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Strict nomenclatural rules or subjective "*best taxonomic practices*": is the Code a confusing factor?

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"*'The President, Australian Society of Herpetologists' proposes to suppress three works of WELLS & WELLINGTON for several reasons, which have only one important aspect in common: all are totally irrelevant according to the – either current or past – provisions of the Code and accepted nomenclatural procedures!*": this was the first sentence of the chapter 7. of my paper (HOŁYŃSKI 1994) published 26 years ago. The main reasons justifying – in *The President*'s (ANONYM 1987) opinion – nomenclatural "annihilation" of the incriminated papers were their having been "*published in a journal controlled by the authors*", where "*The Managing Editor ... is ... Wells and ... Advertising Sales Manager ... Wellington*", and "*subscriptions are payable ... at Wells' address*"; having not "*been subject to some form of independent referee or editorial consideration*"; being – "*in the contention of the Society*" – "*not based on sound taxonomic research*"; *&c*. There is no need to repeat here the extensive analysis of the case presented by me on the following 5 pages (chapter 7: **Freedom of scientific expression; or: can irrelevant decide?** – HOŁYŃSKI 1994: 26-30), anyway the Commission's decision ("*not to vote on this application, which it considers to be outside its remit. The case is therefore now closed*" – ANONYM 1991) was in line with my argumentation: the application to suppress the WELLS & WELLINGTON's ***validly*** published papers, only because they did not match ***somebody's idea*** of "*broadly accepted conventions*" from among ***somebody's idea*** of "*the intellectual foundations of biological classification*" (STONE 1988), has been – in accord with the Code's pledge:"***The Code refrains from infringing upon taxonomic judgement, which must not be made subject to regulation or restraint***" – effectively rejected, and it seemed that not only the case of the respective three papers was "*closed*", but the general question (as to what – compliance with the Code, or subjective criteria preferred by some *Besserwisser*-s? – should determine the nomenclatural validity of publications) has been settled once for all.

And indeed, for many years no further arbitrary (without clear evidence of transgressing the rules laid down and formulated as Articles of the Code) attempts to invalidate taxonomic publications or nomenclatural acts came to my attention. But self-styled controllers of self-created "best practices", for whom freedom of scientific research in general, and of taxonomic judgement (solemnly guaranteed by the Code) in particular, does apparently mean something totally different from what I am able to imagine, have not given up... The first signal of their continuing activity struck me when, in 2016, I (fed up with hopeless, time-consuming and nerve-racking windmill fights against censorial endeavours of publishers, editors and reviewers) established *Procrustomachia – Occasional Papers of the Uncensored Scientists Group*: some of my Colleagues almost immediately launched vehement attacks, decreed and proclaimed *urbi et orbi* that the "*pamphlet Procrustomachia*" with all my papers published in it is "*nomenclaturally unavailable*", and two years later formulated application (BÍLÝ *& al.* 2018b) requesting the Commission to use plenary power to suppress it! They tried to support their verdict with allegedly not fulfilled criteria required by the Code, but as these allegations successively proved false, only one has been quoted in the formal application – also false, but... when no valid objection can be found, invalid must serve: in such situations the truth seems to be the least (if at all...) important aspect! Interesting – and, I think, highly symptomatic – is the fact, that accusations of *Procrustomachia* allegedly "*promoting bad science*" has ***never*** been supported by ***any*** concrete evidence beyond the unpardonable crime of being uncensored ["*privately published, not peer-reviewed*"]: ***nobody ever*** tried to seriously criticize ***any result, opinion, interpretation*** of ***any*** of the hitherto published 25 papers, ***nobody ever*** tried to show that my works published in *Procrustomachia* are of lower quality than those from "renowned" journals!

Until recently, I considered this assault as isolated action, very painful to me but not directly affecting others. However, in the last time I came across a paper (TRONCOSO-PALACIOS *& al.* 2019) being in essence further degeneration of the attitude expressed in ANONYM 1987 and BÍLÝ *& al*. 2018b: the Authors do not like the book "*Reptiles en Chile*" by D. DEMANGEL MIRANDA (2016) because of its being "*self-published*" and in their opinion, "*lack the necessary scientific rigor in terms of replicability, specimen work, lack of peer-review*", "*do not follow the best practices accepted by herpetological community*", *&c.*, therefore, ***admittedly*** aware that these reproaches – even if true! – are ***irrelevant*** ("*these could be considered validly published according to the International Code of Zoological Nomenclature*") they (like earlier BÍLÝ *& al*. 2018) decided to... concede themselves the capacity of *sui generis* Super-Commission or Supreme Nomenclatural Authority: "*we hereby invalidate all 13 taxonomic changes proposed in this book*"! For the moment I started to wonder if it was not the attack against *Procrustomachia* that has stimulated TRONCOSO-PALACIOS *& al.* (2019) to launch their campaign against DEMANGEL-MIRANDA (2016), but the references immediately showed that the idea originated from some earlier publication, probably KAISER *& al.* (2013): a lengthy (7 pages) presentation of the Authors' ideas concerning "*best practices*", accompanied by (also 7 pages long), rather bizarrly introduced ("*These recommendations are not formal nomenclatural proposals according to articles of the Code, but temporary treatments until the ICZN has developed a suitable response to actions of taxonomic vandals*") list of (with few exceptions not specifically justified) substitute names suggested to be used instead of the "*unscientific*" ones proposed by two (R.W. WELLS and – mostly – R.T. HOSER) Australian herpetologists. A suggestion to replace – for undefined, but certainly not short, time – ***validly*** ("*Like those published by Raymond Hoser, works by Wells follow the basic requirements of the Code*") proposed names by

admittedly invalid ones seems itself rather, to say the least, original concept of "*best scientific practices*", but this is apparently only one of the bizarre consequences of the fundamenal misunderstanding that has led ANONYM (1987), KAISER *& al.* (2013), TRONCOSO-PALACIOS *& al.* (2019), and perhaps some others (quoted by them but not consulted by me), to postulate – almost exactly "copying" *The President*'s request and argumentation against WELLS and WELLINGTON – suppression of papers (and/or even entire journals!) "*for several reasons, which have only one important aspect in common: all are totally irrelevant according to the – either current or past – provisions of the Code and accepted nomenclatural procedures!*".

This basic misunderstanding lies the ***confusion of nomenclature with taxonomy***, evident already from the formulation of the title of KAISER *& al.* (2013)'s paper: "*taxonomic decisions ... are acceptable only ...*": there is no such thing as "***taxonomic*** *decisions*", taxonomy is a branch of science, all taxonomic statements are ***opinions*** (hypotheses), which anybody can (***and should!***) "accept" (share, consider true) or reject (consider false) according to ***his/her*** evaluation of available evidence (HOŁYŃSKI 2017b). On the other hand, nomenclature is not, in itself, a science but a tool which must be used acording to fixed rules, one of them (in fact, fundamental) is that a ***nomenclatural*** act – *e.g.* application of a name to a "new" taxon – is to be treated as a ***decision*** which, unless ***demonstrably*** contradicting specific Article[-s] of the Code or formally, using "plenary power", invalidated ("supressed") by the Commission, ***must*** obligatorily be accepted and followed by everybody! So, when *e.g.* HOSER (2012) described a new species under the name *Cryptophis edwardsi*, but KAISER *& al.* (2013) consider it taxonomically identical to *C. nigrescens*, they of course can (indeed: ***should***) not accept the validity of the ***taxon*** and in their works can (indeed: ***should***) place *C. edwardsi HOSER 2012* in synonymy of *C. nigrescens*, but they ***must not*** neglect HOSER's ***name*** as if never published, and taxonomists disagreeing with the synonymization ***must*** use the ***name*** as valid (unless ***demonstrably*** contradicting specific Article[-s] of the Code)! Of course it is anybody's right to apply to the Commission for using its "plenary power" to suppress the name (and/or the respective ***paper*** – suppression of the entire ***journal*** is rather extraordinary stipulation...), but such application should be supported by indication of ***nomenclatural*** (violation of the provisions of the Code), ***not*** taxonomical (disagreement of somebody's idea of "*best practices*") flaws! In other words HOSER (2012), in describing *Cryptophis edwardsi*, made in fact two statements:

1). Taxonomical: "*I think that the specimens mentioned in the description belong to a separate species of the genus Cryptophis, different from C. nigrescens*" – this is HOSER's personal ***opinion***, a hypothesis open to verification or falsification by anybody.

2). Nomenclatural: "*For the new species I have coined the name edwardsi*" – this is a nomenclatural ***decision*** binding for everybody referring to this nominal taxon (whether as valid species or as synonym).

Suggestions to compromise or even totally dismiss scientific freedom in taxonomy are also made by activists of nature conservation, whose proposals go sometimes as far as to, in fact, destroy taxonomy as a serious branch of science ("*the IUBS should create a process that does exactly what that effort* [the declaration that "*Nothing in this Code may be construed to restrict the freedom of taxonomic action*"] *avoids* – ***restrict the freedom of taxonomic action***"!!! In particular, "*the IUBS should create a taxonomic commission to establish what rules (if any) should be applied across all life forms and, if taxon-specific definitions need to be developed, what those should be*" ["*for instance, agreed differences in calls and songs ... to delineate species of birds and primates; for fungi, genetic barcodes ... Such differences must be explicitly stated and agreed*"], then "*establish subcommittees*" which "*would review taxonomic papers for compliance with agreed standards*", and "*judicial committee*" as "*the*

final arbiter ... responsible for upholding the rules" – quotations from GARNETT & CHRISTIDIS (2017) [boldface mine – RBH])! I have already (HOŁYŃSKI 2017a) analysed these suggestions in detail and am referring to them here only because the alleged "*major negative consequences*" generated for nature protection by violation of "*best taxonomic practices*" has also been claimed by KAISER *& al.* (2013), MEASEY (2013) and TRONCOSO-PALACIOS *& al.* (2019).

I am not herpetologist, have not seen any of DEMANGEL MIRANDA's, HOSER's or WELLS' publications, and so – like in case of WELLS & WELLINGTON's papers [HOŁYŃSKI 1994] – "*I am neither qualified nor willing to discuss the soundness of* [their] *taxonomic work*", nor wish I to defend bad practices in taxonomy. What I ***do*** wish to defend, is my firm conviction, that the Code's disavowal of "*infringing upon taxonomic judgement, which must not be made subject to regulation or restraint*" is absolutely ***fundamental*** principle and should remain uncompromised; that not only KAISER, BÍLÝ, TRONCOSO-PALACIOS, *&c.*, but also DEMANGEL MIRANDA, HOSER, WELLS and anybody else (including me...) – have the right to publish their taxonomic views in journals ***they*** consider appropriate and in the form ***they*** consider optimal; that the soundness of their taxonomic interpretations should be judged ***individually*** by competent specialists, who will accept what is good and ignore what is wrong (doing it easily without intervention of the Commission); and that (inevitably subjective) evaluation of ***taxonomic*** soundness is absolutely irrelevant to ***nomenclatural*** availability. So, arbitrary "suppression" of publications and/or nomenclatural acts in them by some authors ["*we will treat "Procrustomachia" as an unavailable publication and all names published in it so far as unavailable names*" – BÍLÝ *& al.* (2018a)], communities, or journals ["*to deal with many examples of bad taxonomy that affect African taxa, African Journal of Herpetology will not use names listed in Kaiser et al. (2013)*" – MEASEY (2013)] is absolutely unacceptable not only as malicious foul play but also as ***really*** very bad scientific practice (introducing, in fact, alternative nomenclature[-s], *i.e*. just what the Code is aiming to prevent)!

But also the eventual formal suppression by the Commission would be a very bad decision: unfair (the incriminated papers have been published in accordance with the *Code*), confusing (as admitted *e.g*. by TRONCOSO-PALACIOS *& al.* 2019, many of these names have already been accepted and used in several important publications), and dangerous ["*a precedence would be established, encouraging attempts to disqualify any work only because somebody does not like it! Such attempts – claiming incompetence, lack of sound taxonomic study, methodological imperfections, etc. – would be virtually impossible to evaluate (there is no clear-cut border between good and bad; there is no agreement as to what is needed for a taxonomic work to be "sound"; members of the Commission are not likely to be specialists; etc.), so decisions would be unavoidably highly subjective, with consequent impairment of the authority of both the Commission and the Code*" – HOŁYŃSKI 1994]!

Last but not least, no article of the *Code* interdicts private publishing [indeed, as observed by KRELL (2020), "*private publications (i.e., published by the author) have a long tradition in taxonomy*"] or conditions nomenclatural availability of a publication upon external interventions, but – in the opinion of the fault-finders – apparently the most horrible mortal sin of DEMANGEL MIRANDA's, HOSER's, WELLS' or, for that matter, my publications is just being "*self-published*" and not "peer"-reviewed, therefore they expect ICZN to "*develop a suitable response*" by introducing the requirement of "*publishing via peer-review*" as an indispensable prerequisite for *e.g*. new names being considered "*acceptable*". Evidently, however, such a requirement would be in direct opposition to the **Preamble**, which states *expressis verbis* that none of the *Code*'s provisions or recommendations should "*restrict the freedom of taxonomic thought or action*", this being evidently an empty word unless freedom

of publishing one's views in the form considered (by the ***author***!) optimal is also granted (interestingly, also "*The authors of this paper* [KAISER *& al.* 2013] *understand that the right to freely interpret scientific data as it relates to taxonomic decisions must remain inviolate*"; how they can reconcile that understanding with their postulate of – to say it in plain words – ***obligatory censorship***, is beyond my comprehension): there is ***no science without freedom of expression*** of the author's ***true*** opinions, and freedom of expression is a ***meaningless***, in fact ***sneeringly fraudulent phrase*** if ***uncensored*** ("not peer-reviewed") publication is prevented or declared non-existent from the viewpoint of the very purpose it has been aimed at (this is exactly what suppression really means)!

To conclude:

– Freedom of expression of scientific views in general, and of *taxonomic thought or action* in particular, must remain the most fundamental, supreme principle: curtailment (no matter whether done by political censorship – like in "Lysenkoism era" in Soviet Union – or by KAISER *& al.* (2013), TRONCOSO-PALACIOS *& al.* 2019, "*herpetological community*", or even by International Commission on zoological Nomenclature) of the authors' right to publish ***their*** views in the form ***they*** consider optimal would transform the serious branch of science into its caricature!
– ***Taxonomic*** hypotheses (diagnostic value of characters, validity of taxa, their taxonomic rank, classification *&c.*) are personal opinions of particular taxonomists that can (and should!) be accepted and followed by the scientists who consider them apt and rejected by those who think otherwise – but the decision should be made ***individually*** based on the ***merits of particular statement***, not according to where and by whom it has been published or whether somebody's ideas of "*best practices*" were or were not adhered to!
– The right (in fact, the obligation) of scientists to ***honestly*** – in light of the ***available evidence*** (***not*** of any preconceptions like "*best taxonomic practices*"!) – evaluate and ***accordingly*** accept or reject each published taxonomic conclusion does ***not*** imply the right to deny the ***fact*** of its having been published: the respective publication (whoever could have been its author, and whichever journal it could have appeared in!) should be uprightly cited wherever relevant, with the results earnestly referred to!
– Soundness (or "*body of evidence*", to say nothing of "*best practices*") of ***taxonomic*** conclusions is perfectly irrelevant to the availability of ***names*** (or other ***nomenclatural*** acts) which, if introduced in concordance with the Code, are ***binding*** and ***must*** be accepted unless formally, using "plenary power", suppressed (not only proposed/expected to be suppressed!) by the Commission.

Or – if KAISER *& al.* (2013)'s version of "*academic freedom*" ("*it is the judgement call of authors, editors and readers whether a proposed name should be applied*") is accepted – we would go by 200 years back to the time when every author decided himself which name to accept and which to ignore; if so, the Code would become but a superfluous disturbing factor...

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