

The science of herpetology is built on evidence, ethics, quality publications and strict compliance with the rules of nomenclature.

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ABSTRACT

This is a rebuttal of a dangerous and dishonest blog by Hinrich Kaiser and eight other renegades. These are Mark O'Shea, Wolfgang Wüster, Wulf Schleip, Paulo Passos, Hidetoshi Ota, Luca Luiselli, Brian Crother and Christopher Kelly. It was published in *Herpetological Review* (Kaiser *et al.* 2013). The journal is edited by one of the authors (Schleip) and the "paper" evidently bypassed all standard peer review and editorial quality control as outlined in the Society for the Study of Amphibians and Reptiles (SSAR) ethics statement (Anonymous 2013a), the SSAR being publisher.

Kaiser *et al.* make numerous false and defamatory statements against this author (Raymond Hoser) as part of an obsessive 15-year campaign.

The claims made without evidence against Hoser are in fact shown to be true for the accusers.

These include, "evidence free taxonomy", fraud, "unscientific taxonomic publications", "taxonomic terrorism", plagiarism, "unscientific taxonomy", "unscientific practices", "unscientific incursions" and "deliberate acts of intellectual kleptoparasitism".

Kaiser *et al.* seek to break and destroy the rules of Zoological Nomenclature (Ride *et al.* 1999) including the three critical rules of:

- 1/ Homonymy (Principal 5, Article 52 and elsewhere),
 - 2/ Priority (Principal 3, Article 23 and elsewhere),
 - 3/ Stability (Principal 4, Articles 23, 65 and elsewhere),
- as well as the ethics of the Code (Appendix A).

They seek to do this in the first instance by boycotting established nomenclature and the established rules in a war plan that must by their own account run for decades (Kaiser *et al.* 2013, p. 20).

They then seek coin their own names for hundreds of taxa already properly named by others and attempting to take credit for the research work of the earlier authors. This will create unprecedented taxonomic instability and confusion.

Their actions will effectively:

- 1/ Freeze the progress of herpetological taxonomy and if copied, perhaps all of zoology;
- 2/ Put lives at risk;
- 3/ Increase the likelihood of extinctions of rarer taxa.

Their alleged loophole in the Zoological Code which they assert allows them to create hundreds invalid junior synonyms to usurp the proper names, as quoted by them, does not in fact exist!

This is because Kaiser *et al.* misquoted the Zoological Rules in their badly written paper.

Furthermore the repeated claim by Kaiser *et al.* to have the official backing of the ICZN for their scheme is also shown to be a lie.

Keywords: Hinrich Kaiser; Wulf Schleip; Wolfgang Wüster; Mark O'Shea; Peter Uetz; Raymond Hoser; Richard Wells; *Herpetological Review*; *Australasian Journal of Herpetology*; *Australian Biodiversity Record*; *Journal of Herpetology*; peer review; fraud; ethics; taxonomy; ICZN; rules; nomenclature; homonymy; priority; stability; synonym; boycott; *Leiopython*; *Laudakia*; *Adelynkimberlea*; *Spracklandus*.

SUMMARY

The following refutes the claims in a dangerous and dishonest blog by Hinrich Kaiser and eight other renegades. The others are Mark O'Shea, Wolfgang Wüster, Wulf Schleip, Paulo Passos, Hidetoshi Ota, Luca Luiselli, Brian Crother and Christopher Kelly.

The blog was published in *Herpetological Review* in March 2013 (Kaiser *et al.* 2013). The journal is edited by one of the authors (Schleip) and the "paper" clearly avoided all standard peer review and editorial quality control as outlined in the Society for the Study of Amphibians and Reptiles (SSAR) ethics statement. The SSAR are the publishers.

The first four listed authors have waged an obsessive campaign against myself (Raymond Hoser) since at least 1998 (Hoser 2001). Their campaign has ruthlessly and dishonestly attacked all the taxonomic and nomenclatural changes proposed by myself for various reptile species, genera and the like (Hoser 2009a). These individuals have also unlawfully attacked my Australian reptile education business, Snakebusters, including calling for armed raids on and closure of, the lawful business (Various authors 2011a). They have even launched criminal attacks on my family via Facebook hate pages including one they called "Ray Hoser, Melbourne's biggest Wanker", shut down in July 2011.

Mark O'Shea, Wolfgang Wüster and Wulf Schleip have in their obsessive campaign which I have previously documented in a 64-page paper cited herein as Hoser 2012a, consistently advocated a general boycott of use of names proposed by Hoser for previously unnamed taxa. This means the general basis of, and stated aim within Kaiser *et al.* 2013 is nothing new.

What is new is their false claim that they have backing of the ICZN itself, when the opposite is in fact the case. Likewise for their false claim to have found a loophole to allow them to legally boycott use of valid scientific names.

Many lies and misrepresentations by these pseudoacademics in their *Herpetological Review*, "Point of View" article, were previously rebutted by myself in a paper titled, "Robust taxonomy and nomenclature based on good science escapes harsh fact-based criticism, but remains unable to escape an attack of lies and deception". *Australasian Journal of Herpetology* 14:37-64 (Hoser 2012as). This was based on a similar document prepared by Kaiser *et al.* in 2012, circulated *en masse* by Kaiser himself and published at the same time and place (within Hoser 2012as).

As much of this matter is not revisited, that paper (Hoser 2012as) and Hoser (2012a) should be read in full before reading what follows.

Herein I demonstrate that all the false claims levelled by these individuals against me of undertaking "evidence free taxonomy", fraud, "unscientific taxonomic publications", "taxonomic terrorism", plagiarism, "unscientific taxonomy", "unscientific practices", "unscientific incursions", "deliberate acts of intellectual kleptoparasitism" and the like are without factual basis and foolishly contradicted by themselves elsewhere in their own documents.

This actually is most readily demonstrated by reference to the original papers by myself cited herein.

More significantly I show herein by cross-referencing their statements, that these individuals have repeatedly engaged in the very acts the falsely accuse me of including; "evidence free taxonomy", fraud, "unscientific taxonomic publications", "taxonomic terrorism", plagiarism, "unscientific taxonomy", "unscientific practices", "unscientific incursions" and "deliberate acts of intellectual kleptoparasitism" including in many cases in the 2013 Kaiser *et al.* paper itself. These individuals have repeatedly broken the rules of Zoological Nomenclature (Ride *et al.* 1999). This particularly includes the three critical rules of, 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere) and the ethics of the Code (Appendix A).

What I present here is just a fraction of the totality of the fraud, lies and the like by this consortium of individuals over the relevant period and in terms of the document Kaiser *et al.* (2013).

This article is the ultimate call of their 15-year long campaign to boycott all Hoser taxonomy and nomenclature that has appropriately failed thus far. This is nothing more than a desperate last ditch effort to attack the very rules of Zoological Nomenclature that underpin the science of taxonomy that has been undertaken over the last 200 years.

I say their call should be rejected.

Their call to boycott any and all valid scientific names of their choice and by any author of their choosing, including those of myself and others for usurpment by their own junior synonyms, a practice they have unlawfully engaged in since 1998, should be resisted.

This is especially as their allegedly newly identified loophole in the ICZN rules to enable this, as spelt out in Kaiser *et al.* (2013) is defective and been shown as such previously (Shea 2013a, ICZN 2991, ICZN 2001) when others have attempted to exploit the same alleged loophole (including Sprackland, Smith and Strimple 1997).

Such plans involving reversal of priority for modern names have been condemned by the ICZN before (ICZN 2001) on the basis that if widely adopted, this would ultimately put any and all established scientific names for taxa at risk, thereby undermining the Code.

In their blog that is largely devoid of factual information or evidence, Kaiser *et al.* 2013, proceed to list all species as described by me (Hoser) and Richard Wells in the arbitrary period between 2000 and 2012 as cited below and numbering hundreds of valid names, to have the proper names "boycotted" and then renamed by themselves and others.

Their evidence-free actions are a direct attack on the established rules of Zoological Nomenclature and would if upheld create unprecedented instability. It is a scheme that has already been derided by other taxonomists as "clearly ridiculous and unworkable" (Shea 2013a), confirmed by the earlier views of the ICZN on this topic (ICZN 1991, ICZN 2001).

With Kaiser *et al.* (2013) (p. 20) reserving the right to add further names and species to their suppression and usurpation hit list, no taxon named by anyone is safe in this unprecedented attack on the Code.

Furthermore, by effectively boycotting established nomenclature and the established rules in a war plan that must by their own account run as a controlled and coordinated conspiracy for decades (Kaiser *et al.* 2013, p. 20), Kaiser *et al.* threaten to create unprecedented confusion among herpetologists and others.

Their plan, if allowed to execute, will effectively freeze the progress of the science of herpetological taxonomy and if copied, potentially all other branches of zoology.

Their plan if enacted will also put lives at risk.

The scheme of Kaiser *et al.* (2013) will increase the likelihood of extinction for rarer and poorly known taxa whose proper identities, Kaiser *et al.* might seek to subsume elsewhere, if they actually knew these species well enough to do so.

THE RULES OF NOMENCLATURE: PREAMBLE

It is trite to state the obvious.

But it makes sense that living things be classified according to the evidence. That is the science of taxonomy.

It also makes sense that scientists can use a single set of rules to name what they have classified to enable others to accurately identify the same organisms. That is the aim of the rules of zoological nomenclature.

With these facts in mind, the Linnaean system of nomenclature was developed in the 1700's to deal with the issues of taxonomy of the time.

So while taxonomy is the classification side of things and nomenclature is the act of naming organisms and they are separate but entwined entities/processes, in Zoology they are often treated together.

The Linnaean system, amended slightly in the ensuing centuries, in use by zoological taxonomists globally, has worked well. It has allowed all living animals (including the reptiles) to be classified in a consistent manner and understood by scientists everywhere, the system being used primarily to group species (the fundamental unit) according to relationships to one another.

The fundamental rules of the system (known as "the rules" or "the Code") has for many years been written up as a set of binding rules available to all practicing zoologists, the latest 1999 edition (known as the 2000 Code), being in force since that date and herein cited as Ride *et al.* (1999).

The Code includes three critical rules:

- 1/ Homonymy (Principal 5, Article 52 and elsewhere),
- 2/ Priority (Principal 3, Article 23 and elsewhere),
- 3/ Stability (Principal 4, Articles 23, 65 and elsewhere).

The first states that no two different types of organism (species) can have the same name. That is to avoid obvious confusion in identification. Principal five of the Code states:

"To avoid ambiguity, the use of the same name for different taxa must not occur and is prohibited. This is the Principle of Homonymy."

The priority rule in Article 23 of the Code states:

"23.1. Statement of the Principle of Priority. The valid name of a taxon is the oldest available name applied to it"

This means that the first name correctly assigned to a given taxon, is the name to be used for all time. It means that organism names remain stable and are not subject to undue change by later authors wishing to merely overstamp "their name" on an organism or over-riding a properly assigned name due to reasons such as a personal dislike of the original author.

These first two rules effectively encapsulate and support the third. That is the rule of "stability".

So important is the stability of zoological nomenclature, that the global governing body, the International Commission for Zoological Nomenclature, or ICZN, has stability of names as a stated chief objective in its current rules (Principal 3 of the Code).

Confirming the above at 23.2 the Code also explicitly adds:

"Principle of Priority is to be used to promote stability".

No zoologist of credit or note would dare to challenge these fundamental objectives as enshrined in article 23.2 of the Code.

However, Kaiser *et al.* (2013) (p. 20) no doubt seeking notoriety have boldly stated that they want to ignore 23.2 of the Code so they can also ignore and then rename hundreds of validly described taxa and in the process create instability of an unprecedented magnitude. More on this shortly.

PUBLISHING THE SCIENCE OF ZOOLOGY

Of course for science to progress, inputs including evidence, experimental results and the like must be presented in an honest manner with strict adherence to nomenclatural rules. That way other scientists can duplicate inputs, get the same results and in an ideal world make the same conclusions on the basis of the same evidence.

In a less than ideal world, including the real one we live in, scientists are allowed to arrive at different conclusions, even when the evidence is not in dispute. This occurs regularly in the science of taxonomy. Provided the debate is honest and facts are accurately stated and in context, science may progress.

Hypotheses can be tested and supported or refuted only when every relevant input is known and quantified and results properly published.

Repeated visitation to published scientific papers and making reference to results, conclusions and the like by third parties is not only desirable, but in fact essential so that any potential errors can be identified or conclusions reassessed and science as a whole can progress.

On the other hand if and when experimental data is flawed or fraudulent, this may not be possible and so the progress of science is slowed down, caused by an inability to duplicate results. This includes the study of taxonomy, where molecular and other results in more recent times can be used to validate or refute findings based on morphology, behavior, ecology and zoogeographical evidence and the basis for earlier species descriptions.

Hence honesty and integrity in science at all levels is of paramount importance.

Fraudulently attacking sound scientific data and results can create confusion and put scientific progress back many years.

In taxonomy, false claims about the validity of a taxon that is clearly distinct, can not only delay science, but potentially put one or more species at risk of extinction as has already happened at many times in the recent past.

The common saying "two heads are better than one" is also very true in science. Research often benefits from collaboration by scientists. Sharing of data between scientists is routine.

Published papers often benefit from the input of more than one person.

Good scientists will, as a rule, have all their material checked. This is at all stages of the research and publishing chain. Inputs are calibrated and checked. Experimental data checked, as are hypotheses, theories, results, conclusions and the like.

At the publishing stage, again, all inputs are checked and re-checked and when possible, other experts are invited to review all aspects of the publication before printing.

At the final proofing stage, a few extra sets of eyes may not only find factual or contextual errors, but even such humble things as typographical mistakes.

To this end, the majority of scientific journal editors claim to engage in a process known as "peer review".

"Peer review" as generally defined, involves a paper being sent to one or more reviewers with relevant expertise to check for ways to improve the draft and if need be, reject the paper as unsuitable if material within is plainly wrong, out of context or similar.

While it is not a sure-fire way to guarantee a quality end-product, the statistical reality is that peer reviewed publications are on average of higher quality than those that are not.

In other words peer review in its ideal form is a form of quality control.

To that end, both journals I have edited, namely *Monitor: Journal of the Victorian Herpetological Society* in 1998-1999 and *Australasian Journal of Herpetology (AJH)* from 2009 to the present (2013) have had vigorous peer review, the full history of the *AJH* journal is detailed by Hoser (2012a). In the case of the *Journal Monitor: Journal of the Victorian Herpetological Society*, it was not peer reviewed until I took over as editor and instituted that procedure.

Using world's best practice, I had four reviewers for each paper (as opposed to the usual two) and a streamlined process whereby reviews were quick and effective, with reviewers being given clear guidelines as to what they should be looking out for.

The same procedure was adopted for *Australasian Journal of Herpetology* in 2009.

For the record, and to correct a common lie peddled by Wüster and associates, I have published numerous papers in various peer reviewed journals and over which I have no editorial control since my first paper in 1980 (Hoser 1980, Hoser 2012a).

As a result, I find myself in the first instance, in support of the general statement:

"Best Practices: In the 21st Century, Taxonomic Decisions in Herpetology are Acceptable Only When Supported by a Body of Evidence and Published via Peer-Review".

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Of particular relevance here however is that if I were asked to vote in terms of the principal as espoused, I most certainly would be voting in favour of it.

However, I note that some excellent research findings and publications have been produced in the absence of peer review and in some other cases some terrible papers have made it through a peer review process! As with most things there are exceptions, and in terms of peer review, this is a system easily open to abuse as shown below.

Authors may “shop” papers to journals known to have lax quality control or peer review processes. Alternatively errors can slip past even the most well-meaning of reviewers and editors.

And so one of these outcomes is the case within the “Point of View” section of *Herpetological Review* with the title “Best Practices: In the 21st Century, Taxonomic Decisions in Herpetology are Acceptable Only When Supported by a Body of Evidence and Published via Peer-Review” *Herpetological Review* 44(1):8-23, cited throughout, as requested by Kaiser as, Kaiser *et al.* (2013).

That paper, better described as a dangerous rant, should never have been published!

It is so replete with lies, false information and misrepresentations including being designed to incite hatred of bona-fide scientists, as to be reckless. If its objectives came to fruition, the safety of members of the public would be put at risk.

Of course the comparison between peer reviewed and non-peer reviewed publications is neither new, or in fact relevant to the final “Items of Action” (pp. 19-20) of Kaiser *et al.* Discussion of the relative merits or otherwise of peer review as done by Kaiser *et al.* is in reality merely a distraction from the main and, in reality, only issue of the factuality of the content of my published articles. Their ‘rant’ does nothing more than present a false assertion that there has been no peer review at *Australasian Journal of Herpetology* and is therefore somehow inferior and lacks taxonomic merit even though no evidence is forthcoming on any specific taxonomic changes.

In the context of Kaiser *et al.*'s claims about peer review, Australian reptile taxonomist Glenn Shea wrote:

In the context of Kaiser *et al.*'s claims about peer review, Australian reptile taxonomist Glenn Shea wrote:

“The second issue is that there is no clear dividing line between the non peer-reviewed literature and the peer-reviewed literature. Even the peer-reviewed literature results in taxonomic and nomenclatural changes that are controversial and are either not supported by the evidence that is claimed in the paper, or which can be subject to differing opinions. See, for example, the recent dispute over the division of *Anolis* in the pages of *Zootaxa* (3626(2): 295-299 which, by the way, has an awesome introduction!).

There have been papers published which have somehow made it through the peer review process, which are inadequate under the Code of Zoological Nomenclature. The erection of the gecko genus *Saltuarius*, for example, somehow overlooked nominating a type species for the genus. There was a

recent case of new species of *Hemidactylus* being described without the provision of diagnostic characters, which means those names are unavailable under the provisions of the Code of Zoological Nomenclature. The recent paper dividing up the Caribbean *Mabuya* by Hedges and Conn, with the description of numerous new species and genera, included a reclassification of skinks, raising many lineages within the Scincidae to family level, with the only argument provided being that having lots of species in a family made it unwieldy (try telling that to the coleopterists, who still happily deal with vast

numbers of species in the family Curculionidae (weevils)). I’m still happy to deal with a single family Scincidae, rather than a number of different families. The *Anolis*, *Saltuarius*, *Hemidactylus* and *Mabuya* papers were published in major peer-reviewed journals. On the other hand, there are numerous papers published in fringe journals (entomologists in particular often publish new names in books, which aren’t peer reviewed in some cases) that apply to clearly valid taxa. The descriptions are accompanied by clear arguments and evidence. So where do you draw the line?”

(Shea 2013d).

To further qualify Shea's comments, the *Saltuarius* paper he referred to was in fact: Couper, P. J., Covacevich, J. A. and Mortiz, C. 1993. A review of the leaf-tailed geckos endemic to eastern Australia: a new genus, four new species, and other new data. *Memoirs of the Queensland Museum* 34(1):95-124. That is in fact an “in house” scientific journal in which Museum workers publish their own material that effectively by-passes standard peer review processes. Although Shea raises the above examples, there are numerous other relevant papers he could have also cited to press his point, some emanating from an organization with which he is affiliated, the Australian Museum within its own ‘in-house’ publication the *Australian Museum Records!*

But notwithstanding the above, the fact is that the arguments provided in favour of peer review by Kaiser *et al.* (2013) are generally sound and broadly not in dispute the utopia espoused by peer review or at least the perception of its objective application is clearly a situation that rarely happens in reality. The problem in this instance however is that the arguments and quotes from publications such as Yanega (2009) are presented as a basis to support a false claim against the papers of myself (Hoser) in *Australasian Journal of Herpetology* in 2009-2012, that false claim being that they are not peer reviewed. The fact is that they were!

If Kaiser *et al.* were not running a false claim against *Australasian Journal of Herpetology*, a better title for their blog would have been “Taxonomic Decisions in Herpetology are Acceptable Only When Supported by a Body of Evidence”, because in reality they will only be accepted in time by other scientists if presented with hard evidence.

In taxonomy, as in most other areas of science, it is the evidence that matters the most, not some idealized or

false perception of the adequate application of peer review!

The latter done properly is used as a means to check the former, although history and published experimental results shows that peer review at its best and without bias can and does routinely fail to find mistakes (Ware 2011).

KAISER ET AL. 2013.

Kaiser has had a long working relationship with the editor of *Herpetological Review*, Robert Hansen, having published there many times.

Well before Hansen had even seen a draft of their paper (ultimately published as Kaiser *et al.* 2013) in 2012, Hinrich Kaiser had sent out a SPAM email on 5 June 2012 with the same "paper" saying it would be published at *Herpetological Review* (see copy of the email in Hoser 2012a, the email and attachments cited herein as Kaiser 2012a and Kaiser 2012b). In other words, he knew in advance that the alleged peer review process at *Herpetological Review* was not going to threaten the detail of the "paper" of which he was listed as the lead author.

There were other reasons raising serious questions of ethics, which further explain why Kaiser knew his blog would be published effectively unchanged, but we'll deal with this later.

The detail of the SPAM version as compared to the final version was virtually unchanged, save for the notable removal of all references to Bill McCord and his tortoise taxonomy. This removal arose after McCord had his lawyers write to Kaiser in July 2012 and told him he and the vehicle of publication would be sued if the draft article was published containing his false and defamatory material about their client McCord.

The first section of this published point of view (pages 8-9) are not at issue as they rather do nothing but verbosely explain what I have done in my preamble.

Yes, it makes sense to base taxonomic decisions on the basis of evidence and I can't imagine anyone openly arguing the contrary.

Where Kaiser *et al.* (hereon sometimes cited merely as "Kaiser" for convenience sake) come unstuck is in the second part of their "paper", where he (as senior author) essentially makes a series of false claims against myself and fellow herpetologist Richard Wells. Then on the basis of the false claims, then asserted as "fact", that he seeks widespread support for his position. His position is in two parts, the first being to gain widespread hatred for myself and Wells. On the basis of his "facts" if believed without scrutiny of what is said, this would not be hard to obtain (see also O'Shea 2012a, 2012b, 2013a, 2013b and 2013c).

The second part of Kaiser *et al.*'s position is in fact a wholesale usurping of the established rules of Zoology, including 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere).

Their aim is disclosed (on page 20 of the document) as being to enable themselves (and closest friends) to rename dozens of validly described and already named

reptile genera and species.

This action would cause chaos, not just for herpetologists, but to all zoologists everywhere, if the same tactic of suppression of names purely for the purpose of enabling the re-naming of taxa were to be followed. In fact Kaiser *et al.* (2013) claim they hope this very chaos happens (p. 20). The concept of stability in zoological names as defined in the Zoological Code (Principals 3-5, Articles 23, 52, 65 and elsewhere) would literally be a thing of the past!

The first of many targets of this attack on the Code is all taxonomic papers of myself (Hoser) and Richard Wells (published separately and totally independently) as published in the period 2000 to 2013, listed by Kaiser *et al.* (2013).

Included are the entire contents of issues 1-15 of *Australasian Journal of Herpetology* from 2009 to 2012, all published by myself (Hoser) as well as numerous papers by myself in several other recognized herpetological journals from 2000-2008 and a large number of papers by Wells in the *Australian Biodiversity Record*, a Journal of which he is editor.

Kaiser *et al.* seek to gain widespread support for a boycott of all names proposed within these journals (and perhaps others as they see fit) and then the renaming of all relevant taxa by themselves.

Of course, putting a proposal forward to literally ignore all the rules and conventions of zoology built up over more than 2 centuries would/should never gain support.

On the other hand to propose to somehow suppress the work of a person (or persons) who (allegedly) is a "taxonomic vandal" who habitually likes to "plagiarize others" and engages in "taxonomic vandalism", "fraud", "taxonomic piracy", "bogus taxonomic accounts" and "deliberate acts of intellectual kleptoparasitism" would in the absence of evidence to the contrary only be supported by uninformed people as a popularised view (or others with the very same attributes they say they despise), this scenario would set a dangerous precedent in zoology.

This is especially so, if the plan was marketed (falsely) as being fully endorsed by the ICZN (O'Shea 2013c, Coritz 2013)!

However, here I now demonstrate serious and fundamental flaws in Kaiser *et al.*'s false claims against myself, just listed, including the false claims of endorsement by the ICZN.

I include details of his own unethical activities in breach of the SSAR's own ethics statement, (the controller of the publication he published his blog in), actions in breach of the rules of zoological nomenclature (including those sections previously cited), his ignorance of the differences between taxonomy and nomenclature and why the Kaiser *et al.* attempt to usurp the rules of Zoology for their own personal self aggrandisement should not be allowed.

Before detailing specific errors and flaws in Kaiser *et al.*'s document published in *Herpetological Review*, I draw readers attention to earlier instances of documented and unarguable fraudulent, dishonest and unethical conduct involving the authors of Kaiser *et al.* (2013), including

numerous instances by the co-authors Wolfgang Wüster (see Hoser 2012a), Wulf Schleip (see Hoser (2009a), Mark O'Shea (Hoser, 2012as) and Hinrich Kaiser (Hoser 2012as, cross-referenced with Kaiser *et al.* 2013) as well as listed "supporters" of Kaiser *et al.* (2013) such as David John Williams (Hoser 2001) which are generally not repeated here due to space considerations.

SPECIFIC CLAIMS AGAINST KAISER ET AL. 2013

I now address some of the claims by Kaiser *et al.* (2013).

"Unscientific taxonomy, embodied"

The claim made against myself under the following subheading

"Raymond Hoser's Private Taxonomy"

Automatically makes adverse inferences against me and without basis.

First fact is that if my taxonomic judgments are in error and have no scientific basis, then as a matter of course none would ever be used (Wüster 2003). This would not be the first time in history where such a situation had occurred and would not be the last.

The rules of zoological nomenclature deal with so-called "non-taxa" (a term used by Wüster *et al.* 2001) by relegating them to synonymy with other taxa, via the law of homonymy (Principal 5, Article 52 and elsewhere), and has done this effectively for many years. This means there is never any need to step outside the rules, effectively making Kaiser *et al.*'s (2013) alleged reason to exist effectively redundant.

Recall that the claimed purpose of their paper is to deal with "unscientific incursions", but as already shown, no action outside the zoological Code is in fact required to deal with any taxonomic or nomenclatural decisions/acts made by Hoser or Wells (or anyone else in the Kaiser firing line).

However this most basic claim by Kaiser *et al.* of "unscientific incursions" by Hoser and/or Wells is shown to be false by O'Shea *et al.* (including Kaiser) (2012), where for example they concede that *Broghammerus* Hoser, 2004 is named in recognition of a genus of snakes for which there is sound scientific evidence to support the new designation. In that paper they wrote:

"Rawlings *et al.* (2008) determined that *reticulatus* and *timoriensis* were sufficiently phylogenetically distinct from other species in the genus *Python* to warrant separate generic recognition. However, we believe that the generic name assigned to these two species by Rawlings *et al.* (2008) is taxonomically unavailable"

without giving any reason or basis to the claim of being "taxonomically unavailable".

Specific claims made under the heading "*Raymond Hoser's Private Taxonomy*" are invariably either false or taken out of context, easily shown as such if quotes are read in conjunction with the rest of the relevant descriptions. Of course Kaiser *et al.* hope that readers don't look that far!

Amazingly, out of the dozens of descriptions published, by myself (Hoser) from 2000 to 2012, Kaiser *et al.* are unable to identify even one that they can show, with evidence, is in any significant way, defective!

Instead they have resorted to unscientific, unsupported and evidence free personal attacks of "taxonomic vandalism", "fraud", "taxonomic piracy", "bogus taxonomic accounts", "unscientific incursions" and "deliberate acts of intellectual kleptoparasitism" as a basis to call for a total rejection of the Zoological Code and the fundamental laws of 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere).

The heading "*Raymond Hoser's Private Taxonomy*" is also insidious. It implies there is something fundamentally wrong with how or where my papers were published, when this is not the case. The publications were routine, normal and "public". There was nothing "private" at all!

At best they could claim that *Australasian Journal of Herpetology* was not published as a result of a taxpayer funded handout! And not 'peer reviewed' by them!

Kaiser *et al.* (2013) wrote:

"Hoser does not coin and assign names for the purpose of scientific need, taxonomic clarity, or improved characterization of biodiversity, but rather for personal reasons, as explained by the author in most of his etymology sections, as well as in several Internet blogs and social media environments. Hoser's genus and species names are all patronyms, and many include the author's surname (N = 43; Table 1) or the names of his relatives, employees, and even pets."

Is collectively spiteful and not true. Firstly a new unnamed taxon needs to be identified before it is named. I refer again to the rule of Homonymy!

Once an unnamed taxon is identified, it must be named, according to the rules of zoology. There is a well-established "**scientific need**" to name unnamed taxa! Scientists do this daily!

It is standard and common practice to recognise persons or entities who have provided benefit to herpetology, the author or the wider community when naming taxa. In a breach of the SSAR's own Code of ethics, the co-authors of Kaiser *et al.* (2013) fail to tell readers that they themselves have engaged in the practice of naming new species after friends, a practice sometimes known as nepotism.

There is no basis whatsoever in the claim that species were named "rather for personal reasons, as explained by the author in most of his etymology sections, as well as in several Internet blogs and social media environments".

Because there was no basis for the claim by Kaiser *et al.* they failed to provide a shred of evidence to support it!

That is not a very ethical or scientific approach to publishing in a supposedly scientific publication such as *Herpetological Review*!

In any case there are institutions that receive payment in support/funding of future research and in return name things after these financial benefactors seeking taxonomic immortality, with the ICZN itself being forced to make an official statement on the matter on their website

From: glenn.shea@sydney.edu.au
 To: viper007@live.com.au
 CC: rwrossco@gmail.com; chelodina@aol.com; envirodata@hotmail.com
 Subject: RE: Ash conference and submission of article adverse to myself
 Date: Fri, 8 Mar 2013 02:27:13 +0000

Hi Ray,|

ASH was asked to approve/resolve a prewritten statement, supplied by Kaiser, supporting a Kaiser manuscript submitted to Herp Review. I have no idea whether the ms. is actually to be published there, or when, and I am assuming the ms. is similar to the one which was circulating last year. Despite several requests by Kaiser last year that I provide a letter of support for his manuscript, I repeatedly advised him that was not interested in supporting it. The prewritten statement he provided to ASH was clearly ridiculous and unworkable, and full of the usual gobbledegook that resolutions written by Americans contain, so I did some rewriting for it to make the points clearer and correct several errors in it (e.g., distinguishing between nomenclature and taxonomy – the former is not science, the latter is, and it is only the former that is under the control of the ICZN – they do not make any rulings on taxonomy). As one of the authors of the rewritten resolution, I had to second the motion in order for it to be put to the meeting. However, as I do not agree with the premise of the motion, I abstained from the actual vote. Once the minutes of the meeting have been approved, they should be published on the ASH website (and if the Kaiser manuscript is eventually published, the resolution should accompany it). Until then, they are unavailable (in the same way your own manuscripts are unavailable to other researchers until they are published in your journal).

Regards,
 Glenn

at www.iczn.org Anonymous 2013f), this being, “What is the ICZN official position on selling scientific names? There is no official position taken by the Commission.”

However, it is noted here that the Rules of Zoology do call for wide dissemination of nomenclatural acts (Recommendation 8A) to the extent of ‘posting’ details of my own publications and access to them on my personal facebook pages, websites I control, and relevant internet lists, for which I make no apologies. Again this is in line with the same practices of most other practicing taxonomists, including the authors of Kaiser *et al.* (2013) and not something for which I should be held up for public hatred.

Cases in point including Kaiser *et al.* author, Wulf Schleip and his privately owned “leiopython.de” (Schleip 2012) as well as similar sites controlled by Wüster, O’Shea and Kaiser himself! Who are the hypocrits yet again!

For the record within 24 hours of Kaiser *et al.* being published online, it was SPAM posted on dozens of websites by the 8 authors of the rant and their close associates as part of the entire issue of *Herpetological Review*, posted online, as well as on numerous Facebook pages, in an apparent serious breach of the SSAR’s own publishing and open access guidelines (see Bartholemew 2013a, 2013b), resulting in Bartholemew himself sending out, unsolicited, a SPAM email to hundreds of recipients on his commercial email list advising of the breach of SSAR rules (Bartholemew 2013a) and damage caused.

Mark O’Shea repeatedly posted the Kaiser *et al.* rant as a link including with the header on his facebook page to read “A very important herpetological paper has just

been published - read about it here first.” (O’Shea, 2013a, 2013b).

In this case, wide dissemination and blatant self-promotion had been taken to a ridiculous extreme.

“Hoser’s genus and species names are all patronyms”

As noted already, this claim is broadly correct and no apology is required.

A patronym is a scientific name after a person or thing (like a pet dog), as opposed to one describing the animal in some way, usually using the dead language Latin.

There are several sensible reasons for this. Firstly and most importantly the Zoological Code allows this. Secondly the practice is widespread, acceptable and effectively a standard in modern zoology as noted already, including among the authors of Kaiser *et al.* 2013.

It is not as if I am somehow a lone renegade worthy of “boycotting” because I bestow patronyms. Thirdly, the alternative is naming via a Latinized description of the taxon. The only benefit of this course of action is on the presumption that the reader knows and understands the dead language of Latin and relative descriptive terms as applied to a given taxon. Outside of a limited section of the small community of taxonomists, most people on the planet have no understanding of Latin. More significant is the rule of Homonymy; that is no two organisms can have the same scientific name. When the Linnaean system of

nomenclature was devised there was little conflict in terms of names, as the total number of described species taxa was only numbered in the thousands. Now with an estimated million or more metazoan organisms formally named it has become nearly impossible to coin a descriptive Latin name for a taxon without finding it already occupied by another (animal) organism.

In fact I even found difficulty assigning some patronyms on the basis of prior occupation by another organism or group, meaning some I had earmarked for potential use, were simply not used. I also note that running an argument against a person's taxonomy or nomenclature on the alleged basis of use of patronyms shows how devoid of merit the case of Kaiser *et al.* really is!

The complaint about naming more than one species after a given person also lacks merit. Here are a few examples from the many thousands of patronyms in use: *Boiga wallachi* Das, 1998, *Leptotyphlops broadleyi* Wallach and Hahn, 1997, *Nothophryne broadleyi* Poynton, 1963, *Elapsoidea broadleyi* Jakobsen, 1997, *Leptopelis broadleyi* Poynton, 1985, *Dipsadoboa broadleyi* Rasmussen, 1989, *Atheris broadleyi* Lawson, 1999, *Platysaurus broadleyi* Branch and Whiting, 1997, *Pelusios broadleyi* Bour, 1986, *Lygodactylus broadleyi* Pasteur, 1995, *Ptychadena broadleyi* Stevens, 1972 (has anyone yet suggested banning naming things after Donald Broadley?) or *Oedura coggeri* Bustard, 1966; *Oxydactyla coggeri* Richards and Menzies, 2004; *Ctenotus coggeri* Sadlier, 2005; *Hydrophis coggeri* Kharin, 1984; *Emoia coggeri* Brown, 1991; *Lampropholis coggeri* Ingram, 1991; *Geomyersia coggeri* Greer, 1992; *Mixophyes coggeri* McDonald, Richards and Alfred, 2008 (has anyone yet suggested banning naming things after Hal Cogger?) or *Rhynchophis boulengeri* Mocquard, 1897; *Neolamprologus boulengeri* (Steindachner, 1909); *Cylindrophis boulengeri* Roux, 1911; *Mantidactylus boulengeri* (Methuen, 1920); *Hynobius boulengeri* (Thompson, 1912); *Atelopus boulengeri* Peracca, 1904; *Cryptobatrachus boulengeri* Ruthven, 1916; *Scinax boulengeri* (Cope, 1887); *Morethia boulengeri* (Ogilby, 1890); *Scutigera boulengeri* (Bedriaga, 1898); *Pseudepidalea boulengeri* (Lataste, 1879); *Gephyromantis boulengeri* Methuen, 1919; *Cornufer boulengeri* Boettger, 1892; *Epipedobates boulengeri* (Barbour, 1909); *Amblycephalus boulengeri* Angel, 1920; *Liolaemus boulengeri* Koslowsky, 1896; *Lepidolamprologus boulengeri* (Steindachner, 1909); *Bryconaethiops boulengeri* Pellegrin, 1900; *Trachyboa boulengeri* Peracca, 1910; *Lamprologus boulengeri* (Steindachner, 1909); *Boulengerinia* Dollo, 1886 and many other *boulengeri* species noting that yet again no one has ever suggested too many were named after Mr. George Boulenger!

Or if scraping the bottom of the barrel, *Euseya irwini* Cann, 1997 and *Crikey steveirwini* Stanisic, 2009, both named in honour of Steve Irwin! Recall he's the vandal who ripped off the original Crocodile Hunter Mick Pitman's trademark name "The Crocodile Hunter" and then made a fortune in unspeakable acts of animal cruelty that was broadcast on international TV.

At the same time he was simultaneously having his

mates in the Queensland wildlife department shutting down countless other competing zoo businesses and getting government funds diverted to himself instead of other wildlife "charities" that he also viewed as "competitors".

Kaiser *et al.* (2013) wrote:

"Without exception, Hoser's taxonomic decisions have been published in outlets with evaluation processes that, if they exist, are not designed to safeguard scientific rigor."

Is a false statement. Period!

Notwithstanding the false claim, the merits of the publications speak for themselves.

As already noted, the critical make or break factor for any publication is its evidence and content, not who publishes it, where it was published and who may or may not have reviewed it prior to publication.

If the conclusions within are correct, they will be adopted, just as Darwin's theory of evolution was eventually adopted by a skeptical scientific community, noting that Darwin's publication was most certainly NOT peer reviewed and widely condemned at the time.

On the other hand, if the conclusions in my papers are wrong due to a lack of scientific rigor, then they will not be cited or used and the names unused as well. There is no need for a formal suppression order for this or any need to step outside the established Rules of Zoology including the key rules of 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere) (Wüster 2003).

I also note that the papers I have published have not been submitted to vehicles of publication that the likes of Kaiser or Wüster have editorial control or influence over.

This may irk them and their desire for control, but noting that there have been allegations of their own "peer review" process unnecessarily holding up publications, allowing them to potentially use the submitted data in their own papers and by so doing potentially 'scoop' naming rights for species over others. Consequently, there is no way I would be taking the risk of sending an important taxonomic paper their way!

I now note however, that assuming the recent "Hoser" papers do have scientific rigor, and describe valid species, then the taxonomy used within will come into general usage and in accordance with the established rules of Zoological Nomenclature. This is actually what Kaiser *et al.* (2013) know and fear and is the prime motive for their 'rant'! Proof of this comes from the Facebook page of Mark O'Shea himself where he has posted his knowledge of the fact in one of his ever increasing outrageous angry outbursts.

On 25 March 2013, O'Shea wrote on fellow author Wulf Schleip's Facebook wall:

"150 years from now Herpetologists will consider hoser the greatest herpetological taxonomists of OUR generation. This must not be allowed to happen."

In other words it is clear that it is personal hatred and a mentally twisted state bordering on psychosis that drives

Kaiser *et al.* (2013) and nothing scientific at all!

Kaiser *et al.* (2013) wrote:

“Although the *AJH* masquerades as a scientific journal, it is perhaps better described as a printed “blog” because it lacks many of the hallmarks of formal scientific communication, and includes much irrelevant information (Ross *et al.* 2012). Examples of the latter include private email messages in their entirety, as well as polemics against taxonomic herpetologists (e.g., Hoser 2001:48-56; Hoser 2009a:3-21, 30; Hoser 2012a:1-34), taxonomic journals (*Zootaxa*; Hoser 2012a:15ff), wildlife officials (e.g., Hoser 2012f:12), and even judges in courts of law (e.g., Hoser 2012i:45).”

This is a ridiculous criticism. The vitriolic language above written by Kaiser *et al.* is inappropriate in the first instance. More importantly, there is no rule against any of the material cited above being in scientific journals. As science is indeed a search for the truth and discussion of the same, it is important to include all relevant information, including if necessary e-mails and the like. If asserting a case of fraud, as outlined in *AJH* Issue 10 (Hoser 2012a) against Wüster, it is important to include all relevant documents, in this case including emails (which is nothing different to any other personal correspondence which has often been used and cited for centuries) to document the detail of the fraud and to verify what is a serious claim against a man with a position as an academic at a taxpayer funded University. Furthermore authors of relevant e-mails may accuse me of misrepresenting them out of context should full copies of e-mails not have been printed where appropriate.

By the way, there is no law against citation of other herpetologists, taxonomic journals, wildlife officers, court officials and so on! In fact Kaiser *et al.* did it in the above paragraph!

Kaiser *et al.* (2013) wrote:

“We maintain that *AJH* should not be considered a “public and permanent scientific record” and therefore fails a requirement of the *Code* (Art. 8.1.1; emphasis added) in both style and substance. The *AJH* is not a journal in the scientific sense. It is instead personally distributed by Hoser for unscientific purposes, and should therefore perhaps be best classified as advertising.”

This is a patently ridiculous statement.

The facts are *Australasian Journal of Herpetology* is “public and permanent scientific record” and complies with all rules for the same, including Article 8.1.1 of the *Code*. I know this as fact because the ICZN has confirmed this obvious fact by e-mail on 19 March 2013 (Nikolaeva 2013a)!

So the two questions beg: Why did Kaiser *et al.* publish such a ridiculous claim to the effect that *AJH* is in breach of the *Code* when it clearly is not?

And:

How did such a patently ridiculous (and demonstrably false) claim get past the alleged peer review at *Herpetological Review*?

The claim it is “personally distributed by Hoser for

unscientific purposes” is also ridiculous. Kaiser *et al.* are herein arbitrarily setting themselves up as a group usurping the ICZN itself by redefining the *Code* and what is a legitimate publication, that being one that must be published exclusively by a person who meets their personal approval as a close friend. The claim that *Australasian Journal of Herpetology* is “best classified as advertising” is outrageous.

The fact is that the journal carries no paid advertising!

Whilst talking about advertising, we find from the SSAR's own website at: <<http://www.ssarherps.org/pages/HRinfo.php>>

that:

“*Herpetological Review* accepts commercial advertising. For rates and ad submission details, please check here for display advertising information in (pdf).”

On that basis, should the rant of Kaiser *et al.* be disregarded as “advertising” and having no taxonomic weight?

(This also ignores their disclaimer on page 10 of the document that reads: “These recommendations are not formal nomenclatural proposals according to articles of the *Code*” which as written makes one question why they published their ‘rant’ at all!)

In terms of the issue of taxonomic weight, I will argue the merits or lack of them in Kaiser *et al.* (2013) and not waste time with diversionary ‘red-herring’ arguments and claims against them or *Herpetological Review*.

By the way, copies of each issue of *Australasian Journal of Herpetology* can be downloaded as identical pdf's of the printed version at <<http://www.herp.net>> and again they speak for themselves.

They are clearly scientific literature!

In each issue the following is also written:

“Australasian Journal of Herpetology

Publishes original research in printed form in relation to reptiles, other fauna and related matters in a peer reviewed journal for permanent public scientific record, and has a global audience.

ISSN 1836-5698 (Print)

ISSN 1836-5779 (Online)

Full details at: <http://www.herp.net>

Online journals (this issue) appear a month after hard copy publication.

Minimum print run of first printings is always at least fifty hard copies.”

This text alone makes a mockery of the false claims against *Australasian Journal of Herpetology* by Kaiser *et al.* (2013).

Now this text is not exactly hidden either. It commonly appears prominently placed in the journal, including on the very pages of papers they have sought to specifically condemn (see for example, Hoser 2012aa, bottom of last page and cross match this with their specific complaint number five and also the rest of the page with their false claims of “evidence free” descriptions).

Again I note that it's even more disturbing that false claims like these by Kaiser *et al.* made it past the alleged

peer review process at *Herpetological Review*!

The claim that my descriptions are:

“irreconcilable with published evidence”

is false and itself irreconcilable with Kaiser *et al.*'s other claim that I have engaged in:

“harvesting of clades from published phylogenetic studies for description as new genera or subgenera”.

Hence the central claims of Kaiser *et al.* that my publications are unscientific, a term usually associated with such bogeys as “creationism” and “intelligent design” is shown to be false on his own published evidence!

“Harvesting of clades” is itself at the minimum, a statement I have based my descriptions on some kind of evidence from somewhere!

Kaiser *et al.* (2012) accused me of having used evidence “lifted from others”, which was in fact true in many cases in as much as all the molecular evidence cited had come from other people's work, that I had appropriately referred to and cited.

This 2012 statement, removed from the 2013 document confirms that Kaiser *et al.* knew at all times that my descriptions were based on the most robust of evidence.

Notable is that in 15 pages of rant, Kaiser *et al.* (2013) don't once challenge the evidence I have gained from “Harvesting of clades” and “lifted from others” to challenge the taxonomic and nomenclatural judgments I made arising from this obvious evidence. I note that they had dozens of papers to choose from (listed at the end of this paper) and none was apparently weak enough to be credibly criticized.

Again I note it is even more disturbing that false claims like the descriptions made “in the absence of evidence” by Kaiser *et al.* made it past the alleged peer review process at *Herpetological Review*, even when the claim of “Harvesting of clades” was in the same paper and evidence being “lifted from others” was in the 2012 draft that they were also aware of!

There is also the issue of those papers by myself (Hoser) published in journals over which I have no editorial control.

While citing my own publication and editorial control of *Australasian Journal of Herpetology* as the basis for seeking to boycott all scientific names proposed by myself, this does not sensibly explain why the proposed Kaiser *et al.* boycott must include those descriptions of mine predating 2008 and as far back as year 2000.

These include such widely used names as *Broghammerus* Hoser, 2004, that were published in other journals and over which I clearly had no editorial control.

Kaiser *et al.* (2013) cite and list publications and scientific descriptions of new taxa by myself for the period 2000-2008 within no less than four other scientific journals (they overlooked a fifth with a taxonomic paper naming new taxa from 2003), one of which Wüster and O'Shea have themselves also published a taxonomic paper (Hoser 2000b, Wüster *et al.* 2001)!

Kaiser *et al.* wrote:

“This accountability extends to the taxonomies we create or use. We also believe this responsibility includes monitoring the evidence presented as

justification for taxonomic decisions. Normally, this is a key function of peer review (McPeck *et al.* 2009; Perry *et al.* 2012; Thompson 2010).”

Anyone who thinks peer review is a reliable means of eliminating falsehoods, errors and other inappropriate information being promulgated is a fool (Ware 2011). One look no further than *Herpetological Review*, and in particular the very article in question written by Kaiser *et al.* (2013) to see this. Kaiser himself is a profile elevation seeking nobody from an even less significant institution in backwater California. This crummy little institution is a ‘vanity degree’ issuing organisation peddling creationist theory and intelligent design (Berni 2010, Diaz 2010, Pamplin 2013) and PhDs by mail order that gets a sizeable chunk of its funding selling degrees to students over the internet. Here Kaiser and his other northern hemisphere nobodies are telling (in part) Australians who have worked with local reptiles for decades how we should or shouldn't classify herpetofauna here in Australia.... Can they be serious?

Kaiser *et al.* said:

“Differentiating between science and non-science in taxonomy is a challenge.”

Well Kaiser *et al.* said it and for them this is clearly the case. They have automatically declared Hoser and Wells publications “non-science” and as shown later haven't even read the allegedly offending publications (see below for statements by Schleip and other evidence, such as their inability to even properly reassign taxa in their hit-list).

In other words it is Kaiser *et al.* that deal in “non-science” in taxonomy.

I note also that as alleged scientists, none of the gang of nine have been able to actually rebut a single taxonomic conclusion with a shred of evidence against Hoser or Wells in their sizeable blog! Now if they were scientists they would either “put up or shut up”.

Kaiser *et al.* wrote:

“Taxonomists are relegated to “re-describing” valid taxa that were named prematurely in acts of mass naming or in deliberate acts of intellectual kleptoparasitism (e.g., Aplin and Donnellan 1999; Rawlings *et al.* 2008).”

The above is copied herein as part of Kaiser *et al.*'s rant and not because it applies in my case, although they are falsely arguing it does.

The claim is ostensibly supported by two cited references who had correctly used my taxonomy and nomenclature. However neither of the references contain anything remotely resembling the claim attributed to them, that being I had taxa “named prematurely in acts of mass naming or in deliberate acts of intellectual kleptoparasitism”. In other words the claim fails as fraudulent on the basis that the cited references don't support it. This means the document by Kaiser *et al.* is produced with a veneer of purported truthfulness and verification when in fact there is none!

There are numerous similar cases in the same rant, which I have not detailed herein on the grounds of tedium.

Yet again, I note it is even more disturbing that false claims like these by Kaiser *et al.* made it past the alleged peer review process at *Herpetological Review!*

Obviously their alleged reviewers didn't read the cited sources!

Also I note that the claim that "**Taxonomists are relegated to "re-describing" valid taxa that were named prematurely in acts of mass naming or in deliberate acts of intellectual kleptoparasitism**" could be equally leveled at the likes of Gray, Boulenger, Cope, Fitzinger, Peters and others who created hundreds of new reptile species and genera, thereby depriving others of the right to stick their names on given taxa when these same taxa were revisited at a later date.

It's also worth revisiting the exact taxa that Aplin and Rawlings were allegedly left to redescribe as a result of my own taxonomic work.

In the case of Aplin it was a species of Death Adder, genus *Acanthophis* I had named in 1998 (see Hoser 1998a). Fact was, in 1980, I had deferred naming the same species to allow Glen Storr of the West Australian Museum the right to name the same species.

Instead he chose to regard it as a variant of *Acanthophis pyrrhus* (see Storr 1981). My holotype of the species *Acanthophis wellsei* was in fact one of several specimens of the taxon that Storr had inspected himself and in my view incorrectly classified as *A. pyrrhus* as listed in his material examined. I named that and other *Acanthophis* variants as new species as part of a 20 year project working on the taxonomy of Death Adders.

This was in fact my first ever paper naming new taxa in a period spanning nearly two decades of publishing in peer reviewed journals.

My first peer reviewed publication was in 1980 in the journal *Herpetofauna* (Hoser 1980).

To allege I have somehow sought to rush in and usurp another herpetologist is patently outrageous.

Aplin did subsequently publish a paper further validating my description of *Acanthophis wellsei* with new molecular data, but there is absolutely no question that I had been working on the said snakes for many years longer than him (Aplin and Donnellan 1999).

The same applies in terms of the Reticulated Pythons, that I placed in the genus *Broghammerus*.

At the time of my publication (Hoser 2004a), I had been publishing papers on python taxonomy since 1982 (Hoser 1982) a span exceeding more than 20 years. So I was no new kid on the block seeking to rip off someone else's work as scandalously inferred by Hinrich Kaiser *et al.*!

The 1982 paper was notable for transferring the species *carinata* Smith, 1981 from the genus *Python* to *Morelia*, a move for which I was widely condemned at the time. But since about 1990 the concept has been generally adopted, (although in 2009 and 2012 I have now since transferred the species to a new genus; see Hoser 2009a, 2012b).

At the time of publishing Hoser (2004a), no other people were working on the taxonomy of the relevant taxa including the Reticulated Pythons and of note is that

numerous expert taxonomists including McDowell (1975) or Kluge (1993) had published monographs on python taxonomy and chosen to place the species *reticulatus* in the genus *Python*.

In the period leading to the publication of Hoser (2004a) and relying on obvious morphological differences as detailed by McDowell (as in the same data he had available to himself), I interpreted the data (along with my own) differently and had no hesitation in transferring *reticulatus* to a separate genus. In the absence of an available name, I did in accordance with the rules of Zoological Nomenclature (Ride *et al.* 1999) create one, this being, *Broghammerus* Hoser, 2004.

There is nothing at all unreasonable with what I did and to be condemned by Kaiser *et al.* a decade after the fact is totally outrageous, especially when the only proven grievance is a long term personal hatred of myself by the other key authors of his "paper".

Wüster and others published widespread condemnation of my taxonomic act with the taxon *reticulatus*, including on www.kingsnake.com <<http://www.kingsnake.com>> chat forums .

In 2004, I was not able to read into the future and see that four years later a student biologist in the form of Leslie Rawlings would inspect the DNA of the same species and confirm my splitting of the original genus *Python* on the basis of then unknown molecular data (Rawlings *et al.* 2008).

To blame me for relegating Rawlings to ""re-describing" valid taxa" is an outrage.

Should all living herpetologists stop describing new taxa so that later generations won't be relegated to ""re-describing" valid taxa"?

Should we allow species to go extinct in order to allow later generations the prospect of being able to describe a given taxa perhaps then long since gone?

What is perhaps most surprising in hindsight is that no other herpetologist had in the previous 200 years seen the obvious and that being that the species *molurus* and *reticulatus* were not remotely alike!

I further note the size and abundance of both species, they are neither exactly small or obscure.

With Google showing over 20,000 uses of the validated name *Broghammerus* Hoser, 2004 on the web alone, as of March 2013, including more than 100 uses online in the week following publication of Kaiser *et al.* (2013), I must say that to expect Kaiser and friends to be able to violate the fundamental rules of zoological nomenclature, including 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere), so as to assign their own name to the same taxon group name of pythons really is an outrage. Finally I note that as recently as March 2012, under the user name "Mokele" Wüster edited the Wikipedia page for the Reticulated Python to ensure that the snake was not listed under the name *Broghammerus*.

In other words he'd been boycotting all Hoser names since well before the publication of Kaiser *et al.* (2013).

The statement "**Taxonomists are relegated to**

“re-describing” valid taxa that were named prematurely in acts of mass naming or in deliberate acts of intellectual kleptoparasitism” is also notable when reconciled with the rest of Kaiser *et al.* (2013), and can easily be seen as a case of some would-be taxonomists getting their noses out of joint (‘sour grapes’) because a man with whom they have fought for more than fifteen years has named species they are now left re-describing and having to otherwise use his names in their own publications (e.g. Schleich and O’Shea 2010), when they would clearly rather not, as seen in O’Shea 2013b, where O’Shea posted on Schleich’s Facebook wall:

“150 years from now Herpetologists will consider hoser the greatest herpetological taxonomists of OUR generation. This must not be allowed to happen”.

See also “Repeated plagiarisation by Mark O’Shea” detailed below.

Now as for the claims repeated by O’Shea regularly (e.g. O’Shea 2012a, 2012b), that I, Hoser have rushed out and named too many species, thereby depriving others of this alleged right, a reality check is in order.

Scientific consensus is that there are a lot more unnamed species of animal than named (Goldenberg, 2011, Osborn 2010), including about 20% of reptile species remaining unnamed, giving a total of reptiles unnamed, still in the thousands.

Now rather than bitching and moaning and wasting everyone’s time publishing hate and lies about a man they don’t like who has named a few dozen species and genera, perhaps Kaiser *et al.* can set about describing some of the unnamed biodiversity before people exterminate it!

It is also interesting to note they hypocrisy and double standards of O’Shea, Wüster and friends in their attacks on myself.

O’Shea (2012a, 2012b) complains that I (Hoser) have named too many species, thereby depriving future generations of herpetologists of the opportunity to name taxa (see also Kaiser *et al.* 2013), and along with Wüster, have successfully convinced their good friend Peter Uetz to boycott the use of any Hoser names on his “reptile database” (Uetz 2013c), relying on these claims within Kaiser *et al.* (2013). Yet on the very page Uetz announces the boycott of the Hoser names, he also pays homage to others including Hobart Smith and Aaron Bauer for the large number of taxa they have formally named!

We do in fact get a glimpse of the false marketing by Kaiser *et al.* as seen below:

“We have learned that better placed or marketed falsehoods may supplant truths in public perception. Thus, a taxonomic fact can become obscured by nonscientific information, misleading those who are unable to discern whether the information was appropriately generated.”

Here they delude themselves into thinking if they say something often enough it becomes true. The intended outcome of which is to supplant the original discoverer and describer with one of their own publications that

results in the intellectual theft of someone else’s work. Otherwise an objective scientist would just prove that the taxonomic change proposed is not correct (e.g. Wüster 2001, Wüster *et al.* 2001, both of which over the next decade failed to convince other better and quite properly sceptical scientists of any factual basis to reject the Hoser taxonomic proposals in the period predating 2001). Why else would you want to suppress a proposed taxonomic change by demanding a boycott and yet without presenting a scientific basis for it?

THROWING AWAY THE RULES OF ZOOLOGICAL NOMENCLATURE

In the June 2012 draft of the same document, Kaiser *et al.* (2013), Kaiser *et al.* wrote:

“we propose that a 9-member herpetological consortium with rotating, global representation is formed to establish a List of Available Names in Herpetology.”

My response to this in Hoser (2012as) was a statement that this was:

“simply a grab for power by the truth-haters and their self appointed “consortium”. They seek to usurp the ICZN, to assert naming rights on all previously named reptiles, to rename all species as they see fit, shamelessly ripping off the work of other zoologists and in violation of a Zoological Code that’s operated for more than 200 years!”

The same authors have now altered their claim to:

“List of herpetofaunal taxa published on or after 1 January 2000 that can be objectively classed as unscientific, non-peer reviewed, misguided in intent or presentation, fraudulent, or lacking evidence. These names *should not be used* in herpetological nomenclature, pending suitable action by the ICZN. Instead, we urge that these names be treated as listed in the column titled Recommendations by reverting to the older name of record, or by another suitable name as indicated.”

This statement is then followed by a list of hundreds of names.

Which in effect is much the same thing!

What hasn’t been properly revealed by Kaiser *et al.* (2013) is the real basis behind the false claims in their “paper” and what they are in fact seeking.

In 1993 and 1996, I published a pair of books about wildlife smuggling and corruption in Australia (Hoser 1993, 1996).

Adversely named therein was David John Williams, identified in the books as being found guilty in a court of law of extreme animal cruelty and wildlife smuggling (Hoser 1996) as well as another smuggling incident (Hoser 1993).

Whilst dealing with the subject of illegality and movement of specimens of animals between jurisdictions and the conservation impacts of such activities the recent activities of listed “supporter” of Kaiser *et al.* and major advocate for them, Bryan G. Fry are worth noting.

In 2011 Brian Fry, whilst working at Melbourne University was alleged to have illegally imported and exported reptiles to and from Victoria as well as from and to

various other jurisdictions. Brian Fry was ultimately caught and had a search warrant executed upon him by Victorian Police and the DSE. This search warrant ultimately triggered a further 5 simultaneous search warrants in NSW and Victoria. According to Victorian Department of Sustainability and Environment (DSE) official Tom Thyuss, Brian Fry then fled to Queensland in an effort to avoid the prosecution that would otherwise have followed, being forced to remain out of state for at least two years to escape charges (DSE are undecided on whether to exercise extradition). But in any case Fry has left a trail of devastation among the other amateur herpetologists lured into his illegal activities on the naïve pretext that they thought it was all legal due this research status at the University of Melbourne.

Shane Black a noted NSW amateur Elapid keeper and breeder was raided and his massive collection ultimately dispersed and he soon after lost his wife who died as a direct consequence of the devastating outcome. He now also lives in that refuge for people on the run from the law called "Queensland".

Other amateur's lured into Fry's web that were subject of heavily armed raids in his wake included noted Victorian amateur herpetologists Robert Valentic and Chris Hay, both of whom had wildlife seized and destroyed, including the only captive Pilbara Death Adders *Acanthophis wellsei* in Victoria, charges laid and in due course very heavy fines, which at the time of writing this in 2013, they are still paying off. Their lives were similarly trashed in Fry's wake. This is the calibre of individuals with whom Kaiser *et al.* co-publish and, with whom, they purport to practice ethical science (Thuys 2013)!

Or, applying the legal litmus to Mark O'Shea would not come without blemish, noting that in 1993 he left a trail of indecent or sexual assault investigations in his wake after leaving Australia.

Williams and friends, Wüster, Mark O'Shea, Wulf Schleip and others have in effect waged war against myself ever since, doing all they can to undermine my good name and tarnish my reputation built over more than 40 years of working with reptiles.

Each and every taxonomic proposal I have published since the first taxonomic paper in 1998, has met with published protest by these same individuals, making directives to others to not use the names and further increasingly ridiculous claims against the relevant publications.

Examples of such papers and claims against my own taxonomic works by these people have included Williams and Starkey (three versions of the same paper listed as 1999a, 1999b, 1999c), Wüster *et al.* (20001), Williams *et al.* (2006, 2008), Schleip (2008), Wallach, Wüster and Broadley (2009), Wüster and Bernils (2011) and others.

Using the internet as a key weapon and often posting under false names, they have harassed journal editors and others and tried to ensure that none of my taxonomic proposals have been recognized or names used.

These individuals have used a range of often fraudulent claims and methods and have had some degree of success.

However, with science being a search for the truth and my taxonomic papers being based on hard evidence, the names proposed by myself for what were then hitherto unnamed taxa, have since come into general usage. Better known examples include *Acanthophis wellsei* Hoser, 1998, *Leiopython hoseerae*, Hoser, 2000, *Pailsus* (or *Cannia* or *Pseudechis*) *pailsei* Hoser, 1998, *Pailsus* (or *Cannia* or *Pseudechis*) *rossignollii* Hoser, 2000, *Broghammerus* Hoser, 2004 and so the list goes on. In 1998, Wüster and others simply claimed my taxa described weren't different to other named species. An example of the often repeated "non-taxa" claims are seen in Wüster (2001a) and Wüster *et al.* (2001).

By 2003, Wüster commenced running claims that my papers were not validly published according to the Code. In the face of the false claims against my 2003 paper, this was dealt with most easily by describing the same taxa again in 2003/4 in another publication (Hoser 2004a).

In 2009 I commenced publication of *Australasian Journal of Herpetology* for several reasons, including so that Wüster and others could not harass journal editors to either not publish my material, or alternatively make them retract my papers to enable them to republish descriptions of the same taxa using my data.

It should be noted that unlawful approaches to do exactly this were made in 2000 (two editors), 2001 and again in 2004 (see Hoser 2012a for details and citations). To the credit of each editor, they resisted the threats of Wüster and Williams.

Put simply, I wouldn't tolerate their illegal threats and that was the basis for me taking control of my own journal. I am not the first to have done this and will not be the last. I also note that with Schleip as an editor at *Herpetological Review*, Kaiser *et al.* have hypocritically committed the very act, they seek to demonize me for!

It was also believed (in hindsight erroneously) that by being in active control of the publications, Wüster would not again raise a false claim that they were not published according to the Zoological Code.

However following a series of taxonomic papers in *AJH* issues 1-7, naming numerous species and genera, Wüster and two co-authors (Wallach and Broadley) made an outrageous claim that none were published according to the Zoological Code and they falsified their data in a paper the same year to support the claim. At the same time the authors sought to rename a genus of Cobras, I had named in *AJH* Issue 7. In the same paper (Wallach *et al.* 2009) and elsewhere (e.g. Schleip and O'Shea 2010) the claims were repeated and they invited or incited others to breach the Code by attempting to usurp the names validly published in *AJH* Issues 1-7.

In the earliest period 1998 to about 2008 when condemning the "Hoser names" Wüster had written things similar to what he had written in 2001 when he stated:

"it is clear that all the taxa described by Hoser are validly described under the provisions of the Code. The names are thus available, and, where they are the oldest available names for biologically valid species or genera, they must be used. However,

Hoser's descriptions are much less convincing when it comes to establishing the biological reality of his taxa." (Wüster 2001a, Wüster *et al.* 2001).

Hence he was advocating people not believe the taxa were valid.

As each were revisited by other scientists, including the likes of Aplin and Donnellan (1999), Rawlings *et al.* (2008), Eipper (2012) and so on, the taxa were validated and so the names came into usage.

In 2009, Wüster decided to reverse the original claim and say that while the taxa may be valid, the publications were not, as he did in Wallach *et al.* (2009).

Then in 2012 and 2013, when all his tactics failed (see below), he has now said he has no respect for the rules of Zoology and wants to rewrite all Hoser's names in his own peculiar way (e.g. Kaiser *et al.* 2013).

The central theme spanning this 15 year period has been a continual campaign to boycott use of the "Hoser" names.

Of course science should deal with facts and not personal dislikes, and the end point of taxonomy and nomenclature should be that organisms are properly named and in accordance with the rules, not who named them!

Fifty years from now, no one will care who Hoser or Wüster were!

The claim that I have named too many species, inferred explicitly in page 16 of Kaiser *et al.* (2013) and stated openly by O'Shea (2012) is ridiculous in the extreme. Naming dozens of taxa is insignificant when compared to hundreds by the likes of Boulenger, Gray, Fitzinger, Cope and others. Will these individuals have their valid scientific names put on the embargo list because they named too many species?

In passing I note that Wüster's close friend, Van Wallach, has tried the stunt of renaming validly named taxa in breach of the Zoological Code's three critical rules of, 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere) several times. He did this in 2006, when erecting the genus *Austrotyphlops* to usurp the properly named genus *Sivadictus* Wells and Wellington 1985, using the same type species. Coauthoring with Donald Broadley (of Wallach, Wüster and Broadley 2009 fame) in 2009, Wallach created the genus name *Afrottyphlops* to retrospectively usurp the valid Fitzinger 1843 name *Aspidorhynchus* for the same type species.

These acts of "taxonomic vandalism" in direct breach of the rules of Zoology are the same sort of nomenclatural chaos that await all legitimately publishing herpetologists if the Kaiser *et al.* (2013) proposal to retrospectively suppress hundreds of names gains traction.

In 2012, and well aware of the non-stop antics of Wüster and others to claim my publications were not valid, a series of six more issues of *AJH* were published (issues 9-15), all taxonomic in nature, with printed receipts from the printer for the hard copies of the journal printed within each issue.

This was to negate further false claims that the journal

was only published online or "on demand" (in violation of the Zoological Rules).

The same year, and again in 2013, Kaiser *et al.* have continued their campaign against usage of "Hoser names" not on any scientific or moral basis, but rather due to hatred and spite and their own total contempt for the rules of zoology, including 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere).

The paper Kaiser *et al.* 2013, runs 15 pages and yet in its entirety fails to address a single taxonomic act I have performed and identify it as being in error. The closest they get to this is on page 16, where they have quoted sections of descriptions (usually a few words from descriptions running paragraphs) for a handful, or claimed without factual basis "invention of evidence" totally out of context to allege the entire descriptions are somehow in error.

This is done even though the evidence presented for each description in the original papers was vastly greater than inferred/stated and most certainly not invented as alleged.

Even more bizarrely, Kaiser *et al.* (2013) concede I have made correct taxonomic judgments, (page 9, top of second column), which in the normal course of events should lead to an endorsement of my taxonomy and use of my nomenclature ... except for the fact that these so-called scientists have a personal dislike and put that ahead of the rules of nomenclature, including 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere).

Hence when the veneer of lies and misrepresentation is viewed objectively, there can be no basis for suppressing the zoological Code in favour of a gang of 8 who in summary seek to retrospectively usurp the rules by over 12 years to have them rename taxa that already have valid names and many in wide usage.

Put simply, Kaiser *et al.* are well aware that the taxa named by Hoser in 2012 and earlier have been named on the basis of firm molecular and morphological evidence and are therefore valid taxa and also properly named according to the rules of zoological nomenclature. What they fear and dread is that a herpetologist they have decided to wage war against has names proposed by him that end up in widespread general usage.

It is also notable that these same authors make no ambit claims against generic names resurrected from synonymy by myself or Wells, even though these number in the dozens and would in theory have been created by the same allegedly "evidence free taxonomy". This absence confirms that the Kaiser *et al.* claims and intent is motivated solely by malice (or petty jealousy) and no noble scientific intent as claimed. Running out of false claims to make against individual taxa described, which have in 2012 become too numerous to try to deny, and unable to again claim that *AJH* is not validly published, they have launched their final attack against Hoser and in effect the entire Zoological Code in order to suppress dozens of valid names for valid taxa.

The clear (and virtually stated) end point is so that they may in time themselves seize naming rights for the same organisms.

The statement that the taxa in their list are “**objectively classed as unscientific, non-peer reviewed, misguided in intent or presentation, fraudulent, or lacking evidence.**”

is also patently false.

There has never been any objectivity by Kaiser and his co-authors. This has been admitted by O’Shea and Wüster on numerous online chat forums. Kaiser himself has lied about this as well. In his SPAM email of 5 June 2012, he said he was disseminating the first draft of his rant as a neutral secretary, not at the time identifying himself as nominated lead author.

Elsewhere in Kaiser *et al.* (2013) they conceded *Australasian Journal of Herpetology* was in fact peer reviewed (p. 17) making their above quoted hit list statement on page 10 of their manifesto fraudulent.

As for the part stating the Hoser (and Wells) publications were “lacking evidence”, well, I’ve already mentioned the other claim of “harvesting of clades from published phylogenetic studies” making a mockery of “lacking evidence” bit.

As the publications of *AJH* followed normal scientific processes, procedures and methods, the claim they were unscientific is also false. And as already noted, if false, can be dismissed as such forever, without need to enforce some kind of “boycotting” (Wüster 2003). There is not a shred of evidence to suggest anything fraudulent in the papers, that is unless Kaiser *et al.* are going further and suggesting that perhaps some of the references cited in my papers, like Pyron *et al.* (2011), Kluge (1993), McDowell (1973) and the like are themselves fraudulent. However again, they have not produced any evidence of this.

And returning to the often repeated claim that *Australasian Journal of Herpetology* was not peer reviewed is also false. For the record, repeating a lie many times does not change things.

It remains a lie!

I also note that the “Items for Action”, essentially an in text qualification of the stated demands of their “Table 1”, statement is a direct breach of the Zoological Rules which in its preamble states “none restricts the freedom of taxonomic thought or actions.” However their call to all herpetologists, editors and the like to “*strictly avoid the use of the taxon names listed in Table 1*” is a very serious breach of the Code in that it is a direct censorship of freedom of individuals to make taxonomic judgment’s!

The plan by Kaiser *et al.* (2013) to restrict taxonomic publications to the journals over which they alone exercise editorial control and censorship (p. 19) is both a direct breach of the above-quoted preamble to the Code, and the stated intent of recent amendments to it enabling wider publication outlets including electronic, provided the well-defined criteria of publication are adhered to (Ride *et al.* 1999).

Could you imagine the anarchy that would ensue if all taxonomic publications had to be vetted by individuals

such as the despotic internet trolling Wolfgang Wüster or an angry little man named Mark O’Shea?

PEER REVIEW - OR IS IT A JOKE AT HERPETOLOGICAL REVIEW?

Throughout this paper (above and below), I detail clear factual errors by Kaiser *et al.*, consistency errors and mistakes that should have been eliminated in any sensible peer review.

I will state that I am of the view that the Kaiser *et al.* rant should have been rejected wholesale.

This is because it’s central factual claims are in error.

Noting that I am a proponent of free speech, only tempered by the caveat that truth should be a part of this, I note the following points on the basis that Kaiser *et al.*’s piece was invariably destined to be published in *Herpetological Review* on the basis of a lack of effective editorial control.

While it is clear that the alleged peer review at *Herpetological Review* was not viewed by Kaiser as an impediment to his material being published, I do note that the alleged peer review at *Herpetological Review* has failed to even properly cross reference the allegedly unscientific publications of Hoser and Wells to get Kaiser *et al.*’s own taxonomic proposals correct.

This is significant, as it shows that both Kaiser *et al.* and the peer reviewers at *Herpetological Review* (if they actually existed) have in fact failed to even read most, if any of the publications they seek to condemn!

I give but a few examples.

In the list of Hoser and Wells taxa that they seek to relegate back to their proposed alternative, there are numerous errors including but not limited to:

Costinisauria couperi is not *Lampropholis couperi*, but is a northern population of *Eulamprus kosciuskoi* (the original description makes it clear that the species was described from within what others consider *Eulamprus kosciuskoi*). For readers, like Kaiser *et al.*, totally unfamiliar with these Australian species *Lampropholis* and *Eulamprus* belong to separate tribal groupings within the family Scincidae, Kaiser *et al.* have demonstrated by this evidence free taxonomic act that they have no idea what they are talking about when it comes to making an assignation of an Australian skink to the wrong tribe;

Cyrtodactylus abrae is not a synonym of *Cyrtodactylus tuberculatus* (this matter was dealt with by Shea in 2011, when designation of a neotype made the species a direct synonym of *Cyrtodactylus pulchellus*)

Zeusius sternfeldi is not a synonym of *Cyclodomorphus casuarinae*, but is most similar to

Cyclodomorphus venustus (the population named *sternfeldi* was considered part of *venustus* when Shea described that species).

See Shea 2013c.

For those unaware of Australian skinks, the first mistake is akin to placing a species of Garter Snake (*Thamnophis*) in the same genus as an Indigo Snake (*Drymarchon*).

In other words, if readers were to adopt the taxonomy proposed by Kaiser *et al.* (2013), there would be even greater taxonomic chaos with species being totally

misidentified and being placed in radically different genera to where they should be, even in the highly conservative taxonomic arrangements that predate Hoser and Wells!

All this again shows that the hit list of Hoser and Wells taxa to be suppressed is in itself unscientific and is totally "evidence free".

It is the accusers, Kaiser *et al.* who are guilty of the very offences they are accusing others of!

The continual reference to Hoser and Wells names being "unscientific names" and calling for these to be "boycotted" is as Shea stated "clearly ridiculous and unworkable" (Shea 2013a).

In all that Kaiser *et al.* have printed, by far the most "unscientific name" formally proposed in writing by anyone is the transfer of *Eulamprus kosciuskoi* to the genus *Lampropholis* as advocated WITHOUT EVIDENCE by Kaiser *et al.*!

BYPASSING PEER REVIEW BY BEING THE EDITOR!

This claim has been thrown at myself (Hoser) ever since I first published *Australasian Journal of Herpetology* in 2009 and is cited repeatedly as a basis for condemnation by Kaiser *et al.* (2013) and posts on the web by the authors before and since (e.g. Schleip 2009, Schleip 2013). As mentioned previously, several factors led to my commencing publication of the journal, not least being the non-stopping harassment of several journal editors by Wüster in particular, seeking the formal retraction of Hoser papers. Some editors even published their distaste at Wüster's unethical conduct, this being for the sole purpose of giving him the right to rename Hoser named species, with two even editorialising about Wüster's misconduct, namely Newman (2000), Van Aken (2001).

Of course with Kaiser *et al.* spending a large part of their paper condemning myself for allegedly bypassing proper peer review and quality controls and manipulating things in my role of editor, it would come as a shock to readers here to know that Kaiser *et al.* and Schleip in particular have been guilty of these very things.

You see Schleip is in fact an editor at *Herpetological Review*!

Now of course under the SSAR's ethics statement, such conflict of interest should be disclosed with the paper. It is not!

By contrast, my own editorial role at *Australasian Journal of Herpetology* is disclosed in the proper way.

In spite of dozens of internet posts by Schleip himself condemning myself for being editor of the journal I publish in, he has refused once to disclose this relevant fact in terms of himself.

Of course, knowing Schleip's form for fraudulent conduct (see below) and the obvious breach of standard peer review or quality control in the Kaiser *et al.* blog, it is now patently clear that the conflict of interest with Schleip being an editor at *Herpetological Review* is relevant, not just in terms of how the defective Kaiser *et al.* (2013) blog was published, but also how his defective 2008 *Leiopython* paper (see elsewhere) got published in the SSAR's other journal, namely *Journal of Herpetology* in 2008!

While talking a lack of ethics in terms of Schleip, Kaiser

and the whole editorial process at the SSAR, it is worth contrasting that with the higher standards at *Australasian Journal of Herpetology*.

In 2012, in Issue 14 of *Australasian Journal of Herpetology*, I published the entire document Kaiser (2012b) without any editing whatsoever. At the same time and place I published my rejoinder and critique of the same blog.

That way unbiased readers could read and view both and make an informed conclusion in terms of the papers.

At the time Kaiser sent his blog via SPAM globally, I wrote to Hansen by email and asked him for the proper right of reply at the same time and place as that which he published a piece by Kaiser and others.

While Hansen (2012) then said that he wouldn't be printing the material, which he then stated he hadn't yet got, he was then put on notice I would be seeking such a right of reply and at the same time and place.

It is notable that in 2013, and well after he had received Kaiser *et al.* (2013), Hansen broke all ethics, including the SSAR's own published ethics statement by 1/ not informing me he had received the Kaiser *et al.* blog and 2/ not inviting me to publish a rejoinder to this Kaiser *et al.* blog at the same time and place as would be expected.

All the above shows that the claims of malpractice against myself (Hoser) and *Australasian Journal of Herpetology* lack basis or merit, while Kaiser *et al.* (including Schleip and the fellow *Herpetological Review* editor Robert Hansen) have engaged in systematic ethical breaches and scientific malpractice.

Even if one were to accept the dubious claim that Schleip and Hansen were in fact scientists as some kind of defence for their editorial malpractice, the statement by North (2013), "that having a journal run by scientists does not, by any means, preclude poor judgment. But then we knew that, didn't we." Certainly rings true in the case of both the SSAR's *Herpetological Review* and *Journal of Herpetology*.

Now as to how I found out the critically relevant fact that Schleip was an editor at *Herpetological Review*, it came from a series of posts he made on a German language chat forum "pure-reptiles.de" where in a series of posts in March and April 2013, he effectively bragged to readers that he was able to push the publication of Kaiser *et al.* through the editorial process and bypass proper peer review because he was an editor at the magazine (Schleip *et al.* 2013b)!

Cross-checking with the SSAR's own website confirmed this important fact at the bottom of one of their own webpages (Anonymous 2013e).

While talking about the improper relationship between Kaiser, Schleip and Hansen, allowing for a clear breach of standard peer review and the SSAR's own ethics statement (Anonymous 2013a) to get Kaiser *et al.* (2013) published, there is further evidence of misconduct on the parts of both Hansen and Schleip. This is seen in their relevant private cross-posts as far back as 2011 (Schleip and Hansen 2011). On 26 April 2011, Hansen told Schleip:

Date: Tue, 19 Jun 2012 22:23:05 -0700
 Subject: Re: False and defamatory material about myself - potential submission of paper to Herp Review - To the editor.
 From: herpreview@gmail.com
 To: viper007@live.com.au

Dear Mr. Hoser:

As a general statement, inasmuch as we do not presently have any paper such as you described scheduled for publication, any and all submissions to *HR* undergo external peer review before acceptance for publication. We do not publish personal attacks, as our track record over the last 40+ years should clearly demonstrate.

Sincerely yours,

Robert Hansen

“Hey Wulf. No need to freak. I would not worry too much about “biological truth” as it is an elusive thing.”

Hansen's own disdain for the peer review process was seen in a paper he coauthored, when he said “the review process has become increasingly difficult. The resulting delays slow publication times, negatively affect performance reviews, tenure, promotions, and grant proposal success. It harms authors, agencies, and institutions” (Perry *et al.* 2011), which of course is a ready-made explanation as to why proper peer review was bypassed under his watch for Kaiser *et al.* (2013).

It also explains why he refused to name his alleged peer reviewers for the Kaiser *et al.* paper in 2013, (Hansen 2013), because in reality, there almost certainly were none!

MAXIMUM DAMAGE IS THE AIM OF KAISER ET AL.

The stated long term plan of Kaiser *et al.* was published without obvious contrary view in order to maximize potential effect (or damage) to be caused by the Kaiser *et al.* (2013) blog, which was presented with a veneer of unanimity in the herpetological community, which in itself was a fiction.

The so-called paper, published in breach of the SSAR's own published ethics statement seeks to force a “boycott” on the use of Hoser and Wells names for more than ten years while simultaneously breaking the zoological Code, including the three critical rules of, 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability

(Principal 4, Articles 23, 65 and elsewhere) and publishing and using junior synonyms, would if allowed to succeed could in effect spell the end of the zoological Code as it is known.

In effect a group of renegade pseudo herpetologists could at any time target a scientist or group of taxa, (e.g. McCord, Fitzinger, Cope or genera *Python* or *Chelodina*) or declare the validly assigned scientific names “unscientific names” boycott them and then usurp them with names of their choice by simply redescribing the taxa.

This has already been seen in part with Wallach in his bare-faced attempts to steal naming rights for taxa as diverse as Blind Snakes and Cobras and if Kaiser *et al.* get what they desire, will extend to all areas of herpetology and perhaps in time to other zoological disciplines as stated by Kaiser *et al.* (2013), p. 20 second column, second paragraph.

Of course the herpetologists in the Kaiser's hit list this week are Hoser and Wells. Last year the list included McCord, but he was too rich and powerful to take on in the same legal jurisdiction as the publication, so Kaiser *et al.* dropped their claims against his named taxa for fear of financial decimation via legal proceedings.

But in Kaiser *et al.* they reserve the rights to make similar boycotts against anyone else this unelected group deem “unscientific”. This in effect puts all taxonomists at risk of having their life's work arbitrarily suppressed by renegades, especially those from third world countries and without funds to legally threaten the likes of Kaiser *et al.*

THE LIES ABOUT THE ICZN

Of note: Kaiser *et al.* wrote near the end of their paper:

“We applaud the discussions held by the ICZN on how best to curb taxonomic vandalism, and we encourage the Commission to proceed with all due speed in their deliberations. Time is of the essence, especially given the recent emergence of instances (described above) where individuals have flagrantly violated the spirit of the Code and have used taxonomic publications as a vehicle to defame and inflict professional harm on those working within ICZN guidelines.”

This is a total misrepresentation of the truth. Fact is the ICZN is NOT deliberating in relation to myself or Wells as of the date 20 March 2013 (when Kaiser *et al.* (2013) was published) and have not done so at any time in the preceding 15 years, having never deliberated in terms of myself and dealing favourably with Wells and Wellington in 1991 and 2001 decisions.

This was confirmed by e-mail on 26 March 2013 (Nikolaeva 2013b), where the ICZN officer reported “we have no application from Kaiser”, which corroborates with the published history of applications received and published in *Bulletin of Zoological Nomenclature* and the ICZN’s official website as recently as 30 March 2013.

In other words, Kaiser *et al.* have lied about any official dealings with the ICZN in terms of Hoser and Wells. The fact is there have been none!

Even more alarming is the associated false claim of O’Shea and Coritz to potential supporters telling them the ICZN has formally backed their position (O’Shea 2013c, Coritz 2013)(see below).

That Kaiser *et al.* have not had any official dealings with the ICZN and that none are pending makes a mockery of other relevant sections of their rant.

At the top of their taxon “hit list” on page 10, they wrote:

“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.”,

which besides making a bold and incorrect assertions that the publications of Hoser and Wells were by “Taxonomic vandals”, infers that their boycott was temporary and that a ruling by the ICZN against Hoser and Wells was imminent, when in fact at this stage, none was even possible as there had been no submission made or even accepted for a vote!

Thus Kaiser *et al.* have fraudulently encouraged a mass breaking of the fundamental rules of 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere) on the falsified basis that the ICZN was deliberating in terms of Hoser and Wells and to hand down a judgment against both, when they knew at all materially relevant times this was not the case.

In other words Kaiser *et al.* are liars plain and simple! They have engaged in a serious act of taxonomic and nomenclatural misconduct that not only jeopardizes the

integrity of the Zoological Code, but also puts lives at risk (see below).

If such pseudoscientists will stoop to such patently false actions what does this mean for the factuality of everything they have ever written in any publication?

However, the ICZN is presently dealing with a case of gross taxonomic vandalism and the use of taxonomic publications to “defame and inflict professional harm on those working within ICZN guidelines”.

The submission they are dealing with is Case 3601, running since mid 2012, which involves the demonstrably false claim by Wallach, Wüster and Broadley (2009) that Hoser (2009e) was not published according to the Code and their illegal attempt to usurp the Code by renaming the Cobra genus *Spracklandus* Hoser, 2009, *Afronaja* Wallach, Wüster and Broadley, 2009.

Of course readers of Kaiser *et al.* 2013 are never advised of the fraud perpetrated by one the key co-authors (Wüster) and that it was the subject of ICZN deliberations against them as published in 2012 in the *Bulletin of Zoological Nomenclature* 69(3), page 160 (Nikolaeva 2012). That they were fully aware of the matter is because it is also detailed in a publication they cited, namely *Australasian Journal of Herpetology* Issue 14, (assuming they read what they cited), published way back on 30 June 2012 and indexed in *Zoological Record* shortly thereafter, although I should here note that on 7 April 2013, Schleip said he hadn’t read the Hoser papers that himself as co author of Kaiser *et al.* (2013) wanted to have boycotted (Schleip 2013).

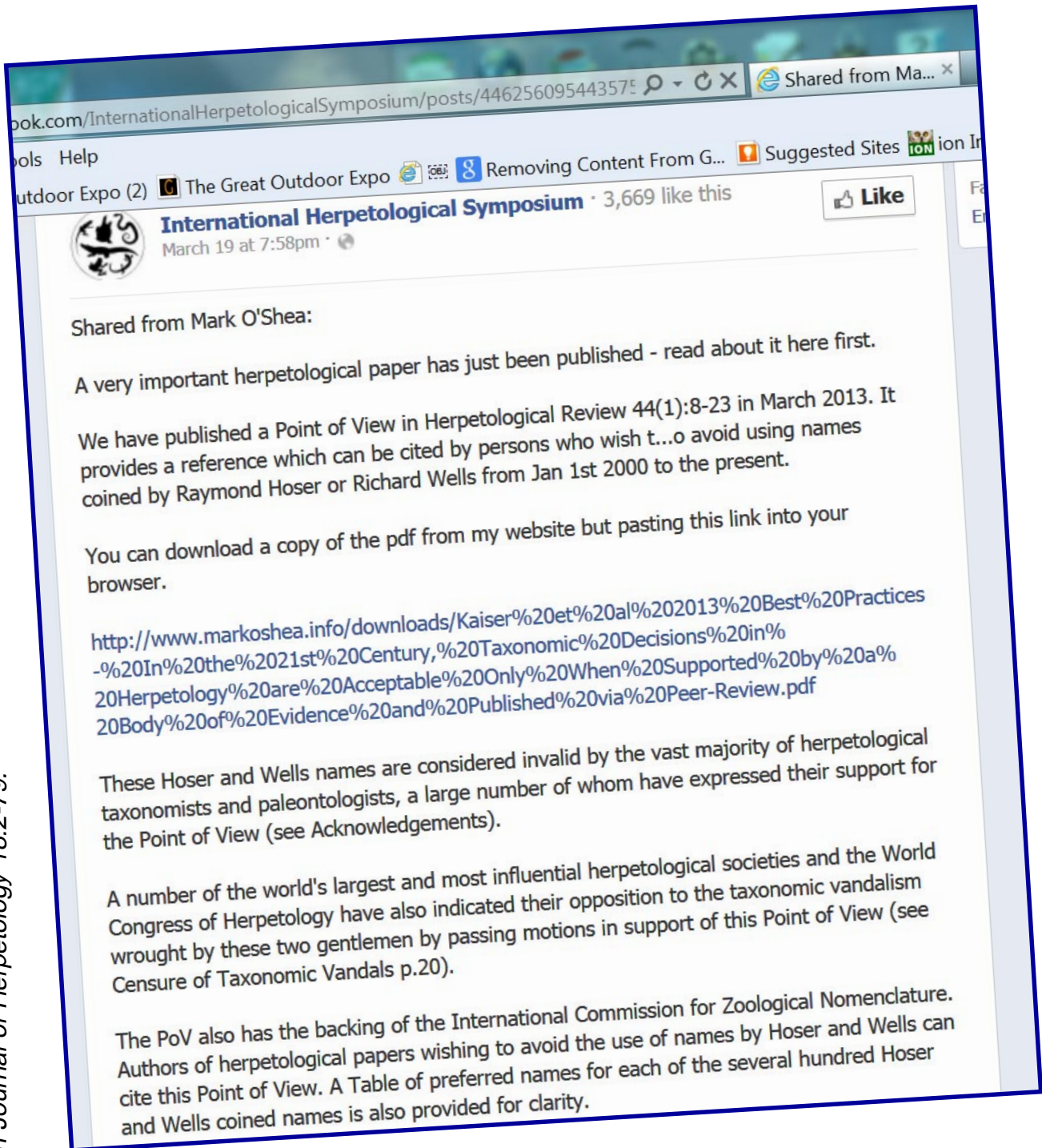
Yes these are the ones branded as “unscientific” and yet one of the alleged authors of the paper Kaiser *et al.* (2013) has been so unscientific as to not read the relevant papers and to pass judgment on them!

But returning to the subject of the ICZN, the authors of Kaiser *et al.* (2013) have instead fraudulently duped the readers of their self controlled publication *Herpetological Review* into thinking that the ICZN are deliberating against Hoser and Wells. In fact they are not. But by inferring the ICZN are, they are in effect lending weight to the illegal call on people not to use names formally proposed by Hoser and Wells on the (falsely) alleged basis that the ICZN will in a short time be ruling against Hoser and Wells (as stated explicitly on page 10 first paragraph and page 20 at a number of places in Kaiser *et al.* 2013).

More disturbing was a post on Facebook by Wüster himself on 20 March 2013, where he advocated the breaking of the ICZN’s Zoological Code, including the three critical rules of, 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere) in order to forcibly suppress usage of “Hoser names”.

Similarly disturbing was a post on Facebook by close Wüster friend, the morbidly obese Al Coritz, calling himself by the pseudonym “Viperkeeper” and who posted a link to the Kaiser ‘rant’ from his Facebook page where he wrote:

“The PoV also has the backing of the International Commission for Zoological Nomenclature. Authors of



herpetological papers wishing to avoid the use of names by Hoser and Wells can cite this Point of View. A Table of preferred names for each of the several hundred Hoser and Wells coined names is also provided for clarity.

The Point of View is an open-access paper so please feel free to circulate this pdf on websites, forums and social network sites where other herpetologists may access and benefit from it."

This in turn had been lifted from an identical Mark O'Shea post on the Facebook wall of the International Herpetological Symposium dated 19 March 2013 (O'Shea 2013c), which is copied by a screen dump print above to show beyond any doubt that the angry little Mark O'Shea is a pathological liar!

The claim that the ICZN backs the Kaiser *et al.* 'rant' was totally fabricated. They had not made any official statements or rulings on the matter!

However here I do note that the ICZN had in fact ruled against an identical scheme when executed by Sprackland, Smith and Strimple in 1997 in their formal opinion number 1970 (ICZN 2001).

The lie of ICZN support by Coritz, uncorrected by Wüster and O'Shea who both posted on the same Facebook walls with more hate posts after the above quoted posts, shows that lies are the main tool of trade of Wüster, O'Shea, Schleip and their friends.

It also gives rise to question what other lies Kaiser, O'Shea and the other listed authors spun to get their potential supporters to agree to list their names at the end of the paper.

Of course, if herpetologists are told that the ICZN already backed the Kaiser *et al.* "point of view", the inference that it had been cross-checked as accurate and endorsed by them would have been invaluable in terms of convincing otherwise skeptical scientists to lend their support to the proposal.

In a clear act of defiance of the Zoological Code built up over 200 years, the clear aim of suppression of the names proposed by Hoser and Wells was clearly the sole motivation behind the Kaiser rant.

The dishonest nature of its promotion and dissemination, embellished further with yet more hatred and lies removes any credibility that the rant may have originally had.

I note again the over-use of "social media" by Kaiser *et al.* to peddle their hate and yet simple dissemination of taxonomic papers via the internet by myself in accordance with Recommendation 8A of the Code, has been held up by Kaiser *et al.* as a contemptible act worthy of sanction and "boycotting" (see Kaiser *et al.* 2013, p. 17 top left).

Speaking of social media and the Zoological Rules, Wüster's (and Schleip's) contempt for the latter was demonstrated by his active posting of derogatory remarks on a hate Facebook page created in 2011 called "Ray Hoser, Melbourne's biggest wanker" (Various authors, 2011).

The ICZN rules are explicit and state that discussions between zoologists should be in a civilized manner, meaning Wüster had breached the Code of ethics of the Rules of Zoological Nomenclature (no. 5). Hate posts on Facebook, hate internet pages (e.g. leiopython.de) and non-stop internet trolling on chat forums including "kingsnake.com" and others sit outside this ICZN ethics guideline (number 5).

The person who said he was the creator of the page, novice Victorian snake handler Sean McCarthy and three others (Andrew De Groot, Benny Moylan and Danny Wynn) later had a court order placed on each of them prohibiting them from engaging in such conduct, which happened to be illegal under Australian Federal Law.

UNSCIENTIFIC PUBLICATIONS?

There is no formal written definition as to what constitutes a scientific publication. As a result the claim by Hinrich Kaiser *et al.* that the Hoser and Wells publications are "unscientific" cannot be definitively accepted or rejected, which is the same case for all other such publications,

including those of Hinrich Kaiser *et al.* themselves.

However, critically important here is that what makes a legally acceptable scientific description for the purposes of zoological nomenclature is spelt out clearly in the Rules at Article 8 (Ride *et al.* 1999).

Fact is in the recent past, Kaiser, Schleip and other key authors of Kaiser *et al.* have repeatedly confirmed that the Hoser publications do comply with the Code in its strictest interpretation (e.g. Wüster 2001a, Wüster *et al.* 2001, Kaiser 2012b, O'Shea 2012).

This also appears to be the case for the relevant Wells papers.

Hence, even if for example the Hoser publications also happened to contain unwanted material such as detailed etymologies, details of relevant legal matters, or even pornography (not that this third element has been alleged yet), these do not detract from the legality and utility of the relevant descriptions.

According to the Rules of Zoology (Ride *et al.* 1999), they must be used if and when they identify hitherto unnamed taxa.

I note here that the rules do not seek to impede an individual's freedom of taxonomic judgment, although I note that Kaiser *et al.*'s (2013) Table 1(p. 10) breaches this very rule!

In the introduction to the current rules (Ride *et al.* 1999) is written:

"The 4th edition of the *International Code of Zoological Nomenclature*, like the preceding editions and before them the *Règles internationales de la Nomenclature zoologique*, has one fundamental aim, which is to provide the maximum universality and continuity in the scientific names of animals compatible with the freedom of scientists to classify animals according to taxonomic judgments."

Now in terms of the relative merits of the Hoser (and Wells) descriptions, the importance lies not just in the physical words accompanying the formal and legal taxon descriptions, but also all other evidence brought to the table to qualify the evidence and reasoning for the new description.

Typical of all taxonomic descriptions, Hoser and Wells included is the completely appropriate citation of studies by other workers that provide evidence in support of the new taxonomic proposition and any that may be contrary if it exists and is relevant.

Each and every paper of both authors (Hoser and Wells), which I note have been published totally independently of one another, meticulously list all the evidence on which the taxonomic judgments are based and the nomenclatural actions that flow from them. This is appropriate and in my own case, the Hoser papers have cited references totaling thousands of documents and many thousands of pages of evidence, including countless peer reviewed journal articles, PhD theses and the like.

All this has been summarily dismissed by Kaiser *et al.* as not constituting evidence!

Importantly and in the face of all this cited evidence, Kaiser *et al.* have not produced a single shred of

evidence against Hoser or Wells to substantiate the scandalous claim of either author trying to “plagiarize others”, although below, I’ve provided undeniable proof here that O’Shea and Schleich have done exactly this in terms of my own publications!

Of immediate relevance here is that it is not illegal under the Code to provide a description without adding any new evidence at all, if it in fact relies on a reinterpretation of evidence provided elsewhere.

This situation is commonplace and reliance on pre-existing evidence accumulated by the studies of others formed the main basis of a number of my recent taxonomic descriptions.

Besides making no apologies for this, I further note that my views have been confirmed and validated by other more recent workers either revisiting the same data, or by adding further to it.

However of critical importance in all this is that by making the claim the publications of myself and Wells are “unscientific” Kaiser *et al.* are making the equally outrageous claim that the sources cited within these papers must by definition also be unscientific.

Hence the monographs of the likes of Kluge (1993), McDowell (1975), Rawlings *et al.* (2008), Pyron *et al.* (2011) and so on are also according to Kaiser *et al.* “unscientific”.

That such a patently ridiculous claim was allowed by the editors of *Herpetological Review* and their alleged peer reviewers to find its way into Kaiser *et al.* some 30 times in the single document clearly was “unscientific” in itself and again calls into question the alleged peer review process at *Herpetological Review*.

CLAIMS AGAINST RICHARD WELLS

The claims against him appear fanciful and are not appropriate in a so-called scientific journal like *Herpetological Review*.

By way of example, to claim the 361 page *Lerista* paper by Richard Wells is “unscientific” is pure fantasy!

Did any of Kaiser *et al.* read the paper?

Have any of the gang of nine ever set eyes on a *Lerista* skink?

We know the answer to the first question is an emphatic “no” as Schleich himself has admitted to this (Schleich 2013b).

As for the answer to the second question, well most if not all the authors of Kaiser *et al.* have never set foot in Australia which is where *Lerista* come from.

So it’d be fair to assume that Richard Wells, having been studying and collecting thousands of these tiny little skinks for more than 40 years would carry a little more expertise on them than the gang of nine!

To claim that an extremely detailed 361 page “book” on these skinks has no factual or scientific basis is an outrage. It is something Kaiser *et al.* and the editors at *Herpetological Review* should be condemned for by publishing the claim.

You’d think that a man who has worked on a group of lizards for over 40 years would have earned some kind of right to make taxonomic judgments in compliance with the rules of zoology.

That one paper by Wells contains a lot more science and fact than the ‘rant’ by Kaiser *et al.* in *Herpetological Review*!

I note for example an alleged (by Kaiser *et al.*) threat made by Wells to threaten “taxonomic terrorism” which is uncited and unreferenced, meaning it cannot even be checked for veracity!

Even if the two words could be lifted from a quote Wells made somewhere at some time on the web, Kaiser *et al.* fail to place it in context or allow readers to do so either, which is a breach of basic scientific reporting and publication as well as the SSAR’s ethics statement. Was Wells serious? Was he joking? or was Wells in fact referring to the actions of Kaiser himself?

Besides breaching the SSAR’s Code of ethics referred to below, the alleged threat as related by Kaiser has little weight noting that Wells is now about 60 years old and hasn’t done anything to undermine taxonomy to date, and he’s been publishing taxonomic papers for decades. Likewise for the central claims that the other taxonomic papers of Wells post 2000 lack “taxonomic merit”.

This I know to be false and can verify easily.

By way of example, the paper by Wells describing the genus *Notopseudonaja* Wells, 2002 a taxon group I am very familiar with, includes the description based on considerable morphological evidence. No one is forcing Kaiser *et al.* to adopt or agree with the ultimate conclusions of Wells, but the claim his paper lacks taxonomic merit or evidence is patently false.

Wells has given his evidence and/or properly cited it, then given his judgment on the basis of this. He has formalized his judgment according to the zoological Code. Nothing could be more straight forward!

Of note is that the more recent molecular studies published by Pyron *et al.* 2011 and others (totally independent of Wells) gave weight to the removal of the taxon within the Wells genus from *Pseudonaja*, at least at the subgenus level.

Furthermore, if Wells hadn’t named these snakes as taxonomically distinct in 2002, I would have done so in 2012!

And what is equally certain is that in the absence of a Wells or Hoser name for the group, someone else will name them!

Perhaps even Wüster, Schleich, Kaiser, O’Shea or one of the other gang of nine!

So in accordance with the ICZN rules, I am stuck recognizing the Wells name and would expect others to be similarly stuck with it and use it if they agree with his judgment!

Why should Kaiser *et al.* be allowed to overturn what is clearly a stable nomenclature made according to the rules of the Code that can be used if the underpinning taxonomy is accepted?

Now I don’t uncritically agree with all taxonomy of Wells. For example his sea snake reclassification (Wells 2007d) does not fit my view of the group and unlike Kaiser *et al.* I have both read the papers of Wells and have extensive experience with many of the relevant taxa. However there is no need for me to engage in false claims or

taxonomic misconduct to deal with my disagreement with the Wells thesis in this case. Nor do I need to censor the work of other taxonomists.

Now having made many dozens of taxonomic acts and descriptions, I sincerely doubt Wells or anyone else would necessarily agree with everything I have done, even if presented with exactly the same data I utilized. The Zoological Code actually allows for such disagreements without the need to go outside the Code. For example and as already noted the preamble reads:

“The 4th edition of the *International Code of Zoological Nomenclature*, like the preceding editions and before them the *Règles internationales de la Nomenclature zoologique*, has one fundamental aim, which is to provide the maximum universality and continuity in the scientific names of animals compatible with the freedom of scientists to classify animals according to taxonomic judgments.”

Knowing that the claims against myself either lack merit in total or are so out of context to the degree that they are best treated as lacking merit, it must by reason be assumed that the same applies in terms of adverse claims against Richard Wells.

Kaiser *et al.* have failed to substantiate them and yet managed to scrape through the alleged peer review process at *Herpetological Review*, although having an author as editor at the journal may have helped the original draft get through effectively untouched and replete with obvious defects.

Furthermore we know that Kaiser *et al.* failed to read the Wells papers they so strongly criticize.

This is because they haven't even been able to follow the intellectual exercise of cross-matching his newly created genera with those from where he derived the given species and instead resorted to the totally unscientific game of guessing, (yes guessing!) what genera the said species came from.

Plus of course, Schleich (2013b) did in April 2013 admit to not having read the papers he so roundly condemned in Kaiser *et al.* (2013).

So in summary, Kaiser *et al.* were incapable of effectively “joining the dots” in terms of the Wells papers, which is something a normal 7 year old could do!

Until now, no one in history has ever contemplated moving species from *Lampropholis* Fitzinger, 1843 to *Eulamprus* Lonnberg and Andersson 1913, but this is exactly what Kaiser *et al.* (2013) has told the world to do. ...and “without evidence”!

As I do not have extensive expertise on skinks of the genus *Lerista* (sensu stricto) I am not going to offer a view on the Wells generic subdivisions or whether or not I agree with them, other than to say that in a brief reading of his paper of 2012, I found it broadly made sense. And yes, I have caught many of the relevant taxa in the last 4 decades!

Regardless of my current views, if Well's findings are ultimately found to be correct by others, then his names should be adopted as per the rules. Again, I do not view an unreasonable hatred of him by Kaiser *et al.* as valid grounds to usurp the rules of zoological Code of

nomenclature, including the three critical rules of, 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere) in order to rename validly named taxa.

Generic splits are often controversial and those of myself and Wells are in reality no different to many others, but none require a breaching of the rules of zoology to be dealt with.

One need look no further than the 2012 split of *Anolis* by several authors including Wüster and O'Shea's close friend and listed co-author of Kaiser *et al.* (2013) Brian Crother, to get an idea of what I mean.

Should myself and Wells perhaps declare the publication of Nicholson *et al.* (including Brian Crother) (2012) “unscientific”, embargo their names and then in ten years time apply to the ICZN to break their own rules and make “Raymond Hoser's *Anolis* bust up” legal?

Working on the crackpot theories of Kaiser *et al.* we'd have grounds. After all numerous other qualified authors have made the same judgment that Nicholson *et al.* were “unscientific” in their paper, including Losos (2012), Poe (2013), Jackman (2012) and Castañeda and de Queiroz (2012).

Poe (2013) wrote:

“Nicholson *et al.* (2012) selectively adopted results of their own flawed, unstable, and conflicting analyses, selectively incorporated pertinent published data and results, and changed names for over 100 species that have never been included in a phylogenetic analysis. The proposed taxonomy is unnecessary and unwarranted according to standard taxonomic practice. It should not be adopted by the scientific or nonacademic communities.”

Now on the basis of the qualified determinations of shortcomings in the *Anolis* paper to which Brian Crother contributed, perhaps and based on the logic of Kaiser *et al.* (2013) the next issue of *Herpetological Review* could have a “hit list” of Crother taxa from the last 13 years in urgent need of “boycotting” to allow myself and others to rename!

In short, the taxonomic papers of Hoser, Wells, Crother, Wüster and Schleich should not be treated any differently to others or by contempt for the Zoological Code and Kaiser *et al.* have not presented any good reason to do so.

Personal hatreds spanning more than a decade are not grounds to try to retrospectively usurp the rules of Zoological Nomenclature by more than 12 years with an option to add to this as desired!

If Kaiser *et al.* were allowed to get away with their long-term plan to rename all taxa named by Hoser and Wells, no other taxonomist would be able to guarantee the safety of their work and names proposed from potential theft by thieves of naming rights at any future date.

Persons like Kaiser *et al.* may at any time seek to break the Zoological Code and retrospectively usurp their names because of some invented or fabricated claim against their work, perhaps as trivial as the journal itself, where it was published and who edited it. An example

here includes the patently false claim that *Australasian Journal of Herpetology* is “advertising” and in violation of 8.1.1. of the Zoological Code.

CO-DEFENDANT WELLS

In the case of the charges against Wells, Kaiser *et al.* (2013) has in effect made us both co-defendants to their patently false allegations.

I do not intend to comprehensively defend Wells here, although based on what Kaiser *et al.* have published, I find he has in effect no case to answer!

I also know Wells sufficiently well to know he is more than capable of pointing out many of the false assertions and hypocritical acts within Kaiser *et al.* himself and will probably do so in his own good time.

Wüster has long been critical of both myself and Wells and has habitually lied to further his own pre-determined agenda.

Neither Wells or myself work together and as such we run our own things.

However Wüster in particular has sought to paint a picture that we are renegades running as a duo, supporting one another without criticism of either, which is certainly not the case.

To this end he wrote in Wüster (2001a) and Wüster *et al.* (2001) of my own “uncritical acceptance of the arrangements of Wells and Wellington (1984, 1985).”

Wüster himself knew the statement to be a lie, because he had spent time in the same 2001 paper (same copy twice) criticising my 1998 *Acanthophis* taxonomy, which he would have known happened to formally reject the 1985 Wells and Wellington proposal recognizing west Australian Death Adders formerly identified as *Acanthophis pyrrhus* as the species *Acanthophis armstrongi*.

The fact is, I have never uncritically accepted anyone’s taxonomy or nomenclature and Wells and Wellington are no different!

SPECIFIC CLAIMS AGAINST THE HOSER PAPERS - WHY THE KAISER ET AL. CASE AGAINST HOSER IS FRAUDULENT AND FAILS

Kaiser *et al.* (2013) make a grand total of just five specific claims, against Hoser papers as listed and cited, which is astounding as they did without a shred of evidence condemn dozens of papers.

This lack of specific criticism implies that they were unable to find fault with the rest, beyond their five listed claims, however I note here that in April 2013, Schleip said they hadn’t in fact read them (Schleip 2013b).

In terms of the five claims held up as the basis for condemnation, which must be taken as the best they have, all are, if taken at their highest possible level and accepted as true and correct, still so trivial as to be meaningless.

However all five of the claims made on page 16 of Kaiser *et al.* are either false or quoted so seriously out of context by omission of other relevant material, that the claims against me as an attack on me may as well be treated as false.

As these five claims form the entire basis by which Kaiser *et al.* have condemned dozens of my papers, and

warranting their call to launch an unprecedented attack on the Zoological Rules, I deal with each specifically. This is to leave no doubt as to the lack of evidence these authors have for their claims against me.

I also note that the totality of these actual claims against me is just a total of 201 words, out of a heavily padded 12,638 word blog attacking me!

Kaiser *et al.* (2013) wrote:

“(1) naming of putatively allopatric populations without primary evidence, but listing the current distribution as the sole or primary distinguishing character (e.g., the diagnosis of *Oxyuranus scutellatus barringeri* Hoser 2002a:47).”

What the authors have failed to note is that this defect was identified in the first instance by myself (not them) and also properly corrected some years earlier.

As a result the same taxon was described as new by myself in Hoser (2009c) with considerably more diagnostic information to comply with the rules of Zoology, under the name of *Oxyuranus scutellatus andrewwilsoni* and for the proper purpose of stabilizing the nomenclature under the Zoological Rules.

Kaiser *et al.* have been dishonest in not noting the 2009 paper with their alleged criticism of the 2002 description and I note that as they have claimed to have read all my papers and cited Hoser (2009c) themselves, they could not have been unaware of the new description that corrected the error, four years prior to Kaiser *et al.* being published.

Also see their third specific criticism for further rebuttal of this first one!

Kaiser *et al.* (2013) wrote:

“(2) invention of evidence (e.g., body color of *Oxyuranus scutellatus adelynhoserae* Hoser 2009c, based on a holotype that is actually an isolated head: BMNH 1992.542)”

is a false statement. The evidence of body colour for the subspecies diagnosis was not taken from the holotype. Hence the invention of evidence claim is false! I chose not to unnecessarily kill live specimens of the uncommon taxon that were available, as the holotype was sufficient for naming the taxon under the Zoological Rules. It was those specimens from where further data was taken and reported, which is quite ethical and normal in herpetology and scientific descriptions in general.

Unnecessary killing of rare and endangered live vertebrates to pickle in Museums for taxonomic science has long been a bone of contention and it was not an area I wanted to delve into.

Kaiser *et al.* (2013) wrote:

“(3) repeated description of the same taxon as new (*Leiopython albertisi barkeri* Hoser 2000a = *L. a. barkerorum* Hoser 2009a = *L. a. barkerorum* Hoser 2012b; *Oxyuranus scutellatus barringeri* Hoser 2002a = *O. s. andrewwilsoni* Hoser 2009c)”

The claim is correct, but the context is not given, which in itself refutes any adverse claim or inferences against me. Interestingly they have themselves provided rebuttal evidence for their first claim against me in terms of *Oxyuranus scutellatus andrewwilsoni* Hoser 2009.

The claim was raised by Schleip (2008) that *Leiopython albertisi barkeri* Hoser 2000a was a nomen nudum. To stabilize the nomenclature under the rules of zoological nomenclature, the taxon was redescribed as new by Hoser (2009a). Again this is not uncommon in herpetology and in the circumstances, totally appropriate.

Of course this did mean that Schleip and the other truth haters were then excluded from naming that taxon and that was the real "problem" they were dealing with.

Kaiser *et al.* (2013) wrote:

"(4) descriptions of new species and subspecies based on morphological aberrations and vague differences in color pattern (e.g., *Acanthophis barnetti* Hoser 1998:24-diagnosed by the absence of raised supraoculars, which is merely an artefact of preservation [WW, pers. obs.], and "heavier dark pigmentation;" *Pseudonaja textilis cliveevatti* Hoser 2012i:38-diagnosed by stating that "each dorsal scale is darker brown tipped")"

Both claims are rejected. The diagnostic differences lifted from Hoser (1998) for *Acanthophis barnetti* Hoser are only a small part of the total as published.

Furthermore they are not based on the evidence of just "morphological aberrations and vague differences in color pattern" as asserted by Kaiser *et al.*, (2013).

The claim that "absence of raised supraoculars, which is merely an artefact of preservation" is also rejected in context. Fact is that as compared to two of three other known New Guinea *Acanthophis* species (*A. crotalusei* and *A. laevis*), the supraoculars are nowhere near as raised!

A read of Hoser (1998a) states clearly that this diagnosis is based on the inspection of a large number of specimens of each taxon as held at the Australian Museum in Sydney.

The third species, *A. rugosa*, is readily separated from the others by their considerably more rugose scales (Hoser 1998a) and other factors as listed by Hoser (1998a). None of these features are "morphological aberrations", "vague differences in color pattern" or "merely an artefact of preservation" as asserted by Kaiser *et al.*

The full diagnosis of the species *Pseudonaja textilis cliveevatti* Hoser 2012 is in fact perfectly reasonable and accurate. It is also of a nature to enable identification of wild specimens with ease by others. Therefore there is no rebuttal required.

Kaiser *et al.* (2013) wrote:

"(5) harvesting of clades from published phylogenetic studies for description as new genera or subgenera. For example, the division of *Natrix* into three monotypic genera (*Natrix*, *Jackyhosernatrix*, and *Guystebbinsus*) by Hoser (2012aa[1]) stems from the recognition of an unsupported branch in Pyron *et al.* (2011)."

As noted elsewhere, the first statement "harvesting of clades from published phylogenetic studies for description" refutes the claim by Kaiser *et al.* elsewhere that my descriptions have no evidentiary basis.

The second statement is patently false and Kaiser *et al.*

are aware of their lie.

The abstract of the Hoser paper naming the new taxa states that the evidence forming the basis of the new division comes from "several phylogenetic studies involving the Keeled Snakes of genus

Natrix and Smooth Snakes of genus *Coronella* as recognized at start 2012".

The abstract also reads: "Within the last decade, several studies have shown the divergence between the three members of the genus *Natrix* to be from 12 to 27 million years ago (Guicking *et al.* 2006), and probably further back for the three extant members of the genus *Coronella* (see comparative results in Pyron *et al.* 2011)."

As for the extensive published diagnosis of each new genus in the paper, none rely on any way on any molecular studies and results, be they Pyron's, Guicking's or anyone else's.

Those studies were merely cited as evidence in support of the formal descriptions, which is entirely appropriate and correct scientific procedure.

This particular paper also correctly cites and lists many dozens of other papers and studies by other authors as an evidentiary basis for the taxonomic conclusions within, which besides being correct scientific procedure, clearly rebuts the false claims of Kaiser *et al.* of "evidence free taxonomy" or plagiarization of other people's work.

Therefore, as none of the five specific allegations by Kaiser *et al.* 2013 against the Hoser papers have merit or are even true as stated, the case against Hoser papers, as stated by Kaiser *et al.* must fail.

WULF SCHLEIP AND WWW.LEIOPYTHON.DE

Kaiser *et al.* claim that I (Raymond Hoser) have engaged in acts of "grievous taxonomic malpractice", but have failed to provide any evidence to support the claim (see above).

Herein I provide such evidence in terms of one of the authors of that paper, namely Wulf Schleip.

What follows, effectively convicts him of the charge of "grievous taxonomic malpractice".

In 2001, a private snake hobbyist by the name of Wulf Schleip from Germany, created the website www.leiopython.de. Here he professed to disseminate information on the genus *Leiopython*, which happened to be the genus/species of snake he was keeping at the time and clearly his favourite python.

At first his site recognised both taxa (*albertisi* and *hoserae*) as different species, which was in line with accepted taxonomy of 2001, noting that Hoser (2000b) had in the case of the latter, merely formalized a long recognised species arrangement (Schleip 2001).

Schleip gave accounts of both as different species, which was appropriate for a website purporting to be an up-to-date reference for the genus.

Unfortunately (for him) he subsequently befriended Wolfgang Wüster who encouraged him to oppose all things Hoser.

Significantly in the context of this paper, from at least 2004, and after a series of posts on webforums, including "www.kingsnake.com" by Wolfgang Wüster and convicted reptile smuggler David Williams, Schleip

amended his site to deny the legitimacy of the taxon *hoserae*.

He declared it “*nomen nudem*” in numerous places. He further stated that the southern black “race” regularly climbed the central range of New Guinea to hybridise with the northern brown “race” of *L. albertisi* (Schleip 2007b).

No evidence was provided to support the wild hybridization claim.

Put simply, he joined the Wolfgang Wüster and David Williams campaign of lies and hate against “Hoser”.

By way of example, in a 2004 post to <http://www.herpbreeder.com/> Schleip also denied the existence of *L. hoserae*, going so far as to infer that he had mtDNA evidence that didn't support the Hoser 2000a designation (Schleip 2004a).

Based on the mtDNA material presented in a 2008 paper by Schleip himself published in the SSAR's *Journal of Herpetology*, that confirmed the validity of the species *L. hoserae* we now know his 2004 statement to be a lie.

While either of Schleip's “new” 2004 concepts are patently ridiculous, there was no means, or for that matter compelling reason for myself to try to change or remove the offending material denying the existence of the species I had named.

The internet is full of questionable material, and in terms of Schleip's website, it was just one of many being run by persons of questionable integrity with undisclosed (to their readers) axes to grind.

Schleip avidly posted on internet forums and elsewhere his consistently negative views of Hoser (myself), on all matters, ranging on taxonomy, venomoid (devenomized snakes), wildlife legislation, education and so on (Schleip 2004b, 2004c, 2007a).

Schleip also edited the “Wikipedia” webpage for *Leiopython* on many occasions, where he made sure that the view that there was only one species in the genus was peddled and remained so, even when others edited the site to indicate the generally prevailing (post 2000) view that there was two species in the genus (*albertisi* and *hoserae*), giving him the opportunity to edit it back to the single species view. This was at:

<http://en.wikipedia.org/wiki/Leiopython>

and the edit history is publicly available via a link on the page (Schleip *et al.* 2007b).

As late as 12 December 2008, and following editing by Schleip, that site read as follows:

“*Leiopython* is a monotypic genus created for the non-venomous python species, *L. albertisii*, found in New Guinea. No subspecies are currently recognized.”

For the record, in terms of all the Hoser descriptions of taxa, they most certainly conformed with the relevant “Rules” as published by the ICZN (Ride *et al.* 1999).

Hence the names were all “available”. However neither myself or anyone else can force anyone to use those or any other names to describe given taxa.

Furthermore, while anything is possible, it seemed unlikely to me that a forest-dwelling python would be able to climb extremely high, sometimes snow-capped hills of the New Guinea central cordillera to find other snakes to

breed with, especially as in over 100 years no one has ever found any snake that is apparently intermediate or hybrid in any way to the taxa *L. albertisi* and *L. hoserae*. Of significance to this paper is that as of late 2008, Schleip's website was still peddling the false line that the Hoser taxonomy for the genus *Leiopython* was wrong and that all could be assigned to a single species.

THE SCHLEIP 2008 PAPER ON *LEIOPYTHON*

Late in 2008, Schleip removed all material from his website.

In a download (dated 7 December) all that was written there was:

“This site is closed for major updates and will be relaunched in a couple of days!”

(cited here as Schleip 2008c).

The site was in fact reloaded and relaunched on 10 December 2008.

The significance of the relaunch was that all his material denying the existence of the taxon *L. hoserae* was removed and Schleip had suddenly and without appropriate explanation or apology declared the species as valid!

The site's relaunch was based around the simultaneous (within days prior) publication of his 2008 paper, broadly accepting the Hoser taxonomy and in turn “creating” three new species of Brown *Leiopython* from the northern New Guinea region and elevating a Hoser (2000) subspecies to full species status as well.

On 28 December 2008, he posted details of his newly published paper on *Leiopython* taxonomy on the website [www.aussiereptilekeeper](http://www.aussiereptilekeeper.com/) and elsewhere.

A search of the internet yielded abstracts of the Schleip paper only, (at: <http://www.bioone.org/perlserv/?request=get-abstract&doi=10.1670%2F06-182R5.1>) see Bioone (2008), with full copies only available through a so-called “paywall”.

The widespread availability of the abstract, in contrast to the full paper was significant in terms of the major discrepancies that emerged between the two.

I was able to acquire a photocopy of the publication through a Museum-based subscriber to the *Journal of Herpetology*.

Firstly, the abstract was quite definitive in stating its basis for diagnosing and describing new taxa of *Leiopython*, including mtDNA, which one would reasonably assume would be for those species that may otherwise have a questionable diagnosis.

However a read of the paper itself had the data revealing a different picture to that inferred in the abstract and essentially no different to that of Hoser 2000b (see below).

While the Schleip website (all pages) broadly mirrored his findings as published in Schleip 2008a (the paper in the *Journal of Herpetology*), (we'll call all pages on the server as of 28 Dec 08 (Schleip 2008b)), there were a number of notable differences.

The differences in essence were a more vitriolic attack on myself and less editorial discipline leading to his inadvertent and inconsistent statements including some

on various webpages stating that all the northern white-lipped pythons are of the same species, namely "*L. albertisi*"!

These points are only raised here to demonstrate the sloppy methodology of Schleip and how motive dictates what he writes, as opposed to the facts as they should be written.

Note for example that Schleip made at least four substantive changes (edits) to his website/s (at: www.leiopython.de) in December 2008 alone!

He was also apparently active at Wikipedia, this time apparently making anonymous edits to webpages for *Leiopython*. This time however he was changing the pages to indicate all new taxa as recognised in his 2008 paper. He wrote: "In 2008, Schleip redescribed and provided proper descriptions and diagnoses...".

That Schleip was the editor was revealed via a reverse IP address search giving the European address of his internet gateway and seeing that it matched his footprint elsewhere on the web.

In fairness to Schleip he could sustain an argument that he had suddenly as of end 2008, changed his mind about *Leiopython* and reversed his tune denying the existence of the Hoser-named taxa. This is not a hanging offence, but a proper explanation and apology would have been ethical.

Also in fairness to Schleip, the ICZN rules do call for "wide dissemination" of taxonomic work, and Schleip could legitimately claim his stalking the web to (now) promote his published paper fitted this request from the ICZN.

However, it is prudent to point out the hypocrisy here as Schleip, Wüster and Williams have put in print many times that Hoser's wide dissemination of taxonomy papers amounts to nothing more than "self promotion", (see Wüster et. al. 2001, or Williams et. al. 2006) and then as reposted and promoted on the web at "aussiereptilekeeper" by Schleip, and similar comments by Kaiser et al. (2013), p. 17, first paragraph.

However even allowing for Schleip's editorial inconsistencies, complete dishonesty and hypocrisy, the fact remains that Schleip has managed to have a taxonomic paper published.

Regardless of how badly either that or his webpage are written, whether or not his newly "created" species are actually valid ultimately turns on the evidence and it is this that is herein assessed and found to be lacking. In other words he is guilty of the charge Kaiser et al. (that includes Schleip) 2013 have made against me, but in their case without any substantiation.

THE LEIOPYTHON SPECIES

Hoser 2000a taxonomy recognised *L. albertisi* and *L. hoserae* (the latter) as described and formally named for the first time in that paper. Two subspecies, namely *L. albertisi bennetorum* from an eastern extremity of the range and *L. albertisi barkerorum* (name amended) from the northern extremity were also formally described and named at the subspecies level.

At the species level, both the latter are synonyms of *L. albertisi*.

While as recently as 2007, Schleip repeatedly claimed expertise on *Leiopython* and that *L. hoserae* and the Hoser-named subspecies did not exist (see for example his 2007 Wikipedia edits), in his paper published around December 2008 and his website (version end Dec 2008), Schleip accepted that *L. hoserae* was both a valid taxon and validly named (as in the name being available under the ICZN Code).

More dramatically, he elevated the "*bennetorum*" to full species. True to past form he alleged there was no basis to separate *barkerorum* in any way from *L. albertisi* and that it was also "*nomen nudum*" (more on this aspect later).

None of the above so far made the Schleip paper notable in any way, or for that matter worthy of comment.

However what was worthy of analysis here was the dramatic move by Schleip of creating three new species of Brown *Leiopython*, namely *L. fredparkeri* and *L. huonensis* from the mainland New Guinea population of *L. albertisi* and *L. biakensis* for the specimens from the Island of Biak.

MTDNA EVIDENCE NOT IN HIS PAPER

In his abstract published and widely disseminated on the web, Schleip indicated that he had assessed this to confirm that his division of *Leiopython* is in fact correct.

He wrote:

"Additional evidence for some species was obtained by maximum parsimony and maximum likelihood analysis of mitochondrial DNA sequences (cytochrome *b* gene) taken from GenBank. Besides three conventional taxa, two new mainland species and one new island species were recognised in accordance with the evolutionary species concept"

However a read of his paper showed he in fact provided no DNA evidence whatsoever to separate any of his newly named northern taxa from one another or for that matter from the nominate race of *L. albertisi*.

The only conclusive mtDNA evidence given by Schleip is in his Figure 4, which shows separation of *L. hoserae* Hoser 2000 from "*L. albertisi*" from Madang (summarised also in the text of the second page (second column) of his paper).

While that confirms the taxonomic position of Hoser 2000b, in contrast to Schleip's own posts on Wikipedia and elsewhere at least to mid 2007, the non-publication of similar data splitting his own "new" species seems to indicate that the evidence he acquired (if he in fact looked) went against his published argument in favour of the new "species".

Interestingly for his newly created "species" "*fredparkeri*", Schleip wrote:

"this assignment should be subject to future studies on a genetic basis".

Which was in total contrast to what was written in his widely posted abstract!

This comment also showed that he either did not conduct genetic studies on this species, or alternatively his results weren't published as they went against his clear desire to name new "taxa" and be believed by his readers.

That I know Schleip was guilty of the latter came from his

good friend Mark O'Shea who inadvertently let the secret out!

O'Shea (2007) page 125, of his book, confirmed that Schleip did have the mtDNA of all the relevant *Leiopython* specimens.

The key element of the use of genetics in determining new species is that it is essential only when the delineation of taxa may otherwise be difficult or questionable.

Most species known to science were never delineated on the basis of mtDNA due to the fact that it wasn't necessary as the differences between taxa were substantive and obvious.

In the case of *L. hoserai*, versus *L. albertisi*, the need to look at mtDNA to confirm the validity of the species designation was at best slight and in my view, totally unnecessary.

The two taxa are obviously very different, obviously allopatric, being split by a very substantive mountain range barrier and hence obviously different species (see for example O'Shea's comments on this in O'Shea 2007, page 128).

By contrast all the northern *Leiopython* are essentially similar in most respects (phenotypically alike), as conceded by Schleip (2008a), not divided by any obvious and permanent physical barriers, extremely common throughout their range and hence are the obvious targets for mtDNA analysis as inferred in the widely disseminated abstract, but not delivered on in the actual paper.

In other words the abstract as published and disseminated is misleading and dishonest.

SEPARATION OF THE THREE SCHLEIP CREATED SPECIES

Until now, all the Schleip taxa would have been recognised as stock, standard *L. albertisi* for his newly created "*biakensis*", or perhaps "*L. albertisi bennetorum*" for his "*huonensis*" or "*fredparkeri*".

Notable also is that until publication of his 2008, paper Schleip was vocal (on his website) in declaring that separated distribution was not a useful basis to identify taxa (Schleip 2004a, 2004b, 2004c).

This view was taken because it was a key plank in his printed rebuttal of the Hoser-named taxa.

In Schleip (2008a), he relied heavily on so-called "Operational Taxonomic Units" to allege what he called "geographically isolated or disjunctive populations" to separate his new "species".

This is of course based on the limited collection of specimens he allegedly had at his disposal, noting that most of the relevant parts of Island New Guinea (and nearby) is relatively uninhabited and not collected for reptiles, meaning that it'd be almost impossible to claim no *Leiopython* inhabit intervening regions, unless of course one is talking about an island population, which then makes potential "rafting" of snakes an issue and seems obvious in the base of Biak.

Hence, the end point as stated in his paper for defining these new "species" using his relatively newly invoked "evolutionary species concept"(or ESC) is that his

species populations are genetically isolated from one another by being distributionally disjunct.

While the central cordillera can give a safe bet southern New Guinea *Leiopython* have been separated from the northern population for anything up to 5 million years (mtDNA separation of about 10% as stated by Schleip 2008a), no such barrier either recently past or present is known to separate any of the northern populations, including the island race from Biak, which as recently as 12,500 years ago was virtually joined to the rest of New Guinea, (see for example figs 10 and 11 in Harvey et. al. 2000, with specific reference to Biak and it's being effectively joined to part of the Sahul Shelf).

Those authors (Harvey et. al. 2000) found that by molecular analysis the Scrub Python snakes from Biak were effectively identical to those from nearby mainland New Guinea (Fig. 6., p. 153).

Hence, it would be expected that a similar situation would exist for the White-lipped Pythons (*Leiopython*).

Furthermore, noting the findings of Harvey et. al. were published eight years earlier and known to Schleip, it would have been incumbent for him to provide contrary data for his own new taxa from the same place.

Schleip had not done this!

This raises more questions than it answers, and besides raising questions about Schleip's bad methodology, it also raises the ethical issue of whether or not he's deliberately chosen to exclude data he knew wouldn't fit his predetermined aim to "find" new species-level taxa, where none actually existed!

That of course is a breach of the SSAR's ethics statement (Anonymous 2013a), relevant here as Schleip's paper was published in the SSAR controlled *Journal of Herpetology*.

In terms of his morphological analysis, Schleip deliberately excluded a host of characters, such as temporals, parietals and postoculars on the alleged basis that there was an allegedly "random distribution between different populations".

However these scales are routinely used to split other python taxa including some from Australasia (see for example, Hoser 2000a, noting that the relevant diagnoses are in turn adopted from earlier authors and therefore not merely Hoser inventions).

However it is clear that the exclusion of characters that give no statistical standing in favour of one population versus another have been excluded by Schleip solely so as to inflate the relative importance of the obscure characteristics (based on ridiculously small sample sizes) he seeks to rely upon to separate his newly created "species".

In terms of the Schleip created species *huonensis*, it is notable that it is found immediately to the west of the distribution for "*L. bennetorum*". Schleip's diagnosis for the newly created taxon, states that it's effectively inseparable from *bennetorum* save for "the lower number of loreal and prefrontal scales as well as a lower average number of postoculars".

The question then begs, are these minor scale differences observed in pitifully small samples of snakes

sufficient grounds to split these snakes off as a separate species?

Also, what of snakes found between the stated known locations for these two "taxa", are they different again? Or are they simply intermediates, as seems likely.

Hedging his bets each way, in the regions between his newly created "taxa" Schleip has marked his distribution map (Fig 5 in the paper) with a series of strategically placed question marks (notation being "populations of unclear taxonomic status").

If one were to assume the logical Schleip species theory to it's logical conclusion, each question mark would represent a new species, giving several new taxa, and an end-point of many essentially similar species in the "*Leiopython albertisi*" species complex, when for other similarly distributed (and similarly mobile) python taxa in the same region ("*amethystina*", "*viridis*", etc), there is only one of each.

Actually, *Chondropython azureus* is a considerably less mobile taxon than *L. albertisi*, and yet only one species occupies the same range as all of Schleip's newly created *Leiopython* species, which is a result that in the absence of a good reason, simply defies logic. The same applies for the Boelen's Python *Lenhoserus boeleni* (see Austin *et al.* 2010).

Leiopython fredparkeri, according to Schleip yields scale counts intermediate between *L. albertisi*", "*L. bennettorum*" and his newly created "*L. huonensis*", which is of course totally expected as these snakes are found between the known locations for these.

Rather than providing evidence for the existence of a new species of *Leiopython*, Schleip has in fact provided further evidence of clinal variation in the range of the taxon *L. albertisi*, within the region of Northern New Guinea.

The same situation is of course seen with "*L. huonensis*" with it being essentially intermediate in form between "*L. bennettorum*" and "*L. fredparkeri*", the "species" between which it's known.

Again, Schleip has chosen to exclude snakes found in regions between these newly created "species" as they would almost certainly be clinal (again) to those he has named and further show up the fact he has in effect described "non-taxa". This is the term his friend Wüster applied in terms of none other than the properly defined and validated *Leiopython hoseae* Hoser, 2000 (Wüster 2001a, Wüster *et al.* 2001).

Hence the true picture revealed is one of clinal variation in the north New Guinea *Leiopython*, rather than any evidence of speciation, discounting of course "recent" man-made barriers, such as roads, farms, fences and the like, similar to those erected worldwide in the last 2000 years.

However my understanding is that as of 2012, most of this region is still either jungle or relatively primitive farms.

Schleip's diagnosis of his newly created "species", *L. biakensis* is the most hypocritical act in his paper, as shown here.

The use of head scalation characters to separate this

"species" from all other Brown *Leiopython*, breaks down, so he relies on cutting up his samples to give the appearance of consistent differences in his critically important "diagnosis". Yes, he even splits Irian Jaya *L. albertisi* from New Guinea ones to get his statistical gymnastics over the line in terms of diagnosing his "species".

This is of course the hallmark of his paper in that he uses, statistics with dodgy parameters and questionable statistical tests to prove his alleged consistent differences (using carefully selected parameters), based on selected samples and on the exclusion of intermediate (often clinal) specimens that may distort his end figures.

Things are made worse when he concedes that his sample size of his newly created "species" *L. biakensis*, is just two animals (yes two animals), and the best differentiating feature from *L. albertisi* from nearby Irian Jaya he has is merely "two labials entering the orbit" in his newly created *L. biakensis*.

It's must also noted here that the trait of two labials entering the orbit may not be consistent among others from Biak!

Schleip also stated:

"This allopatric population shows little, but diagnosable morphological differences to other species. Brongersma (1956) assumed this population to form an incipient race. Because of the geographic distance to the mainland populations, of *Leiopython albertisi*, it is unlikely that gene flow occurs among these populations. Hence this population is considered reproductively isolated (sensu Wiens, 2004) and in accordance with Frost and Hills (1990) and based on the ESC (sensu Frost and Kluge, 1994), the assignment of specific rank to this population seems justified."

In other words the primary basis for separating this "taxon" is distribution and a crude "assumption" without data from an author 52 years ago.

Jumping the gun is a thought that springs to mind here, but lack of data is another serious problem.

Schleip repeats the distribution argument (allopatric populations) at length in his final justification for the creation of his three new "species", using selective quotes taken out of context from papers by Frost and Hillis (1990), Frost and Kluge (1994) and Wiens (2004), giving an observer like myself the impression that Schleip hopes that no one chooses to read either the detail of the cited papers, or for that matter even the detail of the data he's presented himself.

Taking the Schleip interpretations and argument to it's logical end point, you would have almost all island populations of almost all vertebrate species potentially being elevated to new "species" under his newly warped interpretation of the ESC.

Likewise for every species found in valleys that are separated by barriers such as low hills, poor habitat, roads, farms, factories and so on, even if the habitat barriers were no more than a few hundred years old.

With many hundreds of islands offshore to New Guinea, many separated for less than 12,500 years, you can see the potential for a taxonomic nightmare emerging, not

just for herpetologists, but all biologists, in terms if the idea of naming all island populations full species ever takes hold.

However such an outcome will keep editors of publications like "Zootaxa" busy for a long, long time!

The hypocrisy part of Schleip's use of distribution as the key driver to creating his "species" "*biakensis*" is that for the previous 8 years and even in his 2008 paper, he argues strongly against the recognition of the *L. albertisi* from Mussau Island as distinct, claiming distribution is not a useful character.

That population is however that named by his enemy (Hoser), in Hoser 2000b as *L. albertisi barkeri* (correctly amended to *barkerorum*) regarded by Schleip 2008 and associates (as posted on the internet) as "*Nomen nudem*".

In Schleip's 2008 paper he wrote a diatribe claiming the taxon was a "*nomen nudem*" and also arguing:

"Allopatric distribution may itself separate the Massau Island population geographically, but it is highly questionable if this alone is able to distinguish a taxon from another, regardless of the underlying species concept."

So while allopatric distribution apparently pushes Schleip's own vague "species" *L. biakensis* over the line, it is not sufficient grounds to push a similarly isolated island population (more distant from the main population) over the line as a separate taxon, even at the lower level of subspecies.

The evolutionary species concept (ESC) was employed by zoologists to account for allopatric and other reproductively isolated populations of similar animals that were not ever likely to breed or evolve together as a species and hence would for the indefinite future evolve apart.

In terms of it's use and application in the classification of pythons, recent examples of papers and outcomes include Harvey *et al.* 2000 and others. Schleip's 2008 interpretation of the ESC is so warped and extreme, that taken to its logical end-point, you could foresee two sibling snakes separated in plastic tubs being declared separate taxa on the basis of scalation differences in traits known to be variable if the owner said "I will not put these snakes together, ever!"

I have one such example in my facility in terms of sibling Olive Pythons, both demonstrating different head shield configurations, and using the Schleip theories as practiced could both be renamed as "new" species under his warped ESC interpretation.

Not surprisingly most people agree with me and the Schleip taxa remain generally unrecognized (Natusch and Lyons, 2011). This includes in the hobbyist snake keeper business, where money can be made from the identification of a new species, race or even mutation! Now surely if Kaiser *et al.* (2013) are really serious about "grievous taxonomic malpractice" they should be looking at the paper of Schleip (2008) as their first port of call! Finally, I should note that meanwhile, Schleip's good friend Mark O'Shea (both co-authors of Kaiser *et al.* 2013), had already inadvertently shattered Schleip's

2008a diagnosis for *L. biakensis* even before it had been published.

In his 2007 book (O'Shea 2007) on page 126, there is a picture of a specimen identified clearly as "*Leiopython albertisi*" from Madang, which also happens to have two labials entering the orbit!

With "two labials entering the orbit" as the sole diagnostic feature of the alleged species *L. biakensis*, clearly being seen in bog standard *L. albertisi*, the newly created Schleip species is found to be fictitious.

I also note that in his SPAM email of 5 June 2012 (Kaiser 2012a), Kaiser wrote:

"I am acting as secretary for the purpose of neutral dissemination of the attached Point of View manuscript, put together by an international group of seven respected herpetological taxonomists."

Besides the fact that Kaiser had lied about being neutral (he was in fact listed in Kaiser *et al.* 2013 as the lead author), he also lied by alleging Schleip could possibly be one of "seven respected herpetological taxonomists".

As seen above, Schleip's first and only significant foray into the world of taxonomic publishing in 2008 was marked by the following verified results:

- A case of fraud,
- Reference to data he chose not to produce as it went against his published conclusions of new species,
- A direct breach himself of the SSAR's own ethics statement,
- Description of three new alleged *Leiopython* species that were fictitious or "non-taxa",
- Published in a journal controlled by the SSAR that in spite of alleged peer review, somehow evaded proper scrutiny and as published violated the SSAR's own ethics statement,
- Unnecessarily wasted the time of taxonomists for many generations to come by the creation of three new junior synonyms for a species of snake that already had other available junior synonyms.
- Caused a waste of government resources towards the conservation of "non-taxa" better spent on conserving genuinely threatened species.
- Was an unmitigated disaster that created taxonomic instability for a genus of snakes who's relationships had been properly stabilized according to the science of taxonomy and rules of zoological nomenclature by Hoser 2000a.

In terms of dishonesty, the same can be said for the statement by Kaiser (2012a) with regards to another co-author, namely Mark O'Shea!

SCHLEIP, O'SHEA AND WÜSTER YET MORE LIES, REVERSING ARGUMENTS.

The trio are veritable experts when it comes to telling lies and arguing rubbish to further their argument in internet chat forums and elsewhere, where the readers may not know any better or not have access to criticized papers. In 2009, the three were trolling the web attacking the Hoser papers, of that year, including the paper Hoser (2009a), which exposed Schleip's fraudulent "creation" of three new *Leiopython* species.

The trio posted on a forum at:

<http://pleiotropy.fieldofscience.com>

/2009/03/taxonomic-war.html#comment-form

where they wrote their usual unsupported claims about Hoser papers being “without evidence”, “evidence free” (Schleip), “evidence free taxonomy” (Wüster), and “taxonomic vandalism” (O’Shea) with the argument that the Zoological Code needed rewriting, leading to the response from a person posting as “Siglind” who wrote: **“Guys, chill out, he can say what he wants, in the end, taxonomy works by making PROPOSALS, not demands. So, the next author looks at it, makes up her/his own mind, and decide that those names are synonyms for already describe species. And that will be the end. Same for taxonomy. He is a annoyance, but not one that requires rewriting of The Code.”**

Of relevance here is that at the time Schleip, O’Shea and Wüster were aware of the fact that all Hoser papers were published according to the Zoological Code and valid.

In fact Schleip himself wrote:

“everybody can write a taxonomic paper, rename or invent taxa, and if the author gets it published in accordance with the basic requirements of the code, the names are available.”

and

“So, as you can see, there is no tool within the “Code” to stop Raymond Hoser and others from publishing evidence-free and offensive papers in self-published journals.”

Of course, this was a case of Pot, Kettle, Black, with the author of a holotype evidence free paper just months earlier making the false accusation against me, but that fact is not relevant here.

After the publication of Kaiser *et al.* (2013), co-authors Schleip, O’Shea and Wüster posted links to their new “paper” paper widely and reversed their argument. They now argued widely that the Zoological Code actually allowed them to boycott and rename all Hoser species. The basis of his new claim being the Kaiser *et al.* argument that no Hoser publications were ““public and permanent *scientific* record” and therefore fails a requirement of the *Code* (Art. 8.1.1).

Schleip repeated this on German snake forum:

<http://www.pure-reptiles.de/>

[index.php?page=Thread&threadID=1290](http://www.pure-reptiles.de/index.php?page=Thread&threadID=1290)

on 19 March 2013.

What Kaiser *et al.* (2013) failed to do was quote the relevant parts of the Code which actually in large part define articles 8.1.1.

So here it is:

“8.1.1. it must be issued for the purpose of providing a public and permanent scientific record,

8.1.2. it must be obtainable, when first issued, free of charge or by purchase, and

8.1.3. it must have been produced in an edition containing simultaneously obtainable copies by a method that assures numerous identical and durable copies.”

and

“Recommendation 8A. Wide dissemination. Authors have a responsibility to ensure that new scientific names, nomenclatural acts, and information likely to affect nomenclature are made widely known. This responsibility is most easily discharged by publication in appropriate scientific journals or well-known monographic series and by ensuring that new names proposed by them are entered into the *Zoological Record*. This is most easily achieved by sending a copy of the work to the *Zoological Record*, published by BIOSIS U.K.

Recommendation 8B. Desirability of works on paper. Authors and publishers are strongly urged to ensure that a new scientific name or nomenclatural act is first published in a work printed on paper.”

Now while *Australasian Journal of Herpetology* had a direct statement of fact that it was produced for the “purpose of providing a public and permanent scientific record”, this is generally assumed of other journals so is in reality a non-argument. Then of course is the other issue of the other five or more journals that Kaiser *et al.* have ruled “unscientific publications” recognized by everyone else as being scientific publications for up to 12 previous years and include papers by countless other authors besides myself, including none other than Wüster himself (Wüster *et al.* 2001)!

Again while Schleip, O’Shea and Wüster were claiming in 2009 and earlier (e.g. Wüster *et al.* 2011) that Hoser had not described valid species or “non-taxa”, the fact was, they knew I had, and the real problem was the fact they had to use the Hoser names, something O’Shea and Wüster had both made clear was something they would avoid at all costs and even if the taxa named by myself (Hoser) were in fact valid (O’Shea 2013a, 2013b, 2013c, Wüster 2013a, 2013b).

In other words there was never actually a problem with the science, taxonomy or nomenclature of the Hoser descriptions, but rather a problem, O’Shea, Wüster, Schleip and the others had with “Hoser”.

FUTHER RELEVANT STATEMENTS BY KAISER AND OTHERS

I don’t intend repeating the detail of false claims by O’Shea and others in the period preceding publication of *Australasian Journal of Herpetology* Issue 14, which contained details of Kaiser *et al.*’s “paper” as circulated at the time and other claims made against me. The details of this scandalous campaign and various false statements made by the relevant parties are covered at length there (Hoser 2012as). These should be read by all interested parties in detail.

Of note again is that not once did any of the authors of Kaiser *et al.* 2013 ever argue the merits or otherwise of the taxa named by Hoser in 2012!

That is because all were defined as a result of good robust molecular and morphological data, all of which was cited within the papers!

As I didn’t do all the research myself, I did according to proper convention cite all relevant sources.

There is nothing out of the ordinary in any of this!

Kaiser *et al.* and others instead resorted to publishing of

hate images in violation of the Zoological Code on Facebook and elsewhere and making further false statements about myself.

In June 2012, and after receipt of the Kaiser SPAM e-mail of 5 June 2012 (Kaiser 2012a), I emailed the editor of *Herpetological Review*, Robert Hansen and asked him questions in relation to the "paper" by Kaiser *et al.*

In a reply email dated 19 June (Hansen 2012), he said he wouldn't be publishing the material, as "We do not publish personal attacks".

Now falsely calling a person guilty of "grievous taxonomic malpractice" (Kaiser *et al.* 2013) and without a shred of evidence must surely be some kind of personal attack!

On 7 March 2013, I was advised of the impending publication in *Herpetological Review* of the Kaiser *et al.* (2013) rant and I sent another e-mail to Hansen, who rebutted his earlier email and said he was in fact publishing the "paper" that month (Hansen 2013).

It is of note that the paper by Kaiser *et al.* (2013) breaches the SSAR's own ethics statement as downloaded from <http://www.ssarherps.org/pages/ethics.php> (Anonymous 2013a) on 20 March 2013, which in part reads:

"2. Veracity. Members will not commit scientific fraud (e.g., through fabricating or falsifying data, suppress results, or deliberately misrepresent findings). All statements made regarding methods used and data collected will be factually correct. All interpretations made in the Introduction and Discussion will be truthful representations of the author's understanding. Relevant literature and data not compatible with the conclusions must not be intentionally omitted."

Now we know that Kaiser's paper breached the rules for several reasons.

In 2012, Australian reptile taxonomist Glenn Shea was approached by Kaiser to support his hate campaign. Shea later recalled, "Despite several requests by Kaiser last year that I provide a letter of support for his manuscript, I repeatedly advised him that was not interested in supporting it."

This was not reported by Kaiser *et al.* in Kaiser *et al.* (2013) in direct violation of the SSAR statement of ethics above.

In terms of the 2013 final draft of Kaiser *et al.*, which Shea described as being "similar to the one which was circulating last year" Shea told Kaiser it was "ridiculous and unworkable" and "gobbledegook" (Shea 2013a).

Shea happens to be Australia's best known reptile taxonomist who even went to Facebook to proclaim to the world that the Kaiser *et al.* rant was totally "nonsensical" (Shea 2013b).

Of course it is uncertain as to how many other dissenting herpetologists have had their views suppressed by Kaiser *et al.* in breach of the SSAR's own published ethics Code.

He has only listed those whom he claims support him to present a false picture of unanimous support within the herpetological community for his position,

Clearly Kaiser *et al.* are not going to tell us who else

declined to get involved in their crackpot plan to usurp the rules of zoological nomenclature. We do know however that Hal Cogger, another key taxonomist in Australia didn't want to touch the Kaiser *et al.* plan with a bargepole even though he was harassed by Kaiser to join the Hoser hate campaign. Cogger received email directly from Kaiser, which he chose to ignore and indirectly passed on to myself via a third party.

Then of course there is the list of names who are identified as supporting the proposal. While numbering some dozens of individuals, it is in fact only a fraction of the global herpetological community and a lousy end number considering the gang of seven, then (add two who didn't contribute) nine co-authors had spent a whole year harassing herpetologists around the world to join their campaign.

I also note that this support base was also gathered with the assistance of O'Shea's widely repeated lie to potential supporters that the ICZN had already backed the "Point of view" (O'Shea 2013c, Coritz 2013) even before it had been published and the false statement by Kaiser (2012a) that the piece had been written by "seven respected herpetological taxonomists."!

The number pales into insignificance when compared to the tens of thousands of votes, listed supporter and convicted wildlife smuggler, David Williams got for himself, with the extreme support of Wüster, O'Shea and snake fancier Al Coritz in his failed attempt to get the most votes in the now infamous Accor Hotels "unsung hero" competition. Those thousands of votes of support were for Williams to win himself a free holiday valued at \$20,000 (see Hoser 2012a for details).

In that case O'Shea, Wüster, Coritz and Williams himself fraudulently marketed himself as some kind of "unsung hero", spreading lies about good work he hadn't performed, alleging he had been saving people's lives in the jungles of New Guinea, whereas he was in fact at the time dealing in antivenom for his own benefit and illegally trading in the same commodity. He had also recently been convicted of a number of smuggling and animal cruelty charges. While Williams gathered many votes through misleading appeals on internet chat forums from reptile hobbyists, Williams and the others couldn't match other people in the same contest and so had to resort to generating vast numbers of votes themselves.

As the contest drew to a close Williams sent a message out, also reposted by his helpers, including Wüster at: <http://www.reptileforums.co.uk>, which read:

"I am especially grateful to my friends Shane Hunter from ARK in Australia, Mark (O'Shea) and Wolfgang (Wüster) in the UK, Al Coritz and Chris Harper in the USA, and Wulf Schlep (sic) from Europe, who promoted this contest fiercely, spending many long hours at the keyboard or on the phone to mates stirring up interest."

Williams was then disqualified from the contest as he had been a party to the generation of 4,000 "votes" for himself from the same IP address. He also provided untrue and misleading statements to the contest organisers at the time he entered the contest.

However, even in terms of their alleged supporters Kaiser

et al. (2013) gained by making false claims of ICZN support and the “seven respected herpetological taxonomists” who allegedly wrote the piece, they have gone further and falsified data in the paper.

On page 20 they report, support of “Australian Society of Herpetologists (per unanimous Annual General Meeting vote)”. Yet according to Glenn Shea who attended the meeting, the vote was far from unanimous as he himself abstained! (Shea 2013a, 2013b, ASH 2013).

It also turned out that according to a member of the Australian Society of Herpetologists, Scott Eipper, the vote itself was sold to the membership as a condemnation of “taxonomic vandalism” that had already been formally endorsed by the ICZN! And putting it that way, who wouldn’t vote in favour of the proposition!

Not one person was shown a single copy of any of the Hoser or Wells papers attacked by Kaiser *et al.* (2013) to verify the claims within. Furthermore, no documentary evidence to support the claim of ICZN support was produced and that is because we know from the ICZN that there was in fact none (Nikolaeva 2013b).

However the real failure of Kaiser *et al.* to get support for their attack on the rules of zoology or to condemn the papers of Hoser and Wells was seen in the final motion actually passed by the Australian Society of Herpetologists (ASH 2013). It was written as follows:

“Whereas the Australian Society of Herpetologists recognises the importance of peer-review when proposing taxonomic decisions and the nomenclatural acts that follow from them,

acknowledges that works published outside of the peer-review process are damaging the integrity of herpetological science,

does not condone the naming of taxa for reasons that are unscientific or not based on a trail of evidence, and

applauds the efforts by the International Commission on Zoological Nomenclature to curb the misuse of nomenclature,

and a motion was proposed by Mark Hutchinson that “ therefore, be it resolved that the Australian Society of Herpetologists joins the other listed societies in endorsing the point of view presented by Kaiser *et al.* in the March 2013 issue of Herpetological Review, namely that in the 21st Century, taxonomic decisions in herpetology and their nomenclatural consequences are acceptable only when supported by a body of evidence published within the peer-review process”.

The Australian Society of Herpetologists, including prominent members Hal Cogger, John Scanlon and Glenn Shea did not want to be a part of breaking the Zoological Rules or condemning papers of Hoser that they accepted and used taxonomy and nomenclature from, and so deliberately restricted their endorsement of Kaiser *et al.* to the preamble which set out the above points and in the absence of obviously false claims about Hoser or Wells (Shea 2013a, Scanlon 2013, Cogger 2012).

And as I mentioned earlier, what scientist would not

endorse the concept of taxonomy being supported by a body of evidence within the peer review process.

That is of course exactly how it works at the present time!

Without evidence, a taxonomic proposal fails!

Now in terms of this, no one needed Kaiser *et al.* to state the obvious!

In fact a read of the minutes of the 2013 ASH meeting shows how the meeting was quite disgusted at the idea of having a resolution attacking Hoser papers and so stopped proceedings to allow time to make a new resolution in support of the general and obvious principals of evidence based taxonomy, and subject to peer review.

Notable is that AHS members also wanted to explicitly support the rules of the Zoological Code, but this was removed from the final resolution as that was against the second part of the Kaiser *et al.* blog and they were trying to keep the peace.

So on the basis of the preceding we know that Kaiser *et al.* have made a false statement about the Australian Society of Herpetologists in terms of the alleged unqualified support.

Thus there is a strong probability that the somewhat limited list of names given as supporters (obtained through the making of false claims) and alleged herpetological societies support, may also be compromised in one or more ways.

DID THEY READ THE PAPERS?

We know from the taxon “hit list” that Kaiser *et al.* failed to read the papers of Hoser and Wells properly to be able to re-assign taxa to their original genera and so made some significant taxonomic blunders.

If that alone doesn’t create taxonomic and nomenclatural confusion, then nothing will!

That there have been no substantive criticisms of the factual basis of the Hoser papers in any way, implies that they haven’t been read in detail by Kaiser *et al.* or any other potential critics either.

However readership of the papers by myself (the main target of Kaiser *et al.* 2013) is important in terms of myself and the alleged support for the Kaiser *et al.* claim. In the absence of having read each and every paper and the sources cited within, one must conclude that support for the Kaiser *et al.* position of 2013 is fundamentally flawed and untenable.

For the record, the combined word count of the relevant Hoser papers as cited by Kaiser *et al.* is nearly a million words, or the equivalent of ten full sized books by word count. The cited references and source information forming the basis of the taxonomic conclusions and acts (totalling about 500, when resurrections of old names are included) would in turn exceed several million words.

Who if any of the alleged supporters of Kaiser *et al.* (2013) would have read this material before lending support to the Kaiser *et al.* claims that all Hoser’s papers were unscientific?

It should be noted that none of the alleged supporters have explicitly stated that they have read all the Hoser papers subject to the taxon “hit list” on page 10 of Kaiser *et al.* (2013) or the supporting reference papers.

Noting the previously documented practices of Kaiser, Wüster and Williams of improperly inflating their author and support listings, (as detailed below), no weight should be placed on the alleged claims of support for the vague proposals of Kaiser and the general notion of taking action against "Taxonomic vandalism" as alleged by Kaiser *et al.* (2013).

In other words Kaiser *et al.* (2013) do not have a mandate from the herpetological community to engage in a mass boycott of scientific names, and their renaming of valid taxa in violation of the Zoological Code including the three critical rules of, 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere).

FURTHER EVIDENCE THE ALLEGED SUPPORTERS OF KAISER ET. AL. 2013 DID NOT READ THE HOSER PAPERS!

While few if any people would genuinely believe that all the alleged supporters had read all the relevant Hoser papers as listed in Kaiser *et al.* 2013, or the supporting references, the claims made within Kaiser *et al.* are not supported by the publications of the alleged supporters themselves.

By way of example Ruud de Lang and L. Lee Grismer are listed by Kaiser *et al.* (2013) as being in total support of their claims that the Hoser papers totally lack merit and that the Hoser names should never be used, (and that the rules of Zoology should be broken in doing so).

In fairness to the pair, they probably bought the lie from O'Shea of ICZN backing for their document and were understandably misled (as were perhaps each and every other listed supporter).

However I do for completeness sake mention that Ruud de Lang and L. Lee Grismer in fact both comply with the relevant rules of Zoology and use one or more Hoser names from Hoser papers listed by Kaiser *et al.* (2013) as being without evidence in their own publications (de Lang 2011, Grismer 2011).

In his book, *The Snakes of the lesser Sunda Islands (Nusa Tenggara), Indonesia*, de Lang uses the name *Broghammerus* Hoser, 2004 (pp. 175-188), citing Hoser, without dispute of any form. The same use applies for Grismer in his book *Field Guide to the Amphibians and Reptiles of the Seribuat Archipelago* (pages 167-168).

These facts don't match the claims of Kaiser *et al.* (2013).

MORE FABRICATIONS BY WÜSTER AND COMPANY

Falsification of co-authors, fabrication of data, conclusions and the like is not something new to the Wüster camp. In 1999, Wüster lent his support to a paper posted on the web by his friend, the convicted wildlife smuggler David John Williams, alleging that the elapid species described by myself as "*Pailsus pailsei*" in 1998 (Hoser 1998b) was "synonymous with *Pseudechis australis*".

The online paper was altered three times over the following 18 months or so, to accommodate the ever changing views of Williams, listed Brian Starkey as a co-author, clearly to add weight to the ridiculous claims and

conclusion in the paper (Williams and Starkey, 1999a, 1999b, 1999c).

That was that *Pailsus pailsei* Hoser, 1998 was synonymous with "*Pseudechis australis*".

I might add also that molecular data published by Kuch *et al.* in about 2005 confirmed my original taxonomy.

For completeness sake, I mention that the second two alterations to the 1999 Williams and Starkey paper was to support a false claim that I had somehow stolen his naming rights for the New Guinea species *Pailsus rossignollii* Hoser 2000 (Hoser 2000b), a claim Wüster himself formally withdrew on 28 January 2001 as did Williams at about the same time (Wüster 2001, Williams 2001).

In 2008, Starkey stated he had nothing whatsoever to do with the Williams and Starkey paper of 1999 in any of its three incarnations and that he totally disagreed with the central claims and conclusion within.

Starkey (2008) wrote:

"I had absolutely nothing to do with time alteration and the reposting on web. In fact I was in two minds about the whole paper, without even seeing a specimen of pailus. I didn't want to pass judgment until I had got out there and looked for myself. I did four trips asap to the area and found a couple of specimens 40-50 km from Cloncurry. I knew as soon as I saw my first

DOR, that you were right! When I showed David a few pic's and close ups he knew too! Then I got a live specimen amongst a small group of rocks, so fast I nearly lost it. I have probably seen about 3 live and 4-5 DOR specimens in 9 or more trips. I wish we didn't jump the gun.

But David wrote the paper and added my name. I never actually wrote a word, although he may have quoted things I said during phone conversations. And that's the truth."

In this instance, David Williams listed as a supporter of the Kaiser *et al.* (2013) rant did quite a bit more than just breach the SSAR's own Code of ethics by adding his friend as a listed author, when he had nothing to do with the paper.

In fact he went even further than that!

In both his revised versions of the online paper Williams and Starkey (1999b, 1999c), he cited a paper as being in press by various authors including himself.

Williams (2001) stated that it was to be a published description of a new species of elapid from New Guinea, similar to *Pailsus rossignollii* Hoser, 2000, for which Hoser "fortunately does not have access to specimens".

Williams and Starkey 1999b had the following citation referred to in the text of the paper and at the rear of the paper added to the original text to support the backdated claim of working on this allegedly new species of snake.

The citation read:

"O'SHEA MT, WILLIAMS DJ, WÜSTER W, BIGLALE IH, and STARKEY BA (1998) A new species of highly venomous elapid snake of the genus *Pseudechis* from southern coastal Papua New Guinea - taxonomy, conservation status and medical implications. Unpublished (in preparation)."

Fact was, no such snake existed and no such paper was written, in press or ever planned.

We know this because as of 2013 nothing has been published!

In that case Williams literally invented a paper and invented four alleged co-authors!

The significance of all this is that consistently, Wüster and those associated with him have engaged in all sorts of unethical tactics to claim support for their position in argument, whether by adding listed authors to publications, or by claiming support that is either non-existent or based on false representations.

Or for example the listing posted in 2001 (Uetz, 2001) at: [http://zeta.embl-heidelberg.de:8000/srs5bin/cgi-bin/wgetz?-e+\[REPTILIA-species:'Pailsus_SP_pailsei'](http://zeta.embl-heidelberg.de:8000/srs5bin/cgi-bin/wgetz?-e+[REPTILIA-species:'Pailsus_SP_pailsei')

which is quoted below and read:

"Species:

Pailsus pailsei

Synonyms:

Pailsus pailsei HOSER 1998

Subspecies:

Family:

Serpentes: Elapidae snakes

Distribution:

Australia (N-Queensland)

Comment:

Questionable genus and species. Probably synonymous to *Pseudechis australis* (W. Wüster, pers. comm.) "

Which was a statement to his friend Peter Uetz that Wüster himself knew at all materially relevant times was a lie. This is confirmed by cross-referencing with his own data as published in Wüster *et al.* 2005 (see the abstract, figure A1 and page 12 for examples).

In 2001 and by which stage it was apparent to all that the Williams (in reality without Starkey) claims against my species *pailsei* were false, Wüster did again breach the zoological rules by improperly trying to destabilize the nomenclature again.

In a hate piece full of yet more lies, that he first published on Kingsnake.com on his own on January 22, 2001 at 11:29:07 to be exact (Wüster 2001a), then later shopped to friends to be co-authors and reprinted in *Litteratura Serpentina* in June that year (Wüster *et al.* 2001) he falsely claimed that the species *pailsei* from Queensland, (eastern) Australia, was one and the same as another species, *weigeli*, from Western Australia described by Wells and Wellington in 1987.

Wüster wrote:

"the two names represent the same species", going on to say "*pailsei* Hoser, 1998 would become a synonym of *weigeli* Wells and Wellington, 1987." and then going on a long-winded diatribe alleging I had recklessly destabilized the taxonomy and nomenclature of Australian snakes by describing "non-taxa" and synonyms.

As already noted, at all materially relevant times, Wüster

knew he was lying. His own later paper (Wüster *et al.* 2005), confirmed that all the taxonomic and nomenclatural judgments of myself relating to the species *pailsei* (as published in 1998 and later) and *rossignollii* (published in 2000 and later) had been correct at the times of publication and that both were species distinct from both *australis* and *weigeli*!

Later authors have correctly recognized both taxa, being well defined, geographically, morphologically and via molecular data published by several authors, the most recent author recognizing both species (and the other two) being Eipper (2012).

The paper Wüster *et al.* (2001a) is relevant for several other reasons as well.

As already mentioned, effectively the same Wüster piece published in *Litteratura Serpentina* in June 2001 was first published on Kingsnake.com on 22 January the same year by Wüster on his own as his own writing and then widely circulated elsewhere. It was rapidly discredited by numerous correspondents (e.g. Frome (2001a, 2001b), Brammell (2001a, 2001b, 2001c, 2001d)).

Notwithstanding this, Wüster then "shopped" the same piece among friends and people who owed him favors in order to get some other "names" as "co-authors" to give his wild allegations and claims some added credibility, before it was re-sent to the editor of *Litteratura Serpentina* on 5 May 2001 (four months later) (see van Aken 2001a, 2001b).

This editor (Gijs van Aken), fell for the ruse, and so Wüster's already discredited lies got a second running both in print in a journal and of course online as pdf.

For the record, I then sent the editor of *Litteratura Serpentina* a response systematically rebutting all Wüster's lies and allegations and of the same length as Wüster's original piece.

But the editor, who had in fact invited my response, went back on his word and e-mailed me. He said he wasn't going to print the response. He had been unlawfully pressured by Wüster in the sort of information censorship he's become famous for.

An altered version of that response has since been published in "hard copy" in the latter part of Hoser (2001).

As already noted, when the Wüster *et al.* paper in *Litteratura Serpentina* appeared in print, there were four more alleged authors tacked onto the paper, even though clearly none had been involved in writing it, as the draft was unchanged from what Wüster had previously published on kingsnake.com and identified as being written by himself alone.

Besides breaching the rules of ethics of the SSAR and pretty much everyone else by adding authors to his own papers, Wüster has as far back as 2001 been seeking to present a veneer that the stand of himself and his band of academic thieves against everything Hoser has a far wider support base than is the reality.

This situation has been mirrored in Kaiser *et al.*'s (that includes Wüster) claim of wide support for their reckless attempt to attack the rules of Zoological nomenclature and even in the face of their false claims in print made to gain this support.

These are but some of countless examples of the dishonesty of Wüster in actively creating unnecessary confusion in the taxonomy and nomenclature of reptiles, peddling information he knows to be false, all driven solely by his obsessive dislike for this author (Hoser) manifested in a desire to suppress names for taxa properly described by Hoser and so he can practice intellectual theft at some time later.

WÜSTER'S 15 YEAR HISTORY OF TRYING TO GET OTHERS TO BOYCOTT HOSER NAMES

Space precludes me from listing many of the hundreds of other cases involving fraud and dishonesty by Wüster in particular, usually committed as part of his long unethical campaign to try to stop other people using Hoser names for taxa.

However the paper, "Exposing a fraud! *Afronaja* Wallach, Wüster and Broadley 2009, is a junior synonym of *Spracklandus* Hoser 2009! *Australasian Journal of Herpetology* Issue 9, on 3 April 2012 is a 64 catalogue of yet more of his gross misconduct by Wüster complete with copies of relevant supporting documents.

That paper was published primarily to rebut false claims of Wallach, Wüster and Broadley 2009 to the effect that a paper describing a new genus of Spitting Cobras (namely *Spracklandus* Hoser, 2009) (Hoser 2009e) was not validly published under the Zoological Code (Ride *et al.* 1999).

The paper went further to put Wüster's most recent claims into context by detailing earlier and relevant misconduct.

Included in the paper are examples of the following:

- Through publication of library receipts and the like, Wüster is shown to have knowingly made false claims in terms of *Australasian Journal of Herpetology*, issue 7, notably a false claim that no hard copies were published and that it was only an online journal (pages 3-10 and elsewhere),
- On 29 March 2009 Wüster lied on "venom list" internet chat forums about the content of *Australasian Journal of Herpetology* issue 7 to allege that I (Hoser) had split the recognized genus *Naja* into 20 new genera in order to cause undue alarm to other herpetologists.
- Wüster's friend Bryan Grieg Fry (another man associated with the illegal reptile trade in Australia), on 29 March 2009, did in the wake of publication of *Australasian Journal of Herpetology* issue 7 reverse his previous support for the taxonomic position espoused within the journal by publishing a hate post on the internet opposing the Hoser taxonomy, which contradicted his earlier online posts including one as recently as 6 Feb 2009.
- Wüster co-published a paper in 2009 unlawfully renaming the Hoser genus *Spracklandus*, *Afronaja*, on the basis of the claim he knew to be false, that being that *Australasian Journal of Herpetology* was not published according to the Zoological Code.
- In publishing their junior synonym, Wüster and friends had deliberately chosen to overlook places where hard copies of *AJH* had been sent to, including the most obvious "*Zoological Record*" as specified in

Recommendation 8A of the Zoological Rules (Ride *et al.* 1999) or the Australian Legal Deposit Libraries, for which Hoser was able to produce and publish hard copy receipts.

- Wüster and co-authors, Wallach and Broadley, deliberately misquoted and misrepresented the Zoological Code in their paper, in a manner not unlike that done in Kaiser *et al.* (2013).

- Wüster and co-authors breached peer review and ethics protocols at *Zootaxa* by submitting a second paper with a valuable species description at the same time as his new Cobra description to assist in getting the fraudulent paper published as part of the combined package.

- After being alerted to the falsity of his claims in terms of the alleged non-publication of *Australasian Journal of Herpetology* issue 7, Wüster and close associates, including Schleip and O'Shea continued to knowingly post the same lie on the internet and even in later papers over the following 2 year period, including getting friend and most recent coauthor Brian Crother to do the same in another of his evidence-free taxonomic works (Crother 2012).

- Besides breaking the Rules of Zoological nomenclature including the three critical rules of, 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere), Wüster's co-written Cobra paper of 2009 breached several other sections of the Code (Ride *et al.* 1999).

- Lies peddled by Wüster and associates in the context of attacking Hoser have caused a number of snakebite deaths (two listed) when bite victims have chosen on the basis of their widely disseminated advice, not to seek treatment on the erroneous guess they had received a dry bite.

FURTHER WÜSTER LIES

A read of Kaiser *et al.* (2013) sees Wüster claiming that none of the Hoser taxonomic publications had any scientific merit whatsoever. As a result, he asserted all had to be boycotted (Kaiser *et al.* 2013).

This of course is his last ditch attempt at stopping what after fifteen years of calling for non-use of the Hoser names, was clearly a failed strategy.

As other impartial scientists had revisited Hoser papers from 1998 onwards, all the newly described taxa were being validated one by one and so the names were coming into general useage.

That Wüster himself knew of the validity of the said taxa was true at least as far back as 2001, when he authored Wüster (2001a) later shopped to co-authors and republished as Wüster *et al.* (2001).

In that paper, his attack on Hoser "non-taxa" even included a phylogeny of the Australasian pythons (fig 3) that was clearly nothing more than a total fabrication and with no semblance of the reality.

Produced by Wüster to discredit the Hoser taxonomy published a year later, Wüster's phylogeny was shown as fraudulent when matched with that of Rawlings *et al.* (2008) (Fig. 1).

Notably the published phylogeny of Rawlings *et al.* (2008) (Fig. 1), effectively validated all the genera of pythons named by Hoser in the preceding years as well as those of Wells and Wellington from the 1980's.

However, Wüster has published widely that if and when, Hoser taxa are recognized by other later authors, including for example Aplin and Donnellan (1999) or Rawlings *et al.* (2008), it is a result of good luck on the part of Hoser rather than any scientific skill on my (Hoser's) part (Wüster 2001, 2013b, Kaiser *et al.* 2013). In Wüster 2001, 2013b and Kaiser *et al.* 2013 Wüster cites the case of the Pilbara Death Adder, *Acanthophis wellsei* Hoser, 1998, being validated as correct by Aplin and Donnellan (1999).

The significance here is that Wüster has as far back as the beginning of 2001 been aware of the scientific reality that the Pilbara Death Adder is a species in its own right.

Notwithstanding this fact, Wüster has continued to use all his influence to improperly stop useage of the valid Hoser name for the species.

The best example of this remains on the Peter Uetz edited website "The Reptile Database", where for this species it is as of March 2013 written:

"Synonymy: Not listed by COGGER 2000 ... (WÜSTER, pers. comm. 15 Dec 2010)."

So more than ten years after Wüster himself was clearly aware of the validity of the species, he was dishonestly misrepresenting the species *Acanthophis wellsei* as being a "non-taxa" not worthy of recognition by others. The same sort of totally reckless, misleading and deceptive taxonomic and nomenclatural conduct by Wüster can be seen in Uetz's other pages dealing with Hoser taxa.

These also list comments by Wüster claiming they are non-taxa or similar and that the Hoser names are not to be used.

Examples current as of 30 March 2013, include for *Pseudechis (Pailsus) pailsei*, *Tropidechis sadlieri* and *Broghammerus reticulatus* (Uetz 2013f, 2013g, 2013h, 2013i).

These examples and the content of Kaiser *et al.* (2013) show that Wüster has no interest in the science of taxonomy or merits of classification by anyone. Rather his actions are fuelled by a hatred of myself (Hoser) and a desire to use all available means, no matter how dishonest, to stop people from using "Hoser names" for any taxa.

That this is the case was stated by Wüster himself (Wüster 2013b) on the private facebook wall of Wulf Schleip in March and April 2013, when discussing with Schleip their strategy against Hoser following publication of Kaiser *et al.* (2013).

In his wall posts O'Shea stressed the "need" for himself, Wüster, Schleip and others of their choice to be able to rename all taxa named by Hoser and then they "must" use all means possible, regardless of the ethics involved to achieve this aim (Schleip *et al.* 2013a, Wüster 2013b), a point reaffirmed on the same Facebook wall by Wüster and Schleip.

In his obvious state of excitement immediately following the publication of Kaiser *et al.*, on 20 March 2013, the meglamaniacal Mark O'Shea gloated on his Facebook wall, "Oh the excitement of it all!" before speaking at length of a grand plan whereby O'Shea himself and his 8 other co-authors would from now on be the gatekeepers for all new herpetological scientific names and taxon descriptions and that unapproved (by himself) people would from this time on be confined to publishing only "natural history papers" (O'Shea 2013b).

The trio were making their comments in the context of being unaware that I'd be able to access their posts via a disgusted herpetologist listed as a Facebook "friend" of the trio.

Notable also is that here and elsewhere Schleip conceded that their battle was difficult and unlikely to be won, because he noted "the (Hoser) names are available." under the Zoological Rules (Schleip 2009, Schleip *et al.* 2013a).

Co-author Brian Crother has also copped considerable flak over previous attempts to become gate-keeper for deciding who's scientific names go into general usage and his own earlier evidence-free taxonomic decisions as detailed at length by Pauly *et al.* (2009).

In terms of his earlier attempts to censor the taxonomy of others in 2008, Pauly *et al.* chastised Crother severely in a paper he published running over 8,000 words.

Crother had used his editorial influence at the SSAR back then to produce an "official" list of common and scientific names for North American Reptiles and Amphibians (Crother *et al.* 2008).

That is, Crother had illegally set himself up as some kind of "official" and governing authority, to tell people what scientific names they "must" use.

In line with the Wüster position, in a 2012 reprint (Crother *et al.* 2012), he advocated non-use of the Rattlesnake genus names proposed by myself in 2009, citing the discredited "paper" Wallach, Wüster and Broadley (2009), even though Crother admitted at the same time that phylogenetic studies had in effect further validated the Hoser names!

Countering Crother's dangerous position, Pauly *et al.* (2009) said:

"We argue that no scientific society should suggest or imply that they are regulating scientific names because such regulation is counter to the spirit of the International Code of Zoological Nomenclature (ICZN, 1999). Indeed, the first principle of the Code (1999:XIX) states, "The Code refrains from infringing upon taxonomic judgment, which must not be made subject to regulation or restraint."

When one combines the various publications of Crother (2008 and 2012) as well as those he has co-authored, Nicholson *et al.* (2012), Kaiser *et al.* (2013) and others it is clear that he is a serial offender in terms of grievous taxonomic misconduct, mass-naming of valid taxa and who seeks to rename as many species and genera as possible for the express purpose of personal self-gratification.

Worse still is that Crother is content to knowingly and

deliberately act outside of the rules of Zoological Nomenclature to achieve this!

HINRICH KAISER'S OWN CREDENTIALS

Also there is the other relevant issue of note and that being that Hinrich Kaiser himself, a University Professor of Zoology at what may be politely described as a non-mainstream university, which actively promotes and teaches "creationism" and "intelligent design" (Berni 2010, Diaz 2010, Pamplin 2013). Kaiser is also unable to grasp the simple difference between taxonomy and nomenclature as outlined by myself in the preamble to this paper.

Shea made this point in his email of 2013. In a return email after the fact, Kaiser claimed I had got it wrong and that he did in fact know the difference and that I shouldn't believe Glenn Shea.

However proof that both himself and his co-author Mark O'Shea don't know the difference is seen in the paper O'Shea *et al.* (2012) which is co-authored by both O'Shea and Kaiser, and posted on Kaiser's own website. In that paper they wrote:

"Rawlings *et al.* (2008) determined that *reticulatus* and *timoriensis* were sufficiently phylogenetically distinct from other species in the genus *Python* to warrant separate generic recognition. However, we believe that the generic name assigned to these two species by Rawlings *et al.* (2008) is taxonomically unavailable and therefore follow the more conservative decision by Zug *et al.* (2011) to retain the genus name *Python*."

Actually, and based on the first part of their statement, if they have an objection, it is to the **nomenclature**, not the taxonomy!

The correct nomenclature based on Rawlings 2008 is of course "*Broghammerus reticulatus*".

By the way Zug *et al.*'s 2001 paper dealt primarily with the different species *Python curtus* Schlegel, 1872 and its closest relatives (within describing a new species) meaning the reference to it by Kaiser and O'Shea was also deliberately misleading their readers.

CREATING A BATTLEGROUND BY MISREPRESENTATION AND YET MORE LIES

Widespread publication of Kaiser *et al.* (2013) was in the first instance by posts on the internet on 19 and mainly 20 March 2013, notably on the Facebook pages of Bryan G. Fry, Wolfgang Wüster, Mark O'Shea, Wulf Schleich and others, which is significant as these authors have condemned myself in their rant at page 17 for doing just that ... making posts on the internet.

Their paper was even promoted on twitter.com by serial spammer Darren Naish (Naish 2013b), a close friend of O'Shea (Naish 2013a) and was placed on a link from "Wikipedia" on a Wüster edited "Raymond Hoser" hate page on 21 March 2013 (Anonymous 2013b), a site Wüster regularly has posted lies and hate for many years. Wüster has gone so far as to ensure that the Wikipedia site has a robot installed so that his false and defamatory edits cannot be undone.

This includes a false and invented claim on the site that I have killed four people and seriously injured many others

and been convicted in a court of law for the same.

The various publications of the Kaiser *et al.* 2013 "paper" online were replete with false and defamatory claims made on Facebook as part of the marketing of the paper, including for example, that by Wüster on 20 March 2013 where he wrote: "**Herpetology finally fights back against taxonomic vandalism:**" the inference being he represents all the best interests of herpetology, which is a joke and indicative of his own highly inflated self view and worse is the fact he has deluded himself to the point that he can't see it is he that is the real taxonomic vandal!

His own conduct detailed above shows emphatically that over a fifteen year period he has been a "taxonomic vandal" of the highest degree!

By way of example, the re-assigning species formerly in the genus *Eulamprus* Lonnberg and Andersson, 1913 to *Lampropholis* Fitzinger, 1843, as done by Kaiser *et al.* (2013) without a shred of evidence must rank as one of the biggest acts of herpetological taxonomic vandalism of all time!

Noting his own recent case of scientific fraud involving the spitting Cobras (see Hoser 2012a), Wüster clearly is not a man who can possibly claim to represent the better interests of herpetology or science.

Or the post by Mark O'Shea on 20 March 2013 (O'Shea 2013), who had been trolling my own Facebook page when he wrote about his new 2013 paper co-authored with Kaiser and added:

"this was on Hoser's site on 2 March "Just got an e-mail today from a colleague in the USA who pointed out that more than one in seven genera of snakes in North America was named by myself. This shows that the age of discovery for biodiversity and taxonomy is far from over!" All that from his armchair without ever setting foot in a museum or examining a specimen!"

Of course the final statement is a total lie, easily shown as such by viewing even my first taxonomic paper of 1998 describing new species, which was complete with photos taken by myself of museum holotypes that had been dead and pickled and available to anyone for years!

The animals were shown complete with tags from the Australian Museum in Sydney, just in case the likes of Mark O'Shea are unaware of the significance of these Type corpses.

The significance of these depicted holotypes in Hoser (1998a), or for that matter holotypes depicted in Hoser (1998b and later papers, including many of those cited in the post 2000 period, like Hoser 2001) is that based on his Facebook comment of 2013 (made from his PC somewhere in the UK), made after publication of Kaiser *et al.* (2013), of which he was listed as coauthor, O'Shea had condemned my papers without even having read them (and seeing the pictures of holotype specimens in museums in the papers), even though Kaiser *et al.* 2013 said they had read my papers!

See also Schleich's (2013) statement of fact that he too hadn't read the Hoser papers criticised as "unscientific" in Kaiser *et al.* (2013).

Of course the only other alternative is that he was

knowingly lying on his Facebook page.

Either way, in this case alone O'Shea has lied at least once!

One of many examples of O'Shea's non-stop lies was in a post he put on his friend Wulf Schleip's Facebook wall on 2 August 2012, where he told a global audience of up to a billion people (who could access the page) that I had been jailed for forgery!

In fact I have no such criminal history!

Or another of the many outrageous lies you can find on O'Shea's Facebook wall was the comment also made on 20 March 2013, which read:

"he's done this to a lot of North American species, for example, without ever leaving Australia to study these animals in the wild (in fact, I don't know if he's ever even seen living specimens)."

That O'Shea knew that statement to be a lie is seen in the fact that a journal he himself had despotic editorial control of, namely *Herptile*, published a big paper about a trip I made to North America way back in 1993 (Hoser 1994), complete with photos of various locations!

Details of earlier lies by O'Shea, cross referenced to establish the fact are published in earlier papers including Hoser (2012a) and not repeated here.

In 2012, O'Shea published a rant on Facebook giving his sole claim against the Hoser papers being that I had named too many species on the alleged basis I had deprived future generations of herpetologists the pleasure of naming species.

Fact is that there is no legal limit and at age 51 and having spent a life working on reptiles, my total of named taxa is nothing out of the ordinary and well below that of many other herpetologists.

I also note it is consensus elsewhere that it is critically important for the conservation of wildlife that the biodiversity be formally described sooner rather than later (Newer 2013).

Bacher (2012) even wrote: "We still need to describe species at a much faster rate than we currently do; we still need more taxonomists."

I have detailed fraudulent actions by O'Shea elsewhere, but of note in terms of this latest attempt to usurp the Zoological Code and create nomenclatural instability are his past actions with regards to ethics and his flagrant contempt for the concepts of intellectual property (IP).

In 2004, his close friend David John Williams, a listed co-sponsor of Kaiser *et al.* (2013) and another man Bruce George, worked with Western Australian film producer Ed Punched to create a TV series they illegally called "Snakebuster" in an unlawful direct infringement and use of my registered trademarks "Snakebusters" and "Snakebuster".

After these men scammed three million dollars in government handouts and fees from the broadcasters through use of my registered trademark and as the federal court later found, by even masquerading as myself, I sued them and got judgment against them. After the first judgment, the studio of the production company at Freemantle burnt down in questionable circumstances and at the time the main proceedings

commenced, they effectively pled bankruptcy, claiming just \$300 in the bank.

At the conclusion of the case I was paid compensation to the tune of \$30,000, which was ostensibly all the defendants had available and this money came from the TV broadcasters, not Bruce George and the other reptile handlers.

At the time these proceedings were afoot, Mark O'Shea sought to undermine my case by marketing himself and Williams as "Snakebusters" in another deliberate breach of my IP, even going so far as to print a "Snakebusters" logo on several covers of the journal "*Herptile*", over which he was in despotic editorial control (see covers of issues 32(3) and 33(1), from 2007 and 2008 (various authors 2007, 2008).

This was to create a false veneer of widespread use of the term "snakebuster" or "snakebusters" to support no less than three separate sets of legal proceedings commenced against me by the production company to try to strike out my registered trademarks.

Ultimately the film company lost all cases, but not after I spent an inordinate amount of time and money defending the actions, including two long trips from Melbourne to Sydney and another from Melbourne to Canberra (Anonymous 2013c, 2013d) to attend legal proceedings in courts.

Fortunately *Herptile* is not widely distributed in Australia, so the actual damage to my business reputation was minimal, especially when compared with the online efforts of O'Shea and the others. But the actions by O'Shea clearly displayed his lack of morals and respect for simple IP and his attempts to create a false view of reality, in that case to pretend the term Snakebusters was used by many people in Australasia as a trading name, when in fact at the time, my business was the only one.

Therefore it comes as no surprise he would seek to be involved in a plot to retrospectively usurp names that have been in use for more than 12 years in breach of the Zoological Rules and to remorselessly try to induce others to similarly break the rules by renaming validly named taxa.

It also comes as no surprise that based on prior conduct, O'Shea, Wüster and the other authors of Kaiser *et al.* will say and do virtually anything to present a picture of widespread support for breaking the rules of the Zoological Code, when this is not in fact the case.

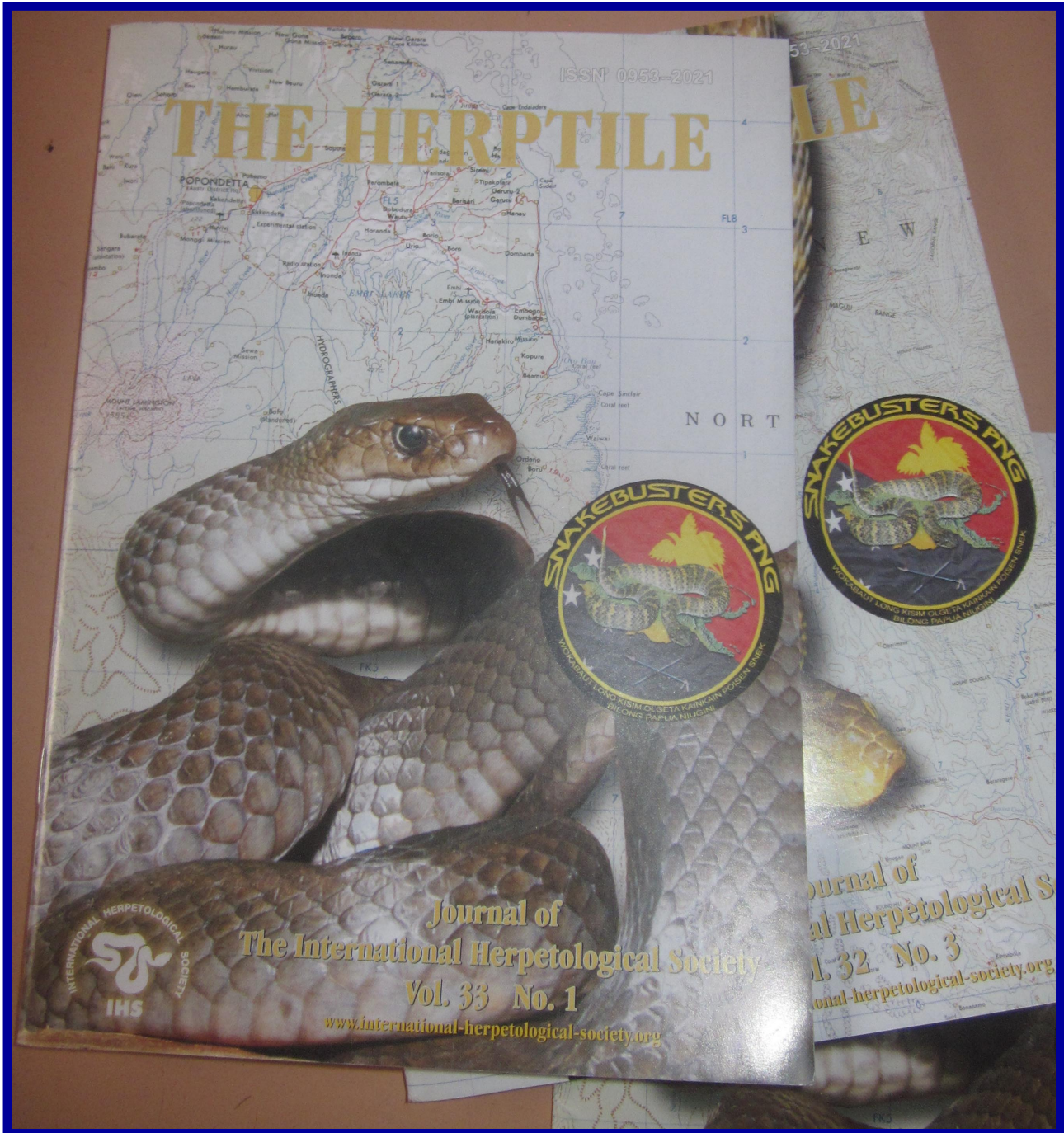
NOMENCLATRURAL CHANGES AND PUTTING LIVES AT RISK!

The claim was alleged against myself by Kaiser *et al.* (2013) and more explicitly by Mark O'Shea's allies on his Facebook wall and elsewhere.

Kaiser *et al.* cite state:

"this may literally be a matter of life and death, when name changes spread via media outlets by attention-seeking authors may cause uncertainty among medical personnel as to which antivenom is appropriate in cases where the name of the source snake species has changed (Sutherland 1999)".

The problem with this statement attributed to Sutherland (1999) is that he never made any such statement against



Hoser 2013 - Australasian Journal of Herpetology 18:2-79.

This is what happens when despotic editorial control gets into the wrong hands!

In this case from 2007/2008 serial offender and truth-hater Mark O'Shea simultaneously plagiarises Hoser papers and bootlegs Hoser registered trademarks.

The illegal trademark usage was in order to fabricate "evidence" against Hoser for legal proceedings afoot at the time.

The legal proceedings were a series of attempts to deregister the long-registered Snakebusters trademarks in which O'Shea tried to falsely claim common-usage for the trademarked term.

The actions against Hoser ultimately failed and all trademarks remain registered.

The plagiarization was the theft of data on the subspecies *Pseudonaja textilis pughii* Hoser, 2003 from a paper by Hoser (2003) in a paper by O'Shea in these 2007/8 issues of *Herptile* where O'Shea masqueraded the Hoser evidence as his own and without any attribution of the Hoser 2003 paper as the source.

You'll even note the subspecies of snake named by Hoser in 2003 and a newly made-up David Williams and Mark O'Shea "Snakebusters" logo put on a single magazine cover by O'Shea.

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my taxonomy. In fact he agreed with it!

The fact is that the paper merely asserts that the name King Brown Snake (Genus *Pseudechis* Wagler, 1830) and potential synonyms) is less desirable as a name than the Mulga Snake for these snakes as it is not in fact related to the Brown Snake and requires a different anti-venom. Second fact is that both names, King Brown Snake and Mulga Snake have been in use long before I was born!

Once again, Kaiser *et al.* have cited a reference to support a claim against myself, when in fact there is no such claim or basis for it!

While talking about nomenclatural changes and lives being at risk, if the argument were to be accepted, then the question must be asked: Why is Wüster seeking to rename snakes already renamed by Hoser?

Recall in 2009, the genus *Spracklandus* was created by Hoser (2009e) to accommodate the spitting cobras. If changing the genus from *Naja* (from which the spitting cobras were generically excised) was such a potential medical disaster, why did Wüster first condemn the move on 29 March 2009 (Wüster 2009a, 2009b) on taxonomic grounds only to be then (contradicted in February 2009 by his good friend Bryan Fry (Fry 2009)), a decision made by himself without knowledge of the impending paper (Hoser 2009e)) and then subsequently decide to fraudulently rename the same snake genus himself in September the same year (Wallach, Wüster and Broadley 2009).

Recall that was when as co-author Wüster and Wallach and Broadley sought to allege that *AJH* Issue 7 wasn't a print publication, therefore not legal under the Zoological Rules and thus enable them to rename the same *Spracklandus* taxon group themselves as *Afronaja* (Wallach, Wüster and Broadley, 2009). Of course nothing creates taxonomic and nomenclatural instability and confusion more quickly than the making of unnecessary junior synonyms for already named taxa.

And then nothing further cements taxonomic and nomenclatural instability and confusion better than the continued illegal promotion of a junior synonym by a person who knows the fact but deliberately ignores it.

This is exactly what Wüster has done since 2009 (Wüster and Bernils, 2011).

MARK O'SHEA'S CULPABILITY FOR DEATHS ALREADY!

Of course crocodile tears coming from the likes of Mark O'Shea falsely alleging Hoser or Wells have put lives at risk, is particularly irksome considering that firstly there is not a shred of evidence to support such an assertion.

Secondly O'Shea himself has most certainly been culpable for one or more deaths already. One of these being that of expert UK-based herpetologist Luke Yeomans!

The circumstances surrounding this totally avoidable death on 29 June 2011 are covered in the etymology section of Hoser (2012a).

Without repeating all the detail, I note the historical record of O'Shea's intense attacks against myself in terms of my world first surgical de-venomizing of the

world's deadliest snakes via internal excision (Hoser 2004b, 2004c), known widely as venomoid snakes.

I note the procedure not only removed risk to handlers of the said snakes and the public, but also removed ongoing cruelty endured by venomous snakes through day-to-day handling with sticks, tongs and other metal implements.

O'Shea published in the journal he then controlled as his personal mouthpiece, *The Herptile*, a blistering attack (O'Shea 2004), replete with statements he knew to be lies for the basis of furthering his obsessive hatred against myself. Lies included claims of regeneration of venom in venomoids, venomoids being unable to digest their food, false cruelty claims and similar nonsense.

This he reposted widely and it remains on the internet at: http://www.markoshea.info/blog_venomoid1.php as of 25 March 2013 and other sites he has placed it on.

He later had both myself and Yeomans, members of the *International Herpetological Society* (IHS) of over 20 years standing expelled from that society in 2008 because we both possessed venomoids (Yeomans 2010a).

O'Shea was able to get his way in terms of the expulsions because of his control of a meeting venue the society needed at the time (Yeomans 2010a) when no others were available.

O'Shea, sponsored an online petition in 2004 calling for criminal sanctions to be taken against me for having venomoid snakes (Hunter 2004), the petition running in several forms over more than 8 years, which was ultimately successful in that the procedure was formally banned in Victoria in about 2007. O'Shea was a party to the widespread placement of banners across other internet forums, first calling for a ban on the "surgical enhancement" of snakes, and then when this failed to gain traction, changing the banner to use the words "surgical mutilation".

The scandal here was that neither O'Shea or the others supporting the petition could tell the difference between venomoid and non-venomoid snakes unless told which was which by the owner (Hoser 2004b)!

Yeomans had a venomoid Cobra which ultimately died and he wanted to de-venomize all his venomous snakes in the lead up to his opening of a King Cobra sanctuary in the UK.

This was after he had a near fatal bite from a Mamba and his close friend the herpetologist Wesley Dickinson was bitten by a Cobra and also died.

However other factors made Yeomans make the decision not to get venomoid snakes in the period leading to his fatal bite. The key one was the conduct and threats of Mark O'Shea and his close friend Wolfgang Wüster, two very powerful individuals within the reptile fraternity of the UK.

Of relevance was that in the early 1990's after he was prosecuted for the allegedly heinous crime of feeding live food to a reptile, to wit a Zebra Finch (*Taeniopygia guttata*) to a Wagler's Viper (*Tropidolaemus wagleri*).

For this mortal sin, he was dragged through Britain's criminal courts, prosecuted, convicted and fined. Then he was held up for public hatred in Britain's notorious tabloid

media.

The legal precedent now sits as a threat and if need be, a means to criminally charge any other reptile keeper who dares use live food for any reptiles, including such humble items as mealworms or crickets and then upsets anyone in a government authority.

Yeomans said he was originally “dobbed in” by another reptile person, namely Mark O’Shea, whom he said was “obsessive” and had an axe to grind against him in that O’Shea regarded Yeomans as “upstaging” him (Yeomans2010a, 2010b, 2010c, 2010d, 2010e).

The relevant authority in the Zebra Finch matter, the RSPCA in the UK, ran the prosecution.

In 2010 he corresponded with me in relation to the issue of safety for himself in his own reptile shows that he intended doing at a “King Cobra Sanctuary” he was planning to open in the UK in mid 2011 (Yeomans 2010a, 2010b, 2010c, 2010d, 2010e).

In this, I specifically mean the use of venomoid snakes as described by Hoser (2004).

Yeomans had seen how in the previous 6 years myself and ten staff had done over 10,000 venomous snake shows with the world’s five deadliest snakes and without any fatal or even near fatal snakebites.

He had seen videos of myself taking bites from the snakes to prove they were safe and was well aware of the benefits of the venomoid snakes, not just for the safety aspect, but also the welfare of the snakes.

As already mentioned Yeomans himself had previously owned a venomoid cobra!

Yeomans toyed with the idea of making all his large King Cobras venomoid because he feared that sooner or later he’d make a handling error and get bitten and end up the same way as his good friend Wesley Dickinson. However he decided against doing so and the reason for this is important.

He had no issues with the surgery and the false claims of cruelty to the snakes. In fact in terms of the venomoid snakes, there was no sensible reason for him not to get them except for one.

That reason was the expected attacks he would get from Mark O’Shea, a man he described as his sworn enemy, and Wolfgang Wüster, both within the reptile fraternity and both of the UK and both of whom had been key sponsors of an anti-Hoser and anti-venomoid petition website, run by a convicted wildlife smuggler, David John Williams and his close friend Shane Hunter in Australia.

Yeomans was in extreme fear that should O’Shea or Wüster become aware of him having venomoid snakes, that they would attack and undermine his reptile display business and worse still have him targeted by the RSPCA again.

With one “animal cruelty” conviction already, Yeomans decided the likelihood of attacks by O’Shea and Wüster and another more serious conviction would terminally disable his business.

Therefore he decided instead to take the risk of keeping his snakes that he handled for shows “hot”.

I could devote several pages to the adverse comments made by Yeomans about O’Shea, Wüster and their

unethical behaviour, but these are not particularly relevant beyond what has already been told in terms of how the actions of O’Shea and Wüster made Yeomans choose not to protect himself with venomoid Cobras.

On 29 June 2011, Yeomans made the snake handling error that cost him his life.

Just days before his “King Cobra Sanctuary” was due to open, one of his “hot” snakes bit him and he died.

At just 47 years of age, a herpetologist in the prime of his career was killed.

If Luke Yeomans had not been forced by these other so-called “herpetologists” to put his life at unnecessary risk with snakes that could easily have been de-venomized, he would still be breeding rare and endangered reptiles and educating people at his new “King Cobra Sanctuary”.

Much has been made in recent years of the threats to private individuals and their rights to be allowed to keep and study reptiles. The alleged threat is often identified as coming from outside the herpetological community. The usual bogeyman identified are militant animal rights groups and the like.

They are not the real enemy.

These people lack expertise in reptiles and do not carry any political or legal power in terms of reptiles and the law. Put simply, no one takes them seriously.

By contrast the real enemy is within the reptile community. The reckless conduct of O’Shea and Wüster, both masquerading as reptile experts were in effect directly responsible for the premature death of Yeomans. The death of a “private” individual is then used as a catalyst to call for ever increasing restrictions on individuals to keep reptiles as pets, do scientific research on them and the like.

Of course the events just described are all on the public record and if Wüster or O’Shea were to flag risks of snakebite deaths arising from the actions of a herpetologist, they should as a matter of course have disclosed their own sordid past!

Again they have breached the SSAR’s ethics statement in their paper Kaiser *et al.* (2013).

In the period 2006-8 and at the behest of O’Shea and associates, the Victorian Department of Sustainability and Environment (in Australia) outlawed the surgical procedure to create venomoids, thereby stopping other wildlife demonstrators besides myself getting venomoids, even though most did in fact want them for the obvious safety reasons.

As a result, in the time period since 2006 there has been at least two deaths and several near misses (requiring emergency hospitalization every time and usually a lot of expensive anti-venom at the same time) involving government licenced snake handlers who handled venomous snakes at their displays instead of venomoids. The handlers to die were Ron Siggins and Malcolm Biggs both killed in 2007. Near misses, include Glen Clapton and Mike Taylor (twice) at Healesville Sanctuary, 3 keepers at Australia Zoo, Simon Watharow and Paul Fisher of company “Snakehandler”, Jonno Lucas (twice) of “Educational Reptile Displays”, Mark Dorse, Peter Mirtschin (several times), Bob Withey “Of Aussie Wildlife

Displays", John Deutscher (numerous times), Jon Birkett (Melbourne Zoo), Jason Hall, Stuart Parker of Ballarat Reptile Park (twice), Tony Johnson, Neville Burns, Tom Parkin, Tony Harrison, Kurt Ison, and others (see Hoser 2012au).

The death and destruction caused by O'Shea's successful anti-venomoid campaign has in fact spread to innocent members of the public. In 2009 a member of the public was bitten in an accident at a reptile display run by Peter Carter at Bathurst, NSW.

The victim, a man bitten by an adult Tiger Snake (*Notechis scutatus*), was carted to hospital and comatose for days. He is now permanently disabled.

Carter was charged by the NSW Agriculture Department for allowing the venomous snake to bite the member of the public. He was finally acquitted of the charge after he said that had sought to obtain venomoids to remove the risk of bites in 2007 and was prohibited from doing so by the department on advice from the DSE of Victoria. They in turn had instituted O'Shea's sought after ban on venomoid snakes.

DRY BITE LIES AND MORE DEATHS

Then there's yet another scandal involving snake handlers and the lie of dry bites.

To refute false claims that venomoids regenerate venom as repeated on the petition site sponsored by O'Shea, Wüster, Williams and others, I first in 2006 produced several videos of myself taking bites from venomoid Taipans (*Oxyuranus scutellatus*, *Parademansia microlepidota*), Tiger Snakes (*Notechis scutatus*), Death Adders (*Acanthophis* spp.) and the like, made in front of a public audience, showing no ill effects from what would normally be extremely deadly species.

The videos were removed from Youtube on protest from O'Shea and others, for fear that their lie about venomoids being dangerous because of alleged venom regeneration may be shattered.

The videos were then uploaded onto "smuggled.com" and remain there as of 2013.

They also newly asserted in 2006 that my own survival and that of my staff from venomoid snake bites was as a result of lucky so-called "dry bites" as in bites whereby a snake does not inject venom.

Contrary to the belief of some uniformed people, dry bites from venomous snakes are in the real world extremely rare.

By way of example, none have ever been reported for Taipans and yet a series of bites from several venomoid Taipans of myself not requiring any treatment was written off by our detractors as a lucky escape from dry bites.

O'Shea and others then peddled the "dry bites" lie, often by reposting the same sort of statements by others, to assert that the Hoser venomoids were dangerous when they were not, including by regularly posting internet links to the Wikipedia hate page "Raymond Hoser" which among other things has also falsely claimed the Hoser venomoids are dangerous and that survival of myself from bites has only been because of lucky dry bites (Various authors 2013).

After my eldest daughter volunteered to be filmed taking

bites from the same snakes in front of a public audience to shatter the claims of public risk from venomoids and the dry bite claims, O'Shea and his allies resorted to false claims of child abuse, even though nothing of the sort occurred.

In 2008, O'Shea and Wüster, via their obsessive creation and editing of Wikipedia hate pages, linking to them and the like, actively reposted an erroneous series of claims by the DSE alleging dry bites involving Hoser venomoids (see various authors 2008-2013).

The false claim by O'Shea, Wüster and others that dry bites were common, has had a measurable and negative effect on keepers likely to be influenced by them through their obsessive online presence.

As a result there is now a widespread and erroneous belief among herpetologists globally that dry bites are common with venomous snakes and that if bitten by a pet venomous snake, they too may be able to survive the bite like Ray Hoser and his kids who according to Wüster and O'Shea, luckily had dry bites.

One victim of this misinformation, in this case allegedly from O'Shea's close friend, the morbidly obese Al Coritz, was snake keeper Aleta Stacey, who on 14 June 2011 was bitten by her pet Black Mamba (*Dendroaspis polylepis*), and following advice attributed to Coritz about dry bites, did not do immediate first aid. As a result she died from the bite (see Various authors 2011b).

In the wake of her death has been a raft of adverse legislation passed in the United States further restricting the rights of reptile keepers.

Recently there have been at least two snake bite deaths in Australia involving persons who have taken a punt that they had a "dry bite".

Bradley Hicks died on 3 March 2013, after being bitten by a Stephen's Banded Snake (*Hoplocephalus stephensi*) in New South Wales and after not seeking treatment until some hours after the bite.

By that stage it was too late to save him (Noone 2013).

On 26 November 2010 in Western Australia, Michael Thorpe, aged 43 was bitten by a Brown Snake (*Pseudonaja affinis*). He did not do anything for the bite in terms of first aid or seeking medical help (Ninemsn staff 2010). This was on the basis of what he'd been led to believe about the high frequency of "dry bites". He died shortly thereafter (Fenech 2010).

REPEATED PLAGIARISATION BY MARK O'SHEA

Wüster, O'Shea and Schleip have dealt with their problem of the Hoser names by boycotting the use of them for most of the past 15 years and telling others to do so (see dozens of examples cited and listed in Hoser 2001, 2009a, 2012a, 2012as).

A good example is seen with (Wüster et. al. (including O'Shea) 2001, where they repeatedly refer to all Hoser named species as ""non-taxa" and therefore not worthy of recognition. They justify that claim by pointing out non-recognition of said forms by earlier authors such as Kluge (1993).

However the deliberate fraud of O'Shea is seen when cross referenced with O'Shea 2007, p. 148, which is a book by O'Shea covering Boas and Pythons.

In the former publication O'Shea (as listed co-author, who was actually improperly listed by Wüster for a paper he had written on his own) condemns the Hoser 2000 original description of the species *Leiopython hoseerae*, formerly regarded as a variant of *L. albertisi*, describing it as a "non-taxa".

In O'Shea 2007, he refers to it as an obviously distinct and separate species! But instead he calls it *Leiopython* sp. making a point of not in any way citing the original Hoser 2000 paper (Hoser 2000a), even though he remorselessly lifts data from the Hoser 2000 paper! I again note that while other large pictorial books on pythons quite appropriately use and properly cite more Hoser references than from any other authors, e.g. Kend 1997 and Barker and Barker 1994, noting the countless definitive papers on pythons I have published since 1980, O'Shea is content to rip off the material from Hoser papers, using the material liberally in his own publications, sometimes near verbatim and yet without citing them. This is the serious academic offence of plagiarism and an action he should be condemned for. For the record, there is not a single reference to a Hoser paper in O'Shea 2007, even though he has liberally used data published within them.

In the wake of the series of false and unsubstantiated claims by O'Shea in the Kaiser *et al.* (2013) rant, it is important I show where the real acts of fraud are coming from. It comes from those making the false allegations! O'Shea had in 2007 unethically boycotted the Hoser name *Leiopython hoseerae* and the Hoser reference. This in effect puts his more recent 2013 calls for the same into context as a long-term agenda on his part and not anything new in response to 2009-12 papers in *Australasian Journal of Herpetology*.

However it is clear that from O'Shea (2012) and later rants online that the little angry man Mark O'Shea fears that the names legally proposed by Hoser for a larger number of taxa will come into widespread use.

I refer to his 2013 statement of fear posted on co-author Wulf Schleip's Facebook wall that said:

"150 years from now Herpetologists will consider hoser the greatest herpetological taxonomists of OUR generation. This must not be allowed to happen".

Now fraud and plagiarism are second nature for O'Shea and I could probably fill a book with examples of this alone.

However without being too tedious I will mention another such case.

In 2003, I described a new and medically significant taxon of Eastern Brown Snake, namely *Pseudonaja textilis pughii* from New Guinea (Hoser 2003a).

While the morphological evidence alone was more than sufficient to diagnose the taxon, O'Shea, Wüster and Williams vocally derided the description of the snake as a "non-taxa".

In March 2008, O'Shea himself published a paper effectively redescribing the taxon, on pages 31-32 of the journal *Herpstile*, over which he held despotic editorial control at the time (O'Shea 2008).

Readers may recall it was one of those infamous issues in which he repeatedly and unlawfully bootlegged my registered Snakebusters trademarks in order to assist friends in legal proceedings (that ultimately failed) in which they were trying to gain ownership of the registered trademark, with O'Shea putting his own "Snakebusters" logo on the front cover of the magazine to assist in their legal (and false) claim that use of the term "Snakebusters" was widespread, when it wasn't.

In terms of O'Shea's 2008 (effective) redescription *Pseudonaja textilis pughii* (which of course he refused to mention by name), he lifted from my 2003 paper, at times near verbatim, diagnostic features of my subspecies *Pseudonaja textilis pughii*, included a picture of the same animal on the front cover of the magazine, next to his own illegal "Snakebusters" logo and all without so much as even citing the 2003 paper by myself at any point in the magazine (O'Shea 2008).

Now there is no question that O'Shea had plagiarised my 2003 paper and its findings, then improperly masqueraded in 2008 that the findings he was publishing were all his own.

You see a few months prior O'Shea had also published a paper in *Zootaxa*, as co-author with fellow fraudster and convicted reptile smuggler David John Williams (see elsewhere in this paper) actively condemning my description of the same subspecies of snake (Williams *et al.* 2008).

PLAGIARISATION BY WÜSTER

Notwithstanding his non-stop attacks on everything "Hoser", Wüster (and O'Shea, Schleip and listed Kaiser *et al.* supporter, the convicted reptile smuggler David John Williams) have also repeatedly shamelessly bootlegged and plagiarized Hoser papers and findings after the consensus swung in the direction of favouring the Hoser taxonomy and nomenclature.

Instead of continuing to deny the existence of Hoser named taxa as "non-taxa" (like in Wüster 2001a, Wüster *et al.* 2001), Wüster would publish one or more papers himself making the same taxonomic findings and judgments as in the Hoser papers in gross and blatant acts of plagiarization.

This he repeatedly did without citing the original Hoser material, or if citing it, done so elsewhere in the paper with yet more false and baseless allegations attacking Hoser or the original paper.

Examples of these outrageous acts of plagiarization of Hoser papers include:

- Hoser 1998a/2002b *Acanthophis* taxonomy (confirmed by Aplin and Donnellan 1999, Wells 2002d), (also see support from Starkey 2008 dating back many years), then plagiarized by Fry *et al.* 2002 (including Wüster) and Wüster *et al.* (2005):
- Hoser 2000b/2003e/2004a Python Taxonomy (confirmed by Rawlings and Donnellan 2003 ("*Chondropython*"), confirmed by Wells 2005 ("*Morelia*" Carpet Pythons), Rawlings, *et al.* 2008 ("*Brogghammerus*" and other genera)); then plagiarized by O'Shea 2007 ("*Leiopython*"); also then plagiarized by Schleip 2008 ("*Leiopython hoseerae*" and other):
- Hoser 1998b/2000b/2001 "*Pseudechis*" group

taxonomy (confirmed by Kuch *et al.* 2005), then plagiarized by Wüster *et al.* (2005):

- Hoser 2002a *Oxyuranus* taxonomy, plagiarized by Wüster *et al.* (2005):
- Hoser 2003a *Pseudonaja* taxonomy, plagiarized by David Williams *et al.* (including Wüster and O'Shea) (2008).

The context of all this in 2013 is that Wüster and the others, are well aware of the validity of most, if not all the Hoser described taxa in the post 2000 period (to end 2012) and their current actions can be put in perspective with their past. Clearly they dishonestly seek to steal any kudos, Hoser may get from the earlier taxonomic papers.

THE CORRECT WAY TO DEAL WITH GRIEVOUS TAXONOMIC MALPRACTICE

The allegation has been made (and without any evidence) that the papers of myself and Wells constitute in their entirety (and I note unread) "grievous taxonomic malpractice".

Kaiser *et al.* have suggested that the Zoological Code be totally undermined and made redundant in an all out war on the nomenclature of Hoser and Wells.

The want to go outside of the three critical rules of, 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere).

Of course if the Hoser and Wells taxonomy is evidence free and has no basis, the names will never come into use.

If on the other hand the taxonomy is in fact evidence based, then the names can and will be used, as already seen for the likes of numerous Hoser and Wells and Wellington names already.

In terms of Hoser (the author here), names widely accepted include those from all the earlier papers, including *Acanthophis wellsei* Hoser, 1998 (first paper naming new taxa) (later recognized by Aplin and Donnellan, 1999), *Pseudechis (Pailsus) pailsi* Hoser 1998 (second paper naming new taxa) (since recognized by Eipper 2012), *Pseudechis (Pailsus) rossignolli* Hoser, 2000 (third paper naming new taxa) (since recognized by Williams *et al.* 2005), *Leiopython hoseerae* Hoser, 2000 (fourth paper naming new taxa) (since recognized by Schleip 2008) and so on.

From the editors

Gijs van Akou
Marcel van der Voort

We published an original article by Raymond Hoser describing a new species of snake from Irian Jaya in volume 20 (issue 6). In this issue you will find a reaction on Hoser's taxonomic contributions written by Wüster, Bush, Keogh, O'Shea and Shine. Wüster *et al.* criticise Hoser on the way he describes new genera and species. They also criticise the editors of *Litteratura Serpentina* for publishing the article without prior review by several qualified taxonomists.

Prior to publication we did send the Wüster article to Mister Hoser and offered him the opportunity to reply.

From the reactions received we can conclude that we are in the middle of a conflict that has been going on for years. *Litteratura Serpentina* had no intention to become part of, or escalate this conflict.

The publication of the Hoser paper is a fact. That is why we decided to publish the reaction by Wüster *et al.* on that publication. If Mister Hoser decides to take the given opportunity to reply, you will find his response in the next issue. Both reactions are published on account of the authors themselves.

The editors do not wish to judge in this matter. After reading the contributions our readers can make up their own minds.



LITTERATURA SERPENTINA 2007 VOL. 21 N° 3

So we have four out of four of Wüster's alleged "non-taxa" now being generally recognized by publishing herpetologists (themselves) after other suitably qualified herpetologists have properly revisited my original papers, data and of course the animals themselves!

It should also be noted that Wüster also applied pressure on the editor of *Litteratura Serpentina* not to publish my rebuttal of his rant (Wüster *et al.* 1981), contrary to what he'd undertaken in writing, meaning it was instead published as an add-on to another paper in *Boydii: Journal of the Herpetological Association of Queensland* (Hoser 2001) the same year.

So, while Wüster has been consistent in his demands that no one use any "Hoser names" for each and every new taxon named at the time of each publication, (see citation list to justify this claim in Hoser 2009e) and on the basis they have been "non-taxa" for the preceding period, history has consistently shown that his views have not been accepted in the longer term, because he has been consistently proven wrong in each and every case based on hard scientific evidence.

Noting that the bulk of the taxonomic and nomenclatural changes by Hoser 2012 are by the admission of Kaiser *et al.* (2013) based on the "harvesting of clades from published phylogenetic studies" or evidence "lifted from others", there seems to be little realistic doubt that the changes are likely to be widely accepted and used in the future.

Of course, the actions of Wüster, Schleip and O'Shea in particular have for the past 15 years been obsessive and if not couched in the claims of scientific discourse, would probably have had all three land in jail by now for illegal stalking and harassment, or some other kind of institution for obsessive mentally dysfunctional behaviour.

Now there is a correct way to deal with cases of "grievous taxonomic malpractice" and that is in the same manner Zoologists have dealt with it over the last 200 years. This is by the production of evidence and sound judgments to match the evidence.

By way of example, the first major taxonomic judgment I made was in 1982.

In 1981, Laurie Smith published a formal description of the Rough-scaled Python, describing it as "*Python carinata*" (Smith 1981).

I was outraged at the placement of the snake in that genus and so dealt with it in the correct way.

In 1982, I published a paper, making the then radical move of placing the taxon in the genus *Morelia* Gray, 1842. At the time I did this, I presented a logical argument to support my position.

It took about ten years for the rest of the herpetological community to unanimously see my view on the evidence I had on hand and now that is the current generic placement for the taxon.

There was never any hostile character attacks on Smith, a claim his taxonomy was "evidence free", even though his actual published description was just one line, or an attempt to usurp the rules of nomenclature to get renaming rights on the species.

I did not seek to go outside of the Zoological Code and

the three critical rules of, 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere).

More recently and in the wake of further morphological and biological evidence, and the molecular data of Rawlings *et al.* 2008, I have placed the same species taxon in a new genus *Jackypython* Hoser, 2009. If this is indeed an act of taxonomic terrorism, then no one will use the name. However, if it is a correct placement, then the name should be used according to the Zoological Code.

TAXONOMIC TERRORISM - THE LIE!

Kaiser *et al.* (2013) and the same authors elsewhere repeatedly identify Hoser and Wells as engaging in "taxonomic terrorism" using these exact and similar words. The fear they seek to instill among herpetologists is that Hoser and Wells will create instability by renaming taxa already named according to the Code.

Fact is, that the three critical rules of the Code itself, namely, 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere) prevent any such acts from happening or ever going beyond the oblivion of synonymy.

The three rules of Homonymy, priority and stability prevent Hoser or Wells from any feared mass renaming of well-known taxa.

It simply cannot happen!

Nothing published by Hoser or Wells has ever sought to do this.

To the contrary, both myself and Wells have been staunch defenders of the Code, in all its detail, including the correct and proper resurrection of names proposed by others in priority of our own taxonomic judgments and potential renaming with relevant taxa.

Hence the repeated claims by Kaiser *et al.* of taxonomic terrorism against Hoser and Wells are shown to be false, even to the point of alleging the mass production of potential synonyms that haven't happened.

TAXONOMIC TERRORISM - THE TRUTH

However, Kaiser 2012b and the modified document Kaiser *et al.* 2013, directly call for a breaking of the rules of the Code.

In 2012, they wrote:

"Following the *intent* of the Code and its stated mission of promoting "standards, sense, and stability for animal names in science" may require overriding the *letter* of the Code in certain instances."

In other words they wanted to disobey the Code in a form of taxonomic anarchy, as in act in breach of the rules.

This was changed in 2013, to read:

"authors following best practices could legitimately create names that, under strict application of the Code, would amount to junior synonyms of taxa named in an unscientific manner. Unscientific names should be boycotted and scientifically sound names should be used in their place; applications requesting the suppression of unscientific names

could then be filed with the ICZN after 10 years have elapsed, and the Commission would then be able to enforce the Code.”

Meaning in effect that Kaiser *et al.* wanted to create taxonomic and nomenclatural instability so that in the chaos created his people could make numerous frivolous applications to the ICZN to potentially reverse the long-standing rule of priority to get their names to usurp the originals in the same manner that was attempted by Sprackland, Smith and Strimple in 1997 and was ruled against by the ICZN in 2001 (ICZN 2001).

By the way the term “unscientific names” is Kaiser *et al.*'s invented term to describe any names proposed by Hoser or Wells. Although it's already been established that none of the authors of Kaiser *et al.* have even properly read the relevant descriptions, it becomes hard to accept any judgment they may publish about the relative scientific merits of the said papers.

The above statement also in no uncertain terms calls on others to break the rules of Zoological Nomenclature en masse!

In Table 1 of Kaiser *et al.* (2013), the authors call for the Rules of Zoology to be broken on several counts in order to allow their own mass renaming of taxa named long ago according to their grossly distorted and warped application of rules of the Code out of context and missing critical sections. The sections they choose to ignore and breach includes the three critical rules of, 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere). Elsewhere in the same documents Kaiser (2012b) and Kaiser *et al.* (2013), the authors reserve the right to expand their taxonomic terrorism by adding to their hit list of banned publications and authors.

I note that in the eyes of Kaiser *et al.* McCord's various publications went from being “unscientific” in 2012 to “scientific” and with merit in 2013 on the basis of McCord's legal threat to Kaiser, which in itself introduces a new concept to the science of herpetology.

Will scientific names in future only be accepted if the person making the proposal can afford a lawyer?

This year it is the taxa named by Hoser and Wells that they seek to rename in violation of the Code. I also note in some cases they erroneously tell other herpetologists to transfer some species to genera they have never been in previously and cannot be “objectively” placed within (Shea 2013b, 2013c). I note also the word “objectively” is one that is seriously misused at the head of Kaiser *et al.*'s Table 1.

This year they seek to violate the Code by demanding no other herpetologists use the Hoser and Wells names by “boycott”, including for common taxa, identified under these names for many years.

Next year it could any of thousands of taxonomists, living or dead, who could have their life's work usurped and stolen from them in the rush for naming rights of taxa by Kaiser, Wüster and their cronies if they get away with this heinous plan now.

Unlike the non-existent threat of taxonomic terrorism posed by Hoser and Wells, it is the Kaiser *et al.* threat

that is real, ever present and now clearly defined.

Already they have committed acts of scientific fraud and reckless taxonomic terrorism by without warning, seeking to rename no less than three major genera of snakes in a direct breach of the Zoological Code (Wallach 2006, Wallach and Broadley 2009 and Wallach *et al.* 2009), including breaking the three critical rules of, 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere).

In each case the authors violated the rule of priority (Principal 3, Article 23 and elsewhere) and used the same type species for the genus names they sought to illegally remove from nomenclature.

Kaiser *et al.* 2013 directly calls for an expansion to this plan to illegally rename some hundreds of validly named taxa.

Even more alarming is their reserved right to expand their taxonomic terror hit list at any time as detailed in Kaiser *et al.* 2013, p, 19 (left column) and active encouragement to others to copy “the model we present here” (p.20 right column).

Kaiser *et al.* quoted an ICZN commissioner Douglas Yanega (Yanega 2009:423) who said

“I think the present system by which we name species is not policed effectively and has loopholes and ambiguities. For example, scientific names can be published in journals without peer review. Although that freedom is fine, the reality effectively permits taxonomic vandals to plagiarize others or publish without scientific merit.”

Then going on to say:

“This is an apt summary of the problems in taxonomic herpetology (and other disciplines) that are the primary focus of this article: instances where the Code protects names produced unscientifically, including those without sufficient evidence, justification, or privately published to bypass the peer-review process.”

This is of course is the fundamental problem with their *Herpetological Review* article!

It masquerades behind a façade of reasonableness but is still unable to mask the real intent of the publication.

That is the authors wish to circumvent the zoological Code by coercion and conspiring to destabilise it for their own ends, i.e. the ultimate renaming of everything that's been named so they can usurp what has been described before, thereby stealing the intellectual property of others.

Furthermore and noting their false inference that Yanega's comments were directed at people like Hoser and Wells as opposed to Kaiser, Schleip, Wüster, O'Shea and their band of thieves, who were the real targets of Yanega's comments, as seen by the evidence free taxonomic 'hit list' published by Kaiser *et al.* themselves.

Also, given the overwhelming fact that the majority of the Wells and Wellington 1984 and 1985 taxonomic publications and all those of Hoser in the 1990's have stood the test of time, been further validated by other scientists and despite the (intended) lag time to

acceptance being prolonged due to the previous failed suppression attempts (ICZN 1991, ICZN 2001, Wüster 2001, Wüster *et al.* 2001), this latest 'copy-cat' effort by Kaiser *et al.* should be similarly dismissed and seen for what it is. That is the bitter and twisted sour grapes attitude of a bunch of genuine nobodies in herpetology, destined otherwise to muddle on in their own herpetological oblivion as individuals who have never made a valuable contribution to the science of herpetology and are now seeking to gain some kudos or notoriety they can't find elsewhere.

AN ERROR OF PROPOSALS

Kaiser *et al.* are fully aware that to make an open declaration of mass defiance of the Zoological Code, including, the three critical rules of, 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere), would lead to their condemnation by the scientific community as happened in 2012, after the first draft of Kaiser *et al.* (Kaiser 2012b, Hoser 2012as)

To that end, they have marketed their scheme as a proposal to defy the Code, but in the fine print disavowed themselves of it, leaving those who do take their call to boycott valid names to use improper ones literally on their own head when caught out.

Page 19 of their manifesto reads:

"To defend herpetological taxonomy against unscientific incursions, we propose that the herpetological community, including authors, reviewers, editors, users of taxon names in applications, and other interested parties, set aside and strictly avoid the use of the taxon names listed in Table 1."

At the end of their call for action (detailed below), they again state:

"We further propose that the best practices presented above be used as a basis for framing a practical standard for the taxonomic process in herpetology that can be amended and adopted by herpetological societies and the editorial boards of scientific journals."

The underlines are my emphasis of the relevant words "we propose" and "we further propose", noting these are the correct terms for making actions under the Rules of Zoology (Ride *et al.* 1999).

However when you go to the referred to Table 1, on page 10 of their manifesto, it reads:

"These recommendations are not formal nomenclatural proposals according to articles of the Code."

In summary we have a situation of Kaiser *et al.* failing to define whether or not their grand plan is being made to comply with the Code or not to comply with it.

This obvious defect in their paper should not have been allowed by the alleged peer reviewers and editors at *Herpetological Review*.

Alternatively, one may ask, would the ICZN commissioners seriously approve of a proposed non-proposal as outlined by Kaiser *et al.*?

FINDING A LOOPHOLE IN THE CODE IN ORDER TO FIND A WAY NOT TO USE THE HOSER NAMES

Wüster has clearly treated his war against Hoser and Hoser nomenclature like a game of Chess, or perhaps as a kind of legal challenge.

Either way, the regularity by which he tries to argue like a lawyer, often arguing complete tripe as detailed above, shows he'd make a very good lawyer (but not a defamation lawyer) if he ever decided to change occupation.

He went from arguing against the merits of the Hoser taxonomy in 2001 (e.g. Wüster 2001a, Wüster *et al.* 2001), through to the legality of publication under the Code (Wallach, Wüster and Broadley 2009) to a desperate appeal to ignore the rules of Zoology in 2012 (Kaiser 2012b, Kaiser *et al.* 2013).

Following the publication of Hoser 2012as, the article titled, "Robust taxonomy and nomenclature based on good science escapes harsh fact-based criticism, but remains unable to escape an attack of lies and deception" in *Australasian Journal of Herpetology* 14:37-64, it was evident that the attempt of Kaiser 2012b (with Wüster's obvious input, noting he was later cited as a co-author) to openly step outside the rules of zoology to attack the Hoser names and the Code at the same time, would not be popular with other scientists. As a result the Kaiser team decided to look for alternative ways to defeat the Hoser names within the Code and jettison their 2012 plan of attack as detailed within the document.

I am not privy to all the discussions the various players have had, but it is clear that Wüster in particular was desperate to find a "legal" means to deal with his problem of "Hoser names".

The problem for him was that his earlier campaign detailed already to argue the merits (or lack of them) in terms of the Hoser taxonomy and names had by 2009 failed. Recall that is the date that Wüster, with Wallach and Broadley made the totally false ambit claim that no issues of *Australasian Journal of Herpetology* had been published according to the Code as a bid to effectively knock out a huge number of Hoser names in one go.

With all the relevant taxonomic papers bar one, effectively redone in 2012 issues of the same journal, this time with printer receipts within, such a false claim on non publication could not be viably repeated, and so Wüster needed alternative means to try to stop use by others of the "Hoser names" assigned to then hitherto unnamed taxa.

By 2012, the earliest Hoser taxonomy and names from the period before 2000 had been almost universally adopted and Wüster knew the outcome for the more recent names to 2012, most based on superior evidence to the earlier names, would be much the same.

This contrasted with his earlier position, encapsulated in 2003, at a time when his deception was relatively successful in stopping people from using Hoser names, best seen in his comments on Kingsnake.com on 18 December 2003, where he wrote: "the names are about the least of our worries about his taxonomy".

This was because if the taxonomy wasn't accepted, then the names simply fell away!

By end 2012, Wüster was desperate to stop use of any and all “Hoser names”, including by continuing to obsessively edit sites like Wikipedia, and harass webmasters of sites like “Reptile Database”(www.reptile-database.org) to ensure no Hoser names got used.

To that end he also posted widely on various forums and list servers asking for advice on a way to legally avoid having to use Hoser names.

On one such forum list, the conversation commenced by Wüster seeking loopholes in the ICZN Code rules. When I printed the thread, the print out ran a total of 208 pages! But the best outcome mooted for Wüster was a potential synonymising of some Hoser-named tribes by a correspondent who even then didn't think the prospect likely to succeed in the long term (Various authors including Wüster 2013a).

However, sometime between June 2012 and March 2013 they thought a loophole was ostensibly found. However in their haste to use it, someone in the Kaiser *et al.* group apparently failed to quote the relevant section of the Code properly.

Quite amazingly, none of the nine alleged authors, the alleged peer reviewers at *Herpetological Review* and the entire group of listed supporters (more than 60 people in all) all also failed to cross-check the most important quoted section of the Zoological Rules in the final draft of Kaiser *et al.* (2013) with the Zoological Rules itself to find and correct the most obvious defect.

This is particularly alarming considering the professed support for the document by these people!

KAISER ET AL. AND THEIR DEFECTIVE CALL FOR ACTION

While the dominant feature of Kaiser *et al.*'s rant is a repetitive and unsubstantiated collection of lies condemning my own taxonomic papers and then calling for a boycott of them, it is of note that they have taken the view that working within the Zoological Code rules is in their words a “tricky business”.

This isn't so.

In fact as a document, the Zoological Rules are easy to work within and provided the three key rules of homonymy, priority and stability are adhered to, it is very straight forward.

As shown already, repeated statements by Kaiser *et al.* (2013) detailing deliberations of the ICZN in terms of “taxonomic vandalism” are fraudulently misrepresented by Kaiser *et al.* to imply they relate to Hoser papers, when they clearly do not.

The statements by O'Shea and Coritz, (O'Shea 2013c, Coritz 2013) of ICZN support for Kaiser *et al.* (2013) posted to a potential audience of over 1 billion people is also a lie.

Asserting that the taxonomic papers of myself and Wells are some kind of attack on herpetology, requiring Kaiser *et al.* to set themselves up to “defend herpetological taxonomy from unscientific incursions”, they then set about in an elaborate attempt to create a legal loophole within the Zoological Rules to usurp the Hoser and Wells names for taxa with their own names as attempted already by Wüster in his illegal renaming of *Spracklandus* Hoser, 2009.

As already mentioned, they also sell this as a business model to suppress other people's valid names as well (p. 20, second column).

However, by far the most important part of their plan as detailed is the legal loophole in the Zoological Code that they seek to exploit.

Further, as already mentioned, the legal loophole they seek to exploit is in error, because the authors have foolishly misread the Code and misquoted it, with no one it seems bothering to check the quote against the actual source document.

They rely on Article 23.9 of the Code which deals with “Reversal of precedence”, or in layman's terms means when the law of priority can be broken in order to maintain nomenclatural stability.

Now Kaiser *et al.* have repeatedly misrepresented the spirit of the Code to falsely imply I have acted outside it. As I quoted from the Code earlier, the fact is that myself and Wells have in fact operated wholly within the intent of the Code which states:

“The 4th edition of the *International Code of Zoological Nomenclature*, like the preceding editions and before them the *Règles internationales de la Nomenclature zoologique*, has one fundamental aim, which is to provide the maximum universality and continuity in the scientific names of animals compatible with the freedom of scientists to classify animals according to taxonomic judgments.”

So the fact is, that it is Kaiser *et al.* operating outside the spirit of the Code.

Now in terms of the spirit of the Code as stated in the preamble and elsewhere, reversal of priority is intended for things like when a well-established name on the record is found to be the junior synonym of a long overlooked name published in an obscure place, perhaps not properly indexed or in a foreign language and a very long time ago.

Reversal of priority is not intended when a recently (last 100 years) published name that has been widely disseminated and indexed in places like *Zoological Record* is forcibly suppressed by a bunch of thugs intent on having their own more recently coined name over-ride a senior synonym.

However to get their legal loophole up (in their view), Kaiser *et al.* (2013) twist their alleged meaning of the Code about quite dramatically and interpolate it with direct quotes from it, to make their loophole appear a viable way to forcibly suppress the usage of Hoser and Wells names.

On page 20 they wrote:

“According to the *Code* (Article 23.9.1-3; ICZN 1999) it is desirable to avoid the use of names that threaten stability even when this reverses the *Principle of Priority*. This is one area of the existing *Code* where ICZN actions can favor the establishment of names generated within a genuine scientific framework. The *Code* adopts a strict stand against names (including those that could be classed as unscientific) that have not been used in “at least 25 [scientific] works, published by at least 10 authors in the immediately

preceding 50 years and encompassing a span of not less than 10 years” (Article 23.9.1.2; ICZN 1999); thus, authors following best practices could legitimately create names that, under strict application of the Code, would amount to junior synonyms of taxa named in an unscientific manner. Unscientific names should be boycotted and scientifically sound names should be used in their place; applications requesting the suppression of unscientific names could then be filed with the ICZN after 10 years have elapsed, and the Commission would then be able to enforce the Code.”

However they have in fact misquoted the relevant section of the Code, perhaps/likely deliberately, so as to omit the most important part of Article 23.9 of the Code.

So to clear things up and show why the Kaiser *et al.* plan to rename all the Hoser and Wells named taxa is doomed to fail at the outset, I shall quote in full the relevant section of the Code.

It reads:

“23.9. Reversal of precedence. In accordance with the purpose of the Principle of Priority [Art. 23.2], its application is moderated as follows:

23.9.1. prevailing usage must be maintained when the following conditions are both met:

23.9.1.1. the senior synonym or homonym has not been used as a valid name after 1899, and

23.9.1.2. the junior synonym or homonym has been used for a particular taxon, as its presumed valid name, in at least 25 works, published by at least 10 authors in the immediately preceding 50 years and encompassing a span of not less than 10 years.”

The important bit that Kaiser *et al.* 2013 left out was **“the senior synonym or homonym has not been used as a valid name after 1899”**, which I had underlined above to make sure it wasn't overlooked again.

This automatically excludes the Hoser and Wells names which by Kaiser *et al.*'s own findings are those that postdate year 2000!

Hence the one section of the Code that Kaiser *et al.* thought contained a loophole by which they could suppress the Hoser and Wells names in favour of their own, in fact doesn't exist!

Inability to properly read or quote a simple document such as the Zoological Code, or for that matter join the dots in terms of names of taxa described and genera they came from originally in relevant papers subject to condemnation, do not serve as good indicators of scientific method or ability by the nine listed authors of Kaiser *et al.* or those people who allegedly blindly followed them by endorsing the fatally flawed document.

THE LAST FAILED ATTEMPT AT IMPROPERLY RENAMING REPTILE TAXA

Now in terms of people exploiting perceived loopholes in the Code to suppress a modern senior synonym in favour of an improperly created junior synonym, the Code does have catch all sections to allow a person take a grievance to the commission. One such section is 23.9.3. of the Code.

However this section does not get the Kaiser *et al.*

document over the line in defining a loophole in the Code to allow mass suppression of properly constituted scientific names for taxa.

For the record, the last well-known attempt at forcibly suppressing a valid senior synonym in favour of a junior synonym named in full knowledge of the latter involved an Australian species of monitor lizard.

The case was as close a match to the current situation as can possibly be found.

It involved “taxonomic vandalism” and a scientific name “classed as unscientific”.

Now this isn't my view, but it is most certainly that of Kaiser *et al.* (2013) and Wüster in particular.

The species, *Odatia keithornei* was described in 1985 by Wells and Wellington, the senior author of the paper already identified by Kaiser *et al.* as guilty of “taxonomic vandalism” and the other target of this most recent Kaiser *et al.* attempt at mass suppression and renaming of taxa. The 1985 publication itself was identified by Wüster in 2013 as a holotype case of taxonomic “vandalism” (Wüster 2013a).

So we know that Kaiser and Wüster view the Wells and Wellington papers as being of the same caliber as those more recently produced by Hoser and Wells alone!

There was an earlier attempt to suppress the entire Wells and Wellington publication of 1985 (and two others by them) before the ICZN (Anonymous 1987, Hoser 2007, Wüster 2013a) on the alleged basis that the authors had “published their concepts in their own journal independent of any expert opinion and it was stated largely without any solid taxonomic basis” (Ziegler and Böhme 1998) but that application (case number 2531) failed in 1991 (ICZN 1991, Hoser 2007, Wüster 2013a). In 1991, Robert George Sprackland described the relevant monitor species, as *Varanus teriae*, in honor of his wife, usurping the valid senior synonym *Odatia keithornei* Wells and Wellington 1985.

After a time had elapsed, Sprackland invoked the relevant catch-all section of the Code (Third edition) and brought the matter to the ICZN seeking formal suppression of the Wells and Wellington name (Case 3043)(Sprackland *et al.* 1997).

Fearing an avalanche of cases if this was allowed to become a well-trodden loophole, the ICZN did to their credit rule heavily in favour of the Wells and Wellington name (Opinion 1970)(ICZN 2001, Hoser 2007) and went further by placing the name *Odatia keithornei* on their official list of available names to deter other potential offenders (Anonymous 2001, Uetz, 2013a).

This is the same fate almost certainly awaiting anyone who dares to try to illegally rename any taxa validly named by Hoser and Wells!

This can also be inferred from the formal correspondence I have received from the ICZN (Nikolaeva 2013b).

As for Rob Sprackland, the man who made this audacious attempt to exploit a non-existent loophole in the Zoological Code conduct intellectual theft and to rename a previously named species after his wife, this failure effectively killed his career as a herpetologist.

Notwithstanding his previous excellent work with

monitors in particular, he had permanently gained a reputation for dishonestly trying to rip off another person's work in an act of nepotism!

Geoff Whitten tried a similar caper to that proposed by Kaiser *et al.* (2013).

In 1994, he renamed the species *Pogona henrylawsoni* Wells and Wellington, 1985, *Pogona brevis* in a brazen and deliberate attempt to usurp the senior synonym.

Shea (1995) correctly reversed the error before it became entrenched, thereby avoiding the need for ICZN intervention to correct the mistake. As of 2013, the species remains generally known as *Pogona henrylawsoni* Wells and Wellington, 1985 (Uetz, 2013b).

Finally, with all relevant Hoser and Wells names now being published widely, including in the original publications, in *Zoological Record* and now within the widely disseminated *Herpetological Review*, via Kaiser *et al.* (2013), no herpetologist will be able to mount a claim to the ICZN that the original names were not known or were only published in an obscure publication as justification for not using the correct (senior) name for the given taxa.

Incidentally, that is usually the basis used to reverse priority for pre-1899 names.

HOSER'S TAXONOMY AND NOMENCLATURE IS CORRECT AND VIRTUALLY EVERYONE KNOWS IT! - THE NEXT ATTEMPT TO REVERSE PRIORITY!

While it may seem bold to assert that a collection of dozens of papers are broadly correct in their taxonomy and nomenclature and before being scrutinized by others in depth, there are a few good reasons why.

In terms of the scope of the publications, only blatantly obvious species and genera were named. Anything remotely doubtful was ignored or bypassed.

These obvious facts are underpinned by the robust molecular evidence that also supported most of the genera divided for the first time.

It is also obvious that contrary to the fear tactics of Kaiser *et al.* (2013), the vast majority of generic placements of species for the reptiles remain unchanged by the Hoser publications and so there is not any out of the ordinary instability caused by anything done by myself (Hoser).

By and large, the results of studies such as Pyron *et al.* (2011) and others show that most generic placements for reptiles at the present time are broadly correct and nothing done by the Hoser publications changed this.

There has been no "taxonomic vandalism" or "unscientific incursions" by Hoser as alleged without basis.

Confirmation of the above assertion in terms of correctness of my taxonomic decisions is perhaps best seen in the genus of lizards *Laudakia* Gray, 1845 *sensu lato*, subject to a break up by myself in the paper Hoser (2012a).

In that paper genera were resurrected from synonymy and when no names were available, new names were created.

Subsequently, in the second half of 2012 a paper was published by Baig *et al.* (2012), including Wolfgang Böhme as an author dealing with the same lizard group. Böhme of course is named twice in Kaiser *et al.* (2013)

as a supporter of Kaiser *et al.* (2013).

Their break up of the genus *Laudakia* broadly mirrored that of my earlier paper, which came as no surprise, as they had much the same molecular and morphological evidence available from earlier published studies on which to base their decisions.

However notable is that the did not use my earlier name *Adelynkimberlea* Hoser, 2012 for the *caucasia* group, which they renamed in violation of the Zoological Code.

Now there is a possibility that the authors were unaware of *Adelynkimberlea* Hoser, 2012 at the time of submission of their paper to their journal and so there is a possibility that the mistake was inadvertent.

Now the errors of Baig *et al.* (2012), in creating new generic names when there were names available, would as a matter of course see the error identified by other herpetologists and the junior names proposed by them disappearing into synonymy very quickly.

That is how the rules of the zoological Code work (homonymy and priority, which gives stability).

However notable is that Böhme (who is listed twice) as a supporter to Kaiser *et al.* (2013), and others working on the grand plan, have now sought to improperly suppress and boycott the Hoser name *Adelynkimberlea* Hoser, 2012.

Peter Uetz, a close friend of Wüster, has at Uetz (2013c), noted his use of the Baig *et al.* names and the boycott of the Hoser names (all) referring to Kaiser *et al.* (2013) as the basis for doing so. This is particularly worrying as Kaiser *et al.* don't in fact have a sound basis for calling for the boycott in the first place and yet Uetz is blindly following the Kaiser *et al.* call to arms.

On his species pages, Uetz, uses the Baig *et al.* names for all species formerly within *Laudakia sensu lato*, while not even advising readers of the existence of the Hoser senior synonyms, which he is clearly aware of. Further evidence of this is in his *stellio* page (Uetz 2013d), which mentions the Hoser *Laudakia* paper of 2012 (Hoser 2012a).

I also note the conflict in this case with Böhme's stated support for the Zoological Code and the rule of priority in 1998, when he wrote in support of all Wells and Wellington names (co-authored with Ziegler) which were subject of several similar illegal suppression attempts. Then he wrote in support of the senior synonyms and said "the provisions of the Code apply to all names" (Ziegler and Böhme 1998).

The deliberate instability created by Kaiser *et al.* (2013), Baig *et al.* (2012) and Uetz (2013c) if left unchecked, will spread virally through the internet and beyond, which is of course their grand plan.

Then as the plan unfolds, they will waste yet more time by seeking an ICZN ruling to reverse priority for the valid and well-known senior synonym, which in turn will likely fail.

Success here would of course encourage others to try the same trick and within a few short years, renegades in all areas of zoology would be boycotting all sorts of senior synonyms with a view to improperly renaming taxa with their own coined names and totally destabilizing a

zoological nomenclature built up over centuries.

Thus the ICZN would as in cases past, be forced to rule in favour of the senior synonym to maintain stability from that date on, even after instability had been caused by the deliberate improper promotion of the junior synonym to give it wider usage in the immediately preceding years (ICZN 2001, 2007).

Now noting the dates of publication, Hoser 2012ao, Baig (including Böhme) *et al.* (2012) and Kaiser *et al.* (2013), failure to divulge the conflict of interest in terms of Wolfgang Böhme by Kaiser *et al.* with a direct vested interest in boycotting of a Hoser genus name in favour of his own later coined name, is yet another breach of the SSAR's own ethics statement!

I note also that generic name *Plocederma* Blyth, 1854 (a potential genus within *Laudakia*) was also ignored by Baig *et al.* (2012), potentially indicating that the over 30 reptile taxa named by Blyth as currently recognized may also be on the Kaiser *et al.* renaming list (Uetz, 2013e).

OTHER HITLIST TARGETS OF THE GANG OF NINE

There is no doubt that Kaiser *et al.* seek to steal naming rights for species from others besides Hoser and Wells. Taxa named by Bill McCord was removed from the so-called "unscientific" hitlist not on the basis of any new found merit in his papers, as they never changed, but rather due to the threat to Kaiser from his lawyers.

On that basis it seems that the other targets of boycott and renaming in violation of the code will remain people from outside the jurisdiction of the gang of nine. That is either other countries or dead people.

One additional target already mooted for boycott and renaming of all relevant taxa using the Kaiser *et al.* template was named by the group. His name is Mark van Roosmalen (O'Shea *et al.* 2013). On 8 April 2013, Schelip (2013b) also announced on a German language internet chat forum that he intended using his paper (Kaiser *et al.* 2013) as a basis to enable him to rename the Hoser named python genus *Brogammerus*!

This was confirmed by Wüster (Wüster 2013c), when on April 9 he said he hoped that someone "should be encouraged to take the recent Kaiser *et al.* paper as a cue to scrap *Brogammerus* and overwrite it with their own new genus name".

This is of course a simple planned case of intellectual theft and plagiarisation of the most extreme degree, which again makes a mockery of any claimed adherence and respect for the zoological Code.

These two targets for renaming have emerged in less than three weeks of the publication of Kaiser *et al.* (2013) and no doubt more will emerge in the months and years to follow.

The actions of these rogue individuals, if unchecked are clearly the greatest threat to the stability of the zoological Code in its entire 200+ year history!

CONCLUSION: A CALL TO ACTION

An accurate taxonomy and a stable nomenclature is the essential infrastructure upon which all other zoological science is based.

The rules by which it is based must be adhered to by all. It is too important to be victim of attacks by gangs of nine

seeking to steal naming rights for taxon groups over which they did not do the critically important groundwork, or to allow a system built on homonymy, priority and stability to be effectively destroyed.

Now we know and it is proven without doubt in every possible way, that Mark O'Shea, Wulf Schleip, Hinrich Kaiser and Wolfgang Wüster, the main co-authors of Kaiser *et al.* 2013 are serial liars.

For the exact documentary evidence and proof, by way of cross-referencing, see (O'Shea (2013a) cross referenced with Hoser (1998a), O'Shea (2013c) and other relevant sources, see cross referencing of Schleip documents as done by Hoser (2009a), Kaiser 2012a cross referenced against Kaiser *et al.* (2013) and Hoser (2012a) cross referencing various Wüster publications.

We now know they have lied in relation to myself and my papers to further their own warped agenda, which in summary includes a 15 year battle running from 1998 to 2013 to improperly stop herpetologists from complying with the rules of the zoological Code, by illegally encouraging them to boycott all Hoser names.

We know that these individuals have no respect for the rules of zoology or other rules of ethics noting they have broken them repeatedly in the past, and including for their stated objective of undermining the rules of Zoology (Kaiser 2012b, Kaiser *et al.* 2013).

To allow their call to thousands of herpetologists to ignore the fundamental rules of the ICZN, namely the three critical rules of, 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere) and encourage the illegal mass creation of junior synonyms for established taxa will create taxonomic and nomenclatural chaos.

Of that there is no doubt!

The proposed actions by Hinrich *et al.*, which are a repeat of a similar attempt to undermine the Code by Wallach *et al.* (2009) will potentially put lives at risk. This is due to potential misidentification of venomous taxa due to use of improper and superfluous names improperly placed on the record as synonyms.

Failure to stop Kaiser *et al.*'s unprecedented plan to dramatically breach the rules of zoology on an unprecedented scale must be stopped. Failure to do so may see the administration of the rules of zoological nomenclature descend into anarchy where the rules of Homonymy, Priority and Stability are continually at siege and the ICZN required to intervene to maintain stability at ever increasing regularity.

This would especially be the case, were people in areas outside of herpetology to try to copy the Kaiser *et al.* template.

Therefore the most recently published plans of Kaiser *et al.* (2013) to usurp the ICZN for their own nefarious purposes must be stopped.

More importantly, they should be publicly exposed and humiliated for their repeated academic misconduct and fraud so that there is a deterrent to others who may otherwise try to emulate them.

AUTHORSHIP

This paper was written by myself, Raymond Hoser as author. I had assistance, comments and input from several people at the writing stage. The final copy was reviewed by several people, not all identified to me. Further refinements were made as a result. All reviewers were also provided with copies of relevant parts of relevant cited documents to verify all claims within, the volume of which was very large.

All reviewers also read many, if not all the relevant Hoser papers as cited at the rear of this document and had copies of all on hand.

Numerous people, including some reviewers offered to publicly "sponsor" or support this document. I have chosen to list none for several reasons.

Firstly in doing so, I would potentially be open to the charge that some of these people had not read all the relevant publications, which would almost certainly be true noting that the relevant papers of my own total of nearly a million words, or the equivalent of ten full sized books by word count, meaning that my supporters would in effect be taking me (at least in part) on faith, as has obviously happened in the case of Kaiser *et al.* (2013).

Secondly, the issue has emerged over many years of Wüster, Schleip and O'Shea illegally harassing persons who have supported myself, my business, my family and my publications (as detailed in Hoser 2012a) and on that basis I do not seek to put anyone else at further potential risk of such treatment.

Support for my taxonomic conclusions and nomenclature is however best measured by usage of the names to date. Besides the use in numerous books, including those cited herein and those actually published by alleged supporters of Kaiser *et al.* (2013), the usage of "Hoser names" can be ascertained by simple "Google" searches for them.

Obviously older names revisited by others tend to be the most widely used.

Examples of a number of webpages using given "Hoser names" by Google search results as of 1 April 2013 (recorded by screen dumps) are as follows:

Broghammerus - 22,500 separate webpages.

Morelia harrisoni - 391,000 separate webpages.

These numbers indicate the magnitude of taxonomic and nomenclatural instability that Kaiser *et al.*'s proposals would cause if generally acted upon.

Finally, there are no issues of conflict of interest in terms of this paper, the subject matter, or conclusions within, although in case it's been missed by readers, I note I also edit and publish *Australasian Journal of Herpetology*.

The argument within this paper is about the correct use of correct names in Zoology according to the published rules of the Zoological Code. It is not about the name of the person who first proposed the names. That is only an issue to those with inflated egos.

SUMMARY OF KEY POINTS

· Hinrich Kaiser and eight other renegades, namely Mark O'Shea, Wolfgang Wüster, Wulf Schleip, Paulo Passos, Hidetoshi Ota, Luca Luiselli, Brian Crother and Christopher Kelly, herein cited as Kaiser *et al.* (2013)

have made numerous demonstrably false claims about Hoser and Wells.

· The claim by Kaiser *et al.* (2013) that Hoser's descriptions of taxa are unsupported by evidence is refuted by their other claims that Hoser had engaged in "harvesting of clades from published phylogenetic studies for description as new genera or subgenera" and used evidence "lifted from others".

· The papers and taxonomic decisions by Hoser (and Wells) are based on robust cited evidence and comply with the established rules of Zoological Nomenclature (Ride *et al.* 1999) of homonymy, priority and stability.

· Kaiser, O'Shea, Wüster and Schleip have been exposed here as serial liars.

· Schleip and Wüster have both been exposed for "Grievous taxonomic misconduct" by knowingly publishing descriptions of invalid taxa or junior synonyms and falsifying data.

· O'Shea, Wüster and Schleip have for 15 years engaged in a cynical destabilization of taxonomy and nomenclature in breach of the rules, motivated by a deep personal hatred of Raymond Hoser.

· Over time, Hoser and Wells taxonomic and nomenclatural judgments have been accepted as correct by other herpetologists as confirmed by molecular studies and their names widely used (millions of times)(e.g. *Broghammerus*, *Antaresia*).

· O'Shea, Wüster and Schleip have repeatedly committed the crime of plagiarization.

· Kaiser *et al.* have repeatedly misrepresented and misquoted the Zoological Code.

· Kaiser *et al.* have several times made an open call for others to act in breach of the numerous sections of the Rules of Zoological nomenclature including 1/ Homonymy (Principal 5, Article 52 and elsewhere), 2/ Priority (Principal 3, Article 23 and elsewhere) and 3/ Stability (Principal 4, Articles 23, 65 and elsewhere) and the ethics of the Code (Section A).

· Kaiser *et al.* seek to rename hundreds of validly named taxa in breach of the Zoological Rules, with no restriction on other authors or names they may later deem "unscientific" in order to rename taxa properly named by others.

· In an act of "taxonomic vandalism" and "evidence free taxonomy", as co-author of Kaiser *et al.*, Brian Crother did in 2012, change the names of over 100 species of lizard, none of which had ever been the subject of a phylogenetic study. In 2009 Brian Crother engaged in another act of evidence free taxonomy to improperly reassign names to dozens of North American taxa (Pauly *et al.* 2009).

· The proposals of Kaiser *et al.* if acted upon would irreparably destabilize Zoological nomenclature.

· The proposals of Kaiser *et al.* (2013) if copied by others (as they suggested on page 20) would create general taxonomic and nomenclatural chaos and effectively destroy the rules of zoology.

· The proposals of Kaiser *et al.* if acted upon would potentially put lives at risk through misidentification of

venomous taxa, including through excessive numbers of invalid junior synonyms.

- The loophole within the Zoological rules proposed by Kaiser *et al.*, by which they see a means to rename hundreds of species and genera by alleged reversal of priority is flawed. This is because they misquoted the relevant section of Code omitting the key line, that relating to date of first descriptions usage needing to be prior to 1899, rendering the scheme “clearly ridiculous and unworkable” (Shea 2013a).

- The use of the alleged loophole within the Zoological Rules proposed by Kaiser *et al.*, to unlawfully rename validly named taxa, subsequent to deliberate boycott of the correct names has been attempted before and failed. This included by Sprackland, Smith and Strimple (1997) (ICZN case 3043) and their scheme failed. The illegal attempt to reverse priority was emphatically rejected by the ICZN in their judgment, Opinion 1970. *Bulletin of Zoological Nomenclature* 58(1), 30 March 2001 in Volume 58.

- Claims by Kaiser *et al.* of widespread support for their position was fabricated and false. In fact the only support they got from most other herpetologists was for a proposition that taxonomy should be evidence based and subject to peer review, which is the status quo. However it is in fact Kaiser *et al.* who break both “rules” by engaging in evidence free taxonomy and in the absence of effective peer review.

- Contrary to the published claims of Kaiser *et al.* (2013), they did not have support of the Australian Society of Herpetologists to boycott Hoser names and illegally coin names for those taxa themselves (ASH 2013).

- On the basis of the preceding, the assault on the established rules of zoological nomenclature by Kaiser *et al.* (2013) must be rejected by herpetologists. The gang of nine must be condemned for their gross misconduct.

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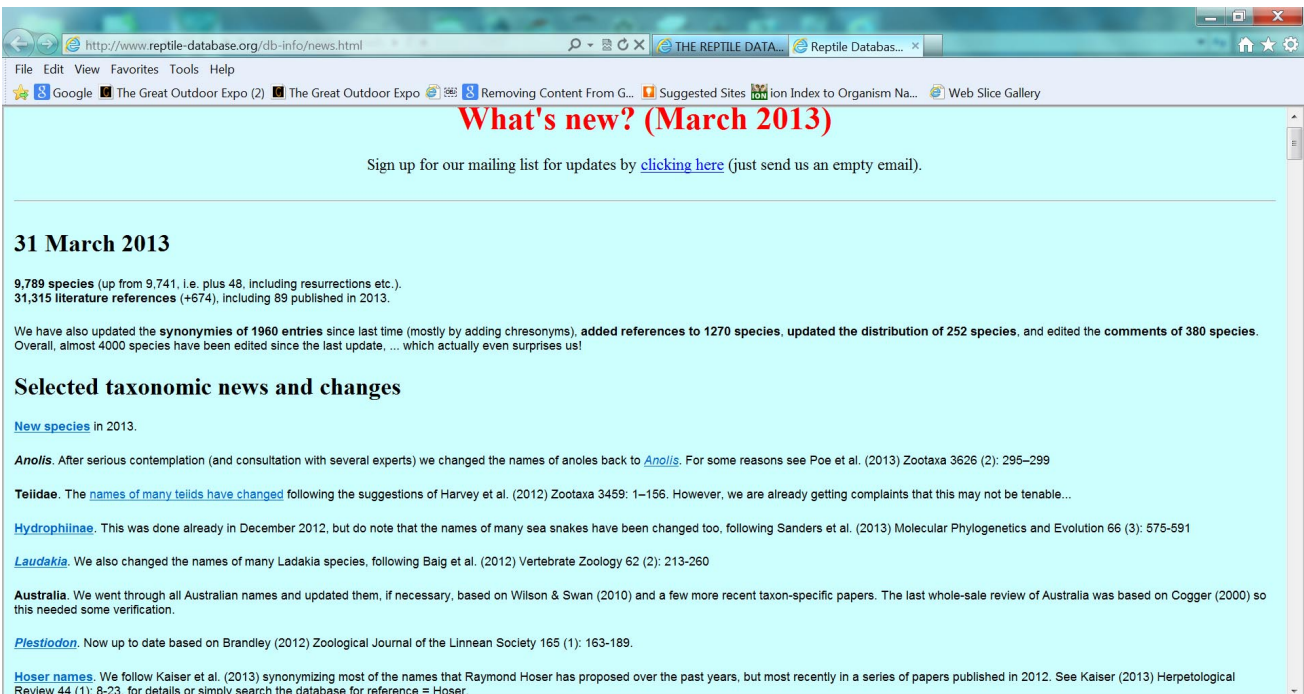
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Taxonomic and nomenclatural chaos in the making!

Serial truth hater Wolfgang Wüster in particular has got his closest friends to play a part in his planned destruction of the Zoological Code. Within hours of the release of Kaiser *et al.* (2013), Peter Uetz, had ensured that readers of his "Reptile Database" site would be made unaware of Hoser-named taxa on his database when they searched for a given taxon. He did however make a brief mention of the fact on his "news" page (screen dump below). Improperly, Uetz had also installed dozens of junior synonyms for species and genera of lizards and snakes in a move guaranteed to cause instability and confusion. This is a direct breach of the rules of the Zoological Code! Uetz had even gone so far as to install junior synonyms of Wüster's mates Van Vally and others over dozens of proper scientific names, including many proposed long ago by a number of other herpetologists including dozens of Fitzinger named taxa from the 1800's, in what is an early step towards their planned annihilation of the Zoological rules. Their game plan is to set themselves up as the arbiters, gatekeepers and deciders of scientific names for animals. Then they will systematically coin new names for everything they desire, by stealing the work of others, to give themselves fraudulent self-gratification.



Hoser 2013 - Australasian Journal of Herpetology 18:2-79.



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Best Practices: In the 21st Century, Taxonomic Decisions in Herpetology are Acceptable Only When Supported by a Body of Evidence and Published via Peer-Review

Taxonomy, the scientific process by which natural groups are identified, described, named, and classified is an exciting research pursuit, not only because it makes an indispensable contribution to biodiversity science but, at a more basic level, because it satisfies the human enjoyment of discovery. However, taxonomy has been an area of biological science in which errors, ethical transgressions, and clashes of egos have been particularly vicious and public, harkening back to the earliest days of the binomial system of nomenclature when Linnaeus (1737) named what he considered an insignificant weed (genus *Stegesbeckia*) after Johann Georg Siegesbeck, a contemporary and very vocal critic.

Taxonomy's Impact.—Taxonomy is a fundamental component of biology because it includes the subdiscipline of biology in which organismal groups are defined and named so that they may fittingly be included in the scientific discourse. Only with

precise taxonomy can biologists, and those who apply biological principles, communicate effectively (Cotterill 1997). As a consequence, dubious taxonomy undermines the underpinnings of science as a whole, with potentially serious consequences in basic and applied research. As scientists, we are fully accountable for all elements of our research, especially when our findings have broad contemporary applications. This accountability extends to the taxonomies we create or use. We also believe this responsibility includes monitoring the evidence presented as justification for taxonomic decisions. Normally, this is a key function of peer review (McPeck et al. 2009; Perry et al. 2012; Thompson 2010), but when peer review is circumvented, biologists are forced to find other means to protect the integrity of their science.

Taxonomy in Practice.—The problem with taxonomy arises when the data used to create taxonomic decisions are shoddily presented, derive from spurious research, or lack evidence. While it is true that taxonomic decisions invariably require detailed descriptive components, when these descriptions are built into a scientific framework, they strictly conform to the scientific method; the hypothesis tested is the existing taxonomy, and this hypothesis can be falsified and reformulated (Crother 2009). To perform the tasks that should define 21st Century taxonomic science, three main steps are key: (1) Generate hypotheses of group membership (e.g., a species, a clade) or evolutionary relationship (e.g., sister taxa) based on available primary sources (e.g., existing or new collections of specimens including whole animals, tissues, and DNA sequences) and the available literature; (2) test these hypotheses via appropriate, rigorous, and honest analysis of the relevant data; and (3) submit proposed taxonomic decisions (e.g., taxonomic rearrangements, descriptions of new species, elevation of subspecies to species rank) to peer-reviewed journals in the form of manuscripts that present the data and provide a rational justification for the proposed decisions. These three responsible steps constitute the information processing system that helps to ensure that taxon names, taxon concepts, and taxonomic arrangements are properly grounded in evidence.

21st Century Developments.—In the post-2000 explosion of electronic information, the rapid publication and quick dissemination of scientific information have been prominent and generally positive trends across all research fields, including taxonomy. In keeping with these developments, the International Commission on Zoological Nomenclature (ICZN) recently amended several articles of the *International Code of Zoological Nomenclature* (hereafter referred to as the *Code*) to allow publication of nomenclatural acts solely via electronic media (ICZN 2012). However, in addition to diverse online publishing channels, universally available desktop-publishing technology has also made production of high-quality booklets, pamphlets, and even journals easy for anyone. For taxonomists, this trend is both a curse and a blessing. Even as the path to publication has been simplified and the time to publication shortened by the emergence of

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reputable, rigorously scientific, peer-reviewed, and well-edited electronic or rapid-print journals (e.g., *ZooKeys*, *Zootaxa*), publishing is no longer a controlled environment and there are outlets where nonscientific and misguided taxonomy is presented as fact. Differentiating between science and non-science in taxonomy is a challenge. The *Official Register of Zoological Nomenclature* (online as ZooBank) is the authoritative ICZN register for nomenclature and can legitimize registered, electronic-only publications (ICZN 2012).

The authors of this paper understand that the right to freely interpret scientific data as it relates to taxonomic decisions must remain inviolate. Furthermore, we acknowledge that as scientists, we identify provisional truths, which are the best approximations of ultimate truths that we are able to produce at the time, and which remain subject to revision and discussion. However, we see a cautionary tale in the manner by which information is disseminated in the fast-paced world of modern science. We have learned that better placed or marketed falsehoods may supplant truths in public perception. Thus, a taxonomic fact can become obscured by nonscientific information, misleading those who are unable to discern whether the information was appropriately generated. To resist such occurrences, the practice of science in general (and taxonomy in particular) first requires adherence to certain standards for generating, analyzing, and disseminating data. Scientists also need to improve information flow regarding matters of taxonomy and nomenclature, and online registration of names (ICZN 2012) may be a suitable first step. While we accept that "bad" taxonomy remains a possible outcome even when researchers follow proper procedure, we feel that it has become necessary to defend taxonomy against misguided, unscientific practices, and to develop a set of principles to guide taxonomic herpetologists in their research, with the intent to promote (to the extent possible) reliable research that contributes to scientific progress.

DOES UNSCIENTIFIC TAXONOMY MATTER?

In herpetology, unscientific taxonomy, under the guise of science, has been presented with increasing frequency in nonprofessional outlets since the year 2000 (Table 1). The many taxon names proposed in these outlets can have serious negative ramifications: they destabilize taxonomy, and in so doing they confound conservation and legislative efforts, medical herpetology, academic processes, grant administration, and the public perception of herpetology as a whole. As a result, the negative practical impact of needlessly destabilizing taxonomy is likely to be more profound than any other type of fraud or error in herpetology.

Information Storage and Retrieval.—The proliferation of superfluous or dubious names can lead to a breakdown in the information storage and retrieval functions of the taxonomy. A change in the name of a genus, for instance, may lead to the establishment of parallel listings for all the species in that genus.

Professional Communication.—Fear of taxonomic piracy, where one author deliberately expropriates the naming intentions of another, creates an atmosphere of mistrust, stifles collegiality, and promotes insular research. In particular, it discourages communication about unnamed taxa, thus delaying research progress and even conservation action (Oliver and Lee 2010).

Bona fide Taxonomic Research.—Unscientific taxonomic acts have several impacts on genuine taxonomic research. For example, scientists are forced to trace unwarranted or bogus

taxonomic accounts in potentially hard-to-locate publications during literature inquiries on synonyms, and they must examine type material in potentially difficult-to-access collections. This not only wastes time and resources, it dilutes legitimate taxonomy with unscientific materials. Taxonomists are relegated to "re-describing" valid taxa that were named prematurely in acts of mass naming or in deliberate acts of intellectual kleptoparasitism (e.g., Aplin and Donnellan 1999; Rawlings et al. 2008). Furthermore, graduate students may have to reformulate thesis proposals or thesis conclusions, and their subsequent publications may be redundant. Nomenclature in grant applications may conflict with unscientific taxonomic publications, resulting in needless delays to ascertain the veracity of the information. In addition, institutional managers not well versed in the details of herpetological research may be unable to follow the mix of validly and unscientifically proposed names or classifications.

Applications of Herpetological Taxonomy.—Confusion about names may cause genuine harm in endeavors relying upon accurate taxonomy of organisms. At the broadest scale, taxonomic confusion will increase the taxonomic impediment to characterizing and managing Earth's biodiversity (Wilson 1985, 2004), including the assessment and protection of threatened taxa and the direction of conservation efforts (Georges and Thomson 2010; Georges et al. 2011; Parham et al. 2006; Pillon and Chase 2007). For example, in the case of species protected by CITES or listed in the IUCN Red List of Threatened Species, dubious taxonomic changes may produce loopholes, where species remain protected according to the rules of these lists, but are not recognized by enforcement agencies. Other areas of particular concern include clinical toxinology, especially the production and use of antivenoms as treatment for the bites of venomous snakes (Fry et al. 2003; Williams et al. 2011; Wüster and McCarthy 1996). In the case of clinical toxinology this may literally be a matter of life and death, when name changes spread via media outlets by attention-seeking authors may cause uncertainty among medical personnel as to which antivenom is appropriate in cases where the name of the source snake species has changed (Sutherland 1999). Wholesale nomenclatural changes at the genus level, especially among medically important snakes, must be carefully considered (even when taxonomically justified) because of the confusion that can arise when the names of relevant species become inconsistent with the names quoted on antivenom products.

Science and the Public.—The public perception of and trust in science is eroded when decisions lacking evidence are presented as fact and permeate what is assumed to be a scientific discourse. The often-strident tone of exchanges surrounding unethical and unscientific taxonomic acts (Borrell 2007) further diminishes the entire scientific discipline in the eyes of the public. In cases where unethical behavior involves illegal activities, international relations, or other similarly sensitive dynamics, the resulting backlash can make it more difficult to conduct *bona fide* research even when good science is demonstrably needed for initiatives such as biodiversity management and conservation.

UNSCIENTIFIC TAXONOMY, EMBODIED

We here present two cases to illustrate unscientific practice. These stand out in the herpetological discipline by the sheer number of taxonomic proposals presented, and the manner in which the authors use the *Code* in contravention of the spirit, if not the letter, of the rules. We use these examples as the departure point for a more general discussion of the scientific

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10 POINTS OF VIEW

TABLE 1. List of herpetofaunal taxa published on or after 1 January 2000 that can be objectively classed as unscientific, non-peer reviewed, misguided in intent or presentation, fraudulent, or lacking evidence. These names *should not be used* in herpetological nomenclature, pending suitable action by the ICZN. Instead, we urge that these names be treated as listed in the column titled Recommendations by reverting to the older name of record, or by another suitable name as indicated. To avoid confusion, in the Recommendation column we list subgenera in parentheses along with the genus name according to standard nomenclatural usage. All other capitalized, italicized names are genera. Where these recommendations are based on previously published taxonomic decisions or errors, citations and explanations are referenced as superscripts and listed at the end of the table. 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.

Taxon	Taxon Level	Citation	Recommendation
<i>Abilenea</i>	gen. nov.	Wells 2007c	<i>Aprasia</i>
Acanthophiina	subtrib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
<i>Acanthophis antarcticus cliffrosswellingtoni</i>	ssp. nov.	Hoser 2002b	<i>Acanthophis antarcticus</i>
<i>Acanthophis groenveldi</i>	sp. nov.	Hoser 2002b	<i>Acanthophis laevis</i>
<i>Acanthophis macgregori</i>	sp. nov.	Hoser 2002b	<i>Acanthophis laevis</i>
<i>Acanthophis wellsei donnellani</i>	ssp. nov.	Hoser 2002b	<i>Acanthophis wellsi</i>
<i>Acanthophis yuwoni</i>	sp. nov.	Hoser 2002b	<i>Acanthophis laevis</i>
<i>Acetyphlops</i>	subgen. nov.	Hoser 2012as	<i>Typhlops</i>
<i>Adelynhoserea</i>	gen. nov.	Hoser 2012o	<i>Trimeresurus</i>
<i>Adelynhoserserpenae</i>	gen. nov.	Hoser 2012c	<i>Atropoides</i>
Adelynhoserserpenina	subtrib. nov.	Hoser 2012d	Viperidae, Crotalinae
Adelynhoserserpinini	trib. nov.	Hoser 2012d	Viperidae, Crotalinae
<i>Adelynkimberleyea</i>	gen. nov.	Hoser 2012ao	<i>Laudakia</i>
<i>Adrasteia</i>	gen. nov.	Wells 2002f	<i>Lampropholis</i>
<i>Adrasteiascincus</i>	nom. nov.	Wells 2010	<i>Lampropholis</i>
<i>Agamatajikistanensis</i>	subgen. nov.	Hoser 2012ao	<i>Laudakia</i>
Agkistrodonini	trib. nov.	Hoser 2012d	Viperidae, Crotalinae
<i>Agressiserpens</i>	gen. nov.	Wells 2002d	<i>Acanthophis</i>
Aipysurini	trib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
<i>Aiselfakhariis</i>	gen. nov.	Hoser 2012am	<i>Salvadora</i>
<i>Alanbrygelus</i>	subgen. nov.	Hoser 2012ah	<i>Tropidonophis</i>
<i>Alcisius</i>	gen. nov.	Wells 2012	<i>Lerista</i>
<i>Alexteesus</i>	gen. nov.	Hoser 2012ai	<i>Rhadinaea</i>
<i>Allengreerus</i>	gen. nov.	Hoser 2009b	<i>Lampropholis</i>
<i>Allengreerus delicata jackyhoserae</i>	ssp. nov.	Hoser 2012ab	<i>Lampropholis delicata</i>
<i>Allengreerus ronhoseri</i>	sp. nov.	Hoser 2009b	<i>Lampropholis delicata</i>
<i>Altmantyplops</i>	gen. nov.	Hoser 2012as	<i>Typhlops</i>
<i>Altmantyplops (Goldsteintyplops) kirnerae</i>	sp. nov.	Hoser 2012as	<i>Typhlops brongersmanus</i>
<i>Altmantyplops (Goldsteintyplops) kirnerae wellingtoni</i>	ssp. nov.	Hoser 2012as	<i>Typhlops brongersmanus</i>
<i>Altmantyplops reticulatus wellsi</i>	ssp. nov.	Hoser 2012as	<i>Typhlops reticulatus</i>
Anelytropsinae	subfam. nov.	Hoser 2012ar	Dibamidae
Anomalepididoidea	superfam. nov.	Hoser 2012as	Anomalepididae
Anomalepiini	trib. nov.	Hoser 2012as	Anomalepididae
<i>Antaresia maculosus brentonoloughlini</i>	ssp. nov.	Hoser 2004	<i>Antaresia maculosa</i> ²
<i>Antaresia saxacola campbelli</i>	ssp. nov.	Hoser 2000a	<i>Antaresia stimsoni</i>
Antaresiina	subtrib. nov.	Hoser 2012b	<i>Antaresia</i>
<i>Aphroditia</i>	gen. nov.	Wells 2012	<i>Lerista</i>
Argyophiini	trib. nov.	Hoser 2012as	<i>Argyophis</i>
<i>Arnoldtyplops</i>	gen. nov.	Hoser 2012as	<i>Typhlops</i>
<i>Asianatrix</i>	gen. nov.	Hoser 2012ah	<i>Amphiesma</i>
<i>Aspidites melanocephalus adelymensis</i>	ssp. nov.	Hoser 2000a	<i>Aspidites melanocephalus</i> ²
<i>Aspidites melanocephalus davieii</i>	ssp. nov.	Hoser 2000a	<i>Aspidites melanocephalus</i> ²
<i>Aspidites melanocephalus rickjonesii</i>	ssp. nov.	Hoser 2009a	<i>Aspidites melanocephalus</i> ²
<i>Aspidites ramsayi neildavieii</i>	ssp. nov.	Hoser 2009a	<i>Aspidites ramsayi</i> ²
<i>Aspidites ramsayi panoptes</i>	ssp. nov.	Hoser 2000a	<i>Aspidites ramsayi</i> ²
<i>Aspidites ramsayi richardjonesi</i>	ssp. nov.	Hoser 2000a	<i>Aspidites ramsayi</i> ²
Aspiditesina	subtrib. nov.	Hoser 2012b	<i>Aspidites</i>
Aspidomorphina	subtrib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
Atractaspini	trib. nov.	Hoser 2012l	<i>Atractaspis</i>
<i>Australiasis funki</i>	sp. nov.	Hoser 2012b	<i>Morelia amethystina</i>
<i>Barrygoldsmithus</i>	gen. nov.	Hoser 2012ai	<i>Rhadinaea</i>
<i>Billmacordus</i>	gen. nov.	Hoser 2012as	<i>Gerrhopilus</i>
<i>Binghamus</i>	subgen. nov.	Hoser 2012f	<i>Micrurus</i>

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TABLE 1. Continued

Taxon	Taxon Level	Citation	Recommendation
Bitisini	trib. nov.	Hoser 2012d	Viperidae, Viperinae
<i>Bobbottomus</i>	gen. nov.	Hoser 2012as	<i>Leptotyphlops parkeri</i>
Bothriechisina	subtrib. nov.	Hoser 2012d	Viperidae, Crotalinae
Bothrocophiina	subtrib. nov.	Hoser 2012d	Viperidae, Crotalinae
Bothropina	subtrib. nov.	Hoser 2012d	Viperidae, Crotalinae
Bothropoidina	subtrib. nov.	Hoser 2012d	Viperidae, Crotalinae
Brogammerini	trib. nov.	Hoser 2012b	Pythonidae
<i>Brogammerus</i>	gen. nov.	Hoser 2004	<i>Python</i>
<i>Brogammerus reticulatus dalegibbonsi</i>	ssp. nov.	Hoser 2004	<i>Python reticulatus reticulatus</i> ²
<i>Brogammerus reticulatus euaneawardsi</i>	ssp. nov.	Hoser 2004	<i>Python reticulatus reticulatus</i> ²
<i>Brogammerus reticulatus haydenmacphiei</i>	ssp. nov.	Hoser 2004	<i>Python reticulatus reticulatus</i> ²
<i>Brogammerus reticulatus neilsonnemani</i>	ssp. nov.	Hoser 2004	<i>Python reticulatus reticulatus</i> ²
<i>Brogammerus reticulatus patrickcouperi</i>	ssp. nov.	Hoser 2004	<i>Python reticulatus reticulatus</i> ²
<i>Brogammerus reticulatus stuartbigmorei</i>	ssp. nov.	Hoser 2004	<i>Python reticulatus reticulatus</i> ²
<i>Brucerogersus</i>	gen. nov.	Hoser 2012y	<i>Thamnophis</i>
Calloselasma	trib. nov.	Hoser 2012d	Viperidae, Crotalinae
<i>Cannia australis aplini</i>	ssp. nov.	Hoser 2001	<i>Pseudechis australis</i>
<i>Cannia australis burgesi</i>	ssp. nov.	Hoser 2001	<i>Pseudechis australis</i>
<i>Cannia australis newmani</i>	ssp. nov.	Hoser 2001	<i>Pseudechis australis</i>
<i>Carettochelys insculpta canni</i>	ssp. nov.	Wells 2002a	<i>Carettochelys insculpta</i>
<i>Carrytyphlopea</i>	gen. nov.	Hoser 2012as	<i>Ramphotyphlops</i>
Cerastini	trib. nov.	Hoser 2012d	Viperidae, Viperinae
Cerrophidionina	subtrib. nov.	Hoser 2012d	Viperidae, Crotalinae
<i>Charlespiersonserpens</i>	gen. nov.	Hoser 2012ac	<i>Dendrelaphis</i>
<i>Charlespiersonserpens (Downieea) papuensis lizeliottae</i>	ssp. nov.	Hoser 2012ac	<i>Dendrelaphis papuensis</i>
<i>Charlespiersonserpens (Macmillanus) jackyhoserae</i>	sp. nov.	Hoser 2012ac	<i>Dendrelaphis lorentzi</i>
<i>Charlespiersonserpens gastrostictus tyeipperae</i>	ssp. nov.	Hoser 2012ac	<i>Dendrelaphis gastrostictus</i>
<i>Chlamydosaurus kingii mickpughi</i>	ssp. nov.	Hoser 2012ap	<i>Chlamydosaurus kingii</i>
<i>Chlamydosaurus kingii pughae</i>	ssp. nov.	Hoser 2012ap	<i>Chlamydosaurus kingii</i>
<i>Chondropython viridis adelynhoserae</i>	ssp. nov.	Hoser 2009a	<i>Morelia viridis</i> ²
<i>Chondropython viridis shireenae</i>	ssp. nov.	Hoser 2004	<i>Morelia viridis</i> ²
<i>Coniophanes</i>	subgen. nov.	Hoser 2012aj	<i>Coniophanes</i> ¹
<i>Copelandtyphlops</i>	subgen. nov.	Hoser 2012as	<i>Typhlops</i>
<i>Costinisauria couperi</i>	sp. nov.	Wells 2009b	<i>Lampropholis couperi</i>
<i>Cottonkukri</i>	gen. nov.	Hoser 2012ag	<i>Oligodon</i>
<i>Cottonserpens</i>	subgen. nov.	Hoser 2012aj	<i>Coniophanes</i>
Cottontyphlopini	trib. nov.	Hoser 2012as	<i>Letheobia</i>
<i>Cottontyphlops</i>	gen. nov.	Hoser 2012as	<i>Letheobia</i>
<i>Cottonus</i>	subgen. nov.	Hoser 2009d	<i>Crotalus</i> ^{3,4}
<i>Crishagenus</i>	gen. nov.	Hoser 2012as	<i>Epictia</i>
Crocodylini	trib. nov.	Hoser 2012an	Crocodylidae
<i>Crossmanus</i>	subgen. nov.	Hoser 2012x	<i>Leptodeira</i>
<i>Crotalina</i>	subtrib. nov.	Hoser 2012d	Viperidae, Crotalinae
<i>Crotykukrius</i>	subgen. nov.	Hoser 2012ag	<i>Oligodon</i>
<i>Crotyreedus</i>	gen. nov.	Hoser 2012ak	<i>Calamaria</i>
Crotytyphlopini	trib. nov.	Hoser 2012as	Typhlopidae
<i>Crotytyphlops</i>	gen. nov.	Hoser 2012as	<i>Typhlops</i>
<i>Crutchfieldus</i>	subgen. nov.	Hoser 2009d	<i>Crotalus</i> ^{3,4}
<i>Cryptophis edwardsi</i>	sp. nov.	Hoser 2012ad	<i>Cryptophis nigrescens</i>
<i>Cummingea</i>	gen. nov.	Hoser 2009d	<i>Crotalus</i> ^{3,4}
<i>Cybelia</i>	gen. nov.	Wells 2012	<i>Lerista</i>
Cyclotyphlopini	trib. nov.	Hoser 2012as	<i>Cyclotyphlops</i>
Cyrilhosserini	trib. nov.	Hoser 2012as	<i>Gerrhopilus</i>
<i>Cyrilhosserus</i>	gen. nov.	Hoser 2012as	<i>Gerrhopilus</i>
<i>Cyrtodactylus abrae</i>	sp. nov.	Wells 2002c	<i>Cyrtodactylus tuberculatus</i>
<i>Dalegibbonsus</i>	gen. nov.	Hoser 2012ar	<i>Dibamus</i>
<i>Dannylefakharikukri</i>	gen. nov.	Hoser 2012ag	<i>Oligodon</i>
<i>Dannyleus</i>	subgen. nov.	Hoser 2012q	<i>Paras</i>
<i>Dannityphlops</i>	subgen. nov.	Hoser 2012as	<i>Typhlops</i>
<i>Daraninserpens</i>	subgen. nov.	Hoser 2012aj	<i>Coniophanes</i>

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TABLE 1. Continued

Taxon	Taxon Level	Citation	Recommendation
<i>Darainus</i>	subgen. nov.	Hoser 2012g	<i>Bothrops</i>
<i>Daviekukri</i>	gen. nov.	Hoser 2012ag	<i>Oligodon</i>
Demansiini	trib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
Dendroaspini	trib. nov.	Hoser 2012e	Elapidae, Elapinae
Denisonini	trib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
<i>Desburkeus</i>	subgen. nov.	Hoser 2012ah	<i>Tropidonophis</i>
<i>Desmondburkeus</i>	subgen. nov.	Hoser 2012ai	<i>Rhadinaea</i>
Dibaminae	subfam. nov.	Hoser 2012ar	Dibamidae
<i>Dorissius</i>	gen. nov.	Hoser 2012ac	<i>Boiga</i>
<i>Downitea</i>	subgen. nov.	Hoser 2012ac	<i>Dendrelaphis</i>
<i>Dudleyserpens</i>	subgen. nov.	Hoser 2012ai	<i>Rhadinaea</i>
<i>Dugitophis</i>	gen. nov.	Wells 2002e	<i>Pseudonaja</i>
Echiini	trib. nov.	Hoser 2012d	Viperidae, Crotalinae
<i>Echiopsis curta martinekae</i>	ssp. nov.	Hoser 2012ad	<i>Echiopsis curta</i>
<i>Edwardstyplops</i>	gen. nov.	Hoser 2012as	<i>Typlops</i>
<i>Edwardsus</i>	subgen. nov.	Hoser 2009d	<i>Crotalus</i> ^{3,4}
<i>Eippertyphloea</i>	gen. nov.	Hoser 2012as	<i>Typlops</i>
<i>Eipperus</i>	gen. nov.	Hoser 2012u	<i>Psammophis</i>
<i>Eksteinus</i>	gen. nov.	Hoser 2012z	<i>Lampropeltis</i>
Elapsoidini	trib. nov.	Hoser 2012e	Elapidae, Elapinae
<i>Elfakhariscincus</i>	gen. nov.	Hoser 2012aq	<i>Chalcides</i>
<i>Elliottmatrix</i>	gen. nov.	Hoser 2012ah	<i>Amphiesma</i>
<i>Elliotttyphloea</i>	gen. nov.	Hoser 2012as	<i>Typlops</i>
<i>Elliottus</i>	subgen. nov.	Hoser 2012u	<i>Psammophis</i>
<i>Elseya dorriani</i>	sp. nov.	Wells 2002b	<i>Myuchelys bellii</i>
<i>Elseya jukesii</i>	sp. nov.	Wells 2002b	<i>Elseya dentata</i>
Ephalophina	subtrib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
Eristicophina	subtrib. nov.	Hoser 2012d	Viperidae, Viperinae
<i>Euanedwardsserpens</i>	gen. nov.	Hoser 2012p	<i>Coelognathus</i>
<i>Evanwhittonus</i>	gen. nov.	Hoser 2012as	<i>Rena</i>
<i>Freudreedus</i>	subgen. nov.	Hoser 2012ak	<i>Calamaria</i>
<i>Freudtyplops</i>	gen. nov.	Hoser 2012as	<i>Typlops</i>
<i>Funkelapidus</i>	gen. nov.	Hoser 2012n	<i>Sinomicrurus</i>
<i>Funkikukri</i>	gen. nov.	Hoser 2012ag	<i>Oligodon</i>
<i>Funkityphlops</i>	gen. nov.	Hoser 2012as	<i>Ramphotyphlops</i>
<i>Funkus</i>	gen. nov.	Hoser 2012h	<i>Nerodia</i>
Furiniini	trib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
<i>Gaia</i>	gen. nov.	Wells 2012	<i>Lerista</i>
Gavialini	trib. nov.	Hoser 2012an	Crocodylidae
<i>Geddykukrius</i>	subgen. nov.	Hoser 2012ag	<i>Oligodon</i>
Gerrhopilidini	trib. nov.	Hoser 2012as	Gerrhopilidae
<i>Gerrhopilus carolinehoserae</i>	sp. nov.	Hoser 2012as	<i>Gerrhopilus hedraeus</i>
<i>Ginafabaserpenae</i>	gen. nov.	Hoser 2012x	<i>Leptodeira</i>
<i>Gleesontyphlops</i>	gen. nov.	Hoser 2012as	<i>Letheobia</i>
<i>Goldneyia</i>	gen. nov.	Wells 2012	<i>Lerista</i>
<i>Goldsteintyphlops</i>	subgen. nov.	Hoser 2012as	<i>Typlops</i>
<i>Greenatrix</i>	gen. nov.	Hoser 2012ah	<i>Amphiesma</i>
<i>Gregshwedoshus</i>	gen. nov.	Hoser 2012y	<i>Thamnophis</i>
Gryptotyphlopidini	trib. nov.	Hoser 2012as	<i>Letheobia</i>
<i>Guystebbinsus</i>	gen. nov.	Hoser 2012aa	<i>Natrix</i>
<i>Harrigankukriae</i>	subgen. nov.	Hoser 2012ag	<i>Oligodon</i>
<i>Hawkeswoodus</i>	subgen. nov.	Hoser 2012as	<i>Liotyphlops</i>
<i>Helioscincus</i>	gen. nov.	Wells 2002f	<i>Lampropholis</i>
Helminthophiini	trib. nov.	Hoser 2012as	Anomalepidae
Hemachatusina	subtrib. nov.	Hoser 2012e	Elapidae, Elapinae
Hemiaspini	trib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
Homoroselapidae	fam. nov.	Hoser 2012e	<i>Homoroselaps</i>
Homoroselapiini	trib. nov.	Hoser 2012e	<i>Homoroselaps</i>
Hoplocephalina	subtrib. nov.	Hoser 2012e	Elapidae, Hydrophiinae

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Taxon	Taxon Level	Citation	Recommendation
<i>Hoseraspea</i>	gen. nov.	Hoser 2012l	<i>Atractaspis</i>
Hoseraspini	subtrib. nov.	Hoser 2012l	<i>Atractaspis</i>
<i>Hoserea</i>	gen. nov.	Hoser 2009d	<i>Crotalus</i> ^{5,4}
<i>Hoserelapidea</i>	gen. nov.	Hoser 2012f	<i>Micrurus</i>
<i>Hoserelapidea</i>	subgen. nov.	Hoser 2012f	<i>Micrurus</i>
<i>Hoserkukriae</i>	gen. nov.	Hoser 2012ag	<i>Oligodon</i>
<i>Hugheskukri</i>	gen. nov.	Hoser 2012ag	<i>Oligodon</i>
<i>Hulimkai</i>	gen. nov.	Hoser 2012i	<i>Suta</i>
Hulimkini	trib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
Hydrelapini	trib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
Hydrophiina	subtrib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
<i>Jackyhosea</i>	gen. nov.	Hoser 2012g	<i>Bothrops</i>
Jackyhoserina	subtrib. nov.	Hoser 2012d	Viperidae, Crotalinae
Jackyhoserini	trib. nov.	Hoser 2012d	Viperidae, Crotalinae
<i>Jackyhosernatrix</i>	gen. nov.	Hoser 2012aa	<i>Natrix</i>
<i>Jackyindigoea</i>	gen. nov.	Hoser 2012ao	<i>Laudakia</i>
<i>Jackypython</i>	subgen. nov.	Hoser 2009a	<i>Morelia</i> ²
<i>Jacobclarkus</i>	subgen. nov.	Hoser 2012af	<i>Lycophidion</i>
<i>Jockpaullus</i>	subgen. nov.	Hoser 2012ai	<i>Rhadinaea</i>
<i>Johnwilsonityphlops</i>	gen. nov.	Hoser 2012as	<i>Ramphotyphlops</i>
<i>Judywhybrowea</i>	subgen. nov.	Hoser 2012as	<i>Typhlops</i>
<i>Karimdaoesus</i>	gen. nov.	Hoser 2012as	<i>Leptotyphlops</i>
<i>Karma</i>	gen. nov.	Wells 2009b	<i>Eulamprus</i>
<i>Katrinahosea</i>	gen. nov.	Hoser 2012r	<i>Rhadinophis</i>
<i>Katrinahoserserpenea</i>	gen. nov.	Hoser 2012q	<i>Pareas</i>
Katrinahosertyphlopini	trib. nov.	Hoser 2012as	Typhlopidae
<i>Katrinahosertyphlops</i>	gen. nov.	Hoser 2012as	<i>Typhlops</i>
Katrinina	subtrib. nov.	Hoser 2012b	Pythonidae, Moreliini
<i>Katrinus</i>	gen. nov.	Hoser 2000a	<i>Liasis</i> ²
<i>Katrinus fuscus jackyae</i>	ssp. nov.	Hoser 2004	<i>Liasis fuscus</i> ²
<i>Kirnerea</i>	subgen. nov.	Hoser 2012ah	<i>Tropidonophis</i>
<i>Kraussus</i>	subgen. nov.	Hoser 2012as	<i>Liotyphlops</i>
<i>Krishna</i>	gen. nov.	Wells 2012	<i>Lerista</i>
<i>Laidlawserpens</i>	subgen. nov.	Hoser 2012aj	<i>Contiophanes</i>
<i>Laidlawityphlops</i>	gen. nov.	Hoser 2012as	<i>Letheobia</i>
<i>Laidlawus</i>	subgen. nov.	Hoser 2012k	<i>Macrovipera</i>
<i>Leiopython albertisi barkeri</i>	ssp. nov.	Hoser 2000a	<i>Leiopython albertisi</i> ^{2,5}
<i>Leiopython albertisi barkerorum</i>	ssp. nov.	Hoser 2009a	<i>Leiopython albertisi</i> ^f
<i>Leiopython albertisi bennetti</i>	ssp. nov.	Hoser 2000a	<i>Leiopython benettorum</i> ^{2,5}
<i>Leiopython hoseae</i>	sp. nov.	Hoser 2000a	<i>Leiopython hoseae</i> ^{2,5}
Lenhosertyphlopini	trib. nov.	Hoser 2012as	<i>Typhlops</i>
<i>Lenhosertyphlops</i>	gen. nov.	Hoser 2012as	<i>Typhlops</i>
<i>Lenhoserus</i>	gen. nov.	Hoser 2000a	<i>Morelia</i> ²
Leptotyphlopoidea	superfam. nov.	Hoser 2012as	Leptotyphlopidae
<i>Leswilliamsus</i>	gen. nov.	Hoser 2012ar	<i>Dibamus</i>
Libertadictiini	trib. nov.	Hoser 2012as	Typhlopidae
<i>Lokisaurus</i>	gen. nov.	Wells 2012	<i>Lerista</i>
<i>Longinidis</i>	subgen. nov.	Hoser 2012as	<i>Myriopholis</i>
Loveridgelapina	subtrib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
<i>Lukefabaserpens</i>	gen. nov.	Hoser 2012x	<i>Leptodeira</i>
<i>Maciachlanus</i>	gen. nov.	Hoser 2012x	<i>Imantodes</i>
<i>Macmillanus</i>	subgen. nov.	Hoser 2012ac	<i>Dendrelaphis</i>
<i>Maconchieus</i>	gen. nov.	Hoser 2012x	<i>Imantodes</i>
<i>Macphieus</i>	subgen. nov.	Hoser 2012as	<i>Anomalepis</i>
<i>Magnellia</i>	gen. nov.	Wells 2009b	<i>Eulamprus</i>
<i>Mariolitus</i>	gen. nov.	Hoser 2012h	<i>Regina</i>
<i>Marrunisauria</i>	gen. nov.	Wells 2012	<i>Lerista</i>
<i>Martinekea</i>	gen. nov.	Hoser 2012m	<i>Orthriophis</i>
Martinwellstyplopini	trib. nov.	Hoser 2012as	<i>Acutotyphlops</i>
<i>Martinwellstyplops</i>	gen. nov.	Hoser 2012as	<i>Acutotyphlops</i>

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Taxon	Taxon Level	Citation	Recommendation
Maticorini	trib. nov.	Hoser 2012e	Elapidae, Elapinae
Matteoa	gen. nov.	Hoser 2009d	<i>Crotalus</i> ^{3,4}
Maxhoserboa	subgen. nov.	Hoser 2012w	<i>Eunectes</i>
Maxhoserini	trib. nov.	Hoser 2012as	<i>Ramphotyphlops</i>
Maxhoserus	gen. nov.	Hoser 2012as	<i>Ramphotyphlops</i>
Maxhoservipera	gen. nov.	Hoser 2012k	<i>Daboia</i>
Maxhoserviperina	subtrib. nov.	Hoser 2012d	Viperidae, Viperinae
Mecistopsini	trib. nov.	Hoser 2012an	Crocodylidae
Michaelnicholsus	subgen. nov.	Hoser 2012t	<i>Leioheterodon</i>
Micropechiina	subtrib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
Micropechiini	trib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
Morelia harrisoni	sp. nov.	Hoser 2000a	<i>Morelia spilota harrisoni</i> ²
Morelia macburniei	sp. nov.	Hoser 2004	<i>Morelia spilota imbricata</i> ²
Morelia mippughae	sp. nov.	Hoser 2004	<i>Morelia spilota</i> ²
Morelia wellsii	sp. nov.	Hoser 2012b	<i>Morelia spilota</i>
Moreliina	subtrib. nov.	Hoser 2012b	Pythonidae, Moreliini
Moseselfakharikukri	gen. nov.	Hoser 2012ag	<i>Oligodon</i>
Mosestyphlops	subgen. nov.	Hoser 2012as	<i>Typhlops</i>
Mullinsus	subgen. nov.	Hoser 2009d	<i>Crotalus</i> ^{3,4}
Mulwanyus	gen. nov.	Hoser 2012ac	<i>Boiga</i>
Najina	subtrib. nov.	Hoser 2012e	Elapidae, Elapinae, Najini ⁶
Ndurascincus	gen. nov.	Wells 2002f	<i>Lampropholis</i>
Neilsimpsonus	subgen. nov.	Hoser 2012x	<i>Imantodes</i>
Neilsonnemanus	subgen. nov.	Hoser 2012y	<i>Thamnophis</i>
Nindibamus	subgen. nov.	Hoser 2012ar	<i>Dibamus</i>
Ninkukri	gen. nov.	Hoser 2012ag	<i>Oligodon</i>
Nintyphlops	gen. nov.	Hoser 2012as	<i>Typhlops</i>
Notechiina	subtrib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
Notopseudonaja	gen. nov.	Wells 2002e	<i>Pseudonaja</i>
Notopseudonajini	trib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
Oceanius	gen. nov.	Wells 2007d	<i>Aipysurus</i>
Oopholis (Philas) adelynhoserae	sp. nov.	Hoser 2012an	<i>Crocodylus novaeguineae</i>
Oopholis (Philas) jackyhoserae	sp. nov.	Hoser 2012an	<i>Crocodylus johnsoni</i>
Ophiophagini	trib. nov.	Hoser 2012e	Elapidae, Elapinae
Ottobreus	subgen. nov.	Hoser 2012as	<i>Leptotyphlops</i>
Oxycrocodylus	gen. nov.	Hoser 2012an	<i>Crocodylus</i>
Oxykukrius	gen. nov.	Hoser 2012ag	<i>Oligodon</i>
Oxynatrix	gen. nov.	Hoser 2012ah	<i>Tropidonophis</i>
Oxynatrix	subgen. nov.	Hoser 2012ah	<i>Tropidonophis</i>
Oxyreedus	subgen. nov.	Hoser 2012ak	<i>Calamaria</i>
Oxytyphlops	gen. nov.	Hoser 2012as	<i>Ramphotyphlops</i>
Oxyuranini	trib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
Oxyuranus scutellatus adelynhoserae	ssp. nov.	Hoser 2009c	<i>Oxyuranus scutellatus canni</i>
Oxyuranus scutellatus andrewwilsoni	ssp. nov.	Hoser 2009c	<i>Oxyuranus scutellatus scutellatus</i>
Oxyuranus scutellatus barringeri	ssp. nov.	Hoser 2002a	<i>Oxyuranus scutellatus scutellatus</i>
Oxyus	gen. nov.	Hoser 2012j	<i>Trimeresurus</i>
Pailsus rosignolii	sp. nov.	Hoser 2000b	<i>Pseudechis rosignolii</i>
Panacedechis papuanus trevorhawkeswoodi	ssp. nov.	Hoser 2009c	<i>Pseudechis papuanus</i>
Parahydrophina	subtrib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
Parapistocalamini	trib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
Paulwoolfinae	subfam. nov.	Hoser 2012ar	Dibamidae
Paulwoolfus	gen. nov.	Hoser 2012ar	<i>Dibamus</i>
Pelamiidae	fam. nov.	Wells 2007d	Elapidae, Hydrophiinae
Pelamiina	subtrib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
Piersonina	subtrib. nov.	Hoser 2012d	Viperidae, Crotalinae
Piersontyphlops	gen. nov.	Hoser 2012as	<i>Ramphotyphlops</i>
Piersonus	gen. nov.	Hoser 2009d	<i>Crotalus</i> ^{3,4}
Pillottyphlops	gen. nov.	Hoser 2012as	<i>Typhlops</i>
Pillotus	subgen. nov.	Hoser 2009d	<i>Crotalus</i> ^{3,4}
Placidaserpens	gen. nov.	Wells 2002e	<i>Pseudonaja</i>

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TABLE 1. Continued

Taxon	Taxon Level	Citation	Recommendation
<i>Plumridgeus</i>	gen. nov.	Hoser 2012af	<i>Aparallactus</i>
<i>Porthidiumina</i>	subtrib. nov.	Hoser 2012d	Viperidae, Crotalinae
<i>Proatherini</i>	trib. nov.	Hoser 2012d	Viperidae, Viperinae
<i>Pseudechini</i>	trib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
<i>Pseudechis porphyriacus eipperri</i>	ssp. nov.	Hoser 2003d	<i>Pseudechis porphyriacus</i>
<i>Pseudechis porphyriacus rentoni</i>	ssp. nov.	Hoser 2003d	<i>Pseudechis porphyriacus</i>
<i>Pseudocerastina</i>	subtrib. nov.	Hoser 2012d	Viperidae, Viperinae
<i>Pseudocerastini</i>	trib. nov.	Hoser 2012d	Viperidae, Viperinae
<i>Pseudonaja affinis charlespiersoni</i>	ssp. nov.	Hoser 2009c	<i>Pseudonaja affinis</i>
<i>Pseudonaja ellioti</i>	sp. nov.	Hoser 2003c	<i>Pseudonaja textilis</i>
<i>Pseudonaja gowi</i>	sp. nov.	Wells 2002e	<i>Pseudonaja aspidorhyncha</i>
<i>Pseudonaja guttata whybrowi</i>	ssp. nov.	Hoser 2009c	<i>Pseudonaja guttata</i>
<i>Pseudonaja textilis clivevatti</i>	ssp. nov.	Hoser 2009c	<i>Pseudonaja textilis</i>
<i>Pseudonaja textilis jackyhoserae</i>	ssp. nov.	Hoser 2009c	<i>Pseudonaja textilis</i>
<i>Pseudonaja textilis leswilliamsi</i>	ssp. nov.	Hoser 2009c	<i>Pseudonaja textilis</i>
<i>Pseudonaja textilis pughii</i>	ssp. nov.	Hoser 2003a	<i>Pseudonaja textilis</i>
<i>Pseudonaja textilis rollinsoni</i>	ssp. nov.	Hoser 2009c	<i>Pseudonaja textilis</i>
<i>Pseudonajini</i>	trib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
<i>Pughus</i>	subgen. nov.	Hoser 2012y	<i>Thamnophis</i>
<i>Ramphotyphlops</i> [sic]	trib. nov.	Hoser 2012as	<i>Ramphotyphlops</i>
<i>Rattlewellsus</i>	gen. nov.	Hoser 2012f	<i>Crotalus</i>
<i>Rawlingspython</i>	subgen. nov.	Hoser 2009a	<i>Antaresia</i>
<i>Rayhammondus</i>	subgen. nov.	Hoser 2012u	<i>Psammophis</i>
<i>Rentontyphlops</i>	gen. nov.	Hoser 2012as	<i>Typhlops</i>
<i>Rentonus</i>	gen. nov.	Hoser 2012ac	<i>Crotalus</i>
<i>Rhiannodon</i>	gen. nov.	Wells 2009b	<i>Glaphyromorphus</i>
<i>Rhinocerophiina</i>	subtrib. nov.	Hoser 2012d	Viperidae, Crotalinae
<i>Richardwellsus</i>	gen. nov.	Hoser 2012m	<i>Zamenis</i>
<i>Robvalenticus</i>	gen. nov.	Hoser 2012ai	<i>Rhadinaea</i>
<i>Rolyburrellus</i>	subgen. nov.	Hoser 2012as	<i>Typhlops</i>
<i>Ronhoserini</i>	trib. nov.	Hoser 2012as	Typhlopidae
<i>Ronhoserus</i>	gen. nov.	Hoser 2012as	<i>Typhlops</i>
<i>Sammykukriae</i>	subgen. nov.	Hoser 2012ag	<i>Oligodon</i>
<i>Sayersus</i>	subgen. nov.	Hoser 2009d	<i>Crotalus</i> ^{5,4}
<i>Scanlonus</i>	gen. nov.	Hoser 2012as	<i>Myriopholis</i>
<i>Sharonhoserea</i>	gen. nov.	Hoser 2012aa	<i>Coronella</i>
<i>Shirenhoserus</i>	gen. nov.	Hoser 2004	<i>Python</i> ²
<i>Simoselapini</i>	trib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
<i>Sinoelaphe</i>	gen. nov.	Hoser 2012ae	<i>Euprepiohis</i>
<i>Slatteryus</i>	subgen. nov.	Hoser 2012u	<i>Psammophis</i>
<i>Smythkukri</i>	gen. nov.	Hoser 2012ag	<i>Oligodon</i>
<i>Smythserpens</i>	subgen. nov.	Hoser 2012aj	<i>Contophanes</i>
<i>Smythtyphlopini</i>	trib. nov.	Hoser 2012as	Typhlopidae
<i>Smythtyphlops</i>	gen. nov.	Hoser 2012as	<i>Letheobia</i>
<i>Smythus</i>	subgen. nov.	Hoser 2009d	<i>Crotalus</i> ^{5,4}
<i>Spectrascincus</i>	gen. nov.	Wells 2012	<i>Lerista</i>
<i>Spracklandus</i>	gen. nov.	Hoser 2009e	<i>Naja (Afronaja)</i> ⁸
<i>Stegonotus adelynhoserae</i>	sp. nov.	Hoser 2012s	<i>Stegonotus diehli</i>
<i>Stegonotus lenhoseri</i>	sp. nov.	Hoser 2012s	<i>Stegonotus modestus</i>
<i>Stegonotus sammacdownelli</i>	sp. nov.	Hoser 2012s	<i>Stegonotus parvus</i>
<i>Strophurus intermedius burrelli</i>	ssp. nov.	Hoser 2005	<i>Strophurus intermedius</i>
<i>Sundanatrix</i>	subgen. nov.	Hoser 2012ah	<i>Amphiesma</i>
<i>Sutini</i>	trib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
<i>Swileserpens</i>	gen. nov.	Hoser 2012t	<i>Buhome</i>
<i>Swiletyphlops</i>	gen. nov.	Hoser 2012as	<i>Afrotyphlops</i>
<i>Teesleptotyphlops</i>	subgen. nov.	Hoser 2012as	<i>Leptotyphlops</i>
<i>Toxicocalamina</i>	subtrib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
<i>Trimeresurusini</i>	trib. nov.	Hoser 2012d	Viperidae, Crotalinae
<i>Trioanotyphlops</i>	gen. nov.	Hoser 2012as	<i>Letheobia</i>
<i>Troianurus</i>	subgen. nov.	Hoser 2012f	<i>Micrurus</i>

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TABLE 1. Continued

Taxon	Taxon Level	Citation	Recommendation
<i>Tropidechis sadleri</i>	sp. nov.	Hoser 2003b	<i>Tropidechis carinatus</i>
Tropidolaemusini	trib. nov.	Hoser 2012d	Viperidae, Crotalinae
<i>Tropidonophis (Alanbrygelus) alanbrygeli</i>	sp. nov.	Hoser 2012ah	<i>Tropidonophis elongatus</i>
<i>Tropidonophis (Alanbrygelus) alanbrygeli sammywatsonae</i>	ssp. nov.	Hoser 2012ah	<i>Tropidonophis elongatus</i>
<i>Tropidonophis (Alanbrygelus) smythi</i>	sp. nov.	Hoser 2012ah	<i>Tropidonophis elongatus</i>
<i>Tropidonophis (Desburkeus) dikkoriae desburkei</i>	ssp. nov.	Hoser 2012ah	<i>Tropidonophis doriae</i>
<i>Tropidonophis multiscutellatus cottoni</i>	ssp. nov.	Hoser 2012ah	<i>Tropidonophis multiscutellatus</i>
<i>Tropidonophis novaeguineae trioani</i>	ssp. nov.	Hoser 2012ah	<i>Tropidonophis novaeguineae</i>
<i>Tropidonophis picturatus pillotti</i>	ssp. nov.	Hoser 2012ah	<i>Tropidonophis picturatus</i>
<i>Tychismia</i>	gen. nov.	Wells 2012	<i>Lerista</i>
Typhlophisini	trib. nov.	Hoser 2012as	<i>Typhlophis</i>
Typhlopini	trib. nov.	Hoser 2012as	<i>Typhlops</i>
<i>Unechis boschmai crutchfieldi</i>	ssp. nov.	Hoser 2012ad	<i>Unechis boschmai</i>
<i>Unechis durhami</i>	sp. nov.	Hoser 2012ad	<i>Unechis nigrostriatus</i>
Vermicellini	trib. nov.	Hoser 2012e	Elapidae, Hydrophiinae
<i>Walliserpens</i>	gen. nov.	Hoser 2012ai	<i>Rhadinaea</i>
<i>Wellingtonatrix</i>	gen. nov.	Hoser 2012ah	<i>Amphiesma</i>
<i>Wellsnatrix</i>	gen. nov.	Hoser 2012ah	<i>Amphiesma</i>
<i>Wellsus</i>	gen. nov.	Hoser 2009e	<i>Naja (Uraeus)</i> ⁵
<i>Whittonserpens</i>	gen. nov.	Hoser 2012aj	<i>Conopsis</i>
<i>Whybrowtyphlops</i>	gen. nov.	Hoser 2012as	<i>Letheobia</i>
<i>Whybrowtyphlops</i> [sic]	subgen. nov.	Hoser 2012as	<i>Letheobia</i>
<i>Whybrowus</i>	subgen. nov.	Hoser 2012y	<i>Thamnophis</i>
<i>Wilsontyphlops</i>	gen. nov.	Hoser 2012as	<i>Namibiana</i>
<i>Wollumbinia</i>	gen. nov.	Wells 2007a	<i>Myuchelys</i>
<i>Wollumbinia dorsti</i>	sp. nov.	Wells 2009a	<i>Myuchelys latisternum</i>
<i>Wondjina</i>	gen. nov.	Wells 2012	<i>Lerista</i>
<i>Woolftyphlops</i>	gen. nov.	Hoser 2012as	<i>Typhlops</i>
<i>Woolfivipera</i>	subgen. nov.	Hoser 2012v	<i>Atheris</i>
Xenotyphlopini	trib. nov.	Hoser 2012as	<i>Xenotyphlops</i>
<i>Yeomansus</i>	gen. nov.	Hoser 2012al	<i>Hierophis</i>
<i>Zeusius</i>	gen. nov.	Wells 2007b	<i>Cyclodomorphus</i>
<i>Zeusius melanops gillami</i>	ssp. nov.	Wells 2007b	<i>Cyclodomorphus m. melanops</i>
<i>Zeusius melanops swani</i>	ssp. nov.	Wells 2007b	<i>Cyclodomorphus m. elongatus</i>
<i>Zeusius sternfeldi</i>	sp. nov.	Wells 2007b	<i>Cyclodomorphus casuarinae</i>

¹ replacement for *Adrasteia* Wells 2002f
² Schleip and O'Shea (2010)
³ Wallach et al. (2009)
⁴ Wüster and Bérnills (2011)
⁵ Schleip (2008)
⁶ unjustified emendation of *Najini* Boulenger 1884
⁷ preoccupied by *Coniophanes* Hallowell 1861

standards that we think should be met for acceptable taxonomic studies (and their taxonomic conclusions).

Raymond Hoser's Private Taxonomy.—Between January 2000 and September 2012, Raymond Hoser named two superfamilies, one family, three subfamilies, 89 tribes and subtribes, 113 genera, 64 subgenera, 25 species, and 53 subspecies of reptiles, including Old and New World snakes, geckos, skinks, and crocodiles (Table 1). These names constitute 76% of genera and subgenera and 16% of species and subspecies newly proposed for snakes over that time period (Uetz 2012). Hoser's invariably single-authored papers are characterized by a lack of scientific rigor and plagued by a variety of other problems, including: (1) naming of putatively allopatric populations without primary evidence, but listing the current distribution as the sole or primary distinguishing character (e.g., the diagnosis of *Oxyuranus scutellatus barringeri*—Hoser 2002a:47); (2) invention of evidence (e.g., body color of *Oxyuranus scutellatus adelynhoserae* Hoser 2009c, based on a holotype that is actually an isolated head: BMNH 1992.542); (3) repeated description of the same taxon

as new (*Leiopython albertsi barkeri* Hoser 2000a = *L. a. barkerorum* Hoser 2009a = *L. a. barkerorum* Hoser 2012b; *Oxyuranus scutellatus barringeri* Hoser 2002a = *O. s. andrewwilsoni* Hoser 2009c); (4) descriptions of new species and subspecies based on morphological aberrations and vague differences in color pattern (e.g., *Acanthophis barnetti* Hoser 1998:24—diagnosed by the absence of raised supraoculars, which is merely an artifact of preservation [WW, pers. obs.], and “heavier dark pigmentation;” *Pseudonaja textilis cliveevatti* Hoser 2012i:38—diagnosed by stating that “each dorsal scale is darker brown tipped”); and (5) harvesting of clades from published phylogenetic studies for description as new genera or subgenera. For example, the division of *Natrix* into three monotypic genera (*Natrix*, *Jackyhosernatrix*, and *Guystebbinsus*) by Hoser (2012aa^[1]) stems from the

[1] Due to the large number of works produced by Raymond Hoser in 2012 (N = 45), we continued the enumeration of citations by beginning the alphabet anew. Thus, in addition to Hoser (2012a–z), 19 additional references exist (Hoser 2012aa–as).

recognition of an unsupported branch in Pyron et al. (2011). Even though the use of patronyms in the naming of taxa is not a contravention of the *Code*, Hoser does not coin and assign names for the purpose of scientific need, taxonomic clarity, or improved characterization of biodiversity, but rather for personal reasons, as explained by the author in most of his etymology sections, as well as in several Internet blogs and social media environments. Hoser's genus and species names are all patronyms, and many include the author's surname (N = 43; Table 1) or the names of his relatives, employees, and even pets.

Without exception, Hoser's taxonomic decisions have been published in outlets with evaluation processes that, if they exist, are not designed to safeguard scientific rigor. Most recently, Hoser (2009a–e, 2012a–ac) has published in the *Australasian Journal of Herpetology (AJH)*, a vehicle produced and mailed by Hoser himself, and primarily geared towards taxonomic articles of which he is the exclusive author and editor. Although the *AJH* masquerades as a scientific journal, it is perhaps better described as a printed “blog” because it lacks many of the hallmarks of formal scientific communication, and includes much irrelevant information (Ross et al. 2012). Examples of the latter include private email messages in their entirety, as well as polemics against taxonomic herpetologists (e.g., Hoser 2001:48–56; Hoser 2009a:3–21, 30; Hoser 2012a:1–34), taxonomic journals (*Zootaxa*; Hoser 2012a:15ff), wildlife officials (e.g., Hoser 2012f:12), and even judges in courts of law (e.g., Hoser 2012i:45). We maintain that *AJH* should not be considered a “public and permanent scientific record” and therefore fails a requirement of the *Code* (Art. 8.1.1; emphasis added) in both style and substance. The *AJH* is not a journal in the scientific sense. It is instead personally distributed by Hoser for unscientific purposes, and should therefore perhaps be best classified as advertising.

The Unscientific Taxonomic Contributions of Richard Wells.—The second case of taxonomic malpractice involves Richard Wells, who has a long history of producing scientifically controversial names, beginning with a near-wholesale alteration of the taxonomy of Australian amphibians and reptiles (Wells and Wellington 1983, 1985). Since 1 January 2000, Wells has described one family, 25 genera, seven species, and three subspecies of reptiles in a publication called *Australian Biodiversity Record*, which he alone edits and produces. Whereas some of the observations in these accounts relating to the natural history of particular taxa may qualify as scientific, the taxonomic decisions proposed by Wells (e.g., Wells 2000a–d) are without scientific merit. Like those published by Raymond Hoser, works by Wells follow the basic requirements of the *Code*, yet lack standard taxonomic data: new taxon names are supported by a diagnosis, but no justification is given for the necessity or authenticity of these names beyond the personal opinion of the author, which is often irreconcilable with published evidence (e.g., Wells 2007d). A failure to specify the material examined and a lack of comparisons with related specimens mean that the taxonomic decisions published by Wells are generally unsupported by well-established sources of evidence. This has resulted in the erection of genera based on characters with unsuitably high degrees of variation, as well as the naming of clinal variants as distinct species. In addition, type designations are often vague, precluding identification of the specimens upon which the names are based (e.g., “an adult specimen in the Australian Museum” in the case of both *Elseya jukesi* and *E. dorriani*; Wells 2002a:8). Furthermore, Wells is very active on blogs, where he has repeatedly threatened “taxonomic terrorism” should his proposals not be accepted by practicing

taxonomists. In summary, while Hoser and Wells are undoubtedly knowledgeable about reptiles and could potentially make meaningful scientific contributions, both are instead producing unscientific herpetological taxonomy for apparently private purposes, based on vague descriptions, insufficient evidence, misrepresentations, and other forms of malpractice, which are defended aggressively by personal accusations and invective.

A Matter of Process.—Whereas taxonomy is considered to be a scientific endeavor, nomenclature is essentially a tool for taxonomists to stabilize the use of names corresponding to particular taxonomic findings and entities (*sensu* Mayr 1969; Simpson 1961). Nomenclature could be viewed as the language that scientists use to communicate about biological diversity, and effective communication requires the linguistic terms (in this case, taxon names) to be explicit, universal, and as stable as possible (de Queiroz and Gauthier 1994). The *Code* and the rulings of the ICZN safeguard and uphold the rules of nomenclature, but unfortunately these safeguards do not extend to the taxonomic processes by which names are established in the first place. There is currently no system in place by which the ICZN can prevent the establishment of nomenclature, and concomitant classification schemes, based on taxonomy produced by unscientific practices, including instances of “taxonomic vandalism” (Jäch 2007a,b). As ICZN commissioner Douglas Yanega expressed (Yanega 2009:423), “I think the present system by which we name species is not policed effectively and has loopholes and ambiguities. For example, scientific names can be published in journals without peer review. Although that freedom is fine, the reality effectively permits taxonomic vandals to plagiarize others or publish without scientific merit.” This is an apt summary of the problems in taxonomic herpetology (and other disciplines) that are the primary focus of this article: instances where the *Code* protects names produced unscientifically, including those without sufficient evidence, justification, or privately published to bypass the peer-review process.

BEST SCIENTIFIC PRACTICES FOR PUBLISHING TAXONOMIC DECISIONS IN HERPETOLOGY

The following guidelines, loosely modeled after those presented by the Turtle Taxonomy Working Group (2007), are a set of recommendations against which authors of taxonomic decisions in herpetology, editors of journals publishing such decisions, and anyone consulting such publications upon their release, may judge the merits of these taxonomic decisions and the methods by which they were reached. They are not intended to serve as the single binding set of rules for how taxonomic decisions should be reached, presented, and published in herpetology. However, from our point of view, taxonomic decisions that do not adhere to these best practices should be considered inadmissible to the body of scientific knowledge (and its applications).

Governance.—For any taxonomic decision that proposes a new taxon name or a change to an established one, the ultimate authority regarding nomenclature lies with the ICZN and its *Code*. To be acceptable, nomenclatural changes should be proposed not only in accordance with the requirements presented in the articles of the *Code*; they should also adhere to its spirit (as detailed in the *Introduction to the Code*) and its ethics (as detailed in the *Code of Ethics of the Code*). However, unless the ICZN formally votes on the conservation or suppression of taxon names, academic freedom governs their use and it is a judgment

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call of authors, editors, and readers whether a proposed name should be applied. Thus, we uphold the long-standing tradition by which taxonomy will stabilize over time by use and acceptance or invalidation and rejection in the scientific literature.

Stability.—Whereas new species will be named and taxonomic changes will periodically be necessary to reflect improved information on inferred relationships between taxa, it is ideal if taxonomists maintain concordance with existing nomenclature, and thereby retain existing classifications, to the extent possible to preserve the stability of the established system. Preservation of nomenclatural stability is one of the primary objectives of the *Code*, and even though the *Code's* articles currently do not set stringent constraints on the naming of taxa, the lack of such constraints must not be misconstrued as license to produce taxonomies according to the letter of the *Code* yet in violation of its spirit, as demonstrated by the examples of Hoser and Wells. Taxonomists should favor nomenclatural continuity unless new, strongly supported analyses make changes unavoidable.

Species.—The biological basis for classification lies with elucidating relationships of evolutionary lineages. Thus, underpinning the presentation of taxonomic decisions are data sets that credibly and reliably assert that the group to be named is on an independent evolutionary trajectory. Names of species should not be coined merely to recognize unusual patterns of distribution or even morphology, but to identify biologically cohesive populations with recent common ancestry, no matter their distribution. The burden of evidence is high in such cases.

Higher Taxa.—Taxonomic decisions regarding taxa above the species level require particular care and demand an even higher burden of evidence, because changes in the names of higher taxa can be especially confusing and destabilizing for users of taxon names and classifications. Names of higher taxa should ordinarily only be coined when data sets reliably identify a monophyletic group containing multiple terminal taxa, although not all such clades necessarily require formal recognition. In this regard, the naming of monotypic higher taxa should be avoided as far as possible, because minimal phylogenetic knowledge is conveyed by such arrangements. However, under some circumstances the establishment of monotypic higher taxa may be justified. For example, this may be the case when an existing generic definition cannot be applied to a sister species with highly divergent morphology, which would otherwise be included in the existing genus. In general, naming of monotypic higher taxa should be avoided and names must be based on the currently available evidence irrespective of hypotheses that the taxon could be expanded in the future.

Evidence.—Information gathering in science is a careful and deliberate process, and it requires the best effort possible to produce a transparent chain of evidence based on reproducible methods. Three lines of evidence are generally accepted for the proposal and testing of taxonomic hypotheses. First, novel evidence is obtained through field and laboratory work, involving samples (e.g., whole specimens, animal parts, tissue samples) from known phenotypes collected in nature, with precisely known provenance, and associated with the obligatory documentation. These samples are deposited in institutions where their long-term curation makes them accessible to other researchers for subsequent hypothesis testing (see Cotterill 1997 on the value of biological collections).

Second, evidence should be sourced from existing samples in museum collections or from published information (e.g., GenBank), both of which are ultimately obtained as described above.

In the case of museum specimens (or specimens linked to published information) whose provenance is not precisely known, or whose phenotypic characteristics were not detailed well in life, scientists know to exercise due caution to judge the merits of the material they choose to incorporate into a study.

One or (typically) both of these lines of evidence should be required for taxonomic investigations. They act as a base for further research, so that later work does not have to begin the evidence-collection process *de novo*. For example, storage of sequence data in GenBank makes these data readily available online. If no records from publicly accessible genetic databases, backed by suitable voucher specimens, are listed in support of a taxonomic decision alleged to have been derived from DNA sequence data, then the decision should be rejected. In the case of morphological studies, a standard requirement is a list of specimens of a proposed taxon and a list of the comparative material examined, with their unique identifiers (i.e., source collections and catalog numbers); therefore, if these are not cited (Cifelli and Kielan-Jaworowska 2005:651) the proposed taxonomic arrangement should be rejected. In each case, the mandated citation of the evidence ensures reproducibility, which is one of the hallmarks of science (e.g., Popper 1972).

The third line of evidence is the existing scientific literature—the body of knowledge produced prior to a new research effort. Investigation of the literature on the taxonomic group of interest can provide direction and perhaps impose constraints on proposed taxonomic changes.

Taxonomic decisions proposed in the absence of compelling supporting evidence should be inadmissible in science and in applications of scientific knowledge. Likewise, equivocal or weakly supported nodes in phylogenetic trees should not be named. Furthermore, taxonomic decisions are ideally based on consilience of multiple data sets (e.g., morphological, morphometric, bioacoustic, behavioral, molecular). In the case of cryptic species that cannot be discriminated morphologically or behaviorally, support from molecular data (e.g., mtDNA, nucDNA, cytogenetics) is usually required. The burden of evidence rests on the author(s) of taxonomic decisions, and in each paper that contains such a decision the rationale must be explicit.

This discussion of evidence would be incomplete if we omitted the next logical next step scientists should ideally take once compelling evidence about taxonomic relationships becomes available. Unresolved taxonomic inconsistencies can cause confusion and uncertainty in the literature, which is undesirable for scientists in other disciplines who rely on taxonomists for clear guidance on issues of nomenclature and taxon definition. We therefore strongly recommend that authors who present data sets with clear taxonomic implications (e.g., well-resolved and well-supported molecular phylogenies, evidence for undescribed species) follow their evidence to its taxonomic conclusion and add suitable, formal taxonomic proposals to their discussion. Additionally, an 'orphaned' data set may invite such mischief as discussed above.

Publication.—We think that proposals of taxonomic decisions invariably require a quality control assessment (i.e., peer review) by a group of qualified taxonomic herpetologists (i.e., the editors and reviewers of a particular manuscript). Proposals should take the form of carefully prepared manuscripts that outline the evidence leading to a justified conclusion. Their assessment would typically constitute the editorial process of peer-reviewed journals, during which competent scientists prepare reviews of the work. Authors and print or Internet outlets

avoiding this process can readily be identified as working outside the acceptable rules of science and taxonomy.

It is our recommendation that taxonomic decisions and their concomitant nomenclatural changes are only acceptable when published in peer-reviewed scientific journals and after meeting the following criteria: (1) The investigation must follow an appropriate scientific methodology, which has to be presented in a section devoted to methods. (2) The investigation must provide a list of publically accessible specimens examined, including collection catalog numbers. (3) Publication occurs in a regularly published outlet for scientific research (i.e., not a popular science or trade magazine), available via subscription or accessible online. (4) The outlet must be supported by an editorial team (e.g., editor, associate editors) and supervised by an expert scientific panel (i.e., an editorial board), whose identities and professional affiliations are printed in each issue. (5) Ideally, the manuscript is reviewed by at least two individuals and an editor who, by their research and publication record, can objectively be considered experts in the field. (6) The publication is indexed in the *Zoological Record*, the *Science Citation Index*, or future equivalent databases. (7) If published electronically after 2011, the work must follow the parameters defined by the ICZN (2012). We further propose that the herpetological community, perhaps through the well-curated and easily accessible *Reptile Database* or *Amphibian Species of the World* and with oversight from the ICZN, electronically publish an annual list of outlets known to meet the standards above, through which acceptable taxonomic decisions can be published. New journals, or those not included in the initial listing, may follow the proposed criteria to establish a record of publishing scientifically rigorous taxonomic decisions and will be added to the list as warranted.

THE TRICKY BUSINESS OF WORKING WITH THE CODE

Taxonomy is a unique area of science because it not only requires research (identification, description, and classification) but also bookkeeping (nomenclature). While the investigative activities are governed by the Scientific Method, bookkeeping is governed, to some extent, by the *Code*. This creates the potential for disharmony in an otherwise logical process; this disharmony is what taxonomic vandals exploit.

"Coded" Challenges.—The *Code* assists taxonomic scientists once their research is completed by providing rules for the proper administration of a name. It is here that the *Code*, grown from a scientific need, fails to adhere to the science it supports. For example, according to Article 13.1.1 of the *Code*, to become available a name must be "accompanied by a description or definition that states in words characters that are *purported* to differentiate the taxon" (ICZN 1999; emphasis added), regardless of the diagnostic utility of these characters or even their existence. Therefore, the inclusion of taxonomic characters in support of a taxonomic decision may, in practice, be viewed as only *pro forma*. Even as taxonomists endeavor to follow the evidence carefully (e.g., by listing the minutiae of species characteristics in descriptions; Mąkol and Gabryś 2005), such evidence is not required by the *Code*! Even though the gatekeepers of taxonomic science (i.e., the editors, reviewers, and users of taxonomy) may require evidence, the omission in the *Code* provides loopholes for the entrenchment of taxonomic vandals. Furthermore, the *Code's Principle of Priority* (Article 23; ICZN 1999) is the dictum that governs the validity of taxon names, whether derived by proper scientific procedures or not.

Consequently, taxonomy becomes prone to abuse by authors like Hoser and Wells.

The ICZN is well aware of the problem of taxonomic vandalism and the resulting destabilization of nomenclature, as exemplified above. Commissioners raise the topic regularly, and the Commission is considering how it can help promote nomenclatural stability in the face of such issues in a way that works within the limits of the *Code* and does not curtail academic freedom (E. Michel, ICZN Executive Secretary, in litt.). It is important to recognize that the purpose of the *Code* is to provide for both continuity and stability of scientific names. The *Code* "has one fundamental aim, which is to provide the maximum universality and continuity in the scientific names of animals compatible with the freedom of scientists to classify animals according to taxonomic judgments" (ICZN 1999: Introduction, line 2). Acceptance of unscientific publications, such as those discussed above, extends the freedom to name animals to people not acting as scientists, which violates the spirit of this "one fundamental aim." Although the ICZN has staunchly advocated and defended the *Principle of Priority* regarding taxonomy, exceptions exist for extraordinary circumstances: "The *Code* recognizes that the rigid application of the *Principle of Priority* may, in certain cases, upset a long-accepted name in its accustomed meaning through the validation of a little-known, or even long-forgotten, name. Therefore the rules must enable the *Principle of Priority* to be set aside on occasions when its application would be destructive of stability or universality, or would cause confusion." (Introduction, Principle 4; ICZN 1999). In the case of unscientific taxonomy, the *Principle of Priority* may be set aside due to lack of usage of a taxon name in *scientific publications*. Thus, boycotting the use of unscientific names proposed since 1 January 2000 and adhering to the recommendations we present (Table 1) will eventually permit the ICZN to rule against them, the *Principle of Priority* notwithstanding.

ITEMS FOR ACTION

The Line in the Sand.—To defend herpetological taxonomy against unscientific incursions, we propose that the herpetological community, including authors, reviewers, editors, users of taxon names in applications, and other interested parties, set aside and *strictly avoid the use of the taxon names listed in Table 1*. These taxon names (proposed since 1 January 2000) can be objectively categorized as unscientific by the criteria we have presented. Users may follow the recommendations we present in Table 1 until the ICZN concludes its deliberations about countering the effects of taxonomic vandalism.

In practice, we suggest a two-tiered implementation of this proposal. Firstly, texts in which unscientific taxon names originate should only be cited in the scientific literature when a new taxon is being proposed to overwrite the unscientific name. Secondly, in circumstances where it is necessary to cite a text in which an unscientific name originated, authors should not use the unscientific name itself but should include a suitable explanatory statement, such as: "We follow the recommendations of Kaiser et al. (2013) and consider the name proposed by [author, year] for the taxon under investigation unscientific and unavailable."

The rationale for strict adherence to this recommendation is found in the *Code* itself. According to the *Code* (Article 23.9.1-3; ICZN 1999) it is desirable to avoid the use of names that threaten stability even when this reverses the *Principle of Priority*. This is one area of the existing *Code* where ICZN actions can favor the

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establishment of names generated within a genuine scientific framework. The *Code* adopts a strict stand against names (including those that could be classed as unscientific) that have not been used in “at least 25 [scientific] works, published by at least 10 authors in the immediately preceding 50 years and encompassing a span of not less than 10 years” (Article 23.9.1.2; ICZN 1999); thus, authors following best practices could legitimately create names that, under strict application of the *Code*, would amount to junior synonyms of taxa named in an unscientific manner. Unscientific names should be boycotted and scientifically sound names should be used in their place; applications requesting the suppression of unscientific names could then be filed with the ICZN after 10 years have elapsed, and the Commission would then be able to enforce the *Code*. Whilst the date of 1 January 2000 as a “line in the sand” is arbitrary, we consider it a suitably clear juncture at which to begin the rigorous defense of taxonomic integrity in herpetology.

Best Practices.—We further propose that the best practices presented above be used as a basis for framing a practical standard for the taxonomic process in herpetology that can be amended and adopted by herpetological societies and the editorial boards of scientific journals.

ICZN Action Against Taxonomic Vandalism.—We applaud the discussions held by the ICZN on how best to curb taxonomic vandalism, and we encourage the Commission to proceed with all due speed in their deliberations. Time is of the essence, especially given the recent emergence of instances (described above) where individuals have flagrantly violated the spirit of the *Code* and have used taxonomic publications as a vehicle to defame and inflict professional harm on those working within ICZN guidelines.

Censure of Taxonomic Vandals.—We further espouse the idea that emerged at the Seventh World Congress of Herpetology, that herpetological societies pass motions of censure against Raymond T. Hoser and Richard W. Wells (and any other agents of similar grievous taxonomic malpractice that may emerge) for their unwarranted and deliberate destabilization of herpetological nomenclature in the absence of evidence and peer review. We apply the term censure in the sense of *Demeter's Manual of Parliamentary Procedure* (Demeter 1969), meaning that these societies express strong disapproval while allowing the opportunity for those censured to subsequently reform. As of this writing, the British Herpetological Society (BHS) and the Deutsche Gesellschaft für Herpetologie und Terrarienkunde (DGHT) have adopted resolutions to censure Hoser and Wells.

The ideas we present have received broad support from herpetological taxonomists and others with an interest in herpetology (see *Acknowledgments*), and the authors have received formal support from the American Society of Ichthyologists and Herpetologists (per email vote taken by the society's Executive Committee), the Australian Society of Herpetologists (per unanimous Annual General Meeting vote), the BHS (per unanimous Council Meeting vote), the Chinese Herpetological Society (per decision by the society's Standing Council), the DGHT (per Peter Buchert, President; Axel Kwet, Vice President; Jörn Köhler, Chief Editor of *Salamandra*), The Herpetologists' League (per Board of Trustees vote), the Societas Europaea Herpetologica (per decision by the society's Council; Claudia Corti, President; Uwe Fritz, Vice President; Anna Rita Di Cerbo, General Secretary; Wolfgang Böhme, Ordinary Member), the Society for Research on Amphibians and Reptiles in New Zealand (per unanimous Annual General Meeting vote), and the World Congress of Herpetology (per Executive Committee vote) to endorse the Point of View

presented here. In addition, the Committee of the Herpetological Association of Africa has expressed its opposition to taxonomic vandalism and has endorsed ethically conducted, scientifically sound taxonomic practices similar to those presented in our Best Practices section. The Crocodile Specialist Group also supports our Point of View, specifically the call to avoid the use of taxon names produced by the type of “vanity publishing” perpetrated by Hoser and Wells (Ross et al. 2012).

Last, it is our hope that the model we present here to safeguard herpetological taxonomy (combining best taxonomic practices, ICZN support, self-policing by authors, reviewers, and editors, and society action) may emerge as a workable solution for other zoological disciplines facing similar challenges.

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