxonomy (or phylogenetic nomenclature)" (Pleijel & Rouse 2000: 627).

9. — In the future, this rule should also be generalized to variety-series nomina, in order to include them also in the *Code*: as a matter of fact, the exclusion from the zoological *Code* of nomina for taxa below the subspecies is an unacceptable case of "restriction of the freedom of taxonomic thought and action". Unlike in zoology, the botanical code (Greuter *et al.* 2000) provides nomenclatural rules for the nomina of lower taxa like varieties or forms.

10. — Johnson (1970: 205) provided the following interesting comment: "In general, whether good or bad, theoretically disposed or otherwise, taxonomists themselves, like other scientists and scholars, find their own activities self-justifying because they bring some degree of intellectual satisfaction. This is doubtless the chief motivation for most of us, whatever rationalizations we offer about usefulness when called upon to show why society would support us. Nevertheless, society does support us, niggardly though we claim this support to be". Such a comment would be only in part true today. "Intellectual statisfaction" may not be the only motivation for chosing a field of research rather than another, even within a general disciplinary field like systematics. If we speak in terms of career, notoriety and income, it is hardly questionable that choosing a "modern", "fashionable" field, especially using a costly methodology (see Dubois 2003a), e.g., molecular phylogeny, will usually prove more fruitful than describing hundreds of new species of obscure tropical mites or worms. This is why the major problem at stake here is not one of individual tastes or orientations but one of political choices by decisions-makers regarding the scientific priorities of the coming century.

11. — Bryant & Cantino (2002: 52) stated that "Taxonomic freedom is also a principle of phylogenetic nomenclature", which is of course a misleading statement, as "taxonomic freedom" means that taxonomists are free to adopt, or not, principles of "phylogenetic taxonomy", not that, in the frame of such principles, they are granted "freedom of taxonomic opinion with regard to hypotheses about relationships"!

12. — To designate a "taxomen of the species-series", Dubois (2000b) had proposed the term "speciesomen". The root used to coin this term was the complete term "species", although a correct etymological formation, based on the stem of the term "species", would have led to the term "speciomen". I had felt that the latter term would have been liable to cause confusion with the term "specimen". However, several colleagues pointed to me that the term "speciesomen" was particularly unpalatable and suggested use of the correct spelling "speciomen", a suggestion followed here. In the plural form, there is no risk of confusion between "specimens" and "speciomina", but in the singular attention should be paid to this possible risk of misprint or confusion.

13. — I do not advocate the use of the root *proto* here, for reasons of euphonism (it would entail a heavy repetition of the letter *t*) and of length of term (especially in composed terms derived from *protaxon*). The root *pro*, from Greek and Latin languages, may take several meanings, including the one used here, which is not the most frequent but exists in several other scientific terms (e.g., *pronephros*).

14. — The new taxonomic term *parotaxon*, similar in aspect to its nomenclatural counterpart *paronym*, is distinct from the term *parataxon* which has the same etymology. The latter term was introduced by Moore & Sylvester-Bradley (1957) to designate classificatory units that do not apply to living organisms but to parts of organisms or to traces left by living organisms. However, strictly speaking, the term taxon is only relevant to designate a classificatory unit for organisms, so that, for the classificatory units that do not apply to complete living organisms, I suggest abandonment of the term of *parataxon* (which would be appropriate to designate a "kind" of taxon) and to replace it by the new term *pseudotaxon* (from the Greek *pseudes*, "liar, deceiver").

15. — Strictly speaking however, it should be clear that the *taxa* may be isotaxic, but not their *nomina*,

as nomina are only labels and have no contents, extension or intension.

16. — Very enlightening examples in this respect were provided in the recent years in amphibians, in the "campaigns" developed by adversaries of the Principle of Priority for the conservation of the nomina *ICHTHYOPHIIDAE* versus *EPICRIIDAE* (see e.g., Dubois 1984a, 1985, 1991b, unpublished-a; Wilkinson & Nussbaum 1988; Smith 1989, 1991; Anonymous 1990, 1993; Tubbs 1991, 1992), *PETROPEDETINAE* versus *HEMIMANTINAE* (Dubois 1981, 1982, 1984b, 1985, 1987a, b, 1994a, 1995b, unpublished-f; Poynton 1995; Frost & Savage 1995; Clarke 1995; Anonymous 1999a) or, even more caricatural, *HEMIDACTYLIINI* versus *MYCETOGLOSSINI* (Dubois 1984a, 1985, 1994b, 1995a; Smith & Wake 1993, 1994, 1996; Webb *et al.* 1994; Anonymous 1997), although these nomina had never or very rarely appeared in non-systematic publications. In such cases, these "campaigns" should certainly be interpreted mostly as a way of "testing" the willingness of the majority of ICZN to replace at least partly the Principle of Priority by a "principle of usage", rather than as a way to preserve a so-called general usage outside taxonomy, which simply did not exist. In fact, "nomenclatural stability" may not be the real motivation of some of the militants for "usage" in the cases above, as suggested recently by the fact that one of them took the exactly opposite attitude when he applied (Savage 2003) for "suppression" by ICZN of a widely known book (de la Cepède 1788a, b) which contains a number of nomina used without discontinuity in zoology since their creation. As shown by Bour & Dubois (1984), if such a rigid application of the Rules was to be followed in this case, it should be so in the case of the classical works of Schlosser (1768) and Boddaert (1770a, b, 1772a, b) as well. If ICZN decided the suppression of de la Cepède's works, simple consistency requires that it also suppresses the seemingly classical book of Laurenti (1768), that contains several trinominal nomina for species, as well a striking non-respect of the Linnaean hierarchical structure of taxonomy in the case of the genus *Proteus* (see Dubois 2004b).

17. — This is an important qualification: of course, an author who does not recognize a taxon will not use its nomen as valid.