

Mitt. dtsh. malakozool. Ges.	86	9 – 12	Frankfurt a. M., Dezember 2011
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Under Threat: The Stability of Authorships of Taxonomic Names in Malacology

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Abstract: Nomenclature must be constructed in accordance with agreed rules. The International Commission on Zoological Nomenclature was founded in Leiden in September 1895. It not only produced a Code of nomenclature, that was refined over the years, but also provided arbitration and advice service, all with the aim of ensuring that every animal has one unique and universally accepted name. Name changes reduce the efficiency of biological nomenclature as a reference system. The Code was established to precisely specify the circumstances under which a name must be changed, and in what way. Name changes are only permitted if it is necessitated by a correction of nomenclatural error, by a change in classification, or by a correction of a past misidentification. Also authorships are regulated by the Code, mainly by Article 50. In a recent paper by WELTER-SCHULTES this Article is interpreted in a way that is different from previous interpretations by the zoological (malacological) community, leading to major changes in authorships. It is here argued that his alternative interpretations (1) are not in line with the spirit of the Code, and (2) will not serve the stability of nomenclature. It is important that interpretation and application of the existing rules be objective, consistent, and clear.

Keywords: authorships, malacology, nomenclature, Code, ICZN, Article 50, *Pisidium*

Zusammenfassung: In der Nomenklatur müssen übereinstimmende Regeln gelten. Die Internationale Kommission für Zoologische Nomenklatur (ICZN) wurde im September 1895 in Leiden gegründet. Sie schuf nicht nur einen Code der Nomenklatur, sondern fungierte auch als Entscheidungs- und Beratungsstelle, dies alles, um sicherzustellen, dass jedes Tier einen einzigen und universell akzeptierten Namen hat. Namensänderungen reduzieren die Effizienz der Nomenklatur als Referenzsystem. Der Code wurde aufgestellt, um präzise festzulegen, unter welchen Bedingungen ein Name geändert werden muss und auf welche Weise dies zu geschehen hat. Echte Namensänderungen sind nur erlaubt, um nomenklatorische Irrtümer oder frühere Fehlbestimmungen zu korrigieren bzw. bei Änderungen der Klassifikation. Auch die Autorschaft des Namens ist durch den Code geregelt, vor allem in dessen Artikel 50. In einer aktuellen Veröffentlichung von WELTER-SCHULTES wurde dieser Artikel in einer Weise interpretiert, die von den bisherigen Interpretationen innerhalb der zoologischen (malakozologischen) Wissenschaftsgemeinschaft abweicht und die zu wesentlichen Änderungen in den Autorenschaften führen würde. Es wird hier die Auffassung vertreten, dass die genannten alternativen Interpretationen von WELTER-SCHULTES erstens nicht dem eigentlichen Sinn des Codes entsprechen und zweitens nicht der Stabilität der Nomenklatur dienen. Es ist wichtig, dass Interpretation und Anwendung der existierenden Regeln objektiv, konsistent und klar sind.

Introduction

In a recent paper by WELTER-SCHULTES (2011), dealing with the authorship of taxonomic names in malacology, the reader is confronted with several problems brought forward by the author, that in reality do not exist (e.g. the possible consideration by WELTER-SCHULTES that a publisher or an artist might legally be the author of a zoological taxon). Apart from these non-existing problems, the author made several statements with far-reaching consequences (i.e. if one agrees with him, major changes need to be made with regard to authorships). Therefore, a critical reply is necessary, in order to halt the threat that is mentioned in the title.

The International Code of Zoological Nomenclature (ICZN), in its Fourth Edition (which is effective as per January 1st 2000), regulates in Article 50.1, 50.2 and 50.3 the authorship of names. In this context, Article 50.1.1 is particularly important. It states that “if it is clear from the contents that some person other than an author of the work is alone responsible **both for the name** or act **and for satisfying the criteria of availability** other than actual publication, then that other person is the author of the name or act”. The Article 50 has already been previously discussed by SABROSKY (1974). Let us discuss some of the proposals of WELTER-SCHULTES in light of the text of this Article 50.1.1; all our conclusions are in line with the statements of SABROSKY.

***Pisidium globulare* CLESSIN, 1873 [not *Pisidium globulare* WESTERLUND & CLESSIN, 1873]**

According to WELTER-SCHULTES, WESTERLUND (1873: 532) “established *Pisidium globulare* with a description and without attributing the name to a special person”. However, consultation of the work of WESTERLUND revealed that he mentioned it as “*Pisidium globulare* CLESS.”, thus attributing the name to CLESSIN. In fact, from the work of WESTERLUND it is clear that CLESSIN was responsible for most of the views regarding the family Sphaeriidae as presented in his work (see e.g. pages 486-487), and that WESTERLUND obtained a manuscript from CLESSIN. In his “Catalogue of the British species of *Pisidium*”, WOODWARD (1913: 131, reference 32) even considers CLESSIN the author of the family Sphaeriidae in the book of WESTERLUND. Several other names were introduced by CLESSIN in the book of WESTERLUND; for more than a century subsequent authors invariably cited CLESSIN as the author of all these names. That CLESSIN provided the names as well as the descriptions to WESTERLUND becomes also clear from parts of the volume “Familie der Cycladeen” written by CLESSIN (1874) for the “Systematisches Conchylien-Cabinet” (e.g. pages 25, 34-35). With regard to *Pisidium globulare*, CLESSIN (1874: 25) stated “Ich habe die Muschel als “*Pisidium pusillum*”” zuerst von Herrn JEFFREYS in London erhalten. Später fand ich selbe unter den schwedischen Pisidien, die mir Dr. WESTERLUND behufs Bearbeitung der Cycladeen für seine Fauna mittheilte“. In summary, CLESSIN **provided the name *globulare*, and is very likely also responsible** for the description/characterization of the taxon (thereby satisfying the criteria of availability). I see no reason for considering WESTERLUND & CLESSIN as the authors of *globulare* (as proposed by WELTER-SCHULTES); therefore, I hope that the malacological community continues to use *Pisidium globulare* CLESSIN, 1873 as the name of the taxon that is now known as such for > 130 years.

If one wants to be a hair-splitter, one may argue that CLESSIN is not entirely responsible for the description, as it is written in the Swedish language (WESTERLUND must have translated the Latin or German description by CLESSIN into Swedish). Consequently, the word “alone” in Article 50.1.1 prohibits the authorship of CLESSIN, and WESTERLUND would take over the authorship. Of course, it all depends on how one interprets the word “alone” of the Article. In any case, WESTERLUND & CLESSIN is not an option: according to Article 50.1.1 it is either CLESSIN or WESTERLUND, not both. Since there are various interpretations possible for the word “alone”, I see (as stated above) no reason (or any obvious advantage) for deviating from a >130 years old tradition, especially if this tradition is in line with the Code (or at least can be defended as such).

***Pisidium lilljeborgii* CLESSIN, 1886 [not *Pisidium lilljeborgi* ESMARK & HOYER, 1886]**

In 1886 ESMARK & HOYER published a paper on the arctic continental molluscs of Norway. In this paper several new taxa are described. One of these taxa is *Pisidium lilljeborgii*. It was published as “*Pisid. Lilljeborgii* CL. n. sp.”; “CL.” is the abbreviation for CLESSIN, who was at that time a celebrated specialist on the genus *Pisidium*. The name *lilljeborgii* clearly originates from CLESSIN. Although not stated specifically, it is likely that the description is also from CLESSIN, as is also believed by WOODWARD (1913: 113 “None are given by CLESSIN”; “CLESSIN when describing the type”). Until proven otherwise, it is wise to follow a century-old tradition, and consider CLESSIN as the author of *Pisidium lilljeborgii*, and not ESMARK & HOYER. If the proposal of WELTER-SCHULTES (2011: 38) for *Pisidium lilljeborgii* is followed, one should also “correct” the authorship of *Oxyloma sarsii* (ESMARK, 1886) into (ESMARK & HOYER, 1886). This taxon was established in the same paper on page 108 under the name “*S. pfeifferi* Var. *sarsii* B. ESM.”. As stated in my title: If one does so, the stability of authorships of many taxonomic names in malacology becomes threatened. The two *Pisidium* examples alone already represent about 10% of the *Pisidium* taxa found in Europe.

By the way, WELTER-SCHULTES (2011: 38) corrected the name *lilljeborgii* (as used by CLECOM and Fauna Europaea) into *lilljeborgi*, and stated “Maybe I was the first after 105 years to have consulted the original source”. This is a strange statement. I have not seen in the original source the spelling *lilljeborgi*, only *lilljeborgii*. The latter spelling should be used; the spelling by WELTER-SCHULTES is an incorrect subsequent spelling, as delineated by Article 33.4 of the Code.

***Helix crombezi* LOCARD, 1882 [not *Helix crombezi* BOURGUIGNAT, 1882]**

In 1882 LOCARD published a review about the continental molluscs of France, and mentions on page 320-321 the taxon *Helix crombezi*. WELTER-SCHULTES (2011: 39) considers BOURGUIGNAT, 1882 as the author of this taxon. Consultation of the work of LOCARD shows that (1) the name *Helix crombezi* is proposed by MILLIÈRE in a manuscript (see LOCARD, 1882: 91), and that (2) the description is from

BOURGUIGNAT. This means that BOURGUIGNAT can never be the author of *Helix crombezi*. Article 50.1.1 is clear on this: LOCARD is the author of *Helix crombezi*, as BOURGUIGNAT is not responsible for **both** the name and the description.

***Truncatellina cameroni* TRIANTIS & POKRYSZKO, 2004 [not *Truncatellina cameroni* TRIANTIS, POKRYSZKO, VARDINOYANNIS & MYLONAS, 2004]**

Consultation of the work of TRIANTIS *et al.* (2004) revealed that the new name is introduced at the top of the relevant section as follows: “*Truncatellina cameroni* TRIANTIS & POKRYSZKO n.sp.”. WELTER-SCHULTES argues that the authors of this taxon should be the authors of the paper (i.e. TRIANTIS, POKRYSZKO, VARDINOYANNIS & MYLONAS). However, it is currently accepted that in modern papers the above mentioned statement is sufficient to indicate that the name **and** the description originates from the person or persons that are mentioned in combination with n.sp. (or sp.n., or combinations thereof). If we do not accept this (as is the case with WELTER-SCHULTES) than this has major implications. For example, in 2010 an excellent book was published by STANISIC, SHEA, POTTER & GRIFFITHS regarding the land snails of eastern Australia. It contains the description of 308 new species and 70 new genera. In each individual case, it is mentioned at the start of the description who is the responsible author in the same way as TRIANTIS *et al.* did (e.g. STANISIC sp. nov., or STANISIC & POTTER sp. nov). If we do not accept this as either a general or an explicit statement, it follows that the authorship of the 378 new taxa in this book belongs to STANISIC, SHEA, POTTER & GRIFFITHS. Other examples are the many new taxa introduced during the last decades from the Canary Islands by the group of ALONSO & IBÁÑEZ. The papers of this group are generally multi-authored, but the individual new taxa are attributed to a subset of authors by placing the responsible authors in front of “sp. nov.”. As an example I would like to refer to the recent paper of YANES *et al.* (2011). If the opinion of WELTER-SCHULTES is followed, the authorship of *Napaeus moroi*, *N. gomerensis*, *N. torilensis* and *N. alucensis* should be attributed to YANES, MARTÍN, SANTIANA, G. HOLYOAK, D. HOLYOAK, ARTILES, DENIZ, ALONSO & IBÁÑEZ. For me (and I assume that this is also the case for > 99% of the malacological community) “it is clear from the contents” (quoted from Article 50.1.1) of this and other papers that this is not the case.

***Clausilia cattaroensis* ROSSMÄSSLER, 1835 [not *Clausilia cattaroensis* ROSSMÄSSLER & ZIEGLER, 1835]**

This is an interesting case. I already came across it when preparing the Fauna Europaea list in 2002. The name is from ZIEGLER, the description is partly from ROSSMÄSSLER, and partly from ZIEGLER. ROSSMÄSSLER is the author of the publication. Although I spent special attention to this phenomenon as from 2002 on, I have not seen a second comparable case. As from 1905 on, ROSSMÄSSLER is considered the author of *Clausilia cattaroensis*. I agree with this, as in this context it is clear that ZIEGLER is not **alone** responsible for the description. Thus, again, I see no reason to change a > 100 years old tradition. WELTER-SCHULTES considers ROSSMÄSSLER & ZIEGLER as the authors of *Clausilia cattaroensis*; he is the first to propose this. This proposal is not in agreement with the Code; according to Article 50.1.1 it is either ROSSMÄSSLER or ZIEGLER, not both. A new Article would need to be incorporated in the Code to accommodate/validate the proposal of WELTER-SCHULTES.

Authors with variant spellings

As WELTER-SCHULTES indicated, inconsistent spellings of authors in zoological species are known to provide serious obstacles to integrating electronic databases. Therefore, it would be very helpful to be consistent. Thus, CLECOM and Fauna Europaea consistently used for example LINNAEUS, although “LINNÆUS”, “LINNÉ”, or “VON LINNÉ” are equally correct. The same is the case with BOETTGER (BÖTTGER is equally correct), and so on. Although WELTER-SCHULTES acknowledges the problem, he still uses in his AnimalBase site (www.animalbase.org) variant spellings, namely the spelling as presented in the original source. Of course, it is an option, but for the malacological community using electronic databases it is clearly not a convenient one. Apart from that, it is an extreme concept (WELTER-SCHULTES’ own words on page 45).

Final remarks

One can endlessly discuss the above-mentioned topics. To end the discussions once and for all, i.e. to come to a standardized way of writing both scientific names and names of authors, it would be wise to prepare a “List of Available Names in Zoology” as defined in Article 79 of the Code. Such a list has the following status (Article 79.4.1): “A name occurring in an adopted Part of the *List of Available Names in Zoology* is deemed to be an available name and to have the spelling, date, and authorship recorded in the *List* (despite any evidence to the contrary)” and, furthermore, (Article 79.4.3): “No unlisted name within the scope (taxonomic field, ranks, and time period covered) of an adopted Part of the *List of Available Names in Zoology* has any status in zoological nomenclature despite any previous availability”. Such a list would be a perfect document. Preparation of such a document (e.g. all names of continental molluscs from Europe published between 1758 and 2000) is an enormous amount of work. Outsiders can get a good impression of the amount of work by consulting the Nomenclator of Gastropod and Bivalve Families from BOUCHET & ROCROI (2005; 2010). This is a work that is meant to become an official Part of the List of Available Names in Zoology (see their introduction on page 4). That is real progress: there is now no need anymore for other scientists to dig into the jungle of suprageneric names! Time is precious: the taxonomic impediment has grown to enormous proportions – let us locate our resources to tackle this urgent problem, which, in my opinion, is much, much more important than trying to destroy a solid nomenclatural framework by means of alternative interpretations of the Code, a Code that normally works well if one uses common sense.

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Mitt. dtsh. malakozool. Ges.	86	13 – 24	Frankfurt a. M., Dezember 2011
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Authorships and Publication Dates in Malacology: some notes on the 2011 French Checklist of WELTER-SCHULTES & al.

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Abstract: Stability and universality of nomenclature are the obvious aims of the International Code of Zoological Nomenclature. The purpose of the Code is to provide rules resulting in unambiguous names. The Code is an important instrument as it provides the tools to come to a language by which we communicate information about organisms. But the Code is a complex and closely integrated document; its complexity results from its network of interdependent Articles. Despite its complexity, the principles on which the modern rules are based have proved to be robust, although it faces some challenges to improve presentation and to reduce ambiguity. It should be stressed that nomenclatural decisions are the result of often painstaking work. Fauna Europaea provides a checklist of accepted names of continental animals, such as the Mollusca. The Gastropoda checklist is the result of this painstaking work, and provides a framework for the malacological community. In a recent paper by WELTER-SCHULTES, AUDIBERT & BERTRAND about the continental molluscs of France, it is claimed that the Fauna Europaea list in its current form contains numerous nomenclatural errors. I will address these errors, and show that the claims of WELTER-SCHULTES & al. are derived from their alternative interpretation of certain Articles of the Code, or are based on inadequate research of the original sources. The abovementioned paper and my subsequent rebuttal is a nice example of the so-called nomenclatural impediment along the following string: controversies are created by alternative interpretations or a defective presentation of facts, previously used nomenclature is destabilized, further work by scientists/committees are required to undo the damage, or failing this, may result in changes that at best will add little credit to taxonomy in the eyes of its customers.

Keywords: authorships, publication dates, nomenclatural impediment, Code, ICZN, France, Mollusca

Zusammenfassung: Stabilität und Allgemeingültigkeit der Nomenklatur sind die offensichtlichen Ziele der "Internationalen Regeln für Zoologische Nomenklatur" (oft kurz "Code" genannt). Dabei soll das vom Code zur Verfügung gestellte Regelwerk für eindeutige Namen sorgen. Der Code ist ein wichtiges Instrument: er stellt die Werkzeuge, mit deren Hilfe wir zu einer gemeinsamen Sprache kommen können, die uns ermöglicht, Informationen über Organismen zu kommunizieren. Allerdings ist der Code ein komplexes und in sich geschlossenes Dokument, wobei seine Komplexität aus dem enthaltenen Netzwerk voneinander abhängiger und aufeinander wirkender Artikel resultiert. Bei aller Komplexität müssen die Prinzipien, auf denen die aktuellen Regeln beruhen, robust sein, um sich den Herausforderungen der Verbesserung der (Ein-)Ordnung und der Reduktion der Mehrdeutigkeit zu stellen. Es ist zu betonen, dass nomenklatorische Entscheidungen oft ein Ergebnis akribischer Arbeit sind. Fauna Europaea hat nun eine Checkliste akzeptierter Namen der non-marinen Tiere (einschließlich der Binnenmollusken) vorgelegt. Die Checkliste der Gastropoden ist ein Ergebnis solcher mühsamer und akribischer Arbeit und versteht sich als Rahmenwerk für die malakologische Gemeinschaft. In einer aktuellen Veröffentlichung von WELTER-SCHULTES, AUDIBERT & BERTRAND über die Binnenmollusken Frankreichs wurde kritisiert, dass die Liste der Fauna Europaea in ihrer aktuellen Form viele nomenklatorische Irrtümer enthalte. Ich werde hier diese Fehler benennen und zeigen, dass die Kritik von WELTER-SCHULTES & al. aus einer alternativen Interpretation einiger Artikel des Codes resultiert, beziehungsweise auf unzulänglicher Recherche der originalen Quellen beruht. Die genannte Publikation und meine darauffolgende Entgegnung sind ein schönes Beispiel für das sogenannte „nomenclatural impediment“: Streitpunkte und Probleme werden geschaffen durch alternative Interpretationen oder eine unvollständige Präsentation von Fakten. Dadurch wird vorher bewährte Nomenklatur destabilisiert. Zusätzliche Arbeit von Wissenschaftlern oder Arbeitsgruppen ist nötig, um den Schaden wiedergutzumachen, oder, wenn dies nicht gelingt, entstehen Änderungen, die der Taxonomie in den Augen ihrer Benutzer bestenfalls wenig Ansehen bringen.

Introduction

The "Mitteilungen der Deutschen Malakozoologischen Gesellschaft" published in number 85 (July 2011) a paper by WELTER-SCHULTES where he explains how he interprets certain Articles of the International Code of Zoological Nomenclature (ICZN) (fourth edition, 1999). I prepared a rebuttal, in order to point to the consequences if one follows his false interpretations, being a warning that the

Code provides no legal ground to share his opinions. However, after finishing the text of this rebuttal, I came across a paper of WELTER-SCHULTES (co-authored by AUDIBERT & BERTRAND) published in number 12 of the *Folia Conchyliologica* (August 2011). These two co-authors are apparently willing to share the opinions of WELTER-SCHULTES. This paper, dealing with the continental malacofauna of France, systematically ignores decades of taxonomic/systematic research of the French malacofauna (and abroad), and in addition shows us a glimpse of the nomenclatural chaos we are now tumbling over. An excellent overview of the continental malacofauna of France has been published by FALKNER & al. (2002), and was recently updated by GARGOMINY & al. (2011); the reader is referred to these two papers to learn about the actual state of research.

At first, I had no idea where to start or what to discuss from the WELTER-SCHULTES & al. (2011) paper: one can easily fill a book if one wants to address all their points. Therefore, a restriction had to be made. The authors noted (: 5) that “Fauna Europaea” comportent un grand nombre d’erreurs nomenclaturales”. So my restriction was made as follows: in this paper I will only discuss the nomenclatural “corrections” that WELTER-SCHULTES & al. have proposed with respect to the molluscan list of Fauna Europaea. Nothing will be said regarding the spelling of names of authors (as this is partly a matter of opinion – this is in contrast to the former, which is based on the Code or on verifiable evidence). In addition, nothing will be said about the numerous taxonomic decisions (and thus nomenclatural consequences) they made at either the genus level (e.g. *Physa* versus *Physella* versus *Haitia*, *Balea* versus *Alinda* versus *Plicaphora*, *Oxychilus* versus *Morlina* versus *Mediterranea*, etc.), the subgenus level (with the exception of *Limax* and *Helix*: all subgenera systematically refused!), the species level, or at the subspecies level (with the exception of *Granaria*, *Abida* and *Clausilia*: all subspecies systematically refused!). The taxonomic decisions by WELTER-SCHULTES & al. regarding the status of (sub)species has resulted in a sharp decrease in the number of endemic molluscan taxa living in France. Fortunately, the dramatic decline of the biodiversity of France sensu WELTER-SCHULTES & al. is on paper/cyberspace only, not in Nature itself.

What nomenclatural corrections are indeed correct, compared to the latest January 2011 Fauna Europaea version at www.faunaeur.org?

- *Tudorella sulcata*: date should indeed be 1805, not 1801
- *Cochlodina triloba*: date should indeed be 1878, not 1870 (an obvious typing error)
- *Limax alpinus*: date should indeed be 1822, not 1821
- *Arion anthracius*: date should indeed be 1866, not 1886 (an obvious typing error)
- *Urticicola moutonii*: date should indeed be 1847, not 1848

These are the only corrections that needs to be executed with regard to Fauna Europaea.

But what about the other “grand nombre d’erreurs nomenclaturales” of Fauna Europaea? The status of all other “errors” of Fauna Europaea and the subsequent proposed “corrections” by WELTER-SCHULTES & al. (2011) are discussed below. The cases regarding *Helix crombezi* LOCARD, *Pisidium globulare* CLESSIN, and *Pisidium lilljeborgii* CLESSIN have already been addressed in my previous paper; I will here use the same format.

***Gyraulus rosmaessleri* (AUERSWALD, 1852) [not: *Gyraulus rosmaessleri* (SCHMIDT, 1851)]**

The name “*Planorbis Rosmaessleri*” has been validly introduced in volume 8 of the journal “*Zeitschrift für Malakozoologie*”. This journal was published 12 times a year; each “Heft” (number) counted 16 pages. It was the intention to publish every month one number (Heft 1 in January, and Heft 12 in December). Although indeed 12 numbers were published for each calendar year, the publication was often delayed. Fortunately, in quite some cases the publication date is indicated at the bottom of the last page of each number. Volume 7 for the year 1850 was published in 1850 (numbers 1, 2, 3, 4, 5, 6 and 7) and 1851 (numbers 8, 9, 10, 11 and 12: at February, February, March, April and April, respectively). Volume 8 for the year 1851 was published in 1851 and 1852. Numbers 1, 2, 3, 4, 5, 6, and 7 are published at May, June, July, 15 July, July, August and September, respectively. Unfortunately, a publication date is not given in numbers 8, 9, 10 and 11, but number 12 is published on 15 January 1852 (as mentioned at the bottom of page 192, the last page of number 12). Also the title page of volume 8 (for the year 1851) is dated 1852. Number 1 and 2 of volume 9 are published at 15 February 1852 (according to page 16) and 25 March 1852 (according to page 32), respectively, which fits within the sequence of 8 (12). WELTER-SCHULTES & al. (2011: 11, 12) “corrected” the publication

date of *Gyraulus rossmaessleri*, that was published in 8 (12), from 1852 into 1851, but as stated above there is simply not a shred of evidence that 8 (12) has been published in 1851. WELTER-SCHULTES & al. (2011: 11, 12) consider SCHMIDT the author of *Gyraulus rossmaessleri*, although AUERSWALD has so far been considered the author of *Gyraulus rossmaessleri*. It was SCHMIDT who published the name and description in one of his papers, but in the text it is stated: “Obgleich ich dem Wunsche des Autors, seine Entdeckungen zu veröffentlichen und mit ein Paar Worten zu begleiten, gern nachkomme....“. The name was published as „*Planorbis Rossmuessleri* AUERSWALD“ under the subtitle “Ueber einen von Hern. BERNH. AUERSWALD, Lehrer der ersten Bürgerschule in Leipzig, entdeckten neuen *Planorbis*“. The entire description is placed between quotes, indicating that the description originates from AUERSWALD. In the same paper, SCHMIDT describes *Helix margaritacea* by himself (“m.” = mihi); this description is not placed between quotes, being further evidence that the description of the new *Planorbis* indeed originates from AUERSWALD. Thus, the name and description is from AUERSWALD; according to Article 5.1.1 of the ICZN, AUERSWALD is the author. That SCHMIDT provided additional remarks to the species itself (at the request of AUERSWALD!) is of no relevance for the authorship. In conclusion, the name of the species that is known as *Gyraulus rossmaessleri* (AUERSWALD, 1852) should not be changed into *Gyraulus rossmaessleri* (SCHMIDT, 1851).

***Cochlostoma conicum* (VALLOT, 1801)**

According to WELTER-SCHULTES & al. (2011: 7), the publication of VALLOT (1801) does not satisfy the provisions of Article 8.1.1 of the ICZN. The Article reads as follows: “it must be issued for the purpose of providing a public and permanent scientific record”. The paper (“Exercice sur l’histoire naturelle”) of VALLOT is a pamphlet of 8 pages, published by the École centrale du département de la Côte d’Or, and printed by L. N. FRANTIN (Dijon). It is not clear to me, why this paper does not satisfy the above mentioned provisions. It is extremely rare nowadays, but it was (and still is) accessible. For example, DROUËT (1855: 7) refers to the paper of VALLOT, and mentions: “Ce catalogue est extrêmement rare. Je n’en connais qu’un exemplaire, conservé à la bibliothèque publique de Dijon“. There are no synonyms available for *Cochlostoma conicum*. If one considers the publication of VALLOT as invalid, a new name has to be introduced for this taxon. Strangely, this was not done by WELTER-SCHULTES & al. They now continue to use - in their opinion - an “invalid” name, which is certainly not in line with the Code!

***Cochlostoma simrothi* (CAZIOT, 1908) [not: *Cochlostoma simrothi* (POLLONERA & CAZIOT, 1908)]**

This taxon is often attributed to POLLONERA. It was FALKNER & al. (2002: 68-69) who correctly changed the authorship into CAZIOT. The name is validly introduced in a paper by CAZIOT (1908) dealing on the malacofauna of the Vallée del la Roya (France), sampled by CAZIOT in accompany with POLLONERA. It is published as “*Pomatias Simrothi*, POLLONERA, sp. nov.”. However, there is no evidence in the text that the description is from POLLONERA. The same is the case with *Pomatias patulus* var. *elongata*, *Pomatias cazioti*, and *Pomatias acutus* that are introduced in the same way in the paper. According to Article 50.1.1 of the ICZN, CAZIOT should be the author of all these taxa. WELTER-SCHULTES & al. (2011: 7) consider POLLONERA & CAZIOT the authors of *Cochlostoma simrothi*. They are the first to propose this. This proposal is not in agreement with the Code; according to Article 50.1.1 it is either POLLONERA or CAZIOT, not both. A new Article would need to be incorporated in the Code to accommodate/validate the proposal of the authors.

It should be stressed, that in the same paper, “*Coryna Locardi*, C. POLLONERA” is introduced. The authorship of that taxon should be contributed to POLLONERA indeed. The reason is that CAZIOT (1908: 467) stated: “POLLONERA établir une description plus complète et plus vraie avec le dessin de cette intéressante espèce. Nous la reproduisons ci-après”. Thus, the name, the description, and the figure originate from POLLONERA. It is a synonym of *Argna biplicata biplicata* (MICHAUD, 1831).

***Physella* HALDEMAN, 1842 [not: *Physella* HALDEMAN, 1843]**

There is no need to repeat it again: see BANK, FALKNER & VON PROSCHWITZ (2007: 46). According to WELTER-SCHULTES, wrappers have no nomenclatural status. An amazing statement.

***Oxyloma sarsii* (ESMARK, 1886) [not: *Oxyloma sarsii* (ESMARK & HOYER, 1886)]**

See the remarks under *Pisidium lilljeborgii* of my previous paper (BANK 2011: 7-10).

***Hypnophila boissii* (DUPUY, 1851) [not: *Hypnophila boissii* (DUPUY, 1850)]**

For the correction of the publication date of 1850 into 1851 see FALKNER & al. (2002: 245).

***Pupilla sterrii* (VON VOITH, 1840) [not: *Pupilla sterrii* (FORSTER & VOITH, 1840)]**

Mentioned for the first time in the paper of FORSTER (1840: 469-470) as “*P.[upa] sterrii* DE VOITH”. In the text it is stated: “Diese neue Art wurde von Herrn Theologen STERR bei Abach lebend entdeckt, von Hrn. Direktor v. VOITH ihm zu Ehren benannt und auf folgende Art beschrieben:“ [follows an extensive latin description of VON VOITH]. Thus, the name and the description originates from I. VON VOITH. The additional remarks by FORSTER (e.g. his summary of the description of VON VOITH) has no effect on the authorship of the taxon.

***Pyramidula pusilla* (VALLOT, 1801) [not: *Pyramidula pusilla* GITTENBERGER & BANK, 1996]**

See the discussion mentioned above under the heading *Cochlostoma conicum* (VALLOT, 1801). WELTER-SCHULTES & al. (2011: 15) consider *Pyramidula pusilla* GITTENBERGER & BANK, 1996 as the correct name for this taxon; they thus follow the same aberrant opinion of MARTÍNEZ-ORTÍ & al. (2007: 78-79). This is not correct. If one considers the publication of VALLOT as invalid (the proposed scenario by WELTER-SCHULTES & al.), the name *Pyramidula pusilla* is then indeed made valid for the first time by GITTENBERGER & BANK. The implication is in addition, that the neotype that these authors selected for *Helix pusilla* VALLOT, 1801 should become the holotype of *Pyramidula pusilla*. However, there are several older names that are most probably available for the taxon under consideration: *Turbo myrmecidis* SCACCHI, 1833; *Helix spirula* A. VILLA & G. B. VILLA, 1841; *Helix aliena* L. PFEIFFER, 1841; *Delomphalus saxatilis* W. HARTMANN, 1842; and *Helix rupestris* var. *pini* ADAMI, 1886. These names have apparently been overlooked by WELTER-SCHULTES & al. (2011). In the above proposed scenario the name “introduced” by GITTENBERGER & BANK then becomes a synonym of one these old names. How odd can one make it?

***Solatopupa psarolena* (BOURGUIGNAT, 1858) [not: *Solatopupa psarolena* (BOURGUIGNAT, 1859)]**

According to WELTER-SCHULTES & al. (2011: 16) the publication date is 1859. They state: “Le texte de la description est daté de 1859, celle de la planche de 1858, mais le nom d’espèce n’a pas été mentionné sur la planche“. Consultation of plate 19 (figure 1-2) published in 1858 in the “Revue et Magasin de Zoologie pure et appliquée” shows at the bottom of the plate the name “*Bulimus psarolenus*”. Thus, according to Article 12.2.7 of the ICZN, the name is validly introduced in 1858, not in 1859. The plate is renumbered in the second volume of the “Aménités malacologiques” as plate 15; it is an identical copy showing the same name on the same place. How is it possible that the three authors state that there is no name on the plate, despite its presence, and thus introduce a wrong publication date for this taxon?

***Granaria stabilei* (E. VON MARTENS, 1865) [not: *Granaria stabili* (MARTENS, 1865)]**

This name was first introduced as *Pupa Stabili* by E. VON MARTENS (1865) in the Zoological Record. STABILE (1868: 34) changed the name into *Pupa Stabilei* (“Per errore tipografico è stampato *Stabili*”). The emendation was accepted by e.g. GITTENBERGER (1973: 53). But under the new (fourth) Code, this is an unjustified emendation. However, Article 33.2.3.1 states that “when an unjustified emendation is in prevailing usage and attributed to the original author and date it is deemed to be a justified emendation”. Thus, we can continue to use the name that is in prevailing use, which is *stabilei*.

***Abida vergniesiana* (KÜSTER, 1847) [not: *Abida pyrenaearia vergniesiana* (KÜSTER, 1850)]**

In his revision of the genera *Pupa*, *Megaspira*, *Balea* and *Tornatellina* for the “Systematisches Conchylien-Cabinet von MARTINI und CHEMNITZ”, KÜSTER introduced for the first time the name *Pupa vergniesiana*. He did this on page 103-104 (Lieferung 97, published 1850) and provided a picture (figure 13-16) on plate 14 (Lieferung 67, published three years earlier in 1847). There is no name on the plate itself. However, it is likely that on the wrappers of the Lieferung 67 the name *Pupa vergniesiana* (in combination with the figure number) is printed. Thus, following Article 12.2.7 of the ICZN, the publication date is 1847. I have seen several of the wrappers of the KÜSTER monograph decades ago (they show the legends of the plates), but have not made notes on this particular plate. That there must be a name on the wrappers is most likely, as one finds in PFEIFFER (1848) on page 342 of the second volume of his “Monographia Heliceorum viventium” the following sentence: “[*Pupa*] *Vergniesiana* CHARP. KÜSTER t. 14 f. 13-16”. As the text of PFEIFFER (1848) was published two years earlier than the text of KÜSTER (1850), it follows that the plate was accompanied with a legend (printed on the wrappers), otherwise PFEIFFER cannot refer to the plate in combination with the name. It seems likely that *vergniesiana* is a writing error of PFEIFFER; it should be *vergniesiana*. If these as-

sumptions are not followed, the taxon must carry the name *Abida vergnesiana* (L. PFEIFFER, 1848). Although the name was published in synonymy by PFEIFFER, it was made available by KÜSTER (1850: 103) by referring to PFEIFFER (see Article 11.6.1 of the ICZN). However, the here presented evidence is, as before, in favour of *Abida vergnesiana* (KÜSTER, 1847): thus, there is no need for changing the name, the author, or the date.

***Chondrina bigorriensis* (DES MOULINS, 1835) [not: *Chondrina pusilla* (DES MOULINS, 1835)]**

This taxon was published by DES MOULINS (1835) under two names: *Pupa megacheilos* var. *pusilla* and *Pupa bigorriensis*. Almost all subsequent authors used the name *bigorriensis*. PILSBRY (1918: 30) formalized the use of the name *bigorriensis* by selecting *bigorriensis* over *pusilla* by acting as a First Reviser in the sense of Article 24.2.1 of the ICZN. The nomenclature of this taxon has been clearly delineated by GITTENBERGER (1973: 188, with additional notes on page 146). For over a century the name *bigorriensis* is used for this taxon. We would like to stress that neither PILSBRY nor any other author treated *bigorriensis* as a substitute name for *pusilla*, as *pusilla* was never considered a junior secondary homonym of *Vertigo pusilla* O. F. MÜLLER, 1774. Therefore, Article 59.3 cannot be applied in this case, as WELTER-SCHULTES & al. (2011: 16) propose. These authors want to change the name into *Chondrina pusilla* (DES MOULINS, 1835), on false grounds, and despite the common use of *Chondrina bigorriensis* (DES MOULINS, 1835) over the last century. This is a serious violation of the Preamble of the Code: “The objects of the Code are to promote stability and universality in the scientific names of animals”.

***Papillifera* W. HARTMANN, 1842 [not: *Papillifera* HARTMANN, 1843]**

The work of HARTMANN, namely the “Erd- und Süßwasser-Gasteropoden”, was published in 8 parts (Heft 1-8) between 1840 and 1844. HEPPELL (1966) made a compilation of this work, and referred to several sources used to substantiate his findings. The name *Papillifera* was first validly introduced on page 156. According to HEPPELL (1966: 87), Heft 5 runs from page 117-148 (1842), Heft 6 runs from page 149-156 (1842), Heft 7 runs from page 157-204 (1843), and Heft 8 runs from page 205-227 (1844). WELTER-SCHULTES & al. (2011: 19) want to change the publication date into 1843, thus assuming that page 156 was published in part 7. On his Website of AnimalBase WELTER-SCHULTES states the following: “The composition of the Berlin copy suggests that pp. 155-156 were not issued in 1842 but in the next Heft (6, 1843) with the descriptions of the other clausiliid species”. It should be noted that he actually refers to Heft 7; this is the Heft where the other clausiliids are described. If he is right, then the publication date of *Papillifera* should be changed into 1843. How does the copy of Berlin look like? In the AnimalBase website the following is stated: “SBPK Berlin < Lq 5935>, text and plates bound separately in Berlin, plates just collected, not bound, volume incomplete, only until p. 154”. Based on this incomplete copy, WELTER-SCHULTES draws the conclusion that Heft 6 stops at page 154, and that Heft 7 starts at page 155. Consultation of the work of HARTMANN reveals that it is printed on sheets containing 8 pages each. The sheets are numbered (have signatures) throughout the book. Sheet 18 starts at page 117, sheet 22 starts at page 149, sheet 23 starts at page 157, and sheet 29 starts at page 205. This fits exactly with the beginning of Heft 5, 6, 7 and 8, respectively, as provided by HEPPELL. If we follow the assumption of WELTER-SCHULTES, sheet 22 must have two blank pages (which is not present in the book), and the start of Heft 7 must be printed on a separate page followed by sheet 23. Clearly, this is not the case. The discussion can thus be closed in favour of 1842.

***Papillifera papillaris* (O. F. MÜLLER, 1774) [not: *Papillifera bidens* (LINNAEUS, 1758)]**

The nomenclature of this taxon has a long and confusing history, and has been debated once again quite recently. The question is: what is the identity of *Turbo bidens* LINNAEUS, 1758, and what is the identity of *Helix papillaris* O. F. MÜLLER, 1774? The history and solution has been explained in detail by KADOLSKY (2009), a paper that is not mentioned by WELTER-SCHULTES & al. (2011: 19). The identity of the nominal species *Helix papillaris* is fixed by the designation of a neotype by GIUSTI & MANGANELLI (2005: 132, figure 1). There is no confusion about the identity of this name. The problem arises with regard to the identity of *Turbo bidens*. A neotype was selected for *Turbo bidens* by FALKNER & al. (2002: 113): it is the shell figured by GUALTIERI (1742, plate 4 figure E). This neotype selection is invalid for several reasons, as delineated by KADOLSKY (2009: 26). A new neotype for *Turbo bidens* has been selected by KADOLSKY (2009: 27-28, figure 4). This shell is not conspecific with *Helix papillaris*, but with *Clausilia incisa* KÜSTER, 1876, the latter being a representative of the genus *Cochlodina* A. FÉRUSAC, 1821. Thus, the local endemic *Cochlodina incisa* (KÜSTER, 1876)

should be renamed into *Cochlodina bidens* (LINNAEUS, 1758). With this action the name of the widespread species *Papillifera papillaris* is secured. The actions of GIUSTI & MANGANELLI and KADOLSKY are in line with the Code, and should therefore be followed. Using the name *Papillifera bidens*, as WELTER-SCHULTES & al. do (2011: 18), is not in line with the Code, and is thus rejected for obvious reasons.

***Macrogastra attenuata* (ROSSMÄSSLER, 1835) [not: *Macrogastra basileensis* (ROSSMÄSSLER, 1835)]**

This species was known for a long time as *Macrogastra lineolata* (HELD, 1836). However, there are two older synonyms available: *Clausilia basileensis* ROSSMÄSSLER, 1835 and *Clausilia attenuata* ROSSMÄSSLER, 1835. Both names are published in the same work (“Iconographie”, (1) 1 (1): 78, 80) and are thus equally available. It was NORDSIECK (1993: 37) who gave precedence of the name *attenuata* over that of *basileensis*. Because of this nomenclatural act (NORDSIECK acted as the First Reviser), the name of the species is according to Article 24.2.1 of the ICZN *Macrogastra attenuata*. Thus, by using *Macrogastra basileensis*, WELTER-SCHULTES & al. (2011: 18, 19) do not act according to the Code, and their alternative is thus rejected for obvious reasons.

***Macrogastra rolphii* (TURTON, 1826) [not: *Macrogastra rolphii* (TURTON, 1831)]**

The name *Clausilia rolphii* has been placed by the International Commission of Zoological Nomenclature on the “Official List of Specific Names in Zoology” by means of Opinion 336 (1955), with TURTON, 1831 as its author. However, this name has been validly published a few years earlier by TURTON (1826), in another publication. Article 80.6 of the ICZN regulates, amongst others, the status of names placed in an Official List. Relevant in this case is Article 80.6.4: “If a name entered in an Official List is thought to be a synonym of another available name their relative precedence is determined by the normal application of the Code”. Clearly, *Clausilia rolphii* TURTON, 1831 is a synonym of *Clausilia rolphii* TURTON, 1826. According to the Code, TURTON 1826 is the author of *rolphii*.

***Balea heydeni* VON MALTZAN, 1881 [not: *Balea sarsii* MENKE & PFEIFFER, 1847]**

The name *Balea lucifuga* LEACH Mss. first appeared as a synonym of *Balea fragilis* in a publication of GRAY in 1824. The name *Balea lucifuga* was not made available by GRAY as it is a nomen nudum (in synonymy). However, BOURGUIGNAT made the name available by adopting it as the name of a taxon (with *Balaea lucifuga* LEACH Mss. as its synonym). By means of Article 50.7 the author of *Balea lucifuga* is GRAY, 1824 (and its type series is defined in Article 72.4.3). GITTENBERGER (2010) selected a neotype for *Balea lucifuga* GRAY, 1824; the shell is identical with the species that is now known as *Balea perversa* LINNAEUS, 1758. Thus, the name *lucifuga* GRAY, 1824 has no priority over *heydeni* VON MALTZAN, 1881. WELTER-SCHULTES & al. (2011: 20) argues that the species delineated by BOURGUIGNAT under *Balia lucifuga* is not identical with *Balea perversa*, but with *Balea heydeni*. They consider BOURGUIGNAT as the author of *lucifuga*. However, the Code is clear with respect to authorship and type series (see above). It is of no relevance that BOURGUIGNAT had another species in his mind than GRAY: the Code does not specify that the author who made the name available should have had the same species in his hand as the author who first mentioned the name in synonymy.

WELTER-SCHULTES & al. (2011: 18, 20) replace *Balea heydeni* with the older name *Balea sarsii* MENKE & PFEIFFER, 1847. This name appears on page 84 of volume 4 of the “Zeitschrift für Malakozoologie” (1847). The article is anonymous; therefore WELTER-SCHULTES & al. consider MENKE & PFEIFFER the authors, being the editors of the journal. However, in the “Inhalt” it is stated that PFEIFFER is the author. Thus, it should be *Balea sarsii* L. PFEIFFER, 1847. It is unclear to me on what basis it is believed that this is an older synonym of *heydeni* (and not a younger synonym of *perversa*) as type-material has not been studied and the type locality is rather imprecise: “Norvegia”, which is Norway. Therefore, to promote stability, the use of the name *heydeni* is continued.

***Cecilioides veneta* (STROBEL, 1855) [not: *Cecilioides janii* (DE BETTA & MARTINATI, 1855)]**

The nomenclature of this taxon has been discussed by BANK, FALKNER & GITTENBERGER (2000) in depth and will not be repeated here again. According to WELTER-SCHULTES & al. (2011: 20) our lectotype selection is wrong, as the lectotype does not originate from Lesina (Hvar). However, the type locality of *veneta* is not the Croatian island Hvar (as can already be concluded from the name *veneta*). The name *Achatina veneta* turned up in the publication of STROBEL (1855: 137) as a synonym of *aciculoides* auct. (non DE CRISTOFORI & JAN, 1832) and *acicula* sensu DE BETTA, 1852 (non O. F.

MÜLLER, 1774). The type series of a name published in synonymy is defined in Article 72.4.3 of the Code and cannot be neglected by WELTER-SCHULTES & al., who want to use the name *janii* DE BETTA & MARTINATI, 1855 for the taxon under consideration. It should be noted that a previous proposal for the conservation of the specific name *janii* was rejected by the Commission in its Opinion 2080 (2004). Thus, there is no legal ground to use the name *janii*.

***Testacella haliotidea* DRAPARNAUD, 1801 [not: *Testacella haliotidea* LAMARCK, 1801]**

LAMARCK (1801: 96) described the genus *Testacella*, and included one (at that time undescribed) species, “*Testacella haliotoides*” from Tenerife (MAUGÉ leg.). According to Article 12.2.6 “a combined description or definition of a new nominal genus and a single new nominal species provides an indication for each name”. Thus, both *Testacella* LAMARCK, 1801 and *Testacella haliotoides* LAMARCK, 1801 are available names. DRAPARNAUD (1801: 99) described a few months later *Testacella haliotidea*; this is not identical with the species of LAMARCK and is also not a homonym of *haliotoides*. The name of LAMARCK has widely been forgotten, or ignored, or its identity has often been falsely interpreted (e.g. most recently by WELTER-SCHULTES & al. (2011: 21), they also wrongly changed the orthography of LAMARCK’s name into *haliotidea*). *Testacella haliotoides* LAMARCK, 1801 is a senior synonym of *Testacella maugéi* A. FÉRUSAC, 1819. An application has to be submitted to the ICZN in order to preserve the widely used name *maugéi*. The name *Testacella* first appeared as a nomen nudum in Table V of the “Leçons d’Anatomie Comparée” of CUVIER (1800).

***Lucilla singleyana* (PILSBRY, 1889) [not: *Lucilla singleyana* (PILSBRY, 1890)]**

The publication date is 1889, not 1890 (*Zonites singleyanus* PILSBRY, 1889, Proc. Acad. nat. Sci. Philadelphia, 41: 84). H. B. BAKER (1929: 264) and PILSBRY himself (1948: 636) have cited the wrong date in a manner that suggested bibliographic correctness, and this error has been copied for decades. The correct publication date (May 14, 1889) is given by CLENCH & TURNER (1962: 166) who curiously omitted the name *singleyanus* in their Catalogue. This was already pointed out previously (BANK, FALKNER & VON PROSCHWITZ, 2007: 52). WELTER-SCHULTES & al. (2011: 21) apparently want to re-introduce the erratic date of 1890, for reasons that remain unknown.

***Hawaiia minuscula* (A. BINNEY, 1841) [not: *Hawaiia minuscula* (BINNEY, 1840)]**

The name *Helix minuscula* was introduced by BINNEY on page 435 of the third volume of the Boston Journal of Natural History. This volume was published in three parts: number 1/2 (page 1-280, “January, 1840”), number 3 (page 281-404, “July, 1840”), and number 4 (page 405-532, “November, 1840”). The title page shows “1840-1841”, and at the bottom of the title page the date “MDCCCXLI” is given. It thus seems likely that number 4 is published in 1841. Indeed, on page 438, 465 and 483 of number 4 it is stated “Read March 17th, 1841”, “Read March 17th, 1841” and “Read February 3d, 1841”, respectively. Furthermore, in the “Bibliography of North American Conchology previous to the year 1860” by BINNEY it is stated on page 348 that number 4 of volume 3 of the journal is published at “Nov. 1841”. COWIE (1997: 39, 45) already noted the correct publication date. Given this evidence, it is difficult to believe that the year of publication is 1840, as WELTER-SCHULTES & al. (2011: 22) are suggesting.

***Retinella* P. FISCHER, 1877 [not: *Retinella* SHUTTLEWORTH, 1877]**

This name has been placed by the International Commission of Zoological Nomenclature on the “Official List of Generic Names in Zoology” by means of Opinion 335 (1955: 49, 58), with FISCHER as its author. The type species is *Helix olivetorum* GMELIN, 1791. According to WELTER-SCHULTES & al. (2011: 23) the author is SHUTTLEWORTH.

Because of this Official List a further discussion is not necessary anymore. However, I would like to make a statement, to ensure that the discussion ends here once and for all. The name *Retinella* appears for the first time on page 7 and at the top of plate 2 of the work “Notitiae Malacologicae”, published 1877 in Leipzig. The 15 plates are from SHUTTLEWORTH, the text (16 pp.) is from FISCHER. The text was prepared by FISCHER after the death of SHUTTLEWORTH, and is mostly an explanation of the plates. If one considers plate 2 as the original source of *Retinella*, and thus SHUTTLEWORTH as the author, one runs immediately into problems. On the plate, four taxa are figured and mentioned: *fuscosa*, *balmei*, *oppressa* and *duboisii*. All shells belong to the genus *Oxychilus*, none to *Retinella* in its current meaning (the supposed type species *olivetorum* is not on the plate). Thus, accepting SHUTTLEWORTH as its author, the genus *Retinella* as currently known would need another name (that

would be *Aegopina* KOBELT, 1878). Fortunately, *Retinella* is on the Official List with FISCHER as its author and is thus a conserved name; in the text of FISCHER the taxon *olivetorum* is mentioned as one of the species. The stability of this name is guaranteed by the ICZN itself.

***Limacus maculatus* (KALENICZENKO, 1851) [not: *Limax (Limax) ecarinatus* O. BOETTGER, 1881]**

This name was introduced by KALENICZENKO as *Krynickillus maculatus*, and was later transferred to the genus *Limax* (and subsequently to *Limacus*, a subgenus of *Limax* that is now considered genus-rank). The well-known name of KALENICZENKO is, in the combination of *Limax maculatus* (KALENICZENKO, 1851), the junior secondary homonym of *Limax maculatus* NUNNELEY, 1837. The latter name has not been used as a valid name after 1899 (it is a synonym of *Limax maximus* LINNAEUS, 1758). That means that Article 23.9 of the Code comes into place (reversal of precedence): prevailing usage must be maintained when (1) the senior homonym has not been used as a valid name after 1899, and (2) the junior homonym has been used frequently as a valid name. Apart from that, *Limacus maculatus* (KALENICZENKO, 1851) is not a homonym anymore, so we are dealing with Article 59.4, stating that a species-group name rejected after 1960 on grounds of secondary homonymy is to be reinstated as valid by an author who considers that the two species-group taxa in question are not congeneric.

***Elona quimperiana* (BLAINVILLE, 1821) [not: *Elona quimperiana* (FÉRUSSAC, 1821)];**

***Caracollina lenticula* (MICHAUD, 1831) [not: *Caracollina lenticula* (FERUSSAC, 1821)]**

The work of FÉRUSSAC (1821) is rather problematic with respect to e.g. the used nomenclature. A nice example is the case of *Helix (Helicigona) barbata*. I will not repeat the discussion here, but instead refer to GITTENBERGER & GROH (1986), GITTENBERGER (1991), KADOLSKY (1990; 1991) and OPINION 1690. The ICZN ruled that *Helix (Helicigona) barbata* FÉRUSSAC, 1821 is a nomen nudum. It was published as a name without a description, but with α and β as variants, that were diagnosed in the text. KADOLSKY (1990: 101) argued as follows: “The usage of Greek lower case letters for infra-specific categories (‘varieties’) was common in the period: the content of the diagnoses implies a variation from the norm which is not specified” and “Obviously, the variants do not define or describe the ‘typical’ form of the nominal species *barbata*; this name is therefore not available from this [1821] work”. The ICZN accepted this reasoning, and declared *Helix (Helicigona) barbata* FÉRUSSAC, 1821 a non-available name. *Helix (Helicigona) lenticula* FÉRUSSAC, 1821 is published on the same page as *barbata*, and shows a similar text structure: the nominal species is not defined, only a variety α is diagnosed. It thus seems logical to follow the ICZN concerning this (*lenticula*) and other cases (e.g. *quimperiana*, *cretica*). As KADOLSKY (1991: 244) stated: “in the long run stability will best be served by a consistent application of the Code to problematical works”.

***Xerolenta obvia* (MENKE, 1828) [not: an invalid name]**

This case has been solved for a long time. Both VON MARTENS (1891: 34-35) and GITTENBERGER (1975: 5-6) showed that the name *obvia* MENKE should be used for the taxon under consideration. GITTENBERGER considered it a nomen novum for *Helix neglecta* W. HARTMANN, 1821 (non DRAPARNAUD, 1805). According to WELTER-SCHULTES & al. (2011: 30) the replacement name is not in agreement with Article 72.7; to be honest, I don’t see why. But let us assume -theoretically- that the authors are right. Then the name *obvia* can still be used. One can consider the reference of MENKE to *Helix neglecta* HARTMANN as an “Indication” in the sense of Article 12.2.1 (the word indication denotes, amongst others, “a bibliographic reference to a previously published description or definition”). It was already VON MARTENS (1891) who pointed into this direction with his remark “wenn die Verweisung auf eine schon publicirte Beschreibung und Abbildung betreffs der Priorität die eigene Beschreibung ersetzen kann, so hat der Name *obvia* den Altersvortrag“. It is bizarre that some authors still want to dispute a case that has been settled now for more than a century.

***Chilostoma acrotricha* (P. FISCHER, 1877) [not: *Chilostoma acrotricha* (FISCHER & DUPUY, 1877)]**

This taxon was mentioned for the first time by P. FISCHER (1877: 52-53) as “*Helix Moulinsi*, FARINES (emend.). – Var. *acrotricha* (*Helix acrotricha*, FISCHER, mss.)”, followed by the locality and a description of the shell. After the description, FISCHER mentioned the following: “M. l’abbé DUPUY m’a envoyé, au sujet de la variété la note suivante”, after which a long text between quotes is following. Both the name (*acrotricha*) and the description is from FISCHER. It is nowhere stated that DUPUY should be the co-author of the taxon. Furthermore, Article 50.1.1 is not applicable. If it was, the author

should be either FISCHER or DUPUY, not both. A new Article should be incorporated in the Code to make the combination FISCHER & DUPUY possible. WELTER-SCHULTES & al. (2011: 32) are the first who propose this combination.

***Chilostoma squamatinum* (ROSSMÄSSLER, 1835) [not: *Chilostoma squammatinum* (MOQUIN-TANDON, 1855)]**

The name *Helix squammatina* was first published as a nomen nudum by FÉRUSSAC (1821: 38) as var. α of *Helix (Helicella) cornea*. The name was mentioned for the second time, but now as *Helix squammatina*, by ROSSMÄSSLER (1835: 6), also as a nomen nudum, but in the synonymy of *Helix cornea*. Since the name was later used as an available name (see Article 11.6.1), the authorship belongs to ROSSMÄSSLER, 1835 (see Article 50.7: “its author is the person who published it as a synonym”). WELTER-SCHULTES & al. (2011: 32) consider MOQUIN-TANDON, 1855 as the author. However, even if they are not willing to accept the Code, or have a different interpretation of the Code, the author can never be MOQUIN-TANDON. The taxon was earlier diagnosed by DUPUY in 1848 (157: L’*Helix squammatina* n’est qu’une légère variété un peu plus petite, plus foncée et plus slide que le type”; plate 6 fig. 5d “Variété (*H. Squammatina*) vue en dessous”).

***Cornu aspersum* (O. F. MÜLLER, 1774) [not: *Helix (Cantareus) aspersa* (O. F. MÜLLER, 1774)]**

Several remarks are necessary. First, *Cantareus* RISSO, 1826, has nothing to do with *aspersa*: it is a monotypic genus with *aperta* BORN, 1778 as its type species. The taxa *aspersa* and *aperta* belong to different genera. Second, if one considers *Cornu* BORN, 1778 an invalid name, then the name *Cryptomphalus* CHARPENTIER, 1837 should be used for *aspersa*. Third, the combination *Helix (Cantareus) aspersa* (MÜLLER, 1774) is wrong: the name was introduced by MÜLLER as *Helix aspersa*, so MÜLLER should not be enclosed in parentheses (Article 51.3 of the ICZN). The presence of a sub-generic name, i.e. “*Cantareus*”, has no effect on the use of parentheses (Article 51.3.2). The same mistake was made by WELTER-SCHULTES & al. (2011: 32) for *Helix (Cantareus) aperta* (BORN, 1778): the taxon was described by BORN as *Helix aperta*. The currently accepted name is *Cantareus apertus* (BORN, 1778). Fourth, the discussion of the validity of the name *Cornu* will soon come to an end: A request for a ruling on the availability of the generic name by the ICZN has been published by COWIE (2011) in the Bulletin of Zoological Nomenclature in order to stabilize the name as *Cornu aspersum*. I fully support this proposal. Not surprisingly, the opinion of WELTER-SCHULTES & al. (2011: 32, 33) is different of that of the submitted application.

***Sphaerium ovale* (A. FÉRUSSAC, 1807) [not: an invalid name].**

WELTER-SCHULTES & al. (2011: 35) stated the following: “*Sphaerium ovale* (FERUSSAC 1807) n’est pas un nom disponible; nous le maintenons dans l’attente de pouvoir le remplacer par un nom valide“. First of all: the name *ovale* is an available name. It is a replacement name (new name; nomen novum) for *Cyclas lacustre* sensu DRAPARNAUD, 1805 [non *Tellina lacustris* O. F. MÜLLER, 1774]. We are dealing here with a misidentification of DRAPARNAUD regarding *lacustris*. Article 72.4.2 of the ICZN reads as follows: “If a new nominal species-group taxon is based, in whole or in part, on a published misidentification by a earlier author, the type series consists of or includes the specimen or specimens which had been misidentified“. Thus, the type series of *ovale* consists of the material that DRAPARNAUD misidentified as *lacustre*. From this material (the collection of DRAPARNAUD is still present in Vienna) a lectotype was selected by FALKNER (2000: 33, fig. a-e) for *ovale*. In conclusion, the name *ovale* is available and its identity is secured by a lectotype that was selected according to the rules of the Code. If WELTER-SCHULTES & al. believe that the name is unavailable, they have to submit an application to the ICZN. By the way, another valid name is available: *radiatum* WESTERLUND, 1897 (a lectotype for *radiatum* was selected by KORNIUSHIN, 2001: fig. 13Aa-c), which is a synonym of *ovale*. This name has apparently been overlooked by WELTER-SCHULTES & al.

***Pisidium pseudosphaerium* J. FAVRE, 1927 [not: *Pisidium pseudosphaerium* EHRMANN, 1933]**

The authorship of this species has been a matter of intense dispute. The following authors have been considered: FAVRE, 1927; EHRMANN, 1933; VAN BENTHEM JUTTING & KUIPER, 1942; VAN REGTEREN ALTENA, 1945; and finally SCHLESCH, 1947. A critical nomenclatural study combined with a lectotype selection by ADLER (1993) finally secured FAVRE as the author of *Pisidium pseudosphaerium*. If WELTER-SCHULTES & al. (2011: 35) do not agree with the conclusion of ADLER, they

have to submit an application to the ICZN. If they are not willing to submit an application, the Code needs to be followed, and according to the Code the author is: J. FAVRE, 1927.

***Mytilopsis* CONRAD, 1858 [not: *Mytilopsis* CONRAD, 1857]**

The genus *Mytilopsis* was validly introduced by CONRAD on page 167 of volume 9 of the “Proceedings of the Academy of natural Sciences of Philadelphia”. According to WELTER-SCHULTES & al. (2011: 35) the publication date is 1857, as at the bottom of page 166+167 it is stated: “June, 1857”. Although volume 9 should have been published in 1857, the volume was partly published in 1857 and partly in 1858 [note: the title page is dated 1858, although the last part of the volume is dated at the bottom of the page as “December, 1857”]. The volume was published in 7 parts; page 167 appeared in part 5. The publication date of part 3 is just before January 7, 1858; the publication date of part 7 is just before May 1, 1858. Thus, page 167 (part 5) must have been published between January 1858 and May 1858. The dates of publication of the “Proceedings” have been collated by ANONYMUS (1913: pages vii-xiv).

Final remarks

LINNAEUS revolutionized animal nomenclature with his binominal system introduced in 1758 in his 10th edition of his work “Systema Naturae”. 250 years later, we are still without a complete and authoritative catalogue of all scientific names of animals. The ICZN was founded in 1895 as a result of the awareness among zoologists of an increasing degree of chaos and controversy in the scientific naming of animals. We have now universally accepted guidelines to ensure stability in zoological nomenclature. The fundamental principles in the Code are rather few, but are complicated by a series of parameters that have to be satisfied. Code compliance determines whether or not the scientific name is available. Adherence to the Code is an example of unity in the zoological sciences, and efforts must be maintained to preserve this situation. This paper is such an effort.

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Frankfurt am Main
Dezember 2011

Herausgeber: Dr. Vollrath Wiese und Prof. Dr. Thomas Wilke, Deutsche Malakozologische Gesellschaft

Redaktion: Dr. Ulrich Bößneck, Hans-Jürgen Hirschfelder, Dr. Ira Richling, Dr. Vollrath Wiese

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Die Zeitschrift ist offen für alle Themenbereiche der Malakozologie. Beiträge zur regionalen Faunistik und Ökologie der Mollusken, Tagungs- und Nomenklaturberichte sowie die Personalien der Gesellschaft gehören zum regelmäßigen Inhalt.

Sie ist in folgenden Literatur-Datenbanken gelistet: Aquaculture and Fisheries Resources, Aquatic Biology, Biological Abstracts (Biosis Philadelphia), Biosis previews, Fish and Fisheries Worldwide (FFW), Ulrich's Periodicals Directory, Zoological Record.

Die Herausgabe der Zeitschrift erfolgt ohne wirtschaftlichen Zweck zur Förderung der Wissenschaft. Über die Annahme von Manuskripten entscheiden die Herausgeber, gegebenenfalls nach der Einholung von Gutachten. Die Autoren sind für den Inhalt ihrer Beiträge verantwortlich.

Titelbild von Heft 86: Stumpfe Federkiemenschnecken *Valvata macrostoma* MÖRCH 1864 (vgl. S. 25ff) (Foto: RICHLING)

Druck: Günther Muchow, Sierksdorfer Str. 14, 23730 Neustadt/Holstein (www.guenthermuchow.de)

Bezugsadresse: Deutsche Malakozologische Gesellschaft
(c/o Haus der Natur – Cismar, Bäderstr. 26, D-23743 Cismar, dmg@mollusca.de)

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