Comments on *Tibicina* Amyot, 1847 and *Lyristes* Horváth, 1926 (Insecta, Hemiptera, Homoptera): proposed conservation by the suppression of *Tibicen* Berthold, 1827 [?Latreille, 1825], and concerning the type species of *Cicada* Linnaeus, 1758

(Case 239; see BZN 41: 163–184; 71: 103–131; 179–180)

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**Relevance of new genera**

A recent proposal by Boulard & Puissant (BZN 71: 119–131), resurrecting and modifying Case 239 (Melville & Sims, BZN 41: 163–184), proposes to suppress the cicada genus *Tibicen* Latreille, 1825 (and Berthold, 1827, its German translation) (Insecta, Hemiptera, CICADIDAE) in favour of *Lyristes* Horváth, 1926 on the grounds that the first name is invalid vernacular and the translation is a nomen nudum. *Tibicen* and *Lyristes* both potentially claim the type *Cicada plebejus* Scopoli, 1763. However, Sanborn (BZN 71: 108–118), supported by Marshall & Hill (BZN 71: 103–107) and Hamilton (BZN 71: 179–180), has argued that Latreille correctly established *Tibicen* with the type *plebejus* and that suppression of the name on the grounds proposed by Boulard & Puissant would unjustifiably imperil other well-established insect genera created by Latreille.

Sanborn also summarized the relative usage of *Tibicen* and *Lyristes* from 1984 (the year in which Case 239 was initiated) through 2010 and showed that *Tibicen* was more widely applied in all world regions during that period. However, Marshall & Hill (BZN 71, p. 105), while supporting the validity of *Tibicen* Latreille, 1825, noted that ongoing molecular and morphological analysis was expected to support new genera for many *Tibicen* species, which is significant because certain Eurasian cicadas have been more consistently referenced under *Lyristes*. This study was recently published (Hill et al., 2015), leading to most of the New World *Tibicen* taxa being moved to new genera (Neotibicen Hill & Moulds, Hadoa Moulds) or to Diceroprocta Stål, 1870. In another major development, *Auritibicen* Lee has been published for most Asian *Tibicen* species (Lee, 2015) – note that *Auritibicen* is synonymous with *Subsolanus* Moulds in Hill et al. (2015) and preceded it in publication by 11 days making *Auritibicen* the senior synonym. In addition, Sanborn (2014) has moved *Tibicen occidentis* (Walker, 1850) to a new genus and tribe, and Sanborn (2015) and Qi et al. (2015) have recently moved five additional *Tibicen* species to existing genera. Consequently, only six species out of 71 listed under *Tibicen* by Sanborn (2013) remain in the genus – *altaiensis* (Schmidt, 1932), armeniacus (Kolenati, 1857), esfandiarii Dlabola, 1970, gemellus (Boulard, 1988), isodol (Boulard, 1988) and *plebejus* (Scopoli, 1763). All are from Eurasia.

With only six Eurasian species remaining in *Tibicen*, the balance of the evidence with respect to nomenclatural stability has shifted, with *Lyristes* now preferred. Most (141) of the approximately 173 publications from 1984 to 2010 cited in the most recent catalogue involve the type species *plebejus*, and of those *Lyristes* is used 75% of the time and *Tibicen* 18%, with 63% of the unique senior authors using *Lyristes* and 25% using *Tibicen* (Sanborn 2013). *Lyristes* also predominates in post-1984
publications involving the other five species, with 92% of papers (22 of 24) and 75% of unique senior authors (six of eight). A recent check of Google Scholar (http://scholar.google.com) suggests that the preference for Lyristes has continued since 2011, with 30 publications referencing Lyristes plebejus or plebeja and seven referencing Tibicen plebejus or plebeja.

Because the usage of Lyristes predominates for the six species remaining in Tibicen, the Commission may have a third option for Case 239: to suppress its use in favour of Lyristes Horváth under Article 23.9.3 and the plenary power. This action would maximize nomenclatural stability for the remaining affected cicada species while protecting the validity of other insect genera named by Latreille (1825) and/or Berthold (1827). Whether the current preference for Lyristes is strong enough to justify suppressing the senior synonym may depend on the balance of factors in this complicated case.

Suppressing Tibicen for its remaining species would also assist with the problem concerning the type of Cicada Linnaeus, 1758, as discussed in the 2014 Comments, and it would remove the last remaining family-group taxon based on Tibicen, the tribe Tibicenini Van Duzee, 1916, which has been confused with TEBICININI Distant, 1905 (also Cicadidae) as a result of its similar spelling. While Tibicenini remains in use, recent authors have more often used CRYPTOTYMANNINI Handlirsch, 1925, which will take precedence if Tibicen is suppressed (47 post-1984 publications with Cryptotympanini, most applying the name for genera in addition to Cryptotympana, vs. 14 with Tibicenini, from Google Scholar).

The International Commission on Zoological Nomenclature is accordingly asked to consider the following alternative option:

1. to use its plenary power to suppress Tibicen Latreille, 1825 (and/or Berthold, 1827) and any use of this name subsequent to that by Latreille (1825) for the purposes of the Principle of Priority but not for those of the Principle of Homonymy;

2. to place on the Official Index of Rejected and Invalid Names in Zoology the name Tibicen Latreille, 1825 (and/or Berthold, 1827) (type species Cicada plebeja Scopoli, 1763 by original designation and monotypy);

3. to use its specific powers to affirm Cicada haematodes Scopoli, 1763 as the type species of Tibicina Kolenati, 1857 by original designation;

4. to designate Cicada ornis Linnaeus, 1758 as the type species of Cicada Linnaeus, 1758, by the plenary power if necessary.

Additional references


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Lyristes Horváth, 1926 was created to replace Tibicen Latreille, 1825, without being universally adopted, and was notably ignored by North-American workers. In order to resolve the inherent nomenclatural problem of Tibicen and Lyristes, Boulard & Puissant, Marshall & Hill, and Sanborn (BZN 71: 103–131 wrote to the ICZN about the validity of these genera. An application concerning their status was originally submitted to the Commission as Case 239.

While Tibicen has been used in North America, Eurasian and Asian species have been more commonly referred to Lyristes during the last three decades, notably in Central Europe, China, France, Germany, Greece, Italy, Japan, Korea, Portugal,
Spain and Turkey: see Boulard & Puissant (BZN 71: 119–131; 2015) for a review of the literature.


Hill et al. (2015) defined three new genera based on a phylogenetic analysis, two for North American and Central American taxa (*Neotibicen* Hill & Moulds and *Hadoa* Moulds) and one for Asian species (*Subsolanus* Moulds). On the one hand, thirty-five of the 38 North American species and subspecies formerly placed in *Tibicen* were transferred to two of these new genera: *Neotibicen* mainly for the eastern and central North American species and *Hadoa* mainly for a western North American clade. Furthermore, five Mexican species formerly placed in *Tibicen* were transferred to *Diceroprocta* Stål. *Subsolanus* Moulds, 2015 was proposed for 14 Asian species. However, *Subsolanus* Moulds, 2015 is a junior synonym of *Auritibicen* Lee, 2015 syn. nov.

Hamilton (BZN 71: 179–180) wrote that rejecting *Tibicen* in favour of *Lyristes* through an ICZN Opinion is not necessary because the suppression of the name *Tibicen* would upset a large number of well-established names in North America. This comment is now unfounded since all the American species formerly placed in the genus *Tibicen* have been transferred to two new genera or placed in *Diceroprocta* (see above).

Therefore, the only species now regarded as belonging to *Lyristes* / *Tibicen* are those from the western Mediterranean area to western Asia. As the genus name commonly used for taxa from this area is *Lyristes*, we ask the ICZN to retain *Lyristes* over *Tibicen* by agreeing to the proposals made by Boulard & Puissant (BZN 71: 119–131).

**Additional references**


Comment on the proposed conservation of *Neobisium* Chamberlin, 1930, *Neobisioida* Chamberlin, 1930, *Neobisiidae* Chamberlin, 1930 and *Neobisiinae* Chamberlin, 1930 (Arachnida, Pseudoscorpiones, Chelonethi), by designation of *Obisium muscorum* Leach, 1817 as the type species of *Obisium* Leach, 1814 (Case 3616; see BZN 70: 75–81)

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Paragraph 2 in Judson’s application takes for granted that *Obisium* Leach, 1814 (p. 429) is a newly proposed genus, independent from *Obisium* Illiger in Kugelann & Illiger, 1798. The former would be a homonym of the latter, and it was replaced with *Neobisium* Chamberlin, 1930 on this basis. This nomenclatural reasoning is in turn based on Judson’s (2012) arguments, as exposed in para. 9 of his application.

However, this paragraph shows several flaws, since it does not explain the real situation in Leach’s text. First, the only genus recognized is *Chelifer* Geoffroy, 1762. Among its synonyms, ‘*Obisium*, Illiger, Walckenaer’ is mentioned. Leach included 3 species by name: *Chelifer cancriformis* ‘Latr.’ (i.e. Linnaeus, 1758) (still in this genus), *C. cimicoides* (Fabricius, 1793) (now *Chernes cimicoides*) and *C. trombioides*, an incorrect subsequent spelling for *Chelifer trombioides* Latreille, 1804 (now *Chthonius trombioides*). The name ‘*Obisium trombioides*, Leach’s MSS.’ appears in the synonymy of the latter and is not used as a valid name.

From the above description, it is clear that such a thing as a new genus *Obisium* Leach does not exist. Thus, Sundevall (1833) and Harvey & Mahnert (2011), among others, were right in their treatments of *Obisium* sensu Leach as a later use of *Obisium* Illiger. The use of *Obisium* sensu Leach as a valid name by later authors is incorrect. Moreover, a genus cannot be considered as new every time a new species not matching the original concept is added to it. If the type species were subsequently excluded, a later use of this modified concept would be a misidentification, and in consequence, it would be unavailable. In this chain of errors, the replacement name *Neobisium* Chamberlin, 1930 fails as an unnecessary replacement name for *Obisium* Illiger, the only available name, and the family name *Neobisiidae* Chamberlin, 1930 is also unnecessary.

Judson’s application is thus based on an incorrect statement of availability for *Obisium* sensu Leach, which, being unavailable, cannot have a type species (*Chelifer trombioides* Latreille, 1804) and cannot be replaced with *Neobisium* Chamberlin, 1930. This would entail a large list of nomenclatural changes which are, as mentioned in the original application, ‘highly disruptive and confusing’ (see paras. 11 and 12). I am of the same opinion, and the best way to keep the current usage of *Neobisium* and *Neobisiidae* is to make available *Obisium* sensu Leach.

The International Commission on Zoological Nomenclature is accordingly asked to consider the following alternative set of proposals:

(1) to use its plenary power to declare *Obisium* Leach, 1814 an available name, not to be considered a later use of *Obisium* Illiger in Kugelann & Illiger, 1798;

(2) to use its plenary power to set aside all fixations of type species for the nominal genus *Obisium* Leach, 1814 (as made available in (1) above) before the
designation of *Obisium muscorum* Leach, 1817 by Simon (1879) as the type species;

(3) to emend the entry for *Neobisium* Chamberlin, 1930 in the Official List of Generic Names in Zoology to record that it was introduced as a replacement name for *Obisium* Leach, 1814 (due to homonymy with *Obisium* Illiger in Kugelann & Illiger, 1798) and that its type species is consequently *Obisium muscorum* Leach, 1817, as ruled in (2) above;

(4) to place on the Official Index of Rejected and Invalid Generic Names in Zoology *Obisium* Leach, 1814 (a junior homonym of *Obisium* Illiger in Kugelann & Illiger, 1798), type species *Obisium muscorum* Leach, 1817, as ruled in (2) above;

(5) to emend the entry on the Official List of Family-Group Names in Zoology for *Cheliferidae* to record that its author is Risso [1827].

**Comment on the proposed confirmation of the availability of the generic name**

*Spracklandus* Hoser, 2009 (Reptilia, Serpentes, *Elapidae*) and for the nomenclatural validation of the journal in which it was published


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In early 2009 Raymond Hoser provided a new generic name *Spracklandus* Hoser, 2009, for a component of what had been, until then, widely considered a single ‘catchall’ Cobra genus *Naja* Laurenti, 1768. Later in 2009, with full knowledge of Hoser’s prior naming of the group, V. Wallach, W. Wüster, and D.G. Broadley (Wallach et al., 2009) published an article in which they attempted to displace Hoser’s earlier name. This article incorrectly asserted that:

1. the Australasian Journal of Herpetology (*AJH*) was not formally ‘published’
2. it did not exist as a ‘print on paper’ publication;
3. it was not produced in multiple identical copies; and
4. it was not produced for permanent scientific record in accordance with the ICZN Code (4th Edition);

A forensic analysis was said to have been undertaken which supposedly invalidated Hoser (2009). Then Wallach et al. interpreted the Code rule on availability and its intent, presented factually incorrect information about the vehicle of publication (i.e. the *AJH*) and at the same time criticised Hoser for being ‘self-published’, and for his publications not being ‘peer-reviewed’. Wallach et al. (2009), by its content, is clearly not an objective paper and judging by the standard of evidence, personal attacks and errors it contains there is little evidence of peer review which, by the same argument, makes the coined name ‘*Afronaja*’ not worthy of recognition or use according to the authors’ own non-Code-compliant criteria. Consequently, their argument is unconvincing but at the same time has created the junior generic name *Afronaja* Wallach, Wüster & Broadley, 2009.

From my observations of the hard copies of *AJH* including Issue 7 and, irrespective of the contents, it is most definitely published and the standard of print
medium is equal to or better than most other similar journals currently available. AJH Issue 7 definitely does exist as a ‘print on paper’ publication in accordance with Article 8 of the Code irrespective of whether PDF electronic versions/reprints and photocopies have also been circulated since. From my observations AJH Issue 7 was produced in multiple copies and I am personally aware of some of these held by friends and colleagues from the time of publication. It is evident that many had been sent out when published, thereby refuting the later allegation by Wallach et al. (2009) that copies had been circulated one at a time as a print on demand publication. The hard copy availability is further confirmed by the unsolicited copies that were sent and received by Museums, National and State Libraries and Zoological Record, at dates corresponding with the initial printing and publication in 2009.

The AJH also contains a statement in print that the articles therein are intended for permanent scientific record. On page 15 of AJH Issue 7, the following is written: ‘Australasian Journal of Herpetology. Publishes original research in printed form in relation to reptiles, other fauna and related matters, including the subjects of classification, ecology, legal, captivity, etc. in a peer reviewed printed journal for permanent public scientific record and with a sizeable print run has a global audience.’ Thus there can be no question that the relevant journal was published in accordance with the spirit and letter of the Code and so the creation of the replacement name Afronaja by Wallach et al. is totally unwarranted. Hoser (2013, 2015a-d) gives numerous examples of published statements by Wüster and others improperly telling people not to use nomenclature for valid taxa named by Hoser (e.g. Wüster et al., 2001). Hoser (2015a) provides a list of many valid taxa first named by Hoser and others for which Wüster, Kaiser and associates have improperly coined their own names. These actions have in fact created instability and are in contempt of the Code, as noted by Cogger (2014), and have been widely promoted by Hoser’s protagonists as a ‘nothing to lose’ position, in that if they did nothing, Hoser’s names would come into general usage, so by fighting they at least have a chance of preventing this. Hoser (2013, 2015a, 2015d) details a long running campaign by Wüster and associates to attack Hoser at all levels, including his business and other matters, indicating that the current actions against his nomenclature are motivated by personal reasons as opposed to any legitimate desire to protect the Code as claimed. This is perhaps best demonstrated by the overwriting in 2014 of long-established and widely used names such as Broghammerus Hoser, 2004 and Leiopython hoserae Hoser, 2000 with names coined by Wüster associates for the express purpose of gaining ‘name authority’ from Hoser in contempt of the Code and in defiance of their own limiting documents, Kaiser et al. (2013) and Rhodin et al. (2015), which specifically exclude those taxa from being renamed by them (Hoser, 2015d).

It was generally agreed by herpetologists across the globe that the Spitting Cobras (Spracklandus Hoser, 2009) should be separated as a distinct taxonomic group. One could easily argue that the ICZN need do nothing as it should be clear to any objective observer that Hoser’s name has priority and his publications, given the test of time, are likely to prevail. His name Spracklandus does apply to a natural assemblage and can be considered sound.

Hoser has been variously condemned for:
- not knowing what he is talking about (yet his taxa are worthy of being renamed by others; examples in Hoser 2013, 2015a, 2015d).
- the new species- and genus-group names he has chosen are unpalatable (one could list many other such names in current use, e.g. *Montypythonoides* Smith & Plane, 1985).
- naming new taxa with patronymic etymologies for people who Hoser chooses to recognise but who are considered inappropriate or not liked by others.
- Stealing the intended work of others – this is difficult to prove as it is virtually impossible to know who knew what first. Furthermore, Hoser has had over 30 years’ experience with reptiles that I am aware of, particularly with snakes; he has developed husbandry techniques, has successfully bred many in captivity and has travelled widely experiencing many species in the wild in their natural habitats. To suggest he has no knowledge of the animals he has published about is a false assertion.
- Not being rigorous in his methods, thereby casting doubt on the validity of his taxonomic arrangements (given the test of time these arrangements will either be proved to be correct or they won’t; this becomes a reputational problem for Hoser but for no one else).
- Publishing in the *AJH*, a journal he controls, even though many of Hoser’s taxonomic articles have appeared in other journals not under his control as cited by Kaiser et al., (2013, but claimed otherwise by Kaiser, 2013). His critics have failed to acknowledge this. Almost without exception the articles condemning Hoser and that have proposed replacement names on invalid grounds have been produced in vehicles of publication that lack any effective peer-review or quality control as demonstrated in detail by Hoser (2015d). The authors of Rhodin et al. (2015), including Kaiser, have asserted that they represent the entire ‘global herpetological community’ in paragraph 1 (twice), as well as paragraphs 4, 5, 6 and 14. Nothing could be further from the truth. Kaiser (2013) proposed a ‘taxon filter’, a concept thoroughly discredited by any rational assessment. This proposal would open the way for the destabilisation of all zoological nomenclature. Kaiser’s stance has already had such a destabilising effect, with overwriting of established names such as those of John Edward Gray, Richard Wells and others. The comments by Rhodin et al. (2015) in their summary seeking to suppress all names by Hoser, including quite evidently any he may publish in the future, are irrelevant to Case 3601 which should in any event only relate to *Spracklandus*.

A similar application was made to suppress the works of Wells and Wellington by another group of herpetologists, several being the same individuals who have co-signed Rhodin et al. (2015) [see list of names provided with Adams et al. (1988)]. In the *Bulletin of Zoological Nomenclature*, 1988, L.B. Holthuis of Rijksmuseum van Natuurlijke Historie, Leiden commented ‘I see no reason whatever to suppress these works. Such a suppression by the Commission would be highly inadvisable, if not ‘illegal’, and would severely damage the image of the Commission as an impartial body.’

George Zug’s comment in BZN 71(4) alleging that Hoser’s articles do not meet the Articles of the ICZN Code is not correct. By any reasonable and objective interpretation of Article 8 and the intent and purpose of it, the *AJH* does conform to the definition of a publication and the new names are available, as outlined above. This is regardless of repeated baseless assertions by others. However distasteful
Hoser’s name changes may be to some they have been published in accordance with the Code. Wulf Schleip’s ‘forensic’ analysis of Hoser’s journal articles in BZN 71 (3) and his assertion that his findings prove that the AJH does not conform to what constitutes a publication, is flawed and unconvincing and should be ignored by the Commission. What he did not disclose was the fact that he later renamed Leio-python hoserae Hoser, 2000 with his own coined name, Leio-python meridionalis (Schleip, 2014), citing Kaiser et al. (2013) as his justification, even though he himself co-signed that document and it specifically excluded this taxon from the renaming list.

Scott Thomson as corresponding author for Rhodin et al. (2015) argues that because he and some of his colleagues don’t like Hoser they are refusing to use Hoser’s names. In doing so they too cite Kaiser et al. (2013) of which they are all co-authors. Logic would suggest it is their actions, not Hoser’s, that have primarily caused nomenclatural confusion by proposing names for taxa already properly named.

Thomson, Sprackland and Georges are all cosignatories of Rhodin et al. (2015). All have already acted in breach of the Code by renaming species previously named by others (besides Hoser) (Georges & Thomson 2010; Thomson, 2015, Thomson & Georges, 2009; Hoser, 2015a, 2015d). It is self-evident that they hope an invalidation of Hoser’s publications, or at least AJH Issue 7, will bring about the suppression of the names they wish to replace (see Thomson & Georges, 2009; Georges & Thomson, 2010; Thomson, 2015). Thomson asserts that legal, conservation efforts and people’s lives are at risk, but conservation and other agencies are not likely to be confused by application of inappropriate nomenclature unless perhaps advised by those with a vested interest.

Before Thomson lost his ‘name authority’ for the genus Myuchelys (Thomson & Georges, 2009), he was a staunch defender of the Code, expressing opposite views to those in his co-written document (Rhodin et al., 2015). As Thomson correctly stated (on internet chat forum ‘kingsnake.com’ 29 December 2003), ‘Ignoring names that meet the requirements of the ICZN is not an option. This cannot be done under strict application of the rules as the names if valid are Available and must be used. The alternative is to refute them. So disagree with the names all you like, ‘Refute or Accept’. That is, publish a valid refutation of the taxon to which the name applies. . . . Nomenclature is pretty black and white. There are a set of rules. Apply them, if the name is valid, use it, if not reject it. If you don’t like it . . . well I don’t recall that being in the rules.’

The fundamental objective of the Code is to promote stability and universality; the intentional renaming of an entity, already validly named, is no way of achieving that. Abolition of the Principle of Priority is a recipe for disaster.

The International Commission on Zoological Nomenclature is accordingly asked:
(1) to confirm that the generic name Spracklandus Hoser, 2009 has been published in accordance with the Code and hence is available;

(2) state that the Australasian Journal of Herpetology (issues 1–24) based on current printing/publication standards as evidenced by the copies they have seen or have in their possession, does constitute a legitimate publication in the sense of the Code.

Additional References


Hoser, R.T. 2015d. PRINO (Peer reviewed in name only) journals: When quality control in scientific publications fails. Australasian Journal of Herpetology, 26: 3–64.


