



Bungled *Bungarus*: lessons from a venomous snake complex illustrate why taxonomic decisions belong in taxonomy-competent journals

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In response to an article in a toxinology specialty journal by Sunagar *et al.* (2021), in which the authors proposed a name for a new species of krait in the *Bungarus caeruleus* group, Dubois *et al.* (2021: 120) reminded herpetologists (for the third time) that it is “high time that taxonomists follow the *Code*.” While the paper in question provided interesting new insights into the genetic basis of variation in venom composition among Indian kraits and revealed previously unsuspected patterns of genetic diversity in the *B. caeruleus* group, the authors also coined the new species name “*romulusi*” in a manner not compliant with the *International Code of Zoological Nomenclature* (ICZN 1999; hereafter the *Code*), which rendered the name invalid. While Dubois *et al.* (2021) outlined the nomenclatural issues, we feel that there were additional lessons that should be learned about best practices in the publication of taxonomic decisions (Kaiser *et al.* 2013) from this unfortunate episode.

A complication with peer-review

At first glance, the issue of coining a species name might appear straightforward to non-taxonomists. However, the definition and description of species involve a complex two-step process. The first is a scientific step, which requires the collection of sufficient evidence for the existence of the proposed new species as an independent evolutionary lineage (Hillis 2019; Padiál *et al.* 2010). This normally involves detailed morphological and/or extensive molecular work, and the careful sampling of potential contact zones (Hillis *et al.* 2021). Once it has been determined that a population warrants formal recognition as an independent evolutionary lineage, it should be named to assert this distinctiveness. This second step in the process needs to follow the often complex requirements of the *Code*. The consequences of such a taxonomic decision could be numerous, affecting narrower, scientific interest (e.g., evolution, biogeography) and broader applications (e.g., conservation), but they are of particular importance to medically important species such as venomous snakes. Thus, any decisions taken about naming such a species must be deliberate, focused, well-supported by evidence, and *Code*-compliant. The burden of proof on the scientific side, and the craftsmanship for producing a well-written manuscript with a clear presentation of the research, first falls on the authors—but it is secondarily supported by the editorial process, which includes peer-review and editorial decisions.

In the case criticized by Dubois *et al.* (2021), the problem arose because the journal selected by the authors to present their toxinological work is a specialty journal dealing with biological toxins—not with taxonomy. The potential for this type of problem, when questions of taxonomy and nomenclature are not being raised during the peer-review process because the focus of the work is on an entirely different realm of biological sciences, has been highlighted at least once before (Dubois *et al.* 2018). It is therefore worth contemplating whether a specialty journal (such as a molecular biology or a toxinology journal) should agree to handle content outside its usual scope, given that in such a journal the expertise and skill set of editors and reviewers is likely limited to that specialty.

Most scientific papers are published in discipline-specific journals, and manuscripts are reviewed rigorously before acceptance. This process requires that papers are read and edited by a group of specialist editors with high levels of sometimes narrow expertise in areas relevant to the manuscript, who are supported by a cadre of reviewers with the know-how to critique the many details of each submission. This collective body of expertise acts as a guarantor, or, perhaps more realistically, as a strong selective factor to maintain a journal’s scientific standards (Hames 2007). One situation where this system of quality control can break down is when authors include in their manuscripts content beyond the main

focus of their research, and taxonomic content in particular. Whereas a standalone submission with the “add-on” would quickly be considered unsuitable based on a journal’s scope, a short add-on to an important or extensive body of research may be overlooked. Such cases pose a risk to a journal’s standards because the extraneous information may lie outside the expertise of its editors and reviewers, and provide a disservice to the branch of science about which information slipped through without appropriate scrutiny. We wish to emphasise here that it is not our intention to assign blame to authors, editors, or any particular journal, but to contemplate how problems of this nature, which ultimately do not benefit authors, the journal, or the scientific endeavour, can be avoided.

Sunagar *et al.* (2021) presented a fascinating account of venom variation in kraits (genus *Bungarus*) across India. They found that while all the populations analysed had similar transcriptomes, their venom proteomes differed profoundly, with serious consequences for venom lethality and the effectiveness of antivenoms. The demonstration that variation in venom composition in these kraits results from translational and posttranslational processes rather than genomic differences is a highly significant contribution to toxinology and our understanding of venom evolution, which made the manuscript entirely appropriate for publication in *Toxins*. Sunagar *et al.* (2021) also quite appropriately analysed novel and existing mitochondrial DNA sequences for a number of *Bungarus* populations, seeking to establish a link between clinically relevant venom variation and krait phylogeny and taxonomy. Their data showed a close relationship between populations of *B. sindanus* from several localities, and also revealed deep mitochondrial divergences within *B. caeruleus*, suggesting the existence of cryptic diversity within the species and the need for a thorough taxonomic revision of the *B. caeruleus* complex.

The unfortunate add-on to this paper, with which the authors inadvertently confounded the usual *modus operandi* of the journal (and which would perhaps have confounded any other non-taxonomic specialty journal), was the inclusion of a species description. Based on high levels of divergence at two mitochondrial loci, Sunagar *et al.* (2021) named a population from a single locality in Karnataka, southern India, as a new species, “*Bungarus romulusi*”, for the iconic Indian herpetologist and conservationist Romulus Whitaker. Unfortunately, this species description presents some critical procedural and scientific problems.

Nomenclature: non-compliance with the rules

As detailed by Dubois *et al.* (2021), the species name proposed by Sunagar *et al.* (2021) for this krait was not validly published according to the *Code*, the set of rules governing this aspect of taxonomy. As a consequence, the nomen is considered unavailable, it does not exist for the purposes of zoological nomenclature (which is why we here use it in quotation marks). This species name should therefore not be used until it has been made available through a *Code*-compliant description, or its nomenclatural status could become ever more complex. Dubois *et al.* (2021) explicitly declined to consider any taxonomic aspects of the publication by Sunagar *et al.* (2021), but aside from the nomenclatural problem that is where some additional, relevant problems are hidden.

Taxonomy I: Will the real *Bungarus caeruleus* please stand up?

Sunagar *et al.* (2021) sampled kraits they attributed to *B. caeruleus* at several localities in northern India, but they did not include samples from the type locality of *B. caeruleus*, Vizakhapatnam in Andhra Pradesh State. This location is almost equidistant between Bannerghatta, Karnataka State (the locality where samples of the purported new species were obtained) and populations of *B. caeruleus* from Maharashtra and West Bengal States, which are also represented in their mtDNA analysis. Including material from the type locality is of critical importance when it comes to taxonomic decisions, as the name of a species is tied to the type specimen. Without inclusion of material from the type locality of *B. caeruleus*, the authors presented no evidence to decide whether that name belongs to the northern taxon they called *B. caeruleus* or to the southern taxon they described as new. In fact, this issue was not lost on the authors, who stated that “in-depth investigations involving broader sampling are necessary to precisely delineate the range distribution and biogeographic histories of various krait species in the Indian subcontinent.” In addition, several other taxon names that are currently considered synonyms of *B. caeruleus* would have to be evaluated before a new nomen can be established in the complex: *Boa lineata* Shaw, 1802, *Boa latotecta* Hermann in Boie, 1827, and *Bungarus arcuatus* Duméril *et al.*, 1854 (Wallach *et al.* 2014). Under the *Code*’s Principle of Priority, if any of these are applicable to the same taxon as the name proposed by Sunagar *et al.* (2021), they would take precedence over the new name, which would then become a junior synonym even if it had been published in compliance with the *Code*.

Taxonomy II: Limits of analysis

While high levels of mtDNA sequence divergence are certainly suggestive of cryptic species, other factors, such as cytonuclear discordance (Burbrink 2001; Burbrink *et al.* 2021; Holding *et al.* 2021) and persistence of deep mitochondrial divisions in a single gene pool (Abreu *et al.*, 2020), can complicate their interpretation. The extremely limited geographic sampling, precluding an assessment of distribution limits and of the nature of contact zones between taxa (Hillis 2019; Padiál *et al.* 2010), and the lack of an intact specimen (as opposed to tissue samples) as a holotype also suggest that a formal taxon description was inappropriate and premature.

Flaws in the process

Authors are ultimately responsible for the content of their papers, but the responsibility for a journal's standards, including scope and quality control, rests with its editors. While infringement on an author's scientific work in general is undesirable as a matter of academic freedom, a journal's scope definitively limits what can and cannot be published in its pages. For example, the Aims and Subject Areas of *Toxins* list the acceptable topics, and taxonomic proposals are not included and would appear to be disqualified from the journal. Yet, the manuscript made it into print.

Given the extensive and novel toxinological content of the paper, it is perhaps understandable that the need for expert taxonomic review took a back seat or was overlooked. Based on its stated scope, the journal would certainly be expected to lack the editorial expertise for assessing taxonomic decisions. Nevertheless, while the rigours of toxinological review likely made this a better paper, the procedural failure with the taxonomic aspect demonstrates a certain editorial laxity, even if inadvertent due to limited experience. Choosing reviewers who between them can assess all parts of a manuscript is a key component of the editorial process and one of an editor's primary responsibilities (Hames 2007). Because this did not happen here, the problems of *Code*-compliance and other taxonomic flaws that would have been obvious to qualified taxonomic reviewers or editors went unnoticed.

In many ways, deficiencies or negligence during the review process matter more in taxonomy than in other sciences: whereas excursions outside the main journal remit into most other disciplines can be judged on their merits by subsequent workers, this is not the case with taxonomic works, where the creation of new taxon names in particular can have long-term consequences due to the rules of the *Code*. Taxonomy is often perceived as an intuitive and uncomplicated discipline by non-specialists, but the burden of evidence required to support a species description can be considerable, especially in morphologically conserved taxa like kraits (Chen *et al.* 2021). Moreover, independently of the science of taxonomy, the rules of nomenclature, the accounting system supporting the science, can be complex, and seemingly minor actions may have effects that can cause taxonomic confusion for decades.

Improving the process

To avoid problems of this nature, we follow our earlier comments on best practices in herpetological taxonomy (Kaiser *et al.* 2013) and suggest the following guidelines for authors, editors, and reviewers of non-taxonomic zoological journals:

Stay in your lane.—As a general guideline, manuscripts submitted to a specialist outlet should not include add-ons, digressions that fall outside the scope of the journal. Both authors and editors need to consider carefully whether manuscripts encompassing a diversity of scientific topics are most appropriate for a subject-specific journal, or better suited for a multidisciplinary journal, or whether they should be divided between separate specialist publications.

Tell authors about taxonomy.—The guidelines to authors of non-taxonomy specialist journals should explicitly discourage authors from including taxonomic decisions. While significant novel data and analyses, including about toxins, may be taxonomically informative, their translation into formal taxonomic conclusions, in particular the description of new taxa and the establishment of new scientific names, should be identified as lying outside the remit of the journal and disallowed. Errors in taxonomy are more likely to have permanent consequences, and some journals already actively discourage authors from submitting manuscripts with taxonomic decisions (e.g., *Biodiversity and Conservation*).

Editorial engagement.—Editors need to be especially assiduous in ensuring that manuscripts with content outside their area of expertise are reviewed by suitable experts. Lack of familiarity with parts of a manuscript should be cause for additional vigilance. Selection of a panel of reviewers that can adequately assess all parts of a manuscript is a key part of the peer review process, and a responsibility of the editor (Hames 2007).

Failure of peer-review and the editorial process renders a disservice to science, the journal, its editors and publishers, and ultimately the authors themselves (Hames 2007). While the issues highlighted here are in no way unique to any one

journal, it is our hope that the above suggestions, triggered by one specific paper, will assist in further consolidating the quality of how specialty journals handle taxonomic decisions that may be hidden in submitted manuscripts. In this context, we found it particularly disappointing that *Toxins* rejected a previous version of this manuscript on the explicit grounds that it criticised the review process underlying the publication of Sunagar *et al.* (2021) and not just the authors and their work. This is especially unfortunate given that the publisher of *Toxins*, MDPI, has been the subject of extensive and controversial discussions over their approach to quality control in their journals (Beall 2017; Oviedo-García 2021). It is also notable that the rejection of this manuscript's precursor came from MDPI's China-based staff rather than the editorial board of the journal. Willingness to admit errors and improve practice on that basis lies at the heart of progress in science. It is to be hoped that journals issued by all publishers will take on board the lessons to be learned from this case.

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