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THE BULLETIN OF ZOOLOGICAL NOMENCLATURE

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The Secretariat
International Commission on Zoological Nomenclature
Lee Kong Chian Natural History Museum
Faculty of Science
National University of Singapore
2, Conservatory Drive, Singapore 117377
Republic of Singapore
(Tel. +65 6516 8364; Fax. +65 6774 8101; e-mail: iczn@nus.edu.sg)

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Cover image: Nanophyes marmoratus (Goeze, 1777), known as the purple loosestrife weevil, illustrated by Mark Russell (published with his kind permission). The genus Nanophyes and its type species Nanophyes marmoratus were placed on the Official Lists of Generic and Specific Names in Zoology by rulings in Opinion 1526 (BZN 46: 67-68). The purple loosestrife weevil is an important biocontrol agent against the European weed Lythrum salicaria L. in North America and an exceedingly diverse species in colour pattern.

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BULLETIN OF ZOOLOGICAL NOMENCLATURE

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Notices

(1) Applications and correspondence relating to applications to the Commission should be sent to the ICZN at the address given on the inside of the front cover and on the Commission website. English is the official language of the Bulletin. Please take careful note of instructions to authors (available online (at http://iczn.org/content/guidelines-case-preparation) as incorrectly formatted applications will be returned to authors for revision. The Commission’s Secretariat will, where possible, answer general nomenclatural (as opposed to purely taxonomic) enquiries and assist with the formulation of applications and, as far as it can, check the main nomenclatural references in applications. Correspondence should preferably be sent by e-mail to iczn@nus.edu.sg

(2) The Commission votes on applications eight months after they have been published, although this period is normally extended to enable comments to be submitted. Comments for publication relating to applications (either in support or against, or offering alternative solutions) should be submitted as soon as possible. Comments may be edited (see instructions for submission of comments at http://iczn.org/content/instructions-comments).

(3) Requests for help and advice on the Code can be made direct to the Commission and other interested parties via the Internet. Membership of the Commission’s Discussion List is free of charge. You can subscribe and find out more about the list at http://list.afriherp.org/mailman/listinfo/iczn-list.

(4) The Commission also welcomes the submission of general-interest articles on nomenclatural themes or nomenclatural notes on particular issues. These may deal with taxonomy, but should be mainly nomenclatural in content. Articles and notes should be sent to iczn@nus.edu.sg

New applications to the Commission

The following new applications have been received since the last issue of the Bulletin (volume 73, part 1, 31 March 2016) went to press. Under Article 82 of the Code, the prevailing usage of names in the applications is to be maintained until the Commission’s rulings on the applications (the Opinions) have been published.


CASE 3710: HETERODONTIDAE Bonaparte, 1845 (Reptilia, Serpentes) and HETERODONTINA Gray, 1851 (Chondrichthyes, Heterodontiformes): proposed resolution of homonymy and stabilization of the use of the generic name Heterodontus Gray, 1851. Savage, J.M. & Briggs, J.C.


**CASE 3712**: *Berriasella jacobi* Mazenot, 1939 (1939) (Cephalopoda, Ammonitina): proposed conservation of specific name. Salazar, C. & Stinnesbeck, W.

**CASE 3713**: *Scarabaeus ferrugineus* Olivier, 1789 (currently *Metacatharsius ferrugineus*) and *Scarabaeus ferrugineus* Palisot de Beauvois, 1809 (currently *Bradyacinetulus ferrugineus*) (Insecta: Coleoptera: *scarabaeoidea*): proposed conservation of the specific names. Smith, A.B.T. & Paulsen, M.J.

**CASE 3714**: *AdeIgidæ* Annand, 1928 (Insecta, Hemiptera, Aphidomorpha): proposed precedence over *pineini* Nüsslin, 1909 and *chermaphidinae* Hunter, 1901. Favret, C.

**CASE 3715**: Punctuated avian species-group names as abbreviations in the zoological appendix of Linnaeus’s *Mantissa Plantarum Altera*, 1771: proposed conservation of *leucorynchus* and *mascarinus* as justified emendations of their respectively punctuated original spellings as *Lanius leucoryn* and *Psittacus mascarin*. Schodde, R., Dowsett, R.J. & Bock, W.J.

**CASE 3716**: *acridina* McCleay, 1821 (Insecta, Orthoptera) and *acridina* Mivart, 1868 (Amphibia, Anura): resolution of homonymy between family-group names. Savage, J.M. & Frost, D.R.

**CASE 3717**: *xylophagidae* Purchon, 1941 (Mollusca: Bivalvia): proposed emendation of the spelling to *xylophagidae* to remove homonymy with *xylophagidae* Fallén, 1810 (Insecta: Diptera). Coan, E.V., Carlton, J.T. & Evenhuis, N.L.

**CASE 3718**: *Phaenocora evelinae* Marcus, 1946 (Platyhelminthes, *typheplaniidae*, *phaenocorinae*): replacement of the syntypes by designation of a neotype. Houben, A. & Artois, T.

**CASE 3719**: *Rhinoceros incisivus* Cuvier, 1822 (currently *Aceratherium incisivium*) Mammalia: Perissodactyla): proposed conservation of usage by replacement of the holotype with a neotype. Demirjian, V.D.

**CASE 3720**: *Tragulus* Brisson, 1762 (Mammalia: Artiodactyla): proposed designation of *Tragulus javensis* Pallas, 1779 as the type species. Demirjian, V.D.

**CASE 3721**: *Callophrys dumetorum oregonensis* Gorelick, 1968: designation of neotyparatypes (Insecta: Lepidoptera: *lycaenidae*). Gorelick, G.A.

**CASE 3722**: *Scarabaeus gazella* Fabricius, 1787 (currently *Digitonthophagus gazella* or *Onthophagus gazella*; Insecta, Coleoptera, *scarabaeidae*): proposed conservation of usage of the specific name by designation of a neotype. Génier, F. & Krell, F.-T.

**CASE 3723**: *bolitotherus cornutus* (Fabricius, 1801) (Coleoptera: *tenebrioidae*): proposed conservation of the specific name. Bousquet, Y. & Bouchar, P.


**CASE 3725**: *Lema decempunctata* von Gebler, 1829 (Insecta, Coleoptera): proposed conservation of the emended specific name. Schmitt, M.
**Results of ICZN Council Elections**

Council Elections were held during a one-month period from 8 November to 7 December 2016. The four Commissioners to have received the highest number of votes during the elections were Commissioners Alonso-Zarazaga, Krell, Pyle and Rosenberg. Commissioners Alonso-Zarazaga and Rosenberg will serve until they step down during the next IUBS while Commissioners Krell and Pyle will serve for full six-year terms.

**Changes in Commission Membership**

Commissioner Jan van Tol (*The Netherlands*) and Commissioner Peter K. L. Ng (*Singapore*) have relinquished their Commission duties effective 31 December 2016. The ICZN gratefully acknowledges their many years of service.

**Bulletin of Zoological Nomenclature Updates**

Commissioner Evenhuis was appointed as Assistant Editor-in-Chief of the BZN and will provide additional assistance in the production of the *Bulletin*.

This triple part of volume 73 will be the last printed issue of the BZN. From volume 74 (2017), the *Bulletin* will be published electronically, and distributed by BioOne (see below).

From 3 January 2017, the *Bulletin* will be distributed by BioOne. All content beginning with volume 65 (2008) will be available as part of BioOne Complete.

The *Bulletin*’s BioOne page is at: http://www.bioone.org/loi/bzno

Specific subscriptions to the *Bulletin* will still be available through the ICZN Secretariat (see rates on inside front cover).
Declaration 45 — Addition of Recommendations to Article 73 and of the term “specimen, preserved” to the Glossary

International Commission on Zoological Nomenclature
c/o Lee Kong Chian Natural History Museum, 2 Conservatory Drive,
Singapore 117377, Republic of Singapore (e-mail: iczn@nus.edu.sg)


Declaration:

1) The following Recommendations are added to read:
   a) “Recommendation 73G. Specific reasons for designation of an unpreserved specimen as the name-bearing type. An author should provide detailed reasoning why at least one preserved specimen, whether a complete individual organism or a part of such an individual, was not used as the name-bearing type for the new taxon and why the formal naming of the taxon is needed at a point in time when no preserved name-bearing type will be available.”
   b) “Recommendation 73H. Assertion of due diligence. When establishing a new species-group taxon without a preserved name-bearing type, steps taken by an author to capture and preserve a physical specimen of the new taxon and/or locate an existing preserved specimen in natural history collections should be recounted.”
   c) “Recommendation 73I. Consultation with specialists. Before the designation of an unpreserved specimen as a name-bearing type, an author should consult with specialists in the group in question.”
   d) “Recommendation 73J. Comprehensive iconography and measurements. When establishing a new species-group taxon without a preserved name-bearing type, the author should provide extensive documentation (e.g., multiple original high-resolution images, DNA sequences, etc.) of potentially diagnostic characters as completely as possible.”

2) The following term is added to the Glossary under the term “specimen” to read:
   “specimen, preserved. A non-living specimen that is deposited in a scientific collection with the intention to keep it for further study.”

Explanatory note

Whenever feasible, new species-group taxa should be established on the basis of at least one preserved type specimen. Additional information representing diagnostic characters (e.g., illustrations, DNA sequences, audio recording analyses, etc.) should accompany the description of a new species-group taxon whenever possible, but well-preserved biological specimens (either as complete individuals, or parts of individuals) are widely regarded as representing the most generally reliable means for establishing the biological and scientific basis for a species-group name. Establishing new species-group taxa without preserved name-bearing type material is permissible under the Code, but is discouraged unless justified by special circumstances, such as when capture or preservation of specimens is not feasible for technical reasons or for conservation concerns, or when specimens must be destroyed to reliably diagnose a new species. While preserving a whole organism as
the type specimen is preferable and encouraged, in circumstances when whole organism preservation is not feasible a portion (or portions) of the organism sufficient to allow the new species-group taxon to be reliably diagnosed should be preserved.

History of the proposal
On 2 May 2016, the ICZN President officially formed a Committee on the issue of ‘typeless species’. The Committee was comprised of Commissioners Krell (Chair), Ballerio and Bouchard. The mandate of the Committee was “To consider an appropriate action from the International Commission on Zoological Nomenclature on the issue of new species-group taxa proposed after 1999 without an extant name-bearing type, including cases where this type is stated to either not having been collected or having been subsequently released or otherwise discharged or lost to the effect that no original name-bearing type has been (or is likely to be) placed in a relevant collection. In case a Declaration is recommended, at least one suggestion of the exact wording should be provided. In case an Opinion or opinion paper is recommended, a lead author should be suggested”.

Under Articles 78.3 and 80.1 of the Code, a Declaration (provisional amendment to the Code) was drafted by the Secretariat, and on 2 November 2016 Commissioners were asked to vote on whether the amendment to the Code resulting from the proposed Declaration 45 “is not a major change but merely clarifies a provision of the Code” (Article 78.3). Over two-thirds of the Commissioners voted in agreement that it was not a major change to the Code (21 FOR, 3 AGAINST and 3 did not vote). The Declaration is hereby approved and under Article 80.1 shall remain in force until ratified or rejected by the International Union of Biological Sciences (IUBS), the international body from which the Commission derives its functions and powers (Article 77 of the Code).
Case 3705 — *Mantis limbata* Brullé, 1838 (currently *Ameles limbata*)
(Mantodea, MANTIDAE, AMELINAE): proposed conservation of the specific name

Frank Wieland
Pfalzmuseum für Naturkunde – POLICLIA-Museum, Hermann-Schäfer-Str. 17, 67098 Bad Dürkheim, Germany (e-mail: fwielan@googlemail.com)

Julia Goldberg
Mühlbergstr. 13, 67693 Fischbach, Germany (e-mail: jule.goldberg@gmail.com)

Kai Schütte
Universität Hamburg, Zoologisches Institut, Abt. Tierökologie und Naturschutz und Centrum für Naturkunde, Zoologisches Museum, Martin-Luther-King-Platz 3, 20146 Hamburg, Germany (e-mail: kai.schuette@uni-hamburg.de)


**Abstract.** The purpose of this application, under Article 23.9.5 of the Code, is to conserve the specific name *Mantis limbata* Brullé, 1838 (Insecta, Mantodea). It is a junior primary homonym of *Mantis limbata* Hahn, 1835 (Insecta, Mantodea). *Mantis limbata* Brullé, 1838 is currently assigned to the genus *Ameles* Burmeister, 1838 (MANTIDAE, AMELINAE), whereas *Mantis limbata* Hahn, 1835 is assigned to the genus *Stagmomantis* Saussure, 1869 (MANTIDAE, STAGMOMANTINAE). Both names have not been considered to be conspecific after 1899. Therefore, we propose to suppress the replacement name *Ameles canaria* Koçak & Kemal, 2008, that has been suggested for the junior primary homonym, for the purposes of priority in order to avoid nomenclatural confusion within the subfamily AMELINAE.

**Keywords.** Nomenclature; taxonomy; Insecta; Mantodea; MANTIDAE; AMELINAE; STAGMOMANTINAE; Ameles; Mantis; Stagmomantis; Ameles canaria; Ameles limbata; Mantis limbata; Stagmomantis limbata; Canary Islands; Central America; North America; mantis.

1. Brullé (1838, pl. 5, caption) described *Mantis limbata* from the Canary Islands. The written description was published in Brullé (1839, p. 76) but the name was made available from the figure in combination with the name on plate 5, that was published in 1838 (Stearn, 1937, p. 55; see also Wieland et al., 2014, p. 95 for a detailed discussion of the year of publication). As the species was described before 1931 with a clear indication (plate and name), the provisions of Article 12 of the Code are met and the name was made available in 1838.

2. *Mantis limbata* Brullé, 1838 is a junior primary homonym of *Mantis limbata* Hahn, 1835 (pl. “Gen. Mantis, Tab. A”, fig. 2), a species described from a male specimen collected in Mexico.
3. Mantis limbata Brullé, 1838 was classified within Mantis until 1892. Krauss (1892, p. 166) assigned limbata Brullé to the genus Ameles Burmeister, 1838. Kirby (1904, p. 231) listed it under the genus Parameles Saussure, 1869, which was subsequently synonymised with Ameles Burmeister, 1838 by Giglio-Tos (1927, p. 158). Mantis limbata Brullé, 1838 has been classified as Ameles limbata (Brullé, 1838) ever since. The species is endemic to the Canary Islands where it has been reported from Tenerife and La Palma and possibly from Gran Canaria (see review in Wieland et al., 2014).

4. Mantis limbata Hahn, 1835 (pl. "Gen. Mantis, Tab. A", fig. 2) was classified within Mantis until Saussure (1873, p. 249) transferred it to Stagmomantis Saussure, 1869. Giglio-Tos (1917, p. 55, 1927, p. 385) assigned it to Auromantis until Beier (1935, p. 95) reassigned it to Stagmomantis as Stagmomantis (Auromantis) limbata. Mantis limbata Hahn, 1835 has been assigned to Stagmomantis ever since. Stagmomantis limbata has been reported from the United States of America, Mexico, El Salvador and Venezuela (Ehrmann 2002, p. 332; Agudelo et al., 2007, p. 123; Maxwell, 2014, p. 516, table 1). The two species have not been considered to be congeneric after 1899, therefore the conditions of Article 23.9.5 are met.

5. Gurney (1947, p. 251) was the first author to recognise the homonymy. He was aware that a second species of Ameles, Ameles gracilis (Brullé, 1838), had been described from the Canary Islands. Gurney (1947) was uncertain about a putative synonymy of the two Ameles species. Therefore, he decided to not take nomenclatural action until future research on morphological variability and the validity of Mantis limbata could be ascertained. Gurney (1947) merely stated that if Ameles limbata was indeed a valid species and not a synonym of Ameles gracilis or another Ameles species from the African mainland, the junior primary homonym would have to be replaced.

6. The next author to discuss the homonymy was Kaltenbach (1979, p. 523) who mentioned the problem in his review of the Canary Island Mantodea fauna. However, Kaltenbach (1979) did not see reason to introduce a replacement name because limbata Brullé had been transferred to another genus in 1904 (actually 1892, see paragraph 3) and limbata Hahn was transferred to Stagmomantis in 1873 (Kaltenbach mentioned 1872 for Saussure's work but the correct publication date was 1873; see Crosnier & Clark, 1998). As both species were not only assigned to different genera but even to different subfamilies, Kaltenbach (1979) argued, no nomenclatural action was required.

7. Otte & Spearman (2005, p. 145) considered Ameles limbata (Brullé, 1838) as a subjective synonym of Ameles gracilis (Brullé, 1838) (therein assigned to Brullé 1840, but the true date of publication is 1838; see paragraph 1). They referred to Kaltenbach (1979, p. 523), who had allegedly synonymised the two species. This was a misinterpretation. Kaltenbach (1979) treated both species as valid. Instead, Kaltenbach (1979, pp. 517, 518) merely mentioned that he had compared several specimens that had been assigned to Ameles limbata by Chopard (1942; 1954) and had found that they actually belonged to Ameles gracilis (see Wieland et al., 2014, p. 84 for a detailed discussion). The synonymy erroneously listed by Otte & Spearman (2005, p. 145) was mentioned by Battiston et al. (2010, p. 74). However, the authors simply followed Otte & Spearman's catalogue (as becomes evident from the incorrect publication date of Ameles gracilis in both publications) and did not provide any reasons for this decision. The assumption of a putative synonymy of Ameles gracilis and Ameles limbata was neither shared in previous nor in subsequent publications (e.g. Kirby, 1904, p. 231; Chopard, 1942, p. 4; 1954, p. 10; Kaltenbach, 1979, p. 523; Roy, 1987, p. 118; García & Oromí, 1999, p. 103; Bland, 2001,
8. Koçak & Kemal (2008, p. 8) did not consider Article 23.9.5 of the Code by which the discovery of a primary homonymy must not automatically result in the proposal of a replacement name for the junior homonym.

9. Koçak & Kemal (2008, p. 8) decided to act on the limbata-problem. They argued that: a) *Mantis limbata* Brullé, 1838 was a junior primary homonym of *Mantis limbata* Hahn, 1835 (in their publication erroneously spelled “de Haan”); b) *Ameles limbata* (Brullé, 1838) was taxonomically distinct from other *Ameles* species; and c) Kaltenbach (1979, p. 523) had not acted correctly with regard to the rules of homonymy of the Code. However, Koçak & Kemal (2008, p. 8) did not specify which provisions of the Code were violated or ignored. Instead, they proposed the name *Ameles canaria* Koçak & Kemal, 2008 as a new replacement name for *A. limbata* (Brullé, 1838).

10. As *Ameles limbata* (Brullé, 1838) and *Stagmomantis limbata* (Hahn, 1835) are highly distinct from each other (morphologically, biogeographically and taxonomically) and have not been considered to be congeneric after 1899, the requirements of Article 23.9.5 of the Code are fully met. Therefore, there is no need for the use of a replacement name. On the contrary: It merely adds to the confusion of the Amelinae taxonomy (Wieland et al., 2014, p. 95), and might add to nomenclatural instability, without being of any practical value.

11. Regarding the type material of the species mentioned in the present case, only the whereabouts of the type material *Mantis limbata* Hahn, 1835 are known. The male holotype is housed in the collection of the Museum für Naturkunde (MfN) in Berlin, Germany (Ehrmann, 2002, p. 332). The male holotype of *Ameles limbata* (Brullé, 1838) and the female holotype of *Ameles gracilis* (Brullé, 1838) are supposedly housed in the collection of the Muséum National d’Histoire Naturelle (MNHN) in Paris, France (Ehrmann, 2002, p. 59). However, the MNHN collection has four specimens of *Ameles gracilis* (none of which is the holotype) and no specimens of *Ameles limbata* (Roy, pers. comm., 2016). The type material of the two species is not located in the collection of the Museo Nacional de Ciencias Naturales in Madrid (MNCN), Spain (Paris, pers. comm., 2016). They are not present in the Natural History Museum (NHM) in London, United Kingdom (Marshall, 1975), nor in the Muséum d’Histoire Naturelle de Genève (MHNG), Switzerland (Roy & Cuche, 2008). Furthermore, we were unable to locate the Brullé types in the collections of the Naturhistorisches Museum Wien (NHMW) in Vienna, Austria and in the Naturalis Biodiversity Center (RMNH) in Leiden, Netherlands. The fate and whereabouts of the Brullé types have to be thoroughly researched in the future, but this is beyond the scope of the current contribution.

12. The International Commission on Zoological Nomenclature is accordingly asked:
   (1) to use its plenary power to rule that:
      (a) the specific name *limbata* Brullé, 1838, as published in the binomen *Mantis limbata*, is not invalid by reason of being a junior primary homonym of *Mantis limbata* Hahn, 1835;
      (b) to suppress the specific name *canaria* Koçak & Kemal, 2008, as published in the binomen *Ameles canaria*, for the purposes of the Principle of Priority and the Principle of Homonymy;
   (2) to place on the Official List of Specific Names in Zoology the name *limbata* Brullé, 1838, as published in the binomen *Mantis limbata* Brullé, 1838 with the
endorsement that it is not invalid by reason of being a junior primary homonym of *Mantis limbata* Hahn, 1835 as ruled in (1)(a);
(3) to place on the Official Index of Rejected and Invalid Specific names in Zoology the name *canaria* Koçak & Kemal, 2008, as published in the binomen *Ameles canaria* and suppressed in (1)(b) above.

Acknowledgements
We are grateful to Francisco Welter-Schultes for his valuable comments on the manuscript, to Reinhard Ehrmann, Roger Roy, and Mercedes Paris for their information regarding the type material of the species, and to Jörg U. Ganzhorn for covering the costs of the publication.

References


Acknowledgement of receipt of this application was published in BZN 72(4): 268.

Comments on this case are invited for publication (subject to editing) in the *Bulletin;* they should be sent to the Secretariat, ICZN, Lee Kong Chian Natural History Museum, 2 Conservatory Drive, Singapore 117377, Republic of Singapore (e-mail: iczn@nus.edu.sg).
Case 3717 — XYLOPHAGIDAE Purchon, 1941 (Mollusca: Bivalvia): proposed emendation of the spelling to XYLOPHAGAIDAE to remove homonymy with XYLOPHAGIDAE Fallén, 1810 (Insecta: Diptera)

Eugene V. Coan
Santa Barbara Museum of Natural History, 2259 Puesta del Sol Road, Santa Barbara, California 93105, U.S.A. (e-mail: genecoan@gmail.com)

James T. Carlton
Maritime Studies Program, Williams College – Mystic Seaport, Mystic, Connecticut 06355, U.S.A. (e-mail: james.t.carlton@williams.edu)

Neal L. Evenhuis
Bernice Pauahi Bishop Museum, 1525 Bernice Street, Honolulu, Hawai‘i 96817, U.S.A. (e-mail: neale@bishopmuseum.org)


Abstract. The purpose of this application, under Articles 29 and 55.3 of the Code, is to remove the homonymy between the family-group names XYLOPHAGIDAE Purchon, 1941 (Mollusca: Bivalvia) and XYLOPHAGIDAE Fallén, 1810 (Insecta: Diptera), which are homonyms due to the similarity of the names of their respective type genera Xylophaga Turton, 1822 and Xylophagus Meigen, 1803. It is proposed that the stem of the generic name Xylophaga be emended to Xylophaga- to give XYLOPHAGAIDAE, while leaving the fly family name unaltered.

Keywords. Nomenclature; taxonomy; Insecta; Mollusca; Bivalvia; Diptera; xyLOPHAGI-DAE; XYLOPHAGAIDAE; Xylophaga; Xylophagus; Nemotelus cinctus; Teredo dorsalis; fly; wood-boring.

1. Meigen (1803, p. 266) proposed Xylophagus in the Diptera (Insecta) including only the single species Nemotelus cinctus De Geer, 1776 (p. 183), which is the type species by monotypy. Fallén (1810, p. 5) proposed a family-group name (as “XYLOPHAGIDAE”) based on the genus Xylophagus Meigen, 1803 and included two genera: Sicus Fabricius, 1798 (p. 547) [non Scopoli, 1763], and Xylophagus Meigen, 1803. This family-group name (correctly spelled as XYLOPHAGIDAE in Stephens, 1829, p. 57) is in current use within the Diptera for a group of flies found worldwide comprising 134 species in nine genera (Woodley, 2011).

2. Turton (1822, p. 253) proposed the genus-group name Xylophaga, for a group of deep-water wood-boring bivalves, of which the type species is Teredo dorsalis Turton, 1819 (p. 185), by monotypy. Purchon (1941) recognized the uniqueness of this genus and proposed a family-group name [as “XYLOPHAGINIDAE” (Purchon, 1941, p. 32)] based on the genus Xylophaga Turton, 1822. Whereas some authors (Taki & Habe, 1950) maintained...
Purchon’s original spelling for the bivalve family, Turner’s (1955, p. 145) monograph on the Pholadidae treated the group as a subfamily and used the spelling xylophaginae. In her treatment of the family in the Treatise on Invertebrate Zoology, Turner (1969, p. N721) became aware of the family name homonym with the Diptera and changed [“nom. correct.”] the name of the bivalve subfamily to xylophagidae. Three years later, Hoagland & Turner (1971, p. 115) maintained the latter spelling but noted that a petition would be needed to formalize the change. Bernard (1983, p. 61) independently changed the name to xylophagidae, stating “nom. correct., herein pro xylophagidae”, but he did not provide a reason and misspelled Purchon’s original orthography. Since 1983, most authors have used the emended spelling, xylophagidae, and it is in current use (Harvey, 1996; Turner, 2002; Bieler & Mikkelsen, 2006, p. 235; Mikkelsen & Bieler, 2007, p. 389; Haga & Kase, 2008; Voight, 2008, 2009, 2015), although Coan & Valentich-Scott (2012, p. 905) used xylophaginae, but noted the alternative often being used and the still remaining need for a petition. Most authors now rank this group as a separate family with the superfamily pholadoidea. Purchon’s (1941) original spelling is thus not in current use.

3. The International Commission on Zoological Nomenclature is accordingly asked:

(1) to use its plenary power to rule for the purposes of Article 29 of the Code, that the stem of Xylophaga Turton, 1822 is Xylophaga-;

(2) to place on the Official List of Generic Names in Zoology the following names:
   (a) Xylophaga Turton, 1822 (gender: feminine), type species by monotypy Teredo dorsalis Turton, 1819;
   (b) Xylophagus Meigen, 1803 (gender: masculine), type species by monotypy Nemotelus cinctus De Geer, 1776;

(3) to place on the Official List of Specific Names in Zoology the following names:
   (a) dorsalis Turton, 1819, as published in the binomen Teredo dorsalis;
   (b) cinctus De Geer, 1776, as published in the binomen Nemotelus cinctus;

(4) to place on the Official List of Family-Group Names in Zoology the following names:
   (a) XYLOPHAGAIDAE Purchon, 1941, type genus Xylophagus Turton, 1822 (Mollusca: Bivalvia);
   (b) XYLOPHAGIDAE Fallén, 1810, type genus Xylophagus Meigen, 1803 (Insecta: Diptera);

(5) to place on the Official Index of Rejected or Invalid Family-Group Names in Zoology the name XYLOPHAGINIDAE Purchon, 1941, spelling emended to XYLOPHAGAIDAE, as ruled in (4)(a) above.

References


Acknowledgement of receipt of this application was published in BZN 73(2–4): 94.

Comments on this case are invited for publication (subject to editing) in the Bulletin; they should be sent to the Secretariat, ICZN, Lee Kong Chian Natural History Museum, 2 Conservatory Drive, Singapore 117377, Republic of Singapore (e-mail: iczn@nus.edu.sg).
Comment (Case 3601) – Some matters arising from the Case and the broader
issues involved and the need to remove ambiguity in Chapter 3 of the Code

Harold Cogger
Australian Museum, 1 William Street, Sydney, NSW 2010, Australia

Glenn Shea
Sydney School of Veterinary Science, B01, University of Sydney, NSW 2006, Australia

Patrick Couper
Queensland Museum, South Brisbane, Queensland 4101, Australia


1. The issues raised in Case 3601 (see Hoser, 2013), seeking validation of a single
generic name (*Spracklandus* Hoser, 2009) by validating the publication in which the
name was erected (*Australasian Journal of Herpetology*) go far beyond a single name
and publication, but involve a complex of matters affecting the current Code and the
interpretation of several Articles in Chapter 3 (Criteria of Publication).

2. Kaiser et al. (2013), following a discussion as to whether the recent vanity pub-
lications of R. Hoser (*Australasian Journal of Herpetology*) and R. Wells (*Australian
Biodiversity Record*) meet the Code’s publication criteria (and having concluded that
they did not), proposed to ignore the herpetological publications of both of these authors
from the arbitrary date of 2000. They urged fellow herpetologists to follow their lead.
This decision implied that names established by both workers prior to the year 2000, but
under similar circumstances, would remain available and consequently inferred that the
publications in which these earlier names occurred would be considered to have met the
Code’s publication criteria.

3. The consequence of this decision by Kaiser et al. (2013), and their supporters, has
been the subsequent erection of many new names (as replacement names, new names
or junior synonyms) for many Wells and Hoser names that Kaiser and his colleagues
now regard as unpublished, for nomenclatural purposes, under the conditions set out
While their actions were subsequently endorsed by 70 eminent herpetologists (Rhodin
et al., 2015), the fundamental issue remains unaddressed by the Commission: do the
journals *Australasian Journal of Herpetology* and *Australian Biodiversity Record* meet
the requirements of Chapter 3 of the Code to be considered ‘published’ for nomenclatural
purposes?

4. Naish (2013) and Denzer et al. (2016) have outlined the issues involved in the
Hoser dispute (but not those of Richard Wells). Apart from the authors of the Kaiser
et al. (2013) paper, many subsequent commentators (e.g., Kaiser, 2014; Schleip, 2014;
Thomson, 2014; Rhodin et al., 2015) continued to explore the validity of the Wells and
Hoser journals by emphasising issues raised by Kaiser and his coauthors, viz., availability
and distribution, whether the published papers were subject to peer review, and whether they were 'published' under the terms set out in the Code? Kaiser (2014; BZN 71(1): 30–34) forensically examined Issue 7 of the Australasian Journal of Herpetology and concluded that this issue of the journal did not meet the essential criteria. Is he correct? And if so, what about other issues of this journal, and of Australian Biodiversity Record?

5. Several commentators, responding to Harvey & Yanega (2013), have also emphasised perceived deleterious impacts of the Wells and Hoser journals on herpetological taxonomy. We reject these arguments. Any published taxonomic works, whether or not subject to a pre-publication refereeing process, are invariably subjected to post-publication peer review and are ultimately accepted, accepted in part, or rejected by all or part of the taxonomic community. New taxonomic arrangements, therefore, including the erection of new taxa, can be rejected or ignored by any taxonomist who regards them as incorrect or poorly argued. However the names of any new taxa in such works cannot be ignored, and come under the purview of the Code. As stated in the Code’s introduction, the Code and the Commission are concerned solely with zoological nomenclature – principally the stability of names within taxonomy – and not with taxonomic arrangements or standards as such.

6. Our concern here is for nomenclatural stability. We do not argue either for or against a particular submission. Rather, we seek clarification and, where necessary, action by the Commission on the following points:

   (a) Do the journals Australasian Journal of Herpetology and Australian Biodiversity Record unequivocally meet or not meet the publication criteria, for nomenclatural purposes, set out in Chapter 3 of the Code? We would argue that the justifications so far presented in the literature for rejecting these journals – that they are vanity publications lacking scientific credibility, that they have not been subject to conventional refereeing processes, that their contained taxonomies are poor or spurious, that their accessibility has been selective, and that they fail to meet the criteria of availability for nomenclatural purposes set out in Chapter 3 of the current Code – are highly subjective, and require testing by the Commission. So, too, an assertion that the Australasian Journal of Herpetology clearly does meet the publication requirements set out in the Code (Wellington, 2015) is equally subjective and debatable. Nevertheless, rejecting these journals on the grounds so far argued carries implications for many other journals – past and present – in which new taxa in disciplines other than herpetology have been described in publications that arguably meet the Code’s criteria. A ruling by the Commission that these journals do not meet the Code’s criteria for “publication for nomenclatural purposes” would immediately resolve any ambiguity and invalidate all contained nomenclatural acts, including the erection of new names for asserted new taxa.

   (b) To date the rejection of the Hoser and Wells names seems to rest almost entirely on the argument that the journals in which they were published fail to meet basic standards of scientific rigor for taxonomic studies. Further, it has also been argued that they are unavailable because they are not consistent with the non-mandatory provisions of the “Code, Introduction: Development and underlying principles” (e.g., Schlep, 2014), that they arguably fail to meet the Code’s standards of publication availability (for nomenclatural purposes) under Chapter 3 of the Code, and that they cause nomenclatural instability. However, if these journals were to be considered by the Commission to meet the conditions of valid publication set out in Chapter 3 then the Hoser and Wells
names would be validated and the many replacement names currently being established to replace them will simply become junior synonyms. Pending resolution of this issue by the Commission (independently, or in response to application(s)), there continues to be a significant dual nomenclature being created (as set out in Thomson, 2014) to confuse not only herpetologists but also agencies or governments needing to cite the names in regulatory or conservation legislation, and in other scientific disciplines such as medicine.

(c) The arguments by Kaiser et al. (2013) for ensuring scientific integrity and standards in published taxonomic treatments are unassailable, but they are not currently mandated by the Code. However any attempts to establish mandatory rules for a scientific discipline such as taxonomy are toothless unless there is a final arbiter in disputed cases. Currently there is no such arbiter in taxonomy beyond the peer review process by individual journals or by subsequent community adoption (usage). But the Commission is an absolute arbiter in nomenclature while having no role in resolving taxonomic disputes. Urgent consideration by the Commission of some of the suggestions made by Kaiser and his colleagues (such as mandating the Code’s Recommendations and Code of Ethics), or perhaps requiring ICZN approval and registration of journals in which new names may be established – would go a long way towards resolving the problems they have identified. However if an individual taxonomist, or group of taxonomists, determine to take unilateral nomenclatural actions contrary to the provisions of the present Code, or without the sanction of the Commission, they are merely replacing one form of anarchy or vandalism with another. We strongly endorse the call by Kaiser et al. (2013) for the Commission to act urgently to resolve these problems, of which the Commission and its Code are integral parts. Without such resolution a confusing dual nomenclature will continue to plague the herpetological literature.

(d) A further problem with the responses to the original submission in Case 3601, and with subsequently proposed modifications and additions, is that their authors seek to have issues 1–21 of the Australasian Journal of Herpetology suppressed, for nomenclatural purposes, by use of the Commission’s plenary powers. But what is the status of nomenclatural acts published in issues subsequent to issue 22 of this journal? The editor(s)/author(s) of these suppressed issues might well decide to validate papers that appeared in them by republishing them. Such republication has been done previously by Wells & Wellington (1988a, b, 1989), Wells (2002, 2007a, b) and Hoser (2002), sometimes with additional information in order to validate previously-described taxa whose incomplete descriptions rendered them nomina nuda when first published e.g., Chelodina rankini Wells & Wellington, 1985, and Elseya stirlingi Wells & Wellington, 1985, both considered nomina nuda by Iverson et al. (2001), but republished with additional data by Wells (2007a, b) respectively.

(e) However, as indicated above, Kaiser et al. (2013) stated their intention to ignore all works published by Raymond Hoser and Richard Wells after the year 2000. Such actions would require responses by the Commission different from those currently being sought in Case 3601. Indeed, to our knowledge no action has yet been requested to validate their determination to ignore the post-2000 nomenclatural works of Richard Wells.

(f) In a discussion of the history of the availability criteria for determining whether an article or journal was a valid publication for nomenclatural purposes, the late Dr. W.D.L. Ride, chairman of the Editorial Committee for the current edition of the Code, concluded in his Introduction to the Code that “it seems likely, in the longer term, and with the
development of new information systems, that the solution will not lie in patching up a definition of publication but, rather, in scrapping it and finding a means of replacing ‘publication’ as a primary determinant of availability’. We believe that this time has arrived. However, while concurring with the frustration and outrage felt by many commentators, we abhor the intemperate language used by both sides in published or public exchanges, including through social media.

(g) In relation to suggestions that the Commission might adopt a role in the adjudication of taxonomic disputes, phylogenies and taxonomic hierarchies are artificial constructs based on data with various levels of accuracy and rigour that can always be subject to multiple interpretations. Most importantly, any published taxonomic arrangement in any journal and by even the most respected practitioner, can be freely accepted, accepted in part, or rejected by other taxonomists and non-taxonomists at their discretion. Only when a taxonomy has been accepted by the relevant specialist community as the most parsimonious of those currently available, is there peer pressure to adopt it. The use of a particular taxonomic arrangement is not, and never can be, mandated by an individual or group, and always represents a hypothesis. Consequently, calls for the Commission to extend its brief to cover adjudication of taxonomic disputes are both unnecessary and, we suggest, impossible to carry out fairly and objectively. Indeed, any attempt to do so would pit individual against individual, group against group, clique against clique, and would therefore risk dependence of outcome on which ‘side’ could muster the most ‘votes’. For the Commission to undertake this role in all animal groups would involve an horrendous task and responsibility that, in the absence of objective rules, and therefore objective outcomes, Commissioners or group specialists are not qualified to undertake.

(h) We also suspect that the furore about Hoser’s (and Wells’s) taxonomies would dissipate if their vanity publications did not involve the description of a plethora of new, often poorly circumscribed and named taxa, as these and their relevant taxonomic treatments could be ignored by those researchers who did not agree with them. Indeed, we believe than many respondents are unable or unwilling to face the real issue – that most of their objections to these publications are based on new names rather than new taxonomies, especially when those new names, through priority, usurp names intended for those same taxa that they or their colleagues have also planned to describe in the course of their research. While we may criticise this attitude, it is nonetheless immensely annoying and frustrating to be ‘pipped at the post’ by less rigorous, superficial research by authors who do not face the constraints of intensive data gathering and analysis, institutional approvals, journal editors, referees and publication waiting lists, and who ignore the non-mandatory Code of Ethics set out in the Code. Unfortunately, the number of taxa named by any individual worker has an “immortality effect”, whether sought or unsought, whereby the number of names authored by a particular worker can be misinterpreted by present and future non-specialists as a reflection of that worker’s research impact on a particular field of study – often long after that worker’s demise.

(i) Case 3601 neither requires use of the plenary powers of the Commission to suspend application of aspects of the Code (Article 78.1), nor involves reference of the Case to the Commission for determination under the Specific Powers of the Code (Article 78.2.2). Hence it is not mandatory for the Commission to issue an Opinion on this Case (Article 78.2.3). Despite this, we urge the Commission to act quickly, and independently of current open cases if necessary, to resolve what is now a major shortcoming in
the Code that is leading to the establishment of dual nomenclatures, each of untested validity.

7. Finally, it should also be pointed out that following the Commission's decision in 1991 that the Australian Society of Herpetologists' application to suppress earlier works of Wells & Wellington (Case 2531; Australian Society of Herpetologists, 1987) was “outside its remit”, many of the specific and generic names created by Wells and Wellington, and subsequently by Wells, and by Hoser, have been widely adopted by Australian herpetologists, including listing in Australian Commonwealth Government and Australian State Government legislation, although often applied to different taxonomic concepts. Consequently the issues arising from this Case are more complex than presented. We seek only a rapid resolution of the disruptive and nomenclature-destabilising terminological uncertainty in Chapter 3 of the Code, or the implementation of earlier suggestions for the registration of names, or of journals in which new names may be published. Attempts to link the issues raised in Case 3601 to taxonomic instability are in our view spurious. Names, not taxonomies, are at the heart of the present dispute. We suggest that without the authority to erect new names, recalcitrant self-publishing authors would either stop publishing, or would have to subject their taxonomic work to conventional peer review in respected, authorised journals.

8. Many respondents to the call by Harvey & Yanega (2013) for comments on ‘taxonomic practice and the Code’ have advised against the Commission becoming an adjudicator of ethical standards in taxonomy or nomenclature, “even if the Code of Ethics is violated”. However creating a code of ethics without there being any consequence for transgressors defies logic, especially when such transgressions lead to dual nomenclatures for the same taxa – the very antithesis of the nomenclatural stability that underpins the principle objectives of the Code and the Commission. Should Hoser be condemned for creating a plethora of new taxa that are poorly defined and poorly justified, and his papers not subject to rigorous review? Should his critics be condemned for accepting the taxonomic validity of many of his taxa, while rejecting his names for these taxa in favour of their own? Or should both be condemned for exploiting the Code’s ambiguities to permit whichever conflicting interpretation of the rules suits their purpose?

9. Consequently we argue that such conflicting interpretations create nomenclatural instability, and that the Commission must act to remove any existing ambiguities in the Code by modifying the relevant Articles or adding new ones where necessary.

10. We also submit this material in response to the Commission’s call (Harvey & Yanega, 2013) for comments on taxonomic practice and the Code.

References


Thomson, S. 2014. Comment on *Spracklandus* Hoser, 2009 (Reptilia, Serpentes, Elapidae): request for confirmation of the availability of the generic name and for the nomenclatural validation of the journal in which it was published [Case 3601]. *Bulletin of Zoological Nomenclature, 71*(2): 133–135.


Comment (Case 3613) — On the setting aside of *Nyctimystes cheesmani* Tyler, 1964 in favour of *Nyctimystes cheesmanae* Tyler, 1964 (Amphibia, Anura, hylidae) (see BZN 70(1): 30–32 [Case])

Alain Dubois

Institut de Systématicque, évolution, Biodiversité, ISYEB – UMR 7205 – CNRS, MNHN, UPMC, EPHE, Muséum national d’Histoire naturelle, Sorbonne Universités, 57 rue Cuvier, CP 30, F-75005 Paris, France (e-mail: adubois@mnhn.fr)

http://zoobank.org/urn:|sid:zoobank.org:pub:5FD188A3-7469-4857-B068-3F22CFA5D5D4

1. The publication of Case 3613 in the *Bulletin of Zoological Nomenclature* (see Tyler & Menzies, 2013, pp. 30–32) is surprising, as this paper ignores the recent discussions devoted to the question of specific scientific names dedicated to persons (Brandon-Jones et al., 2007; Dubois, 2007; Dubois et al., 2011; Nemésio & Dubois, 2012). It would be too heavy and useless to repeat here the detailed explanations given in these papers, particularly in Dubois (2007), and these original publications should be consulted before any decision on this case is taken.

2. The application did not distinguish between the situations described in Articles 31.1.1 and 31.1.2 of the Code, which result in different rules according to whether a specific name based on a personal name was so on the basis of a Latinized form of this name (Article 31.1.1) or not (Article 31.1.2). If Article 31.1.1 applies, the name must “be formed in accordance with the rules of Latin grammar”, but the latter does not imply in the least that a nomen dedicated to a man must ends in -i or that one dedicated to a woman must ends in -ae, as many other genitive endings are compatible with Latin grammar in these situations (Dubois, 2007, p. 54–58). As for Article 31.1.2, it can be applied only when evidence was provided in the original publication itself that the epithet was derived from the unlatinized stem of the person’s name, as well as for the sex of the dedicatee of the name (Dubois, 2007, p. 54). This was not the case in the original paper of Tyler (1964), where the etymology of the specific name *cheesmani* was not given and the new name introduced with the simple sentence: “*Nyctimystes cheesmani* is therefore proposed to replace *Nyctimystes montana* Parker”. Therefore Article 31.1.1 applies, and under this article the spelling *cheesmani* cannot be construed as having been incorrectly formed and should not be emended. Furthermore, it should not be so even if it had been incorrectly formed because, according to Article 32.5.1, “Incorrect transliteration or latinization (…) are not to be considered inadvertent errors” and do not qualify as incorrect original spellings. Therefore, the spelling *cheesmanae* introduced by Menzies (1976) is an unjustified emendation of the original spelling, with its own author and date, and as such it should be rejected as an invalid junior objective synonym.

3. Although its publication was in my opinion not warranted, this application has now been published, and its proposal should be considered. The fact that, after a period of three years, no comment has been published in the *Bulletin* about this case, suggests that the latter does not appear as an important one to practicing amphibian taxonomists, and that ‘protecting’ the spelling *cheesmanae* is not a major concern for most of them. This is also evidenced by the post-1976 citations given by Tyler & Menzies (2013), which include 7 works using the correct original spelling *cheesmani* and 6 works using the
unjustified emendation cheesmanae. Furthermore, these citations failed to mention four major references very widely used internationally for seeking information on the taxonomy of amphibians, namely the publication of Pyron & Wiens (2011) and the websites Amphibian Species of the World (http://research.amnh.org/vz/herpetology/amphibia/), AmphibiaWeb (http://amphibiaweb.org/) and The IUCN Red List of Threatened Species (http://www.iucnredlist.org/), which all four use the spelling cheesmani. Therefore, and contrary to the statement of Tyler & Davies (2013, p. 31), the spelling cheesmanae is far from meeting the conditions of Article 33.2.3.1, which states: “when an unjustified emendation is in prevailing usage and is attributed to the original author and date it is deemed to be a justified emendation”.

4. In conclusion, the conditions for preservation of the emendation are not complied with, and the original spelling should be maintained. In this case like in many others where individual zoologists appeal to the Commission to protect their ‘preferred’ nomenclature, the Code should be strictly followed, as any unjustified exception to the rules tolerated contributes to nomenclatural laissez-faire and weakens the legislative value of the Code in the eyes of zootaxonomists. At any rate, pending the decision of the Commission on this case, and as there is no reason to follow Article 82.1 since there exists no ‘prevailing usage’ of the unjustified emendation, the correct original spelling cheesmani should be used by all authors who mention this species, and the use by Menzies (2014a, b) of the spelling cheesmanae was unwarranted and should not be repeated.

5. It is clear however that the current Article 31.1 of the Code proves of difficult understanding and usage by many working taxonomists, as shown by this case and by the various examples given by Brandon-Jones et al. (2007), Dubois (2007, pp. 67, 68) and Nemésio & Dubois (2012). In amphibians, following the interpretation of the authors of unjustified emendations cited in these works would result in requiring to emend the spelling of very-well known specific names, such as that of the common European frog species Pelophylax lessonae (Camerano, 1882), dedicated to a man, or of the famous ‘moustache frog’ from China Leptobrachium boringii (Liu, 1945), dedicated to a woman. Such a course would certainly not serve nomenclatural stability.

6. A clarification and simplification of Article 31.1 appears therefore necessary, and the Code would no doubt be improved by proper consideration being given to the proposals of Dubois (2007, pp. 63, 64) in this respect.

References


I would like to comment on the proposal to replace the neotype of *Varanus indicus* by Weijola (2015) and the response by Böhme et al. (2016).

1. The only evidence available about the identity of *Varanus indicus* is the type location, a brief description and an illustration of the type specimen.

2. The type location provides the strongest evidence of the identity of *V. indicus*. Daudin (1802) identifies the person responsible for the discovery of the animal in Ambon. Weijola (2015) suggests that the lost type specimen might not have been the one collected by Riche. Even if this is the case, we can only assume that Daudin procured another specimen that he believed also came from Ambon.

3. Böhme et al. (2016) argue that the illustration of *Tupinambis indicus* (Daudin, 1802, pl. XXX by Adel Daudin) is an “iconotype”. This is not a term recognised in the ICZN Code and an illustration cannot substitute for a lost type specimen. They further argue that the illustration proves that Daudin’s description cannot be of *Varanus cerambonensis* because it lacks a light temporal/post ocular stripe. In fact the illustration lacks any head pattern at all and is atypical of any known species in the *Varanus indicus* group. Similarly, Adel Daudin’s depiction of *Tupinambis stellatus* (Daudin, 1802, pl. XXXI) lacks detail of pattern on the head and tail, atypical of animals of the *Varanus niloticus* group. The other *Tupinambis* illustration (*T. albigularis*, see Daudin, 1802: pl XXXII) is by De Seve and is the only one of the three that attempts to faithfully render the pattern of the entire animal. The lack of head patterning in Daudin (1802, pl. XXX and XXXI) are particularly striking in the colour illustrations that were produced as a luxury edition of Daudin’s work (Bour, 2011). It seems likely that Adel Daudin included only aspects of the pattern that were included in her husband’s diagnosis of *T. indicus*: “supră niger, punctis albidis sparsis; caudā compressa, non carinato-serrātā”.

4. Daudin’s (1802) description and the accompanying illustration indicate that the dorsum has white spots not arranged in bands. Böhme et al. (2016) state that the arrangement of dorsal spots in rows is an important diagnostic characteristic, and that because the spots are not in bands the type specimen could not have been the species known from Ambon. In their description of *V. cerambonensis*, Philipp et al. (1999) state that, in adults, there is “a marked tendency to a cross-banded dorsal pattern”, which is not the same as “every specimen of *V. cerambonensis* has bands on its back”. In both the holotype (ZFMK (MZB) 7061, Philipp et al., 1999, fig. 10) and the paratype (ZFMK (MZB) 7619, Philipp et al., 1999, fig. 3) of *V. cerambonensis* the banding pattern is weak (Philipp et al., 1999). Weijola & Sweet (2015) document specimens of *V. cerambonensis* from various islands in the central Moluccas which show great variations in pattern, with some having virtually no arrangement of banding in dorsal markings (Weijola & Sweet, 2015, p. 14, photograph, fig. 1). They also make reference to an “almost entirely melanistic” specimen
from Buru (ZMA 15416g). The dorsal pattern cannot be regarded as definitive proof that Daudin’s animal was a different species to \textit{V. cerambonensis}.

5. The premise that two species of monitor lizards occurred on the island stems exclusively from the assumption that all four untagged lizards in the jar ZMA1146 marked “Ambon” were collected on Ambon (Philipp et al., 1999). If any of the authors had visited Ambon and found only the single species reliably reported from that island, the argument that Daudin’s (1802) description was of a species from elsewhere is unlikely to have emerged. Rather, the authors would have recognised (as does Weijola, 2015) that the monitor lizard on Ambon (and other islands in central Moluccas) was described by Daudin in 1802 and that another name was required for the wide ranging lizard occupying New Guinea and Australia. Instead, Böhme et al. (2016) suggest that one of two sympatric monitor lizards on Ambon might have gone locally extinct since the end of the 18th century, or that subsequent visitors have failed to find the second species despite its presence. Neither argument is convincing because no examples of historical extinctions of \textit{Varanus} lizards from islands over 200 km$^2$ are known and the fact that only one species has been recorded from all major habitats on the islands makes the hypothesized presence of another generalist species untenable.

6. Böhme et al. (2016) suggest that the proposed changes would cause confusion, not only amongst taxonomists but also “ecologists, conservationists as well as national and international custom and conservation authorities”, particularly because members of the \textit{V. indicus} group suffer from high levels of exploitation. They also cite checklists authored by themselves to demonstrate how much the authorities rely on the current nomenclature. However CITES (comparative tabulations using all trade terms) records less than 8000 items of \textit{V. indicus} and two items of \textit{V. cerambonensis} traded internationally between 2000 and 2015, compared with over 8 and 15 million items of \textit{V. niloticus} and \textit{V. salvator}, respectively, within the same time period (CITES trade statistics derived from the CITES Trade Database, UNEP World Conservation Monitoring Centre, Cambridge, UK). Major changes in the taxonomy of \textit{V. salvator} (e.g., Koch et al., 2007; Koch et al., 2010; Welton et al., 214) and necessary changes in the taxonomy of \textit{V. niloticus} (e.g., Dowell et al., 2015) will inevitably result in considerable confusion, but the amount of confusion caused by correcting \textit{V. cerambonensis} to \textit{V. indicus} and the recognition of \textit{V. chlorostigma} for the animals east of central Moluccas will be negligible, and an insufficient reason to accord universality priority over other aspects of the Code.

7. Böhme et al. (2016) further suggest that the proposed changes would undermine taxonomic stability. But their solution, to change the type locality of the existing neotype from Ambon to “northwestern New Guinea”, is not a correction or a clarification in the sense of Recommendation 76A of the Code, but a rather vague guess about where their specimen might have come from, contrary to the information supplied in Daudin’s (1802) description (Ambon) and contrary to the data accompanying their specimen (Ambon). The cornerstone of taxonomic stability is the existence of type material with accurate locality data, which is not provided by the current neotype.

8. This mistake arose out of a lack of primary evidence about species richness at the type locality of \textit{V. indicus} (Ambon) and the assumption that the contents of a jar with missing provenance was sufficient proof of the occurrence of two species on Ambon, rendering further investigations unnecessary. In this case the solution proposed by Weijola (2015) is the preferable alternative because it would provide type specimens for both
species (V. indicus and V. chlorostigma) with documented provenance and hence reliable type localities, retain the original (and reliable) type localities for both species and allow future work on the taxonomy of the group to be conducted on a solid basis and without any of the current inconsistencies.

References


Comment (Case 3676) – Response to a comment on the proposed conservation of *Tupinambus indicus* Daudin, 1802 by replacement of the neotype

Samuel S. Sweet
Department of Ecology, Evolution and Marine Biology, University of California, Santa Barbara, California 93106, U.S.A. (e-mail: sweet@lifesci.ucsb.edu)

Valter Weirola
Zoological Museum, University of Turku, 20014 Turku, Finland (e-mail: vsawei@utu.fi)

http://zoobank.org/urn:|sid:zoobank.org:pub: DBC688EE-3D70-4580-A035-AFA0A835E866

We here provide a response and additional notes to the comments made by Böhme, Koch and Ziegler (2016) to Case 3676, wherein they dispute the proposal by Weirola (2016) to replace the neotype of *Varanus indicus* with a topotypic specimen.

1. It is agreed that the holotype of *Tupinambis indicus* Daudin, 1802, is lost and that sufficiently diligent searches have been made so that this can be accepted as fact. Daudin’s (1802) illustration is sufficient to confirm that the lost type represents a *Varanus* in the *indicus* group, but not to distinguish among several similar species in that group.

2. The type locality “Ambon” is not in dispute, and has since the original description been associated with the name *Varanus indicus*.

3. Following Boulenger (1885) the name *Varanus indicus* was widely applied to a phenotypically variable mid-sized, mangrove-inhabiting monitor species distributed across the SW Pacific, coastal New Guinea and northern Australia. The range of descriptions applied to it is comparatively broad, and features we now know to vary among species are not typically given as characteristic of *V. indicus* sensu stricto until Brandenburg (1983) (e.g., large dorsal scales, faint dorsal banding, pale throat). Tongue color was not specified until noted by Sprackland (1992) and Böhme et al. (1994).

4. As new species were recognized it became important to establish the character states in *V. indicus*. Unfortunately in doing so the Bonn group evidently did not consult topotypic material, but relied instead on their own typological concept of *V. indicus*. All Ambon specimens with reliable data in collections worldwide represent a single species that does not match this concept. Rather than accept that this animal with faint dorsal bands and a pink and gray tongue was Daudin’s *V. indicus*, Philipp et al. (1999) located two untagged specimens in a jar containing the label “Ambon” and concluded that two *Varanus* species occupied Ambon.

5. This is a simple error stemming from a failure to accept the most logical view, and *Varanus cerambonensis* should correctly stand as a junior synonym of *V. indicus*.

6. As detailed by Weirola (2015) and Weirola & Sweet (2015) all properly documented museum specimens and all field work from Ambon and the adjacent islands of Buru, Seram, and Saparua point to a single-species system. While one can never prove a negative, common sense should take preference over obscurantism at some point.

7. The counterproposal made by Böhme et al. (2016) to change the type locality to match their erroneous selection of a neotype, simply compounds the error. Improvising a locality, as proposed by Böhme et al. (2016) for the undocumented neotype is an
unacceptable solution as it has the potential to cause long lasting damage to future work on the taxonomy of the *V indicus* species group. The only undisputed attribute of *V. indicus* is that it is native to Ambon, and this should be retained as definitive.

References


Comment (Case 3682) – Support for the proposed suppression for nomenclatural purposes of the work ‘The White-cheeked Geese’ by Harold C. Hanson
(Case 3682; see BZN 72: 209–216 [Case]; 311–313)

Frank Rheindt

Department of Biological Science, National University of Singapore, 14 Science Drive 4, Singapore 117543 (e-mail: dbsrfe@nus.edu.sg)

I am a member of the Working Group on Avian Nomenclature (WGAN) and I am writing to lend my full support to the recent Case 3682 (Banks et al., 2015) that seeks the suppression of numerous scientific names of geese proposed in a work by Dr Harold Hanson.

The Commission frequently deals with cases put forth by nomenclatural practitioners from a particular animal group (e.g. reptiles) who have become disenchanted with prolific colleagues who hastily publish works containing numerous novel names, often pre-empting the more comprehensive research of other taxonomists. In such cases, the Commission faces the difficult decision of whether the indignation of a substantial part of the community warrants blacklisting names that may be nomenclaturally available.

It is important for me to point out to the Commission that Case 3682 is not one of those cases. In this case, there is absolutely no doubt on the part of anybody within WGAN and the extended community that the names published by Hanson are nomenclatural rubbish. Most of these names refer to taxonomic nonentities and are entirely based on slight individual variation. While this may be a taxonomic (not a nomenclatural) problem, the nomenclatural catastrophe arises when one considers that many of these Hanson names cannot be attributed to type specimens and/or proper type localities, making them essentially useless not only in a taxonomic sense but also in a nomenclatural sense.

Reference

Comment (Case 3688) — On the proposed suppression of CHARINIDAE Gray, 1849 (Reptilia, Squamata, Serpentes): a counter-proposal
(see BZN 73(1): 25–29 [Case]; 73(2–4): 124–126)

Jay M. Savage
Department of Biology, San Diego State University, San Diego, California 93182–4614, U.S.A. (e-mail: savy1@cox.net)

Brian I. Crother
Department of Biological Sciences, Southeastern Louisiana University, Louisiana 70402, U.S.A. (e-mail: bcrother@selu.edu)

http://zoobank.org/urn:lsid:zoobank.org:pub:1ASB46DF-C677-4263-AAA9-73BBF0E00170

1. A better solution to the homonymy between the family-group names CHARININA Gray, 1849 and CHARINIDAE Quintero, 1986 would be to emend the less used name for the snake family-group to CHARINIDINA Gray, 1849 (see also the original proposal by Quintero & Shear, 2016). The proposal to place the family-group name UNGALIOPHEINAE McDowell, 1987 on the Official List of Family Group Names in Zoology is another matter. All authors prior to McDowell (1987) who was apparently influenced by Smith & Preston (1987), accepted the nominative plural of the Greek ophis as ophios (as indicated in most classical Greek lexicons). These authors consistently proposed and listed snake family-group names as terminating in: -idae, -iinae, -iina, and -iini (e.g., CYLINDROPHIIDAE from Cylindrophis). Indeed, with the exception of McDowell (1987) all the authors cited in the application used the spelling UNGALIOPHIINAE Of UNGALIOPHIDAE when referring to this family-group. The -ii form of these snake family-group names thus qualifies as being in prevailing usage.

2. The International Commission on Zoological Nomenclature is therefore asked:
(1) to use its plenary powers to rule that for the purposes of Article 29 of the Code, the stem of the generic name Charina Gray, 1849 is Charin-;
(2) to use its plenary powers to rule that the spelling UNGALIOPHEINAE McDowell, 1987 is an incorrect original spelling and to emend it to UNGALIOPHIINAE McDowell, 1987;
(3) to place on the Official List of Family-Group Names in Zoology the following names:
(a) CHARINAIDAE Gray, 1849, type genus Charina Gray, 1849, spelling emended by the ruling in (1)(a) above (Reptilia, Squamata);
(b) CHARINIDAE Quintero, 1986, type genus Charinus Simon in Raffray, Bolivar & Simon, 1892 (Arachnida, Amblypygi);
(c) UNGALIOPHIDAE McDowell, 1987, type genus Ungaliophis Miller, 1880 (Reptilia, Squamata);
(4) to place on the Official List of Generic Names in Zoology:
(a) Charina Gray, 1849 (gender: feminine), type species by monotypy Tortrix bottae Blainville, 1835;
(b) Charinus Simon in Raffray, Bolivar & Simon, 1892 (gender: masculine), type species by original designation Phrynus australianus L. Koch, 1867;
(c) Ungaliophis F. Müller, 1880 (gender: masculine), type species by monotypy Ungaliophis continentalis F. Müller, 1880.

References


Koch, L. 1867. Beschreibung neuer Arachniden und Myriapoden. Verhandlungen der kaiserlich-


Quintero, D. 1986. Revisión de la clasificación de amblypygidos pulvinados: creación de sub-


Comment (Case 3688) — On the proposed suppression of CHARINIDAE Gray, 1849 (Reptilia, Squamata, Serpentes): a counter-application
(see BZN 73(1): 25–29 [Case]; 73(2–4): 122–123)

R. Alexander Pyron
Dept. Of Biological Sciences, The George Washington University 2029 G St. NW, Washington DC 20052, U.S.A. (e-mail: rpyron@colubroid.org)


In the recently published Case 3688, Quintero & Shear (2016) outline a proposal for suppressing CHARINIDAE Gray, 1849 in favor of CHARINIDAE Quintero, 1986. This invokes the Commission’s plenary power, as Reversal of Precedence under Article 23.9.1.1 does not apply. The authors instead invoked Article 23.9.3, and petitioned for a special ruling from the Commission, arguing for stability. To give a more complete picture of the nomenclatural history, I provide the following information:

1. Gray (1849, p. 84) placed Charina Gray, 1849 in a family-level group he named CHARININA.

2. Cope (1886a, p. 294) later described a family CHARINIDAE Cope, 1886, containing only Charina Gray, 1849. Cope then used this name in numerous subsequent publications, including Cope (1886b, p. 480, 1889, p. 869, 1892, p. 592, 1895, p. 199, 1900, p. 727).

3. Boulenger (1887, p. 16) made note of Cope’s new family in the Zoological Record. Boulenger later listed CHARININA Gray, 1849 in the synonymy of BOINAE (Boulenger, 1893, p. 93), while also listing CHARINIDAE Cope, 1886 in the synonymy of BOIDAE (Boulenger, 1893, p. 71). Similarly, Boettger (1887) discussed and described Cope’s (1886b) classification, listing CHARINIDAE as a separate family citing Cope (1886a) and giving characters for the group (Boettger, 1887, pp. 186, 187, 193). Boettger (1890) cited Cope’s (1889) classification again, also listing CHARINIDAE as a separate family (Boettger, 1890, p. 198).

4. Whitney & Smith (1914, p. 930) listed a dictionary entry for Charina Gray, 1849, and subsequent entries referring to charinid (snakes of the family CHARINIDAE), CHARINIDAE (the family containing Charina), CHARININA (a subfamily containing Charina), and charinoid (resembling CHARINIDAE).

5. Shufeldt (1903, p. 406) referred to CHARINIDAE in his description of California snakes. He did not cite a taxonomic authority, but referred to Cope elsewhere in the article. Noguchi (1909, p. 3) cited Cope’s (1900) system of snake classification, listing CHARINIDAE as a separate family in a table. Gilmore (1938, p. 7) also cited Cope’s (1900) system of snake classification, listing CHARINIDAE as a separate family in a sentence describing its characters. Von lhering (1911, p. 289) cited Cope’s (1895) classification and listed CHARINIDAE in a table, for his review of Brazilian snakes. Prado (1945, p. 61), in his review of Brazilian snakes, cited Cope’s classification without a specific reference and provided characters for CHARINIDAE. Oguiura et al. (2009) used the names CHARININAE (Oguiura et al. 2009, p. 130) and CHARININI (Oguiura et al. 2009, p. 131) in passing, without citing a taxonomic authority.
6. Pyron et al. (2014) reinstated CHARINIDAE Gray, 1849 as the correct family-group nomen for the clade containing Charina Gray, 1849, discussed the history of Cope’s junior objective synonym CHARINIDAE Cope, 1886, and noted the homonymy issue with CHARINIDAE Quintero, 1986.

7. The name CHARINIDAE Gray, 1849 has begun to be re-adopted by the herpetological community, particularly in regional checklists such as Ray & Ruback (2015, pp. 172, 174, 176), Solis et al. (2014, p. 133), Sunyer (2014, p. 193), and Abbas Rhadi et al. (2015, p. 149), and conservation assessments such as Johnson et al. (2015, pp. 27, 30, 33, 40, 86). Head (2015, pp. 3, 8, 9) also used CHARINIDAE and CHARININAE, citing usage as per Pyron et al. (2014), in his review of fossil snakes. Figueroa et al. (2016), in their systematic phylogenetic assessment and revision of Serpentes, also adopted CHARINIDAE, again citing Pyron et al. (2014) for usage. The Reptile Database has also now adopted CHARINIDAE Gray, 1849; this reference is the community standard for herpetology.

8. CHARINIDAE Gray, 1849 (= CHARININA Gray, 1849) is clearly an available and valid name. There is a strong historical tradition of usage for the junior objective synonym CHARINIDAE Cope, 1886, and a recent prevailing usage of CHARINIDAE Gray, 1849 in the herpetological community since the publication of Pyron et al. (2014). Thus, CHARINIDAE Gray, 1849 is not a nomen oblitum under Article 23.9.2, and its usage does not threaten the stability of serpent nomenclature.

9. Therefore, I suggest that CHARINIDAE Gray, 1849 should not be suppressed, but instead, the junior homonym CHARINIDAE Quintero, 1986 be replaced by an appropriately emended name such as CHARINUSIDAE.

10. The International Commission on Zoological Nomenclature is accordingly asked:

(1) to use its plenary power to rule that for the purposes of Article 29 of the Code:
   (a) the stem of the generic name Charinus Simon, 1892 is Charinus-;

(2) to place on the Official List of Generic Names in Zoology the following names:
   (a) Charina Gray, 1849 (gender: feminine), type species by monotypy Tortrix bontae Blainville, 1835;
   (b) Charinus Simon, 1892 (gender: masculine), type species by original designation Phrynus australianus L. Koch, 1867;

(3) to place on the Official List of Family Group Names in Zoology the following names:
   (a) CHARINUSIDAE Quintero, 1986, type genus Charinus Simon, 1892, spelling emended by the ruling in (1)(a) above (Arachnida, Amblypygi);
   (b) CHARINIDAE Gray, 1849, type genus Charina Gray, 1849 (Reptilia, Squamata).

References


Comment (Case 3700) – Support for the proposed designation of *Diplodocus carnegii* Hatcher, 1901, as type species of *Diplodocus* Marsh, 1878


Cary Woodruff

*Director of Paleontology, Great Plains Dinosaur Museum, 405 N 1st Ave E Malta, Montana 59538, U.S.A. (e-mail: sauropod4@gmail.com)*


I am writing in support of Dr. Emanuel Tschopp and Dr. Octavio Mateus’s petition to change the type species of *Diplodocus* from *D. longus* to *D. carnegii* (see Tschopp & Mateus, 2015). The type species material, YPM 1920 consists of an incomplete and fragmentary caudal series. Yet throughout the vast majority of paleontological literature, referral to *Diplodocus*, or direct comparison to fossil material is done so via the holotype of *D. carnegii*, CM 84. Consisting of a largely complete skeleton, CM 84 was the first widely distributed casted dinosaur. Thus, copies of CM 84 grace many of the world’s major natural history museums, making CM 84 the most recognized and widely known specimen of *Diplodocus*. Due to the world wide fame and extensive history of scientific referral, CM 84 largely appears in an unofficial capacity as an “acting type specimen”. Dr. Tschopp and Dr. Mateus’s petition (Tschopp & Mateus, 2015) to officially make it the type species is a well-constructed and -executed petition, and as a sauropod paleobiologist, I completely agree with and support their endeavor.

Reference

Comment (Case 3700) – Support for designating Diplodocus carnegii Hatcher, 1901 as the type species of Diplodocus Marsh, 1878 (Dinosauria, Sauropoda) (see BZN 73(1): 17-24 [Case]; BZN 73(2-4): 127, 129-131, 132-133, 134-135)

Spencer G. Lucas

New Mexico Museum of Natural History, Albuquerque, New Mexico 87104, U.S.A.
(e-mail: spencer.lucas@state.nm.us)


1. I strongly support the application of Tschopp & Mateus (2016) to replace Diplodocus longus Marsh, 1878, the type species of the sauropod dinosaur Diplodocus, with D. carnegii Hatcher, 1901. As has been well demonstrated by Tschopp & Mateus, the holotype of D. longus is an undiagnostic portion of the tail skeleton, rendering D. longus a nomen dubium (nomen vanum). In contrast, the holotype of D. carnegii is one of the most complete fossil specimens of Diplodocus collected, and it includes diagnostic skeletal material. Furthermore, replicas (casts) of a composite skeleton of D. carnegii are on display at museums worldwide, and have come to represent the concept of Diplodocus in the technical and popular scientific literature.

2. Diplodocus is the genus name of an iconic Late Jurassic sauropod dinosaur, and, as such, it has been used in thousands of scientific and popular science publications. The fact that the type species of the genus is a nomen dubium thus threatens the continued use of Diplodocus as a dinosaur generic name, and may even necessitate proposal of a replacement name for the genus. Therefore, it serves both the universality and the stability of zoological nomenclature to redesignate the type species of Diplodocus as a species based on diagnostic type material, as proposed by Tschopp & Mateus (2016) in their application. I urge the Commission to accept their application by designating D. carnegii Hatcher, 1901 the type species of Diplodocus Marsh, 1878.

Reference

Comment (Case 3700) – A statement against the proposed designation of
Diplodocus carnegii Hatcher, 1901 as the type species of Diplodocus Marsh, 1878
(Dinosauria, Sauropoda)
(see BZN 73(1): 17–24 [Case]; BZN 73(2–4): 127, 128, 132–133, 134–135)

Mickey Mortimer
27988 Maple Ridge Way SE, Maple Valley, Washington 98042, U.S.A.
(e-mail: Mickey_Mortimer11@msn.com)


Tschopp & Mateus (2016) propose to designate Diplodocus carnegii as the type species of Diplodocus, as they view the current type species D. longus as undiagnostic. However, there are problems with this proposal:

1. While the authors view D. longus as indeterminate, this opinion is based on a phylogenetic analysis published as Tschopp et al. (2015), which merely found it had no unique characters within those used to construct the data matrix. This led to it forming a polytomy with the D. carnegii holotype, the D. carnegii paratype, and a clade of specimens forming D. hallorum. Yet the particular characters used to construct a matrix do not represent every potential aspect of morphological variability, and indeed depending on the matrix used many diagnostic species would score identically in many analyses. Nor is the undiagnostic status of D. longus even consensus, as the previous publication to examine the problem (McIntosh & Carpenter, 1998) concluded D. longus was distinct based on having shorter caudals with pleurocoels that extend less far distally in the tail, and being stratigraphically older as well. Tschopp & Mateus (2016) did not engage with these arguments, which even if invalid underlie the point that their proposal is based on a single paper published less than a year earlier.

2. The authors state “retention of D. longus as type species would create insecurities and confusion concerning the use of Diplodocus as a genus”, but provide no evidence of this. They explicitly state D. longus “can be clearly referred to the genus Diplodocus as generally perceived”, so that its use as both a type species and specifier for phylogenetic nomenclature is unharmed. Due to the influence of Wilson & Upchurch (2003), who believed Titanosaurus to be undiagnostic and stated “co-ordinate suprageneric Linnean taxa must likewise be abandoned”, there is a mistaken assumption among some dinosaur researchers that eponymous suprageneric clades must be based on diagnostic species. Yet the Code says no such thing, and indeed Article 11.7.1.1 only states the eponymous genus “must be a name then used as valid in the new family-group taxon” (emphasis mine). As Diplodocus longus was seen as valid by Marsh (1884) when he named Diplodocidae, any eponymous coordinate taxa covered by the Code are unaffected. Similarly, Tschopp & Mateus (2016) merely state that as an eponymous genus, Diplodocus “should not be typified by an undiagnosable type species” without stating either Code rules or functional consequences that support this. Although phylogenetic nomenclature is not covered by the ICZN and the Phylocode is still in draft stage, it should be noted that as long as Diplodocus longus falls within our concept of Diplodocus, its undiagnosability within the genus has no effect on suprageneric phylogenetic definitions. If D. longus might belong
outside the concept of Diplodocus as currently used, there would be a case, but Tschopp & Mateus (2016) agree with every prior author that that is not likely.

3. The claim by Tschopp & Mateus (2016) that a future in which “new studies show that D. longus and D. carnegii are in fact the same species” is improbable, but that is just what Tschopp et al.’s (2015) results would suggest is possible. Recall D. longus forms a polytomy with D. carnegii specimens in their analysis, and is not a member of the D. hallorum clade. Thus D. longus is equally likely to be sister to D. carnegii + D. hallorum, sister to D. hallorum, or sister to or synonymous with D. carnegii. As they claim there are no characters outside those viewed as individual variation that differ between D. longus and D. carnegii, synonymization seems even more plausible.

4. Finally, the Tschopp & Mateus (2016) argue the cases of Cetiosauriscus, Cetiosaurus and Stegosaurus are similar and were accepted by the Commission. However, the first two cases involved type species whose holotypes could not be referred to the contemporary concept of the genus in question. The holotype of ‘Cetiosauriscus’ leedsi is from a different formation than the resulting type species C. stewarti, and was last thought to be a macronarian (Upchurch & Martin, 2003), while C. stewarti has never been assigned to that clade. Similarly, Upchurch & Martin (2003) could only assign the syntypes of ‘Cetiosaurus’ medius to Sauropoda, while the resulting type species Cetiosaurus oxoniensis is recognized as a non-neosauropod eusauropod. The case of Stegosaurus is similar in that the original type was never agreed by consensus to be undiagnostic, had a previous author argue for diagnosability, and was agreed to belong to the genus in question. In that case too, its author argued “Stegosaurus armatus MARSH 1877 is a nomen dubium, and Stegosaurus is not available as a genus or as the basis for the Stegosaurinae, Stegosauridae, Stegosauroida or Stegosauria” without recourse to rule or consequence. The result of that decision, when the original holotype has never even been described in detail or in a modern context, should be avoided in the future.

5. Notably, there is precedent in vertebrate paleontology for an apparently indeterminate type species being retained in the company of diagnostic species. Lihoreau et al. (2014) believe the holotype of the type species of Libycosaurus is lost and cannot be distinguished from the three valid species based on available information, but does belong to the current concept of that genus. Thus they leave the type species as a nomen dubium but also have three valid species in the genus. This is what should be done with Diplodocus – retain the type D. longus as possibly indeterminate, and use D. carnegii and D. hallorum as diagnostic species.

References


Comment (Case 3700) — On the proposed designation of Diplodocus carnegii Hatcher, 1901 as the type species of Diplodocus Marsh, 1878 (Dinosauria, Sauropoda): application should be rejected based on new data (see BZN 73(1): 17–24 [Case]; BZN 73(2–4): 127, 128, 129–131, 134–135)

Vahe D. Demirjian

11 Canyon Terrace, Newport Coast, California 92657, U.S.A.
(e-mail: vahedemirjian@cox.net)


1. In a newly published abstract concerning the temporal distribution of Morrison Formation diplodocid sauropods, Tschopp et al. (2016) retreat from their previous treatment of Diplodocus longus as a nomen dubium (Tschopp et al., 2015) by suggesting that D. longus could be ancestral to D. carnegii and D. hallorum, judging from an overview of the temporal distribution of Morrison diplodocid specimens. When putting specimens of Morrison diplodocines into a stratigraphic context, the type locality of D. longus at Felch Quarry 1 in Garden Park, Colorado is situated low in the Brushy Basin Member of the Morrison Formation in contrast to the localities of known specimens of D. carnegii and D. hallorum, which are situated in the middle section of the Brushy Basin Member (see Turner & Peterson 1999, p. 86, fig. 7). Moreover, the fact that Galeamopus specimens partially overlap with the earliest known occurrences of Diplodocus (the Galeamopus skull USNM 2673 was found at the type locality of Diplodocus, and the Galeamopus skulls AMNH 969 and SMA 0011 were found lower in the Morrison Formation in the Salt Wash Member) lends support to the conclusion by Tschopp et al. (2016) that D. longus might be ancestral to D. carnegii and D. hallorum, because the occurrence of the diplodocine skulls CM 11255 (considered possibly belong to Barosaurus; Melstrom et al., 2016), CM 3452, CM 11161, and USNM 2672 within the range of unequivocal Diplodocus specimens suggests that Barosaurus/Kaatedocus-like diplodocines stratigraphically co-existed with Diplodocus/Galeamopus-like forms.

2. Based on the as-yet-unpublished results of the abstract by Tschopp et al. (2016), I urge the Commission to reject the proposals in Case 3700 (Tschopp & Mateus, 2016) if the “Morosaurus” agilis holotype (USNM 5384) is confirmed to be from the same individual as YPM 1920 as stated by Tidwell et al. (2005).

References


I write in support of the proposal by Tschopp & Mateus (2016) to designate the well-known species Diplodocus carnegii Hatcher, 1901 as the type species of the genus Diplodocus Marsh, 1878.

1. The problem afflicting Diplodocus is a familiar one to dinosaur workers: when working with very large animals that died many millions of years ago, most specimens are incomplete, and often very uninformative. In itself this does not cause difficulties: fragmentary specimens need not be the basis for major studies. But the issue was greatly exacerbated by the “Bone Wars” of E.D. Cope and O.C. Marsh, rival palaeontologists in the late nineteenth century of the USA, who each aimed to outdo the other by naming more species of fossil animals. As a result, many dinosaur species were named on the basis of non-diagnostic remains – as the Commission recognised in the case of Stegosaurus Marsh, 1877, for which it designated a replacement type species in 2013 (Opinion 2320 on Case 3536, ICZN, 2013).

2. Despite being one of the most completely known of all dinosaurs, and among those best known to the general public, Diplodocus suffers badly from this syndrome. It was founded by Marsh on a non-diagnostic fragmentary specimen (YPM 1920), which supposedly functions as the type specimen of the type species, D. longus. Meanwhile, the nearly complete mounted skeleton of Diplodocus carnegii CM 84, the holotype of its species, is on display at the Carnegie Museum of Natural History in Pittsburgh, Pennsylvania. High-quality casts of this skeleton are displayed in London, Paris, Berlin, Madrid and numerous other museums. Due to its completeness and wide availability for study, this specimen has formed the basis of essentially all scientific work on Diplodocus since its description by Hatcher (1901). For example, in my own work alone, half a dozen papers extensively discuss Diplodocus, using or implying D. carnegii throughout: Taylor & Naish (2005), Taylor et al. (2009), Taylor (2010), Taylor & Wedel (2013), Wedel & Taylor (2013), Taylor (2014). This includes the paper that formulated the phylogenetic definitions of the clades Apatosaurinae and Diplodocinae, both of which use Diplodocus as a specifier (Taylor & Naish, 2005). Other related clade definitions either use D. carnegii explicitly, or simply specify Diplodocus, with D. carnegii implicitly understood by long precedent.

3. In its use as the definitive exemplar of the genus Diplodocus, as the foundation for numerous palaeobiological studies of the genus, and as the specifier for numerous important clades, the species D. carnegii is already effectively functioning as the type species of Diplodocus. Therefore the petition of Tschopp & Mateus (2016) requests only that the commission recognises de jure what is already the case de facto.
4. It may be argued that the present holotype, that of *D. longus* (YPM 1920) is adequate despite its non-diagnostic nature, on the basis that it falls in a clade with other *Diplodocus* specimens in the recent phylogenetic analysis of Tschopp et al. (2015). I do not find this argument persuasive. As Tschopp et al. (2015, p. 176, fig. 117) explain, YPM 1920 is one of the most phylogenetically unstable OTUs in their analysis, and was one of those that had to be removed *a posteriori* in order to obtain a reduced consensus tree. It is very possible that future analyses, on adding new specimens or new characters, will resolve a topology in which YPM 1920 falls outside the *Diplodocus* clade, which would greatly disrupt nomenclature and include numerous important clades.

5. For these reasons, I support the petition to establish the well-represented, diagnostic, phylogenetically stable and universally referenced species *D. carnegii* as the replacement type species of the genus *Diplodocus*.

References


Comment (Case 3703) — A statement against the proposed designation of a neotype for *Nautilus pompilius* Linnaeus, 1758 (Mollusca, Cephalopoda, Nautilida) (see BZN 72(4): 274–285 [Case]; 73(1): 48; 73(2–4): 139–143)

Ian G. Gleadall

*International Fisheries Science Unit, Graduate School of Agricultural Sciences, Tohoku University, Amamiya 1–1, Sendai, Japan 981–8555*  
(e-mail: octopus@bios.tohoku.ac.jp)


This comment argues against the proposed designation of a neotype. The authors of the proposal have used the extant syntype series to identify their specimen as the same species as that described by Linnaeus, so there is no apparent justification for designating this specimen as a neotype. The prospective neotype is just a voucher specimen useful in redescribing the species and distinguishing it from closely related taxa.

1. The original description did not designate types, so the proposal discussed at length a number of specimens eligible as syntypes by indication in Linnaeus (1758). This was based mostly on a previous detailed discussion of the types of *Nautilus pompilius* by one of the authors (Nikolaeva, 2015). To summarize, the proposal recognized six specimens as comprising the extant syntype series, although for none of these was the locality stated (neither in the proposal itself, i.e., Nikolaeva et al., 2015, nor by Nikolaeva, 2015).

   a) paragraph 12 discussed the shell of a young animal in the Linnean Society collection: this is a syntype (whatever its developmental stage or condition) because it was a specimen identified as *Nautilus pompilius* and possessed by Linnaeus.

   The number of specimens treated in paragraph 13 is a little obscure and few registration details were given (localities in particular), but it is deduced from the proposal that the following are syntypes extant in Uppsala University Museum:

   b) 4 specimens (1 specimen broken & etched, 2 subadults and 1 adult).

   c) 1 specimen, no. 880 (listed by Linnaeus, 1764, as no. 149). It is a large shell, diameter 180 mm, with a complete aperture, closed umbilicus and a well-preserved characteristic colour pattern.

2. These types were mostly not described in detail (referring the reader to Nikolaeva, 2015, which includes photographs of most extant syntypes), but the last mentioned syntype specimen in particular appears to be a suitable example of the species identified by the authors as *Nautilus pompilius* Linnaeus. The Code discourages lectotype designation unless justified and no doubt was expressed in the proposal as to the identification of any of the extant syntypes. Therefore, the logical action to follow would be to redescribe the species *Nautilus pompilius* with reference to the six available syntypes, accompanied by morphological information from a selection of new voucher specimens at different growth stages and corresponding DNA sequences. The redescriptions would then be sufficient to identify the species (and to distinguish any subspecies) to fulfil the aims of managing its fisheries and survival. There is no apparent necessity for a neotype designation.

3. In paragraph 14, the authors stated that, “it is logical to interpret Ambon as the type locality for the species”. However, this is only true if one of the Rumphius specimens were to be designated as lectotype (or if that happened to be the locality of a valid
neotype designation), otherwise the type locality is interpreted as the locality, where known, of each of the specimens in the syntype series (Article 73.2.3 of the Code), whether or not those localities were cited in the original description. The authors did not mention the localities of the extant syntype specimens, but if Ambon is the only syntype locality recorded then that is the type locality of *Nautilus pompilius*.

4. In paragraph 15, the authors reviewed proposals for subspecific distinctions among different populations of *Nautilus pompilius* noting that, from recent genetic studies, this species is distributed throughout much of the western Central Pacific and Southeast Asia, with distinct population structure within the species noted for Indonesia, Western and Eastern Australia and the Philippines. Subspecies distinctions were not recognized except for one other possible species from the Philippines originally described as *Nautilus pompilius suluensis*. If subspecies were to be recognized subsequently, the type locality Ambon is fairly central within the known distribution of *Nautilus pompilius*, which also would be the type locality of the nominotypical subspecies *N. pompilius pompilius*. Therefore, on the evidence presented in the proposal, the problems of describing other subspecies, species and their respective type localities and distributions can be approached without the necessity for designating a neotype for *N. pompilius*.

5. In paragraph 16, the authors discussed “unidentifiable name-bearing types” but it is not clear what they meant by this statement. It could mean that they have been unable to identify which of the specimens at their disposal are type specimens, but clearly that is not so (apparently there are six extant syntypes). If they meant that name-bearing type specimens cannot be identified as *Nautilus pompilius*, that also does not seem to be so, at least with reference to Uppsala University Museum specimen no. 880. As the authors themselves acknowledge in paragraph 17: “The proposed neotype is consistent with what is known on the shell pattern and morphology of syntypes and from other sources and agree[s] with the prevailing usage”. Since the proposed neotype was identified with reference to the syntype series, the latter takes precedence as type material and, on the evidence provided, there is no justification for designating a neotype.

6. A further point not emphasized in the proposal is that for each of the syntype specimens only the shell is extant, the animal itself not represented (except as illustrations of animals identified as syntypes which are no longer extant). Absence of the animal itself from any type material is perhaps the point at the heart of this proposal. However, Article 72.5.1 of the Code states that, for species described before 1931, “... any part of an animal... or of the work of an extant animal...” is eligible to be a name-bearing type.

7. For this species, then, the only potential problems affecting type material and its designation seem to be the identification and locality of each of the syntypes. If any were to be identified as a different species or subspecies, it could be removed from the syntype series during a redescription of *Nautilus pompilius*, and designation of a lectotype could be considered. Otherwise, Ambon is the type locality and all six extant syntype specimens comprise the type material. The details of morphology and molecular analysis from voucher specimens taken in the vicinity of Ambon would provide the necessary base from which to identify other taxa closely related to *Nautilus pompilius*.

8. Justification for designating a neotype would only arise if more than one *Nautilus* taxon were to be identified in the vicinity of Ambon and none of the extant syntypes could be identified as one or other of those taxa. From correspondence with the authors, it seems clear that a comprehensive review of the extant nautiloids is required to resolve the number of extant taxa. However, in my opinion, a clearly justified case has yet to be
made for setting aside the syntypes and designating a neotype, bearing in mind also that *Nautilus pompilius* is the type species of genus *Nautilus*.

**References**


Svetlana V. Nikolaeva

*Department of Earth Sciences, The Natural History Museum, London; SW7 5BD, U.K.; Borissiak Paleontological Institute, Russian Academy of Sciences, ul. Profsoyuznaya 123, Moscow, 117997 Russia; Kazan Federal University, ul. Kremlyovskaya 4/5, Kazan, 420008 Russia* (e-mail: s.nikolaeva@nhm.ac.uk)

W. Bruce Saunders

*Department of Geology, Bryn Mawr College, Bryn Mawr, Pennsylvania 19010, U.S.A.* (e-mail: wsaunder@brynmawr.edu)

Royal Mapes

*Department of Biological Sciences, Ohio University, Irvine Hall, Athens, OH 45701, U.S.A.* (e-mail: mapes@ohio.edu)

A. Louise Allcock

*Ryan Institute and School of Natural Sciences, National University of Ireland, Galway, University Road, Galway, Ireland* (e-mail: louise.allcock@nuigalway.ie)


These comments are made in response to the criticisms outlined in I. Gleadall's comment on Case 3703 submitted to the Commission Secretariat and to the authors (see Gleadall, 2017).

1. Gleadall believes that the five (he incorrectly says “six”) available ‘syntypes’ are sufficient for recognition of the species, and that the neotype designation is unnecessary. There are several incorrect assumptions in his main line of argument, which we will explain below.

2. Gleadall does not appreciate that not all of the presumed ‘syntypes’ are actually confirmed syntypes. Of five historical specimens now in collections, only two subadult specimens in the Uppsala collection can be confirmed to have been specimens known to Linnaeus prior to 1758. The remaining three specimens (a juvenile in the Linnean Society in London, one adult specimen in Uppsala (no. 880), and a broken and etched specimen in Uppsala) are not confirmed syntypes. This is clearly outlined in the proposal and backed by the historical study by Nikolaeva (2015). For the sake of clarity we will briefly summarize the status of these specimens here. Extensive historical documentation is presented by Nikolaeva (2015).

   (a) There is no evidence that the juvenile specimen in the Linnean Society in London was known to Linnaeus prior to 1758 (the specimen is not mentioned in any edition of *Systema Naturae*, and there is no original label). It could have been placed in the collection at any time before 1823. Gleadall writes “... shell of a young animal in the Linnean Society collection: this is a syntype (whatever its developmental stage or condition) because it was a specimen identified as *Nautilus pompilius* and possessed by
Linnaeus”. This statement confuses the concept of syntypes, which are parts of the type series (Articles 72.4 and 73.2 of the Code); the type series includes only those specimens that served as part of the basis for the original description, and not just any specimens identified as the species in question and possessed by the author in an unspecified period of the author’s life.

(b) There is no evidence that Linnaeus handled specimen no. 880 in Uppsala before 1758. It is not included in the 10th Edition of Systema Naturae (1758), and was mentioned for the first time by Linnaeus in 1767.

(c) The broken and etched shell in the Uppsala collection is most certainly not a syntype, because the original specimen apparently known to Linnaeus had an illustration of insects (Holm, 1957), whereas the shell housed now in the Uppsala collection has an engraving of a pig.

(d) Gleadall concludes that “the logical action to follow would be to redescribe the species Nautilus pompilius with reference to the six available syntypes, accompanied by morphological information from a selection of new voucher specimens at different growth stages and corresponding DNA sequences”. However, no subsequent re-description of Nautilus pompilius based on the two subadult confirmed syntypes can possibly clarify the concept of this species, and it is also possible that they do not even belong to this species. The remaining three ‘syntypes’ should not be taken into consideration because of their unconfirmed syntype status.

3. Gleadall further says that the authors stated that Ambon could only be assumed as the possible type locality if one of the Rumphius specimens were to be designated as lectotype (or if that happened to be the locality of a valid neotype designation), otherwise the type locality is interpreted as the locality, where known, of each of the specimens in the syntype series. Gleadall says that no locality data are given by the authors of the application for any surviving specimen. This is indeed the case, but the data were not omitted because of the carelessness of the authors, but because these localities are not known. Linnaeus’ original description (1758) simply says ‘India’. There is indeed a good, logical case to be made for Ambon, Moluccas Is., Indonesia being the type locality, though strictly speaking it is not known where the specimens that Rumphius figured nor the ‘syntypes’ of Linnaeus were actually from. Rumphius stated that Nautilus occurs in that region. Similarly, there is no way to know where the various possible syntypes were from within the broad range of Nautilus pompilius, nor, in some cases, whether they are even N. pompilius s.s. or one of the various other ‘shell’ species that were named (e.g., N. repertus, N. stenomphalus, N. belauensis, and possibly even N. macromphalus) because of the state of preservation, size, etc.

4. There is no consensus on the number of ‘historical’ species of Nautilus (those based on phenotypic characters). Even since the first genetic/electrophoretic studies of Woodruff et al. (1987) and in 17 DNA works since Wray et al. (1995) (see Saunders & Landman, 2010) speculations have ranged from between one and seven, with sibling, geographically isolated-, sympatric- phylogenetic-, etc. terms being used by various authors. It might be that there is only one ‘superspecies’ (N. pompilius) with a series of geographic subspecies, with various ‘historic’ endemics (e.g., N. p. macromphalus, N. stenomphalus, N. belauensis, N. repertus, N. suluensis); and possibly Allonautilus scrobiculatus with A. perforatus as additional species or subspecies. These were mostly originally based on shells and most are endemic, and all occur within the geographic range of N. pompilius. A few may be sympatric (e.g., N. stenomphalus and N. pompilius
on the Great Barrier Reef [with hybridization?], A. scrobiculatus with N. pompilius in Manus, Papua New Guinea [and probably elsewhere]). N. p. suluensis was named as a subspecies, no genetics have been published, and its only known occurrence (Tubbataha Reef, Sulu Sea, Philippines) is a marine reserve that is difficult to access (but which may serve to protect it). However it too is well within the range of N. pompilius and it may even be sympatric with it.

5. Most material referred to as N. pompilius (live-caught and shells) is just assumed to be N. pompilius s.s. which has traditionally been assumed to occur in the Philippines, but Philippines material (we now know) differs from the material from Ambon, Indonesia, Papua New Guinea, Samoa, Australia, etc. The problem is, there has never been documented material from Ambon to compare with until now: this is the ‘new material’ Gleadall referred to which was collected by Saunders in Ambon in 1987, and referred to in Bonacum et al. (2011), Nikolaeva et al. (2015), etc. and is being described in detail by Saunders et al. (in review). A real, documented, localized neotype is needed to be able to establish what is and what is not N. pompilius s.s. This cannot now, or likely ever, be done using the available ‘possible syntypes’ and until a bona fide neotype is available to rectify this, there will continue to be this vague concept of what the type species, N. pompilius, is . . . but bear in mind, the available ‘possible syntype’ material may not even be from the Ambon, Indonesia region; it is not known.

6. Gleadall says that we acknowledge that the proposed neotype is consistent with what is known of the shell pattern and morphology of syntypes and from other sources and agrees with the prevailing usage. He concludes from that, that “Since the proposed neotype was identified with reference to the syntype series, the latter takes precedence as type material and, on the evidence provided, there is no justification for designating a neotype”. However, the statement of the proposed neotype being consistent with what is known about the species and with prevailing usage is an integral part of a neotype designation and is made in strict adherence to the requirements of Article 75 of the Code. This statement in no way implies that a neotype is not necessary if a specimen proposed as a neotype is consistent with what is known about the species. It simply states that all necessary precautions have been taken to maintain stability and prevailing usage, which is in line with the Code and best taxonomic practice.

7. Gleadall believes that for this species, “the only potential problems affecting the type material and its designation seem to be the identification and locality of each of the syntypes. If any were to be identified as a different species or subspecies, it could be removed from the syntype series during a redescriptions of Nautilus pompilius, and designation of a lectotype could be considered”. This is a confusing statement because, as we explained in the application, only two subadult specimens in Uppsala are unequivocal syntypes, and their identification is indeed problematic. They may belong to any subspecies of Nautilus pompilius, or even to a different species, and there is no way to identify them more precisely. In addition, a lectotype identification (albeit unnecessary in this case) will most certainly require a Commission action, because it is not possible to establish among the more than 500 published papers on Nautilus (Saunders & Landman, 2010), whether or not a lectotype has been already inadvertently designated by citing any specimen (surviving or illustrated) as type prior to 2000 (under Article 74.6 of the Code). To establish a neotype based on well localized live-caught material, with shells and soft parts, genetic and population data, ecological information, with associated fauna, etc., would anchor, once and for all, what N. pompilius is. It would provide a set of both
phenotypic and genetic characteristics that in some cases differ from other named species and the many known isolated population variants of *Nautilus pompilius* that have been or may be named as new species or as subspecies of *Nautilus* (e.g. *N. belauensis* (Palau), *N. repertus* (NW Australia), *N. suluensis* (Philippines), plus a described but unnamed form from American Samoa, etc.). There are a number (hundreds?) of other new and/or differing populations out there that have not yet been discovered or described (e.g. Vanuatu). Without solid data belonging to a certifiable type specimen of the type species from the type locality providing phenotypic characters like mature size, umbilical shape and closure (callus), color pattern, shell sculpture, aperture shape, etc., which cannot be determined from existing potential syntype material, there will probably never be solid, fact-based resolution of the various species (in particular, *N. pompilius*), or other named species, subspecies, variants, etc., using phenotypic and genetic characters. The extent to which molecular studies will resolve the status of these various forms remains to be determined. At present, there seems to be such disagreement and uncertainty that in many ways there has not been much progress since the first cladograms, which were based on electrophoresis by Woodruff et al. (1987).

While more background could possibly have made the proposal easier to follow and digest, we are not sure that there is a need to present a treatise on nautiloids to accompany the proposal for designating a neotype, when there are fairly recent and complete accounts of the historically phenotypic-based species, subspecies and even variants already out there (e.g. Saunders, 1981, 1987; Bonacum et al., 2011; Jereb, 2005). The recent and current appearance of new molecular studies of *Nautilus* and *Allonautilus* would make any attempt at synthesis incomplete at best given the lack of information available about the phenotypic characteristics of the Ambon population, which should, of course, be linked to the genetic data that just recently were obtained from the same Ambon, Indonesia material (and to an additional new Indonesian population from Sumbawa, Indonesia; Saunders et al., in review). The status of types in other extant nautiloids is not much better than that for *N. pompilius*. Only *N. belauensis* (Palau) was described and named with access to reposited soft parts and was supplemented by electrophoretic analysis, and later by DNA. (The description of *Allonautilus* was also, but the whereabouts of the types are unknown.) While a systematic effort to revise all nautiloid taxonomy would be admirable, a major impediment to this happening is and has been the muddy history and circumstances surrounding typology of the type species, which Nikolaeva et al. (2015) unraveled. Unquestionably, establishing a strongly based neotype for *N. pompilius* would be an important precedent and would go a long way toward clarifying what the type species of *Nautilus* really is, and it would unquestionably be quite helpful in aiding evaluation of the status of *N. stenomphalus*, *N. macromphalus*, *N. repertus*, *N. belauensis* and *A. perforatus* (for which a deposited holotype really does exist!).

8. To summarize, *N. pompilius* is by far the best-known and most commonly encountered (and trapped and marketed) species of *Nautilus*. But there is, until now, almost no information on the type species in its type locality; indeed, almost nothing new or substantive on Indonesian *Nautilus* has been published since Rumphius in 1705! This is being rectified, literally as this is being written (Saunders et al., in review) with a wealth of new morphological, ecological and genetic data from Ambon and a new, additional Sumbawa population (which appears identical to the former). The designation of a neotype from Ambon would end the uncertainty that has resulted from not having unquestionable, adequate type material of *N. pompilius*.
References


Comment (Case 3704) – Support for resolving the gender of the genus *Lepisma* Linnaeus, 1758
(see BZN 73(1): 7–16 [Case])

José G. Palacios-Vargas
Facultad de Ciencias, Universidad Nacional Autónoma de México, México

http://zoobank.org/urn:lsid:zoobank.org:pub:7AB1A2B4-B32C-41E0-A81A-817DDD97F70C

I would like to support the proposal of my colleagues, Molero-Baltanás et al. (2016, pp. 7–16) to resolve the ongoing issue involving the gender of the name of the silverfish genus *Lepisma* Linnaeus, 1758 and other generic names derived from *Lepisma*.

Reference

Opinion 2381 (Case 3629) — Vipera latastei Boscá, 1878 (Reptilia, Serpentes, Viperidae): conservation of the original spelling

International Commission on Zoological Nomenclature
c/o Lee Kong Chian Natural History Museum, 2 Conservatory Drive, Singapore 117377, Republic of Singapore (e-mail: iczn@nus.edu.sg)


Abstract. Under the specific powers the Commission has confirmed that Vipera latastei Boscá, 1878 is the correct original spelling of the specific name for the viper named for Fernand Lataste.

Keywords. Nomenclature; taxonomy; Serpentes; Viperidae; Vipera; Vipera latasti; Vipera latastei; Iberian Peninsula; North Africa; Lataste’s Viper.

Ruling

(1) Under the specific powers the International Commission on Zoological Nomenclature has confirmed that:
   (a) latastei Bosca, 1878, as published in the binomen Vipera latastei, is the correct original spelling of the specific name for the viper named for Fernand Lataste, as selected by Bosca (1879), deemed to be the First Reviser (under Article 24.2.4);
   (b) latasti Bosca, 1878, as published in the binomen Vipera latasti, is an incorrect original spelling of latastei Bosca, 1878;

(2) the name latastei Bosca, 1878, as published in the binomen Vipera latastei, and the correct original spelling as confirmed in (1)(a) above, is hereby placed on the Official List of Specific Names in Zoology;

(3) the name latasti Bosca, 1878, as published in the binomen Vipera latasti, and the incorrect original spelling of latastei Bosca, 1878, as confirmed in (1)(b) above, is hereby placed on the Official Index of Rejected and Invalid Specific Names in Zoology.

History of Case 3629

An application to conserve the specific name of the snake Vipera latastei Boscá, 1878 was received from Alfredo Salvador (Departamento de Ecologia Evolutiva, Museo Nacional de Ciencias Naturales, MNCN-CSIC, Madrid, Spain), Stephen D. Busack (North Carolina Museum of Natural Sciences, Raleigh, North Carolina, U.S.A.), Roy McDiarmid (USGS Patuxent Wildlife Research Center, Biological Survey Unit, National Museum of Natural History, Washington, DC, U.S.A.), Ivan Ineich (Muséum national d'Histoire naturelle, Reptiles, Dép. Syst & Evol., Paris, France) and José Carlos Brito (Universidade do Porto, Inst. Ciências Agr. Vairão, CIBIO Inbio, Ctr. Invest. Biodiversidade & Recursos Genet., Vairão, Portugal) on 31 May 2013. After correspondence the Case was published in BZN
The Case was sent for vote on 1 December 2015 (VP 22). A greater than two-thirds majority of Commissioners voted FOR the Case (23 For, 0 Against, 1 Abstain).

Decision of the Commission
At the close of the voting period on 1 March 2016 the votes were as follows:
Negative votes – none.
Abstain – 1: Alonso-Zarazaga.

Voting FOR, Aescht noted that she did not agree with all the points presented by the authors (Salvador et al., 2014) in the original application. Boscá (1878) introduced “multiple original spellings” (viz., “two or more different original spellings for the same name” [Article 32.2.1]), in his first publication on the new species. This is not mentioned in the text and the title, which requests for “conservation of the original spelling” (singular) (Salvador et al., 2014). Likewise, no reference is given to Article 31.1 that governs “[s]pecies-group names formed from personal names”. The species-group names *latastei* and *latasti* are both admissible under Article 31.1.1 (originating from the modern personal name “Lataste”), with the latter also available under Article 31.1.2 (starting from the stem “Latast-”). Both names thus are independently available. No evidence was given that *latasti* was an incorrect original spelling, and Article 32.4 does not apply and should have been removed from paragraph 9(1)(b) and paragraph 3, and the ruling in the Opinion modified. Finally, concerning the alternative spelling *latastii*, it should not just have been “considered a subsequent misspelling” in paragraph 4 of Salvador et al. (2014), but Articles 33.4 and 33.5 should have been applied to treat this as an incorrect subsequent spelling. Also voting FOR, Rosenberg stated that the authors of the case show that the information presented by Ineich & D’Hondt (in Alonso-Zarazaga, 2013) about dates of publication was incorrect because the stated pagination of part 2 did not match the signatures of the work. However, given that Salvador et al. (2014) restated where part 2 ends, they should have demonstrated anew the pagination of part 1. Merely stating that page 108 is blank was in itself not sufficient. Page 107 ends with the centred line “Paris. —Imp. LUCAN, r. Saint-Jacques, 221”. This publisher’s imprint, which does not appear on any previous page, is stronger evidence that part 1 had ended. This last piece of evidence led Rosenberg to accept that both spellings were published simultaneously in part 2.

Original descriptions
The following are the original descriptions to the entries on either an Official List or an Index in the ruling given in the present Opinion:

*latastei*, *Vipera*, Boscá, 1878: 201.

References


Opinion 2382 (Case 3637) – Conservation of the accustomed usage of *Papilio phoebus* De Prunner, 1798 by suppression of *Papilio phoebus* Fabricius, 1793 not approved (Insecta, Lepidoptera, PAPILIONIDAE)

International Commission on Zoological Nomenclature
c/o Lee Kong Chian Natural History Museum, 2 Conservatory Drive, Singapore 117377, Republic of Singapore (e-mail: iczn@nus.edu.sg)


Abstract. The Commission has not used its plenary power to conserve the name *Papilio phoebus* De Prunner, 1798 for the European ‘Small Apollo’ butterfly (PAPILIONIDAE) found in the Holarctic by giving it precedence over the senior primary homonym *Papilio phoebus* Fabricius, 1793.

Keywords. Nomenclature; taxonomy; PAPILIONIDAE; PARNASSIINAE; Parnassius; *P. phoebus*; *P. ariadne*; *P. corybas*; European ‘Small Apollo’ butterfly; Holarctic.

Ruling

1. The Commission has declined to use its plenary power to suppress the species name *phoebus* Fabricius, 1793, as published in the binomen *Papilio phoebus*, for the purposes of both the Principle of Priority and the Principle of Homonymy;
2. The species name *phoebus* Fabricius, 1793, as published in the binomen *Papilio phoebus*, has priority over *Doritis ariadne* Lederer, 1853 whenever the two names are considered to be synonyms;
3. The species name *phoebus* De Prunner, 1798, as published in the binomen *Papilio phoebus*, is a permanently invalid junior primary homonym of *Papilio phoebus* Fabricius, 1793;
4. No names have been placed on the Official Lists or Indexes in this ruling.

History of Case 3637

An application to conserve the specific name of the European ‘Small Apollo’ butterfly, *Papilio phoebus* De Prunner, 1798 was received from Emilio Balletto and Simona Bonelli (both from the Department of Life Sciences and Systems Biology, Via Accademia Albertina 13 – I-10123 Torino, Italy). After correspondence the Case was published in BZN 71(2): 75–80 on 30 June 2014 (Balletto & Bonelli, 2014). The title, abstract and keywords of the Case were published on the Commission’s website. No comments on the Case were received.

The Case was sent for vote on 1 December 2015 (VP 24). Less than half of Commissioners voted FOR the Case (10 For, 12 Against, 2 Abstain).

Decision of the Commission

At the close of the voting period on 1 March 2016 the votes were as follows:

Abstain — 2: Welter-Schultes and Zhou.  
Ng, Pyle and van Tol were on leave of absence.  

Voting FOR, Krell stated that the transfer of a binomen from one species to another one is amongst the most disruptive and confusing actions in nomenclature. The affirmative vote in this case was to ensure that *Parnassius phoebus* (De Prunner, 1798) can continue to be used for the species it has long been used for. Also voting FOR, Rosenberg stated that the application was incorrect in stating that new combinations will result from the requested action; a new association of specific and subspecific names is not a new combination. Furthermore, the lack of any discussion on the type material of De Prunner's taxon was a concern.  

Voting AGAINST, Alonso-Zarazaga stated that the request implied not only a change of authorship and date, but also of the nominal species concept, and in a group of taxa whose taxonomy is not yet settled (judging from the variable amount of subspecies mentioned and from the use of some of these as species as well). Taking into consideration that the species were now correctly identified, the names should be applied according to their true identities. Also voting AGAINST, Dmitriev commented that the authors proposed the rejection the name of *Papilio phoebus* Fabricius, 1793 in favour of *P. phoebus* De Prunner, 1798 in order to preserve the name of the European species, which was erroneously identified as *P. phoebus*. It would appear that the rejection will conserve the name for the European species, only with a different authorship. Technically speaking, two different names are being discussed. Even if the name were to stay the same, it is a different nomenclatural concept (associated with a different author and different nomenclatural act validating this name). The authors mentioned that this will preserve the associations between the subspecific and specific names associated with *P. phoebus*. The fact is the issue would not be resolved. The subspecies were described in association with the species name described by Fabricius, not by De Prunner. New associations will still be needed in order to make an association with a different nomenclatural species name which has exactly the same spelling. Also, European and Asian subspecies probably have associations with different specific names (either Fabricius or De Prunner). Changing the name to the correct identifications (i.e., *Papilio phoebus* Fabricius, 1793 for the Asian species and *P. corybas* for European species) will definitely resolve the ambiguity. Each subspecies will be unambiguously associated with one species or the other. Also voting AGAINST, Halliday stated that it would be premature to take nomenclatural action while the taxonomy of these species and subspecies of *Papilio* is not resolved. Also voting AGAINST, Kojima commented that the two names, *Papilio phoebus* Fabricius, 1793, and *Papilio phoebus* De Prunner, 1798, are homonymous but are not synonymous. Approval of this proposal may possibly bring more serious nomenclatural confusion. The best solution of this case would be that, following the Code, *Parnassius phoebus* (Fabricius, 1793) is applied to the Altai species, *Papilio phoebus* De Prunner, 1798 is a homonym of *Papilio phoebus* Fabricius, 1793 and thus is unavailable, and *Parnassius corybas* Fischer de Waldheim, 1823 is applied to the widespread species.  

References  
Opinion 2383 (Case 3640) – Touit G.R. Gray, 1855 and Prosopeia Bonaparte, 1854 (Aves, Psittacidae): usage of names conserved

International Commission on Zoological Nomenclature
c/o Lee Kong Chian Natural History Museum, 2 Conservatory Drive,
Singapore 117377, Republic of Singapore (e-mail: iczn@nus.edu.sg)


Abstract. Under the plenary power the International Commission on Zoological Nomenclature has ruled to maintain current usage of the names Touit G.R. Gray, 1855 and Prosopeia Bonaparte, 1854 through the suppression of the earlier but little-used, taxonomically ambiguous name Pyrrhulopsis Reichenbach, 1850.

Keywords. Nomenclature; taxonomy; Aves; Touit; Prosopeia; Pyrrhulopsis; shining parrots; parrotlets; Central America; South America; Fiji.

Ruling

(1) The International Commission on Zoological Nomenclature hereby uses its plenary power to suppress the generic name Pyrrhulopsis Reichenbach, 1850 for the purposes of the Principle of Priority but not for those of the Principle of Homonymy;

(2) The name Touit G.R. Gray, 1855 (gender: masculine), type species Psittacus huetii Temminck, 1830 by original designation, is hereby placed on the Official List of Generic Names in Zoology;

(3) The name Prosopeia Bonaparte, 1854 (gender: feminine), type species Coracopsis personata G.R. Gray, 1848 by monotypy, is hereby placed on the Official List of Generic Names in Zoology;

(4) The name huetii Temminck, 1830, as published in the binomen Psittacus huetii (specific name of the type species of Touit G.R. Gray, 1855), is hereby placed on the Official List of Specific Names in Zoology;

(5) The name personata G.R. Gray, 1848, as published in the binomen Coracopsis personata (specific name of the type species of Prosopeia Bonaparte, 1854), is hereby placed on the Official List of Specific Names in Zoology;

(6) The name Pyrrhulopsis Reichenbach, 1850, as suppressed in (1) above, is hereby placed on the Official Index of Rejected and Invalid Generic Names in Zoology.

History of Case 3640

An application to use the plenary power to maintain the current usage of the names Touit G.R. Gray, 1855 and Prosopeia Bonaparte, 1854 was received from Richard Schodde (Australian National Wildlife Collection, CSIRO Ecosystem Sciences, Canberra, Australian Capital Territory, Australia), Walter J. Bock (Department of Biological Sciences, Columbia University, New York City, New York, U.S.A.), Dick Watling (Environment Consultants Fiji, Suva, Fiji) and José Fernando Pacheco (Comité Brasileiro de Registros Ornitológicos,
Sociedade Brasileira de Ornitologia, Rio de Janeiro, Brazil) on 28 August 2013. After correspondence the Case was published in BZN 70(4): 245–248 on 20 December 2013 (Schodde et al., 2013). The title, abstract and keywords of the Case were published on the Commission’s website. A comment in support of the application was published in BZN 71(1): 39 on 31 March 2014 (Dickinson & Gregory, 2014).

The Case was sent for vote on 1 September 2015 (VP 17). A greater than two-thirds majority of Commissioners voted FOR the Case (21 For, 0 Against).

Decision of the Commission

At the close of the voting period on 1 December 2015 the votes were as follows:


Negative votes — none.

Fautin, Ng, Patterson and Štys were on leave of absence.

Original descriptions

The following are the original descriptions to the entries on either an Official List or an Index in the ruling given in the present Opinion:

*huetii*, *Psittacus*, Temminck, 1830: text to pl. 491 in livraison 83.


*Prosopeia* Bonaparte, 1854: 153.

*Pyrrhulopsis* Reichenbach, 1850: 82.

*Touit* G.R. Gray, 1855: 89.

References


Opinion 2384 (Case 3641) — *Ascalabotes sthenodactylus* Lichtenstein, 1823 (currently *Stenodactylus sthenodactylus*; Reptilia, Gekkota, Gekkonidae): conservation of current usage of the specific name by designation of a neotype

International Commission on Zoological Nomenclature
c/o Lee Kong Chian Natural History Museum, 2 Conservatory Drive, Singapore 117377, Republic of Singapore (e-mail: iczn@nus.edu.sg)


Abstract. Under the plenary power the International Commission on Zoological Nomenclature has ruled to set aside all previous type fixations for *Ascalabotes sthenodactylus* Lichtenstein, 1823 (currently *Stenodactylus sthenodactylus*) and to designate as neotype a specimen that conserves current usage of the specific name.

Keywords. Nomenclature; taxonomy; Reptilia; Gekkota; *Stenodactylus*; *Stenodactylus sthenodactylus*; *Stenodactylus mauritanicus*; elegant gecko; Sahara; North Africa.

Ruling

(1) The International Commission on Zoological Nomenclature hereby uses its plenary power to set aside all previous type fixations for *Ascalabotes sthenodactylus* Lichtenstein, 1823 and to designate as neotype specimen MNHN 2012.0250, Muséum national d’Histoire naturelle, Paris (formerly BEV.8989 from the collection of the Biogéographie et Écologie des Vertébrés team, EPHE-UMR 5175 CEFE in Montpellier); an adult female collected from Wadi El Natrun, Egypt (Lat: 30.4233/Long: 30.2928, elevation —10 m), DNA GenBank accession numbers KC190520 (12S rRNA), KC190733 (16S rRNA), KF667509 (RAG2) and KF667510 (MC1R);

(2) The name *sthenodactylus* Lichtenstein, 1823, as published in the binomen *Ascalabotes sthenodactylus* and as defined by the neotype designated in (1) above, is hereby placed on the Official List of Specific Names in Zoology.

History of Case 3641

An application to use the plenary power to conserve current usage of the name *Ascalabotes sthenodactylus* Lichtenstein, 1823 was received from Pierre-André Crochet and Philippe Geniez (Centre d’Ecologie Fonctionnelle et Evolutive, Montpellier, France), Sherif Baha El Din (Dokki, Cairo, Egypt), Aaron M. Bauer (Department of Biology, Villanova University, Villanova, Pennsylvania, U.S.A.) and Salvador Carranza and Margarita Metallinou (Institute of Evolutionary Biology (CSIC – University Pompeu Fabra), Barcelona, Spain) on 4 December 2013. After correspondence the Case was published in BZN 71(1): 17–21 on 31 March 2014 (Crochet et al., 2014). The title, abstract and keywords of the Case were published on the Commission’s website. No comments on the Case were received.

The Case was sent for vote on 1 September 2015 (VP 21). A greater than two-thirds majority of Commissioners voted FOR the Case (20 For, 1 Against).
Decision of the Commission

At the close of the voting period on 1 December 2015 the votes were as follows:


Negative votes — 1: Bogutskaya.

Fautin, Ng, Patterson and Štys were on leave of absence.

Voting FOR, Bouchet applauded the fact that the neotype is a modern specimen with associated tissue and molecular sequences.

Original descriptions

The following are the original descriptions to the entries on either an Official List or an Index in the ruling given in the present Opinion:

stheno\textit{dactylus}, \textit{Ascalabotes}, Lichtenstein, 1823: 102.

References


Opinion 2385 (Case 3642) — *Amalia kaleniczenkoi* Clessin, 1883
(Gastropoda, Stylommatophora, MILACIDAE): specific name conserved

Abstract. Under the plenary power the Commission has conserved the specific name of the terrestrial slug *Amalia kaleniczenkoi* Clessin, 1883 (originally as *Amalia kalenzkoi*; currently *Tandonia kaleniczenkoi*, MILACIDAE) by suppression of its little-used senior subjective synonym *Amalia retowskii* Bottger, 1882, whenever the two names are considered to be synonyms.

Keywords. Nomenclature; taxonomy; Gastropoda; Stylommatophora; MILACIDAE; *Tandonia*; *Tandonia kaleniczenkoi*; *Amalia retowskii*; terrestrial slug; Europe.

Ruling

(1) The International Commission on Zoological Nomenclature hereby uses its plenary power to give precedence to the specific name *kaleniczenkoi* Clessin, 1883, as published in the binomen *Amalia kalenzkoi*, over the name *retowskii* Bottger, 1882, as published in the binomen *Amalia retowskii*, whenever the two names are considered to be synonyms.

(2) The following names are hereby placed on the Official List of Specific Names in Zoology:

(a) *kaleniczenkoi* Clessin, 1883, as published in the binomen *Amalia kalenzkoi* but deemed to be the correct original spelling under Article 33.3.1 of the Code, with the endorsement that it is to be given precedence over the name *retowskii* Bottger, 1882, as published in the binomen *Amalia retowskii*, whenever the two names are considered to be synonyms;

(b) *retowskii* Bottger, 1882, as published in the binomen *Amalia retowskii*, with the endorsement that it is not to be given precedence over the name *kaleniczenkoi* Clessin, 1883, as published in the binomen *Amalia kalenzkoi* but deemed to be the correct original spelling under Article 33.3.1 of the Code, whenever the two names are considered to be synonyms;

History of Case 3642

An application to conserve the specific name of the terrestrial slug *Amalia kaleniczenkoi* Clessin, 1883 was received from Igor Balashov (Schmalhausen Institute of Zoology, National Academy of Sciences of Ukraine, Kiev, Ukraine) on 12 September 2013. After correspondence the Case was published in BZN 71(1): 3–6 on 31 March 2014 (Balashov, 2014). The title, abstract and keywords of the Case were published on the Commission’s
website. No comments were received on this Case. The Case was sent for vote on 1 September 2015. A greater than two-thirds majority of Commissioners voted FOR the Case (19 For, 2 Against).

Decision of the Commission

At the close of the voting period on 1 December 2015 the votes were as follows:


Negative votes — 2: Pape and van Tol.

Fautin, Ng, Patterson and Štys were on leave of absence.

Voting FOR, Kottelat commented that when there are reasonable indications or suspicion that the original spelling of a taxon named in honour of somebody is misspelled, it would be a prima facie case of inadvertent error. Misspelling the name of the person being honoured would be a strange way to honour somebody.

Voting AGAINST, Pape placed on record that Amalia retowskii should have been declared a nomen oblitum under Article 23.9.2 as the conditions of both Articles 23.9.1.1 and 23.9.1.2 were met. Article 23.9.2 cannot be applied retrospectively, however. The authors also provided no specific arguments as to why the Code-compliant use of Amalia retowskii as valid would affect stability or universality, or would cause confusion.

Original descriptions

The following are the original descriptions to the entries on either an Official List or an Index in the ruling given in the present Opinion:

kaleniczenkoi, Amalia, Clessin 1883: 39.
retowskii, Amalia, Böttger, 1882: 98.

References


Opinion 2386 (Case 3643) — *Mutilla clytemnestra* Fox, 1899 (currently *Dasymutilla clytemnestra*) and *Mutilla clytemnestra* Péringuey, 1899 (currently *Mutilla dasya* Péringuey, 1899) (Insecta, Hymenoptera, Aculeata, VESPOIDEA, MUTILLIDAE): current usage maintained

International Commission on Zoological Nomenclature
c/o Lee Kong Chian Natural History Museum, 2 Conservatory Drive, Singapore 117377, Republic of Singapore (e-mail: iczn@nus.edu.sg)


Abstract. Under the plenary power the International Commission on Zoological Nomenclature has ruled to maintain current usage of the names *Mutilla clytemnestra* Fox, 1899 and *Mutilla dasya* Péringuey, 1899 through the suppression of the senior primary homonym and objective synonym of the former, *Mutilla clytemnestra* Péringuey, 1899, for the purposes of both the Principle of Priority and the Principle of Homonymy.

Keywords. Nomenclature; taxonomy; Hymenoptera; VESPOIDEA; MUTILLIDAE; Mutilla; Dasymutilla; Mutilla clytemnestra; Dasymutilla clytemnestra; Mutilla dasya; velvet-ant; Nearctic; Afrotropical.

Ruling

(1) The International Commission on Zoological Nomenclature hereby uses its plenary power to suppress the name *clytemnestra* Péringuey, 1899, as published in the binomen *Mutilla clytemnestra*, for the purposes of both the Principle of Priority and the Principle of Homonymy;

(2) The following names are hereby placed on the Official List of Specific Names in Zoology:

(a) *clytemnestra* Fox, 1899, as published in the binomen *Mutilla clytemnestra*, with the endorsement that it is not to be considered invalid by reason of being a junior primary homonym of the name *clytemnestra* Péringuey, 1899, suppressed in (1) above;

(b) *dasya* Péringuey, 1899, as published in the binomen *Mutilla dasya*, replacement name for the name *clytemnestra* Péringuey, 1899 suppressed in (1) above;

(3) The name *clytemnestra* Péringuey, 1899, as published in the binomen *Mutilla clytemnestra*, and as suppressed in (1) above, is hereby placed on the Official Index of Rejected and Invalid Specific Names in Zoology.

History of Case 3643

An application to use the plenary power to maintain the current usage of the names *Mutilla clytemnestra* Fox, 1899 and *Mutilla dasya* Péringuey, 1899 was received from Denis J. Brothers (School of Life Sciences, University of KwaZulu-Natal, Pietermaritzburg, Scottsville, South Africa), Donald G. Manley (Florence, South Carolina, U.S.A.) and
Kevin A. Williams (Florida State Collection of Arthropods, Division of Plant Industry, Gainesville, Florida, U.S.A.) on 2 September 2013. After correspondence the Case was published in BZN 71(1): 13–16 on 31 March 2014 (Brothers et al., 2014). The title, abstract and keywords of the Case were published on the Commission’s website. No comments on the Case were received.

The Case was sent for vote on 1 September 2015 (VP 20). A greater than two-thirds majority of Commissioners voted FOR the Case (19 For, 1 Against, 1 Abstain).

Decision of the Commission

At the close of the voting period on 1 December 2015 the votes were as follows:


Negative votes – 1: Grygier.

Abstained – 1: Brothers.

Fautin, Ng, Patterson and Štys were on leave of absence.

Original descriptions

The following are the original descriptions to the entries on either an Official List or an Index in the ruling given in the present Opinion:

clytemnestra, Mutilla, Fox, 1899: 233, 246.
clytemnestra, Mutilla, Péringuey, 1899a: 360.
dasya, Mutilla, Péringuey, 1899b: 450.

References


Opinion 2387 (Case 3645) – Orthezia characias [Bosc d’Antic], 1784
(Insecta, Hemiptera, Ortheziidae): generic and specific names available

International Commission on Zoological Nomenclature
c/o Lee Kong Chian Natural History Museum, 2 Conservatory Drive,
Singapore 117377, Republic of Singapore (e-mail: iczn@nus.edu.sg)

http://zoobank.org/urn:lsid:zoobank.org:pub:BFSDE408-21C3-49AF-8B0B-B854F7750791

Abstract. International Commission on Zoological Nomenclature has used its plenary power to ruled that the generic name Orthezia [Bosc d’Antic], 1784 and the specific name characias [Bosc d’Antic], 1784 are available. Orthezia-Characias [Bosc d’Antic], 1784 and d’Orthezia-Characias [Bosc d’Antic], 1784 are ruled to be incorrect original spellings of the former. The work in which Orthezia [Bosc d’Antic], 1784 and characias [Bosc d’Antic], 1784 were published is ruled to be available for nomenclatural purposes and placed on the Official List of Works Approved as Available for Zoological Nomenclature.

Keywords. Nomenclature; taxonomy; Coccoidea; Ortheziidae; Orthezia; Orthezia characias; ensign scale insects.

Ruling

(1) The International Commission on Zoological Nomenclature has ruled under the plenary power that:
   (a) the generic name Orthezia [Bosc d’Antic], 1784 is an available name with two incorrect original spellings (Orthezia-Characias [Bosc d’Antic], 1784 and d’Orthezia-Characias [Bosc d’Antic], 1784), and is available from [Bosc d’Antic] (1784, p. 173);
   (b) the specific name characias [Bosc d’Antic], 1784 is available from [Bosc d’Antic] (1784, p. 173) despite its original proposal as a component of the original spellings Orthezia-Characias [Bosc d’Antic], 1784 and d’Orthezia-Characias [Bosc d’Antic], 1784 of the generic name Orthezia [Bosc d’Antic], 1784 as ruled in (1)(a) above;

(2) Under the plenary power, the provisions of Article 11.4 are set aside and the work [Bosc d’Antic, L.A.G.] 1784, ‘Description de l’Orthezia-Characias’ published in Observations sur la Physique, sur l’Histoire Naturelle et sur les Arts, vol. 24, pp. 171–173, pl. 1, figs. 2–4 is ruled to be available for nomenclatural purposes;

(3) the name Orthezia [Bosc d’Antic], 1784 (gender: feminine), type species by monotypy Orthezia characias [Bosc d’Antic], 1784, is hereby placed on the Official List of Generic Names in Zoology;

(4) the name characias [Bosc d’Antic], 1784, as published in the binomen Orthezia characias (specific name of the type species of Orthezia [Bosc d’Antic], 1784), is hereby placed on the Official List of Specific Names in Zoology;
(5) the following names are hereby placed on the Official Index of Rejected and Invalid Generic Names in Zoology:

(a) Orthezia-Characias [Bosc d'Antic], 1784, an incorrect original spelling of Orthezia [Bosc d'Antic], 1784, as ruled in (1)(a) above;

(b) d'Orthezia-Characias [Bosc d'Antic], 1784, an incorrect original spelling of Orthezia [Bosc d'Antic], 1784, as ruled in (1)(a) above;


History of Case 3645

An application to establish by plenary power the availability of Orthezia [Bosc d’Antic], 1784 and Orthezia characias [Bosc d’Antic], 1784 was received from D.J. Williams (Department of Life Sciences (Entomology), The Natural History Museum, London, U.K.) and Danièle Matile-Ferrero (Muséum national d’Histoire naturelle, Département Systématique et Evolution, Paris, France) on 2 December 2013. After correspondence the Case was published in BZN 71(1): 7–12 on 31 March 2014 (Williams & Matile-Ferrero, 2014). The title, abstract and keywords of the Case were published on the Commission’s website. A comment in support of the application was published in BZN 71(2): 103 on 30 June 2014 (Jansen, 2014).

The Case was sent for vote on 1 September 2015 (VP 19). A greater than two-thirds majority of Commissioners voted FOR the Case (20 For, 1 Against).

Decision of the Commission

At the close of the voting period on 1 December 2015 the votes were as follows:


Negative votes — 1: Kottelat.

Fautin, Ng, Patterson and Štys were on leave of absence.

Voting FOR, Grygier stated that the second sentence of paragraph 1 of the Case (Williams & Matile-Ferrero, 2014, p. 7) was misleading as the elided French article “l” was obviously not part of the generic name, even though the status of the elided possessive particle “d” in this respect was ambiguous, as discussed in the first sentence of paragraph 2 of the Case (Williams & Matile-Ferrero, 2014, p. 8). Also voting FOR, Rosenberg stated that the authors overlooked Article 11.2, which states that the presence of a hyphen does not make a scientific name unavailable. This means that the Commission did not need to rule that the work is available and could have ruled under the specific powers that since the original author stated the intention to name a genus and a species, the species name is available and should be treated as composed of generic and specific names. Also voting FOR, van Tol similarly stated that Article 11.2 states that hyphens in species group names are to be eliminated without affecting the availability of the name and if it applies in this instance, then the names were already available. Also voting FOR, Yanega stated that there was some doubt as to whether the reasoning in the application accurately represented the proper interpretation of the Code, but it was also clear that the list of
suggested rulings would be quite appropriate and effective in resolving the problem, leading him to vote FOR.

Voting AGAINST, Kottelat stated that no vote was necessary and an application of the Code would have been sufficient, and that the application should not have been published. The work clearly uses binominal nomenclature and there is no need to place it on the List of Works Approved as Available for Zoological Nomenclature. [Bosc d’Antic] (1784, p. 173) explicitly created a genus and a species name, and although the author’s use of an hyphen between the genus and species names is odd (in today’s context), this does not make it a genus name; that it seems to have been an idiosyncrasy of the editor is totally irrelevant.

Original descriptions

The following are the original descriptions to the entries on either an Official List or an Index in the ruling given in the present Opinion:

characías, Orthezia, [Bosc d’Antic], 1883: 173.
d’Orthezia-Characias [Bosc d’Antic], 1784: 173.
Orthezia [Bosc d’Antic], 1883: 173.
Orthezia-Characias [Bosc d’Antic], 1784: 173.

References

Opinion 2388 (Case 3655) — Mesocrangon Zarenkov, 1965: conserved by suppression of Mesocrangon Woodward, 1873 (Crustacea, Decapoda, CRANGONIDAE)

International Commission on Zoological Nomenclature
c/o Lee Kong Chian Natural History Museum, 2 Conservatory Drive, Singapore 117377, Republic of Singapore (e-mail: iczn@nus.edu.sg)

http://zoobank.org/urn:lsid:zoobank.org:pub:39E487F5-1AA0-45B8-8F85-4F8B7C300D94

Abstract. The International Commission on Zoological Nomenclature has used its plenary power to conserve the generic name Mesocrangon Zarenkov, 1965 by suppressing the unused senior homonym Mesocrangon Woodward, 1873. Mesocrangon Zarenkov, 1965 is placed on the Official List of Generic Names in Zoology.

Keywords. Nomenclature; taxonomy; Crustacea; Decapoda; CRANGONIDAE; Mesocrangon; Mesocrangon intermedia; Mesocrangon atra; crangonid decapods; Recent.

Ruling

The International Commission on Zoological Nomenclature has hereby:
(1) used its plenary power to suppress the generic name Mesocrangon Woodward, 1873 for the purposes of both the Principle of Priority and the Principle of Homonymy;
(2) placed the generic name Mesocrangon Zarenkov, 1965 (gender: feminine), type species by original designation Crangon intermedia Stimpson, 1860, on the Official List of Generic Names in Zoology;
(3) placed the specific name intermedia Stimpson, 1860, as published in the binomen Crangon intermedia (specific name of the type species by of Mesocrangon Zarenkov, 1965), on the Official List of Specific Names in Zoology;
(4) placed the generic name Mesocrangon Woodward, 1873, as suppressed in (1) above, on the Official Index of Rejected and Invalid Generic Names in Zoology.

History of Case 3655

An application to conserve the generic name Mesocrangon Zarenkov, 1965 for a genus of boreal crangonid decapod crustaceans was received from Martyn E.Y. Low (Raffles Museum of Biodiversity Research, National University of Singapore, Block S6, Science Drive 2, #03–01, Singapore 117546, Republic of Singapore) and Sammy De Grave (Oxford University Museum of Natural History, Parks Road, Oxford, OX1 3PW, United Kingdom). After correspondence the Case was published in BZN 71(3): 153–157 on 30 September 2014 (Low & De Grave, 2014). The title, abstract and keywords of the Case were published on the Commission’s website. No comments on the Case were received.

The Case was sent for vote on 1 September 2016 (VP 1). A greater than two-thirds majority of Commissioners voted FOR the Case (22 For, 3 Against).
Decision of the Commission

At the close of the voting period on 1 December 2016 the votes were as follows:


Negative votes — 3: Aescht, Kojima and van Tol.

No votes were received from Bourgoin and Pyle.

Voting FOR, Ng stated that this application was a logical cause of action for nomenclatural stability.

Voting AGAINST, Aescht commented that this application shows that it may need about 50 years to recognise a homonymy and not one of the “66 different authors using Mesocrangon Zarenkov, 1965 as a valid genus” seemingly consulted Neave’s Nomenclator Zoologicus, which was published 15 years before Zarenkov established his junior homonym. Article 23.9.3 was only mentioned in the Abstract by the applicants, but not in the text, where it would have been adequate in paragraph 9. Since Article 23.9.1.1 is not met, reference to Article 81 would also have been adequate in the Abstract and in paragraph 9. The gender of Mesocrangon / Crangon is treated as feminine in proposal (2) and as masculine in paragraph 4 and proposal (3), thus citing Opinion 334, where the gender of Crangon was given as feminine, would have been helpful. In voting Against, Aescht remarked that since Article 23.9.1.1 is violated and that it is not unusual for a fossil species to be collected only rarely, and that the whereabouts of the type material may be discovered, priority should take precedence over convenience for “modern” taxonomists seemingly disinterested in nomenclatural aspects. Also voting AGAINST, Kojima remarked that as the genus Mesocrangon Zarenkov, 1965 currently includes only three species which seem to have importance only in biology, the present issue should be treated according to the Code to avoid possible future nomenclatural confusion. Namely, a replacement name should have been given to Mesocrangon Zarenkov, 1965.

Original descriptions

The following are the original descriptions to the entries on either an Official List or an Index in the ruling given in the present Opinion:

intermedia, Crangon, Stimpson, 1860: 25.

Mesocrangon Woodward, 1873: 523.

Mesocrangon Zarenkov, 1965: 1762.

References


Opinion 2389 (Case 3656) – Cerambyx striatus Goeze, 1777 (currently Dorcadion glicyrrhizae striatum) and Cerambyx striatus Fabricius, 1787 (currently Chydarteres striatus) (Insecta, Coleoptera, CERAMBYCIDAE): specific names conserved

International Commission on Zoological Nomenclature
c/o Lee Kong Chian Natural History Museum, 2 Conservatory Drive, Singapore 117377, Republic of Singapore (e-mail: iczn@nus.edu.sg)

http://zoobank.org/urn:|sid:zoobank.org:pub:9BF7555A-DD1B-46D8-A352-E2EF7EA125AB

Abstract. The International Commission on Zoological Nomenclature has ruled that the specific name Cerambyx striatus Goeze, 1777 is not unavailable by reason of being originally published in a not consistently binominal work, nor is it invalid by reason of being a junior primary homonym of the name Cerambyx striatus Linnaeus, 1758. It has also ruled that the specific name Cerambyx striatus Fabricius, 1787 is not invalid by reason of being a junior primary homonym of the names Cerambyx striatus Linnaeus, 1758 and Cerambyx striatus Goeze, 1777.

Keywords. Nomenclature; taxonomy; Insecta; Coleoptera; CERAMBYCIDAE; CERAMBYCINAE; LAMINAE; SPONDYLIDINAE; ASEMINI; DORCADIONINI; EBURIINI; TRACHYDERINI; Cerambyx; Asemum; Dorcadion; Styliceps; Chyderteres; Cerambyx striatus; Asemum striatum; Dorcadion glicyrrhizae striatum; Chydarteres striatus; Styliceps sericata; longhorn beetles; Palaearctic Region; Nearctic Region; Neotropical Region.

Ruling

(1) The International Commission on Zoological Nomenclature hereby uses its plenary power to rule that:
   (a) the specific name striatus Goeze, 1777, as published in the binomen Cerambyx striatus, is not unavailable by reason of it being originally published in a not consistently binominal work;
   (b) the specific name striatus Goeze, 1777, as published in the binomen Cerambyx striatus, is not invalid by reason of it being a junior primary homonym of Cerambyx striatus Linnaeus, 1758;
   (c) the specific name striatus Fabricius, 1787, as published in the binomen Cerambyx striatus, is not invalid by reason of it being a junior primary homonym of the names Cerambyx striatus Linnaeus, 1758 and Cerambyx striatus Goeze, 1777;

(2) The Commission hereby places on the Official List of Specific Names in Zoology the following names:
   (a) striatus Linnaeus, 1758, as published in the binomen Cerambyx striatus;
   (b) striatus Goeze, 1777, as published in the binomen Cerambyx striatus, with the endorsement that it is not unavailable by reason of being originally published in a not consistently binominal work and not invalid by reason of being a junior
primary homonym of *Cerambyx striatus* Linnaeus, 1758, as ruled in (1)(a) and (1)(b) above;

(c) *striatus* Fabricius, 1787, as published in the binomen *Cerambyx striatus*, with the endorsement that it is not invalid by reason of being a junior primary homonym of *Cerambyx striatus* Linnaeus, 1758 and *Cerambyx striatus* Goeze, 1777, as ruled in (1)(c) above.

**History of Case 3656**

An application to conserve the specific names of two beetles, *Cerambyx striatus* Goeze, 1777 and *Cerambyx striatus* Fabricius, 1787, was received from Juan Pablo Botero and Mario Cupello (both from the Museu Nacional, Universidade Federal do Rio de Janeiro, UFRJ, Quinta da Boa Vista, São Cristóvão, CEP 20940–040, Rio de Janeiro, RJ, Brazil). After correspondence the Case was published in BZN 72(2): 122–128 on 30 June 2015 (Botero & Cupello, 2015). The title, abstract and keywords of the Case were published on the Commission’s website. No comments on the Case were received.

The Case was sent for vote on 1 September 2016 (VP 2). A greater than two-thirds majority of Commissioners voted FOR the Case (21 For, 3 Against, 1 Split).

**Decision of the Commission**

At the close of the voting period on 1 December 2016 the votes were as follows:


Negative votes — 3: Dmitriev, Kojima and Lamas.

Split — 1: Rosenberg FOR (1)(c), (2)(a), (2)(c); AGAINST (1)(a), (1)(b), (2)(b).

No votes were received from Bourgoin and Pyle.

Voting FOR, Grygier recommended that the word “invalid” in proposals (1)(a) and (2)(b) of this Case as published (Botero & Cupello, 2014) should be broadened to “unavailable and thus invalid” in the Opinion, and that, in proposal (2)(b), the wording “in the not consistently binominal work” should be modified to “in a not consistently binominal work”. In addition to the present proposals, the authors should also have proposed the suppression of Voet (1778) and the placement of that work on the Official Index. Also voting FOR, Ng commented that although the nomenclatural aspects of this application were very complicated, the outcome would best serve nomenclatural stability.

Voting AGAINST, Dmitriev advocated his view for the strict application of Article 57.2 for primary homonyms. Also voting AGAINST, Kojima stated that as the word “invalid” seems to be erroneously used as a synonym of “unavailable”, the proposals do not make sense. The reason why only *Cerambyx striatus* Voet, 1778 is left unavailable is not mentioned even though the author considers that Tavakilian’s (1991) synonymy is incorrect and thus *Cerambyx striatus* Voet, 1778 is a good species. Also voting AGAINST, Lamas remarked that the authors of this application admit that Goeze’s (1777) work is not consistently binominal (Article 11.4). Given this situation, and since no firm resolution has been reached on the availability or non-availability of the “new” names introduced in that work, it appears superfluous to discuss the name “*Cerambyx striatus* Goeze, 1777” in the context of the present application.
SPLITTING his vote, Rosenberg remarked that the application did not present evidence that Goeze’s *Cerambyx striatus* is in widespread use.

**Original descriptions**

The following are the original descriptions to the entries on either an Official List or an Index in the ruling given in the present Opinion:

*striatus*, *Cerambyx*, Fabricius, 1787: 133.

*striatus*, *Cerambyx*, Goeze, 1777: 464.

*striatus*, *Cerambyx*, Linnaeus, 1758: 396.

**References**


Opinion 2390 (Case 3658) — Calyptorhynchus baudinii Lear, 1832 and Calyptorhynchus latirostris Carnaby, 1948 (Aves, CACATUIDA): usage of specific name conserved by designation of a neotype

International Commission on Zoological Nomenclature
c/o Lee Kong Chian Natural History Museum, 2 Conservatory Drive, Singapore 117377, Republic of Singapore (e-mail: iczn@nus.edu.sg)

http://zoobank.org/urn:lsid:zoobank.org:pub:646322E7-CCE9-4D3B-8DA4-08C0F0CFE8B2

Abstract. The International Commission on Zoological Nomenclature has used its plenary power to designate a neotype for Calyptorhynchus baudinii Lear, 1832, thereby conserving usage of the specific names Calyptorhynchus baudinii Lear, 1832 and Calyptorhynchus latirostris Carnaby, 1948, for two species of endemic Australian cockatoos. Both specific names are placed on the Official List of Specific Names in Zoology.

Keywords. Nomenclature; taxonomy; CACATUIDA; Calyptorhynchus baudinii; Baudin’s Cockatoo; Calyptorhynchus latirostris; Carnaby’s Cockatoo; Australia.

Ruling
The International Commission on Zoological Nomenclature has hereby:
(1) used its plenary power to set aside all previous type fixations for Calyptorhynchus baudinii Lear, 1832 and to designate as the neotype specimen WAM A11524 lodged in the Western Australian Museum, Perth, Western Australia;
(2) placed the specific name baudinii Lear, 1832, as published in the binomen Calyptorhynchus baudinii and as defined by the neotype WAM A11524 designated in (1) above, on the Official List of Specific Names in Zoology;
(3) placed the specific name latirostris Carnaby, 1948, as published in the binomen Calyptorhynchus latirostris and as defined by holotype A6436 in the Western Australian Museum, on the Official List of Specific Names in Zoology.

History of Case 3658
An application to conserve the specific names of two endemic Australian cockatoos, Calyptorhynchus baudinii Lear, 1832 and Calyptorhynchus latirostris Carnaby, 1948, was received from Ronald E. Johnstone (Department of Terrestrial Zoology, Western Australian Museum, Locked bag 49, Welshpool DC, Western Australia 6986, Australia), Clemency Fisher (World Museum, National Museums Liverpool, William Brown Street, Liverpool, L3 8EN, U.K.) and Denis A. Saunders (CSIRO Land & Water Sciences, GPO Box 1700, Canberra, ACT, 2601, Australia). After correspondence the Case was published in BZN 71(3): 170–178 on 30 September 2014 (Johnstone et al., 2014). The title, abstract and keywords of the Case were published on the Commission’s website. No comments on the Case were received.
The Case was sent for vote on 1 September 2016 (VP 3). A greater than two-thirds majority of Commissioners voted FOR the Case (24 For, 0 Against, 1 Abstain).

**Decision of the Commission**

At the close of the voting period on 1 December 2016 the votes were as follows:


Negative votes — none.

Abstain — 1: Alonso-Zarazaga.

No votes were received from Bourgoin and Pyle.

Voting FOR, Kojima remarked that the one, and only, weakness of this proposal is that the figure in Johnstone et al. (2014, fig. 2) does not include the values for the holotype of *Calyptorhynchus latirostris*. Also, the figure referred to at the end of paragraph 4 in Johnstone et al. (2014, p. 176) should be “Fig. 2” and not “Fig. 7”. Also voting FOR, Ng noted that while he was fully in support of the application, it should be noted that the supposed Liverpool specimen is probably a lectotype, and not a holotype, as no one really knows how many specimens Lear had. The question would then be whether this is the same specimen as the one in the painting, which is a possibility. Nevertheless, this is irrelevant. Furthermore, as there are no other syntypes on hand and the de facto lectotype is extant, the nomenclatural problem needs to be resolved. Under these circumstances, the designation of a neotype is the right course of action. Also voting FOR, Winston commented that the research behind the application was a job well done, with nice detective work and follow-through to avoid a nomenclatural mess.

Choosing to ABSTAIN, Alonso-Zarazaga stated that this application (and others like it that have come to his attention) has made him wonder if the duties as a community of zoologists (and Commissioners) were being carried out as well as possible. What would the goal of preserving types be if; when they are found to not correspond to current (and possibly erroneous) concepts, they can be set aside and neotypes created, thereby maintaining the results of a job badly done for generations, rather than synonymising a name that is apparently of concern to some people. Two disturbing rationales are thus being propagated: 1) that there is no need for keeping reference collections and types in a museum, since the concept in the literature is more important than the type specimen(s) which the concept should be mirroring – but which it is not, and with just a few years of erroneous “prevailing usage”, disinterest in the original type material, and new neotypes meeting the wrong new concept (perhaps following the same fate as its predecessor in the coming years!); consequently, museums and zoological collections can be disposed of; since buildings, staff, etc. are expensive, concepts cost less to store; 2) the type specimens’ patrimony of any collection can be depleted and transferred to another institution (a sudden gift!), the original institution being deprived of any possibility of protesting. The first rationale is dangerous for Zoology as a science, the second is unethical. A revision of the ethical aspects of the Code is thus urgently needed, in his opinion, and neotypes should be created only when the original type material is lost or lacks diagnostic characters, rather than for not meeting current concepts. Precisely because of these reasons, he opted to abstain.
Original descriptions
The following are the original descriptions to the entries on either an Official List or an Index in the ruling given in the present Opinion:

*baudinii, Calyptorhynchus,* Lear, 1832: unnumbered plate.
*latirostris, Calyptorhynchus,* Carnaby, 1948: 137.

References
Lear, E. 1832. *Illustrations of the family of the Psittacidae, or parrots.* Pt XII.E. Lear, London.
Opinion 2391 (Case 3660) — *Antilope arabica* Lichtenstein, 1827 (currently *Gazella arabica*; Mammalia, Ruminantia): conservation of part of the lectotype designated by Neumann (1906)

International Commission on Zoological Nomenclature
c/o Lee Kong Chian Natural History Museum, 2 Conservatory Drive, Singapore 117377, Republic of Singapore (e-mail: iczn@nus.edu.sg)

Abstract. The International Commission on Zoological Nomenclature has used its plenary power to set aside the lectotype status of a skull of *Antilope arabica* Lichtenstein, 1827, retaining only a skin as the sole lectotype specimen.

Keywords. Nomenclature; taxonomy; Mammalia; Ruminantia; Bovidae; *Gazella arabica*; *Gazella arabica rueppelli*; *Gazella gazella*; *Gazella dorcas*; gazelles; central Arabia; southern Arabia.

Ruling

(1) The International Commission on Zoological Nomenclature has used its plenary power to set aside the lectotype status of the skull ZMB_MAM_2115 in the Museum für Naturkunde Berlin of the name *Antilope arabica* Lichtenstein, 1827, retaining only the skin ZMB_MAM_2115 as the sole lectotype specimen;
(2) the name *arabica* Lichtenstein, 1827, as published in the binomen *Antilope arabica* and as defined solely by the lectotype skin ZMB_MAM_2115 in the Museum für Naturkunde Berlin designated by Neumann (1906), as ruled in (1) above, is hereby placed on the Official List of Specific Names in Zoology.

History of Case 3660

An application to maintain usage of the specific name of the gazelle *Antilope arabica* Lichtenstein, 1827 was received from Eva Verena Bärmann (Museum für Naturkunde, Leibniz-Institut für Evolutions- und Biodiversitätsforschung, Berlin, Germany), Alan W. Gentry (Earth Sciences Department, Natural History Museum, London, U.K.) and Anthea Gentry (Life Sciences Department, Natural History Museum, London, U.K.) on 11 April 2014. After correspondence the Case was published in BZN 71(2): 88–94 on 30 June 2014 (Bärmann et al., 2014). The title, abstract and keywords of the Case were published on the Commission's website. Two comments in support of the application were published in BZN 71(4): 253 on 20 December 2014 (Groves, 2014; Scharnhölz, 2014). Three additional letters in support of this application were also received, and notice of their receipt was published in BZN 71(4): 254 on 20 December 2014.

The Case was sent for vote on 1 December 2015 (VP 27). A greater than two-thirds majority of Commissioners voted FOR the Case (23 For, 1 Against).
Decision of the Commission

At the close of the voting period on 1 March 2016 the votes were as follows:
Negative votes — 1: Kottelat.

Ng, Pyle and van Tol were on leave of absence.

Voting FOR, Rosenberg stated that the definitions of holotype and lectotype both require a single specimen. In the case of a holotype discovered to be composite, Article 73.1.5 allows part of the designated material to retain status as the holotype rather than reverting to a syntype. There is no corresponding article for lectotypes, so the lectotype designation for Antilope arabica is invalid, not being based on a single specimen, and the specimens remain syntypes. Therefore the Commission should have been requested to act under the specific rather than the plenary power as there is no valid lectotype designation to suppress.

Voting AGAINST, Kottelat stated that the Commission did not need to be involved as the authors could have taken the necessary action to resolve the issue on their own.

Original descriptions

The following are the original descriptions to the entries on either an Official List or an Index in the ruling given in the present Opinion:

arabica, Antilope, Lichtenstein, 1827: unnumbered caption to pl. 6.

References


Opinion 2392 (Case 3665) – Musca purpurascens Walker, 1836 (Insecta, Diptera, Calliphoridae): conservation of prevailing usage of the specific name by designation of a neotype

Abstract. The International Commission on Zoological Nomenclature has conserved prevailing usage of the name Musca purpurascens Walker, 1836 (currently Lucilia purpurascens) by setting aside all previous type fixations and designating a neotype. The name is placed on the Official List of Specific Names in Zoology.

Keywords. Nomenclature; taxonomy; Insecta; Diptera; Calliphoridae; Musca; Lucilia; Musca purpurascens; Lucilia purpurascens; purpurascens; blow fly; Neotropical Region.

Ruling

The International Commission on Zoological Nomenclature has hereby:

(1) used its plenary power to set aside all previous type fixations for the nominal species Musca purpurascens Walker, 1836 and to designate as neotype the male specimen in the Canadian National Collection of Insects, Arachnids and Nematodes, detailed in paragraph 9 in Whitworth & Rognes (2014, pp. 167, 168);

(2) placed the specific name purpurascens Walker, 1836, as published in the binomen Musca purpurascens and as defined by the neotype designated in (1), above on the Official List of Specific Names in Zoology.

History of Case 3665

An application to conserve the specific name for a Neotropical blow fly, Musca purpurascens Walker, 1836, was received from Terry Whitworth (Washington State University, Department of Entomology, Pullman, WA 99164–6382, U.S.A.) and Knut Rognes (University of Stavanger, Faculty of Arts and Education, Department of Early Childhood Education, NO–4036 Stavanger, Norway). After correspondence the Case was published in BZN 71(3): 166–169 on 30 September 2014 (Whitworth & Rognes, 2014). The title, abstract and keywords of the Case were published on the Commission’s website. No comments on the Case were received.

The Case was sent for vote on September 2016 (VP 4). A greater than two-thirds majority of Commissioners voted FOR the Case (18 For, 7 Against).

Decision of the Commission

At the close of the voting period on 1 December 2016 the votes were as follows:

Negative votes — 7: Aescht, Alonso-Zarazaga, Bogutskaya, Grygier, Kojima, Kullander and Rosenberg.

No votes were received from Bourgoin and Pyle.

Voting AGAINST, Aescht commented that in paragraph 4 of Whitworth & Rognes (2014, pp. 166, 167), it is stated that Aubertin’s concept of *Musca purpurascens* was also employed in the recent monograph on Neotropical *Lucilia* by Whitworth, whereas according to paragraph 7 “Aubertin’s description does not match the holotype of *M. purpurascens*”. Consequently, there exist two different concepts, but which of these corresponds best to Walker’s original description (the diagnostic characters unfortunately remain unmentioned)? It is thus unclear if the authors consider Aubertin’s interpretation as a misidentification requiring a new name. Moreover, although many details are recognisable, paragraph 9 states that “[t]he taxonomic identity of the nominal species-group taxon *Musca purpurascens* Walker, 1836 cannot be determined from its existing name-bearing type”. If this was the case, then how could the authors be sure about the taxonomic identity of the male? Was there any material of the further authors listed deposited somewhere and how do they interpret diagnostic characters of females and males? Aescht thus wondered why Article 75.3 (“Qualifying conditions”), particularly Article 75.3.4 referring to “destroyed” and Article 76.3 (in relation with Article 75.5) were not invoked. In her opinion, there was no clear evidence given for the “exceptional need” required by Article 75.3 and according to the Article 76.3, a shift of the type locality would be a consequence violating Article 75.3.6 (“evidence that the neotype came as nearly as practicable from the original type locality”). Under the current circumstances, as described in the proposal (i.e., the name *Musca purpurascens* is not threatened), her impression was that the stability of taxonomy, rather than the stability of nomenclature was threatened by the partially non-diagnostic condition of the name-bearing type involved. As the proposed neotype would not resolve the taxonomic issues, she voted against the case. Also voting AGAINST, Alonso-Zarazaga said that from the authors’ description of the holotype in paragraph 7, it is clear that the proposed neotype has nothing to do with the original specimen, and in addition they have not tried to locate similar species in Santa Catarina (or at least in Brazil). Thus, Alonso-Zarazaga did not consider the conditions required by Article 75.3 to have been met—a neotype fitting at least the still numerous characters that can be seen in the holotype should be selected. Also voting AGAINST, Grygier observed that the locality of the proposed neotype was very far from the type locality. Furthermore, the details of the material examined by Auberton and by Hall are not provided, and it was also not clear whether Whitworth examined Auberton’s material. As an alternative solution, the Commission could instead have suppressed *Musca purpurascens* altogether and approve a new name for the Auberton/Whitworth species based on a new type series. Also voting AGAINST, Kojima stated that the present case was either a taxonomic or a biological matter; that is, the fly concerned does not have either negative or positive economic/medical/hygienic importance, and the problem could be solved without intervention of the International Commission on Zoological Nomenclature. That is, *Musca purpurascens* Walker, 1836 should be treated as a nomen dubium and a new species would be described with *Musca purpurascens* of authors as its
synonym. Designation of the neotype collected at the place far from the type locality will cause another future taxonomic confusion. Also voting AGAINST, Rosenberg remarked that the application did not provide evidence that the name was in widespread use, or that the neotype from Costa Rica “came as nearly as practicable from the original type locality” (Article 75.3.6).

**Original description**

The following is the original description to the entry on the Official List in the ruling given in the present Opinion:

*purpurascens, Musca, Walker, 1836: 355.*

**References**


Opinion 2393 (Case 3667) – *Paludina conica* Prévost, 1821 (currently ‘*Peringia* conica’) conserved by suppression of *Paludina conica* Férussac, 1814 (Gastropoda, Prosobranchia, HYDROBIIDAE)

International Commission on Zoological Nomenclature
c/o Lee Kong Chian Natural History Museum, 2 Conservatory Drive, Singapore 117377, Republic of Singapore (e-mail: iczn@nus.edu.sg)

http://zoobank.org/urn:lsid:zoobank.org:pub:5194D287-0DA6-4FAD-A3E1-4CB58C30FF5F

Abstract. The International Commission on Zoological Nomenclature has conserved the specific name *Paludina conica* Prévost, 1821 (currently ‘*Peringia* conica’) by suppressing the senior primary homonym *Paludina conica* Férussac, 1814. The former name has been placed on the Official List of Specific Names in Zoology, and the latter on the Official Index of Rejected and Invalid Specific Names in Zoology.

Keywords. Nomenclature; taxonomy; Gastropoda; Prosobranchia; HYDROBIIDAE; *Paludina*; *Paludina conica*; gastropods; Tertiary; Paris Basin.

Ruling

The International Commission on Zoological Nomenclature has hereby:

(1) used its plenary power to suppress the specific name *conica* Férussac, 1814, as published in the binomen *Paludina conica*, for the purposes of both the Principle of Priority and the Principle of Homonymy;

(2) placed the name *conica* Prévost, 1821, as published in the binomen *Paludina conica*, on the Official List of Specific Names in Zoology;

(3) placed the name *conica* Férussac, 1814, as published in the binomen *Paludina conica*, on the Official Index of Rejected and Invalid Specific Names in Zoology.

History of Case 3667

An application to conserve the specific name of a fossil gastropod *Paludina conica* Prévost, 1821 was received from Dietrich Kadolsky (66 Heathhurst Road, Sanderstead, Surrey CR2 0BA, U.K.). After correspondence the Case was published in BZN 71(2): 221–222 on 20 December 2014 (Kadolsky, 2014). The title, abstract and keywords of the Case were published on the Commission’s website. No comments on the Case were received.

The Case was sent for vote on 1 September 2016 (VP 6). A greater than two-thirds majority of Commissioners voted FOR the Case (20 For, 2 Against, 3 Abstain).

Decision of the Commission

At the close of the voting period on 1 December 2016 the votes were as follows:

No votes were received from Bourgoin and Pyle.

Voting FOR, Ng observed that any action that obviates the need for a new name is to be encouraged.

Choosing to ABSTAIN, Kojima stated that the manner in which conica was listed in Férussac (1814) was unclear. If conica was listed at the beginning of the species list under the heading of Paludina, which followed the list of species under the heading of Cyclostoma, then Férussac’s intention might have been to attribute conica to Cyclostoma but the species was, by printer’s mistake, listed under the heading of Paludina. An alternative interpretation would be that conica Férussac, 1814 was published in the binomen Cyclostoma conica. Also choosing to ABSTAIN, Welter-Schultes remarked that he regarded the senior name as unavailable because of having been published in a not consistently binominal work. Férussac (1814) mixed up French and Latin scientific names, so some names used in this work can be identified as Linnean scientific names, while others cannot. A ruling in this application was in principal harmless, but it should not be taken to imply that Férussac’s (1814) work was regarded as binominal.

Original descriptions
The following are the original descriptions to the entries on either an Official List or an Index in the ruling given in the present Opinion:

conica, Paludina, Férussac, 1814: 64.
conica, Paludina, Prévost, 1821: 427.

References
Opinion 2394 (Case 3668) — *Conus antidiluvianus* Bruguière, 1792: prevailing usage of specific name conserved by setting aside the unidentifiable lectotype and replacing it with a neotype (Mollusca, Gastropoda, Conidae)

International Commission on Zoological Nomenclature
c/o Lee Kong Chian Natural History Museum, 2 Conservatory Drive, Singapore 117377, Republic of Singapore (e-mail: iczn@nus.edu.sg)

http://zoobank.org/urn:lsid:zoobank.org:pub:BF01EC70-BFFE-4786-A781-46C17614C1E3

Abstract. The International Commission on Zoological Nomenclature has used its plenary power to conserve the specific name for the fossil cone shell *Conus antidiluvianus* Bruguière, 1792 by designating a neotype. The name is placed on the Official List of Specific Names in Zoology.

Keywords. Nomenclature; taxonomy; Conidae; Conus antidiluvianus; Conus parisiensis; Paris Basin; Italy; fossil cone shells.

Ruling

The International Commission on Zoological Nomenclature has hereby:

(1) used its plenary power to set aside the lectotype of *Conus antidiluvianus* Bruguière, 1792 designated by Kohn (1992) and replace it with a neotype, specimen MSNM 1 28027 in Museo Civico di Storia Naturale di Milano, Italy, as detailed in paragraph 14 of Janssen et al. (2014, p. 227);

(2) placed the name *antidiluvianus* Bruguière, 1792, as published in the binomen *Conus antidiluvianus*, and as represented by the neotype as designated in (1) above, on the Official List of Specific Names in Zoology.

History of Case 3668

An application to conserve the specific name of a fossil cone shell, *Conus antidiluvianus* Bruguière, 1792, was received from Arie W. Janssen (Naturalis Biodiversity Center, P.O. Box 9517. 2300 RA Leiden, The Netherlands), Ronald Janssen (Senckenberg Forschungsinstut, Senckenberganlage 25, 60325 Frankfurt am Main, Germany), Steve Tracey (Natural History Museum, Cromwell Road, London SW7 5BD, U.K.), Leonard M.B. Vaessen (Les Bonins, 37350 Le Petit Pressigny, France) and Jaap van der Voort (Lutterdamm 19, 49179 Venne, Germany). After correspondence the Case was published in BZN 71 (4): 223–229 on 20 December 2014 (Janssen et al., 2014). The title, abstract and keywords of the Case were published on the Commission’s website. No comments on the Case were received.

The Case was sent for vote on 1 September 2016 (VP 7). A greater than two-thirds majority of Commissioners voted FOR the Case (22 For, 3 Against).
Decision of the Commission

At the close of the voting period on 1 December 2016 the votes were as follows:


Negative votes — 3: Bogutskaya, Kojima and Kullander.

No votes were received from Bourgoin and Pyle.

Voting FOR, Grygier remarked that an erroneous expression, “type species of a nominal family” appears near the end of paragraph 13, and the term “neotype” used in line 12 of paragraph 14 should be “name-bearing type”. Also, paragraph 4, concerning “prevailing usage”, is not clearly documented. If everyone post-Hall (i.e., since 1964) has used the original “e” spelling, then that spelling enjoys prevailing usage. If both spellings have been used by different authors since then, perhaps neither enjoys prevailing usage (which is a better way to state this than saying that both do), but the citations necessary to make a judgement on this point are not given. Since the current name-bearing type is lost, the authors themselves might seem to be empowered to make the requested neotype designation. However, in light of questions as to whether the original type locality was in France or Italy and other doubts about the true taxonomic status of the lectotype, and thus concern as to whether the proposed neotype is conspecific with it, the Commission’s approval of the proposed designation seems necessary. Only in this way will an unquestionably valid neotype be established.

Voting AGAINST, Kojima stated that as far as the type locality and taxonomic status of the lectotype are uncertain, and there has been no certain evidence that the lectotype was lost, the better solution in this circumstances would have been to leave Conus antidiluvianus Bruguière, 1792 as a nomen dubium and to describe a new species with Conus antidiluvianus as used by authors for a species with the prevailing taxon concept as its synonym.

Original description

The following is the original description to the entry on the Official List in the ruling given in the present Opinion:

antidiluvianus, Conus, Bruguière, 1792: 637.

References


Closure of Cases

The following Cases, for which receipts as new applications to the Commission were published though the cases were never published in full, are now closed:


The following Cases were published as nomenclatural notes and are now closed:

*Plumulites ruskini* Lamont, 1978 (Machaeridia): proposed unavailability of the species name. Y. Candela. (Case 3654; acknowledgement of receipt published in BZN 71(1): 1). The author of the Case has since published an article that resolves the relevant nomenclatural issues (see Scottish Journal of Geology, 51: 31–42).


The published Case *Leucopelaea albescens* Bates, 1891 (Insecta, Coleoptera): proposed validation of the lectotype designation. A. Smith (Case 3237, acknowledgement of receipt published in BZN 59(2): 97–98) has been closed without a vote. Following correspondence with the author, it has been determined that the lectotype designation is valid under the changes to Article 74.7.3 in Declaration 44 (see BZN 60(4): 263). The case is now closed.

Corrigenda

There was a misprint in the title of the Opinion 2196 (BZN 65(1): 79, line 1), the Case number should be 3357 and not 3557.

There is a misprint in the Notice of closure of Cases (BZN 72(4): 331), the Case number for “*Geophilus easoni* Arthur, Foddai, Kettle, Lewis, Luczynski & Minelli, 2001 (Chilopoda): proposed conservation of the specific name. L. Bonato & A. Minelli” should be 3679 and not 3649.
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