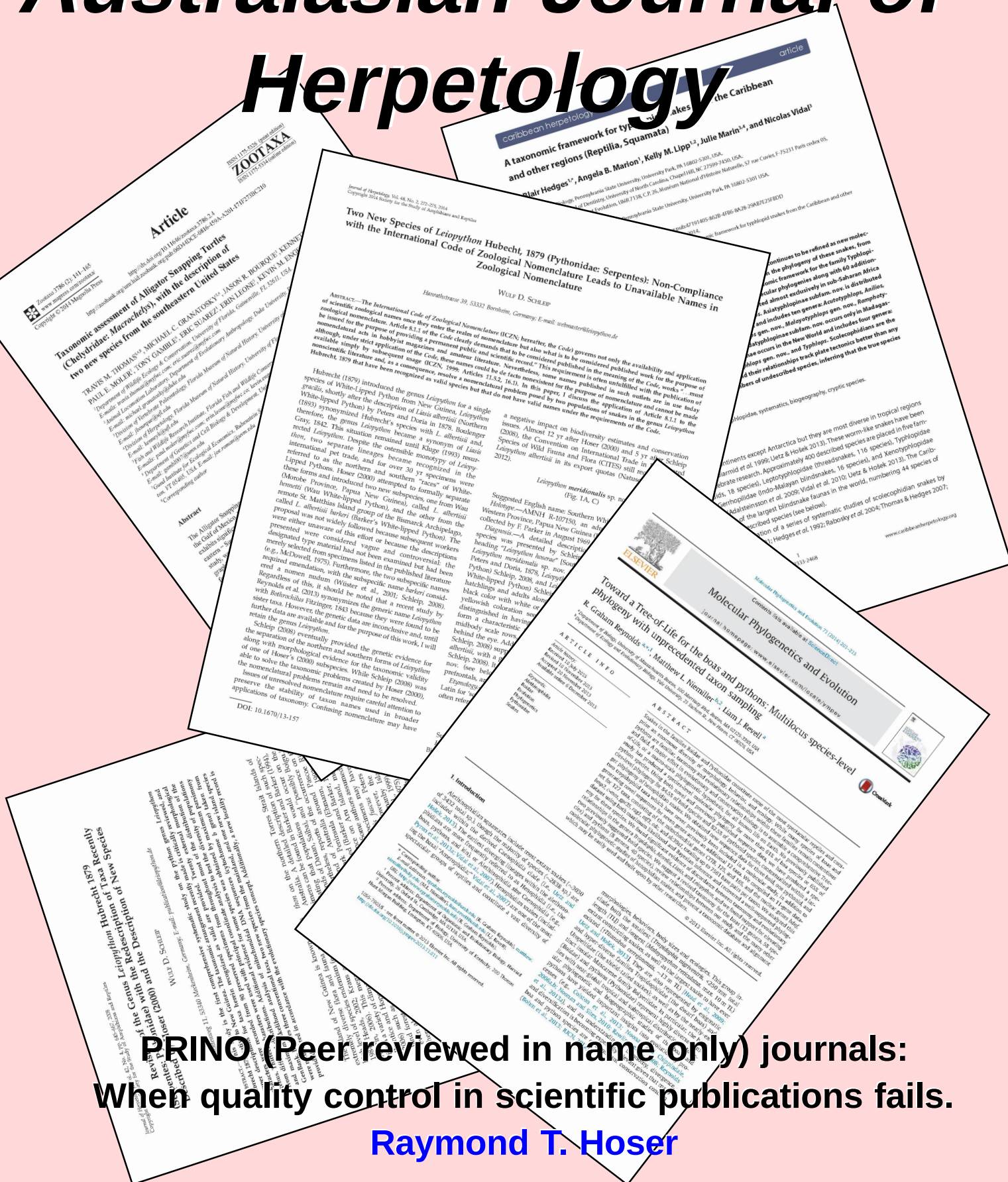


ISSUE 26, PUBLISHED 25 MAY 2015

ISSN 1836-5698 (Print)
ISSN 1836-5779 (Online)

Australasian Journal of Herpetology



PRINO (Peer reviewed in name only) journals:
When quality control in scientific publications fails.
Raymond T. Hoser

Australasian Journal of Herpetology

Contents.

Issue 25

Dealing with the “truth haters” ... a summary!

... Raymond T. Hoser, 3-13 (including):

Timeline of relevant key publishing and other events relevant to Wolfgang Wüster and his gang of thieves.

... Raymond T. Hoser, 8-12.

Table of “*nomen furtum*” synonyms.

... Raymond T. Hoser, 13.

The Wüster gang: Knowingly publishing false information, recklessly engaging in taxonomic vandalism and directly attacking the rules and stability of zoological nomenclature.

... Raymond T. Hoser, 14-38.

Best Practices in herpetology: Hinrich Kaiser’s claims are unsubstantiated.

... Raymond T. Hoser, 39-64.

Issue 26 (This issue)

PRINO (Peer reviewed in name only) journals: When quality control in scientific publications fails.

... Raymond T. Hoser, 3-64.

Issue 27

Rhodin *et al.* 2015, Yet more lies, misrepresentations and falsehoods by a band of thieves intent on stealing credit for the scientific works of others.

... Raymond T. Hoser, 3-36.

Comments on *Spracklandus* Hoser, 2009 (Reptilia, Serpentes, ELAPIDAE): request for confirmation of the availability of the generic name and for the nomenclatural validation of the journal in which it was published (Case 3601; see *BZN* 70: 234-237; comments *BZN* 71:30-38, 133-135) (Unedited version of document submitted to and published in *Bulletin of Zoological Nomenclature* in March 2015).

... Raymond T. Hoser, 37-44.

Raymond T. Hoser, taxonomy publications to end 2014 (about 114 papers) (list).

... Raymond T. Hoser, 45-51.

Raymond Hoser, Nomenclatural Acts to May 2005 (729).

... Raymond T. Hoser, 52-63.

ISSN 1836-5698 (Print)
ISSN 1836-5779 (Online)

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.

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.

ISSN 1836-5698 (Print)
ISSN 1836-5779 (Online)

PRINO (Peer reviewed in name only) journals: When quality control in scientific publications fails.

Raymond T. Hoser

Snakebusters, PO Box 599, Doncaster, Victoria, 3108, Australia.

Phone: +61 3 9812 3322 Fax: 9812 3355 E-mail: [snakeman \(at\) snakeman.com.au](mailto:snakeman@snakeman.com.au)

Received 2 July 2014, Accepted 18 May 2015, Published 25 May 2015.

ABSTRACT

"Peer Reviewed" publications are regarded as both the minimum standard and "gold standard" for scientific research publications. By common definition peer-reviewed articles are those that have been evaluated by two or more researchers or subject specialists in the academic community prior to the journal accepting it for publication.

While not explicitly stated in the definition, it is implied that this ensures a quality and standard of factual information and accuracy, not necessarily present in those publications not subjected to peer review.

In the period 1998 to 2009, a group of renegade reptile enthusiasts known as the Wüster gang decided to engineer a global boycott of established zoological names by this author (Hoser) and other eminent scientists in order steal the results of this work to rename the same taxa.

Their campaign, initially commenced on the internet via chat forums and later through social media sites such as Facebook and Twitter failed (Hoser 2012c, 2013b).

Lacking success and with so-called "Hoser names" moving into widespread usage, in 2012, the same group decided to mount a campaign to try to get others to support their "cause", via a series of publications in ostensibly "peer reviewed" journals. These publications, including (Kaiser 2012a, 2012b) circulated via the web was later published in an ostensibly "peer reviewed" publication *Herpetological Review*, in a paper widely known as Kaiser *et al.* (2013).

That paper was thoroughly discredited by Hoser (2013b) and also rejected by Cogger (2013, 2014a), Dubois (2014), Eipper (2013), Mutton (2014a, 2014b), Shea (2013a-d), Thorpe (2013, 2014a, 2014b), Wellington (2013, 2014a), Wells (2013, 2014), and many others, so this history is not reviewed here.

More recently in the period 2013 and 2014, associates of this same band of thieves have published a series of descriptions of species and genera that overwrite names published in accordance with the *International Code of Zoological Nomenclature* ("The Code", "Zoological Rules" or "Zoological Code") (Ride *et al.* 1999).

This has been in various "peer reviewed" journals.

This paper details the obvious defects in the relevant papers, within the context of documenting obvious ongoing failures of cases of alleged "peer review".

Also shown is how the term "peer review" and the affected publications have been hijacked by dishonest people working as pseudoscientists to put a veneer of respectability to their own less than honourable schemes.

Relevant cases of reckless taxonomic vandalism, also defined herein, by so-called zoologists acting in deliberate violation of the Zoological Code are detailed. These publications have created an unscientific mess that other scientists are forced to spend time correcting.

The term PRINO, an abbreviation of "peer review in name only" is formally coined to identify those journals where the peer review process is so debased or shambolic as to effectively be absent in terms of any real benefit.

Keywords: PRINO; peer review; journal; Wüster; Schliep; O'Shea; Kaiser; Hansen; Baig; Bates; Thieves; Twombly; Creationist; science; Shea; Cogger; Wells, Wellington; herpetology; zoology; taxonomy; nomenclature; scolecophidians; Gerrhosauridae; *Swilesaurus*; *Funkisaurus*; cryptozoology; *Adelynkimberlea*; *Macrochelys*; *Broghammerus*; *Malayopython*; *Laudakia*; *Leiopython*; *Bothrochilus*; *hoserae*; *bennetti*; *meridionalis*; *montanus*; Candoiidae; *Zootaxa*; ICZN, DNA; Zoological Code ; rules; scientific fraud; theft.

INTRODUCTION

"Peer Reviewed" publications are regarded as both the minimum standard and "gold standard" for scientific research publications. By definition "Peer-reviewed articles have been evaluated by several researchers or subject specialists in the academic community prior to the journal accepting it for publication."

While not explicitly stated in the definition, it is implied that this ensures a quality and standard of factual information and accuracy, not necessarily present in un-peer-reviewed publications.

This paper details the obvious defects in several taxonomic papers, within the context of documenting obvious failures of alleged "peer review".

Also shown is how the term "peer review" has been hijacked by dishonest people working as pseudoscientists to put a veneer of respectability to their own less than honourable schemes.

The term PRINO, an abbreviation of "peer review in name only" is formally coined to identify those journals where the peer review process is so debased and shambolic as to effectively be absent.

As a preamble it is fitting that the cases that follow be put in a historical context.

In the period 1998 to 2009, a group of renegade reptile enthusiasts including people with convictions for animal cruelty and wildlife smuggling waged an ongoing war against myself (Raymond Hoser).

I am a herpetologist of global repute and author of many hundreds of scientific papers published in both the peer reviewed and "popular" literature. The first peer reviewed paper was published by myself in 1980 in the Australian journal *Herpetofauna* (Hoser 1980) and my scientific publishing career spans the period from that date to 2015.

The group of renegades, known widely as "the Wüster gang", although also marketing themselves as "truth haters" in order to gain some sort of warped notoriety in the herpetological community, decided to engineer a global boycott of established zoological names in order to rename the same taxa themselves.

Other attempts to do this sort of thing have been tried in zoological science many times over the past 200 years, but have always failed. The best known recent examples were the attempts to suppress and rename several hundred reptile species and genera, formally named by Richard Wells and Ross Wellington in 1984 and 1985 (The president, Australian Society of herpetologists 1987, ICZN 1991, 2001).

These attempts failed, as have all others, with the ICZN Commissioners ruling either unanimously, or

near unanimously against the renegades seeking to step outside the Zoological Code and rename properly named taxa.

It is also significant that the brief of the ICZN is also to enforce the rules of "The Code" and that is what they have done to date.

In every case, where attempts have been made to suppress one scientist's works for the purposes of renaming their taxa, the arguments have been the same. They have included the catch-all claim that the to be suppressed author is a "taxonomic vandal" or "unscientific", easily shown to be a lie, because if that were the case, then the original scientist's works could simply be placed into synonymy with correct taxa as is done regularly in zoology and within the Zoological Code.

If and when the alleged taxonomic vandal's science is shown to be correct to the wider global audience to a degree that denial is pointless, the alleged vandals are then accused of stealing the work from those who first identified the first author's work as taxonomic vandalism.

It is a catch-22 situation engineered to ensure that the target scientist whose work is to be stolen, never gets credit for what they have done.

When the arguments are stripped bare, it invariably gets down to the fact that one group of people are acting in an unprofessional manner and don't want to use the names or recognize the contribution to zoology by their adversaries or competitors.

A common but associated claim mounted is that the original scientists have named "too many" taxa, thereby depriving later workers of the right to name species.

The claim is ridiculous in the extreme as it is in effect a call on scientists to stop working to allow later "scientists" the ability to make discoveries.

Can you seriously imagine one scientist saying to another, "don't publish your cure for cancer, because one day I might get around to doing it"?

I should also add that no vertebrate taxonomists in the 21st Century come anywhere near approaching the scale of mass-naming species and genera as compared to the major contributors from the 1800's and that there has never been a Zoological Code imposed limitation on the number of species or genera a person is allowed to name.

In both the attempt to squash the names of Wells and Wellington in a petition to the ICZN in 1987 and in the Wüster gang's campaign of 2012-2015, the same claim of the alleged "taxonomic vandal" naming too many species has been raised as an argument in favour of suppression of the affected works.

This equally bizarre suggestion must be reconciled with the more reasonable suggestion that perhaps the targeted scientists have simply been more “productive” than their adversaries may have hoped.

This is the usual accolade afforded to persons who have named a large number of taxa.

Such productivity in terms of ability to name valid zoological entities within the boundaries of the Zoological Code (Ride *et al.* 1999) has clearly irked the Wüster gang, members of which led the campaign against Wells and Wellington in the 1980's and later against myself (Hoser) in the post 2000 period.

All three men, Richard Wells, Ross Wellington and myself (Raymond Hoser) are not based at a government facility such as a Museum or University and so are treated by the Wüster gang as private scientists (as opposed to publicly funded), although the term they use is “amateur” in order to present a veneer that the work of their adversaries is “amateurish” and not up to proper standard.

I should note that in science, there is no firm rule that says amateurs must by definition produce amateurish work, or that professionals must produce work of professional standard.

Numerous examples exist of both amateurs (in the Wüster gang's definition of the term) and professionals (in their definition of the term) producing both amateurish or professional works, publications and the like.

In terms of the attempted suppression of the Wells and Wellington papers by members of the Wüster gang, the history of the matter is worth revisiting as seen by checking the original publications of Wells and Wellington (1984, 1985) and the case mounted against them in the *Bulletin of Zoological Nomenclature (BZN)* via both comments by other scientists for and against the proposed suppression and the ultimate decisions by the ICZN commissioners.

The arguments raised against the two men and counter arguments in their favour are identical to those mounted against myself at the current point in time.

Even the reckless creation of junior synonyms in supposedly peer reviewed papers, by the Wüster gang members in the hope that the ICZN will squash the proper, earlier names is something that was done in terms of both Wells and Wellington and the more recent Hoser publications.

THE WÜSTER CAMPAIGN AGAINST HOSER

There is however, one significant difference between the Wüster gang campaign against myself in the post 2000 period and that against Wells and Wellington in the 1980's and early 1990's.

This is the use of the internet to mount the campaign.

Wüster's ability to use the internet in every conceivable way to create a false veneer of the truth and market his lies has been well documented already (Hoser 2012a, Hoser, 2012c and Hoser 2013b).

In terms of volume of posts attacking myself, made both under his real name, aliases he has assumed and via people he has recruited to his cause is astronomical. As of January 2014, it was estimated via Google searches that the Wüster gang had made no less than 80,000 adverse posts about myself in the previous 14 year period!

I need not mention a case from 2008, where Wüster *et al.* attempted to defraud the Accor Hotels chain of an estimated \$US 20,000.00 in 2008 by aggressively using online social media to generate “votes” for David John Williams.

This member of the gang is a man with serious smuggling and animal cruelty convictions (Wüster 2008, Coritz, 2008, Williams 2008). Among his many wildlife-related convictions was one in the Cairns Magistrates Court where he was fined \$7,500 for numerous wildlife smuggling and aggravated animal cruelty offences.

The Wüster gang's scheme worked in that their man, David John Williams won the vote tally, after a very close tussle with an Indian academic who was similarly trying to game the system. However the scheme to defraud the hotel chain fell apart and the Wüster gang was outed when the IT people working for the hotel chain detected many thousands of “votes” for Williams, ostensibly from many different individuals coming from a single IP address (Williams 2008).

Hence while it is evident that Wüster is able to create a veneer of widespread support for all he and his gang does, this is not necessarily the reality.

Furthermore and in terms of the enforcement and use of the rules of the Zoological Code, a majority view is not relevant in any event. Compliance with the rules is the only thing that gives stability in nomenclature, not some kind of lawless online mob-rule!

However I should also note that the hate campaign of Wüster against myself has also had considerable traction, most notably via his use of false names to make false claims against me, then reposted widely.

This has included sites such as Wikipedia, where a page he has edited, named “Raymond Hoser” under the user name “Mokele” has been obsessively edited with a barrage of hate and lies (see for example Wüster 2009). His sidekicks Mark O'Shea (user name “Papblak”), Wulf Schlip and Scott

Thomson, have also made numerous hatred inciting claims including the recklessly false claim that I have killed six people, including my own 10-year old daughter! (Wüster, W., O'Shea, M. *et al.* 2014), or for example the numerous false claims by these men hosted by Hunter *et al.* (2006) as part of a long running campaign to have the family business shut down by authorities.

Of course a disinterested third party would almost certainly form an unfavourable view of a person they were told had committed acts of animal cruelty, academic fraud, theft of material from others and mass killings when denied access to correcting information.

I should also mention that the highly damning false claims about myself by these men goes to every area I have ever worked in or published about and caused immense damage to my wildlife education business here in Australia.

The Wüster gang's campaign that was initially commenced on the internet via chat forums and later social media sites such as Facebook and Twitter failed (Hoser 2012a, 2012c, 2013b).

In spite of an invitation by the Wüster gang in 2009 to others to rename Hoser-named taxa (Wallach *et al.* 2009), widely reposted, no one had as of 2013 heeded their call.

That result alone showed that the Wüster gang's claims of support for their plans as alleged in 2013 and 2014 are false.

In 2012, the same group decided to mount a campaign to try to get others to support their "cause", via a series of publications in ostensibly "peer reviewed" journals. These publications, including (Kaiser 2012a, 2012b) circulated via the web was later published in an ostensibly "peer reviewed" publication *Herpetological Review*, known as Kaiser *et al.* (2013).

Kaiser *et al.* sought (and still seek) to step outside of the Zoological Code and breach the three most important rules being:

- 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) and other sections.

Notwithstanding the fact that Kaiser *et al.* (2013) was thoroughly discredited by Hoser (2013b) in detail as well as Cogger (2013, 2014a, 2014b), Dubois (2014), Eipper (2013), Mutton (2014a, 2014b), Shea (2013a-d), Thorpe (2013, 2014a, 2014b), Wellington (2013, 2014a), Wells (2013, 2014), and many others, the Wüster gang have

adopted the strategy that if a lie is repeated often enough, it will be believed by people. This is especially if no correcting information is presented to the relevant audience.

To this end Wüster and others in the gang have republished the same claims widely.

Ignoring the countless instances on the web, key documents repeating the lies are Kaiser (2013, 2014), Wüster *et al.* (2014), Schleip (2014b) and most recently Rhodin *et al.* (2015).

Because their false claims were all rebutted by Cogger (2014a), Hoser (2013b, 2015a-e) and Shea (2013a-d) and others, there is no need to repeat the rebuttal here.

I should also note however (as shown in Hoser 2013b), that authorship of given documents by members of the Wüster gang is invariably questionable.

In recent years it has become clear that a number of documents with authorship beyond Wüster have in fact been written by Wüster himself.

By way of example, the original version of the document Kaiser *et al.* (2013) was circulated within a SPAM email globally by Kaiser in 2012. In his call to arms letter (Kaiser 2012a), Kaiser stated that he played no part at all in the preparation of the relevant document (Kaiser 2012b) and was merely circulating it on behalf of the author.

However when it appeared in print in the publication *Herpetological Review* a year later, Kaiser was listed as the lead author, with 8 listed co-authors including Wüster. These authors consisted the core of the Wüster gang.

Significantly, other versions of the document posted on the web, including one at www.researchgate.net listed Wüster as the sole author!

It is also worth noting that the Kaiser email of 2012 openly asked others to sign on as co-authors in order to give the document more "weight".

"Author shopping" as it is known in science circles is regarded as a serious crime and is explicitly forbidden in the code of ethics of many journals, including notably in the very journal Kaiser *et al.* (2013) was published in (details as given in Hoser 2013b).

This calls into question how and why this case of author shopping slipped through that journal's peer review and quality control processes. Also called into question is why the journal did not print a correction, retraction or apology when it became clear that the code of ethics had been breached. What is however known, and based on his own posts on Facebook in the relevant period, is that the listed editor of *Herpetological Review* does not seriously believe in proper peer review or quality

control, but prefers to run the publication as a launching pad to engage in reckless attacks on all his enemies and those of his friends.

Similarly in 2001, Wüster alone published online at www.kingsnake.com and elsewhere a blog attacking myself and advocating that people not use scientific names for taxa formally described by myself in the previous three years.

Later that year, the same paper appeared in the non peer reviewed journal *Litteratura Serpentina* under the names of Wüster and several other co-authors (details in Hoser 2001).

Wüster had in that case also shopped the paper to numerous herpetologists to "sign on as authors" in order to give "weight" to the publication. While many herpetologists declined his invitation, eventually he managed to cobble together a group of alleged co-authors.

Significantly, the Kaiser *et al.* (2013) plan (which Richard Wells in 2013 remarked should be properly known as Wüster *et al.*) was marketed to many herpetological societies as a call to improve scientific standards.

This is of course a very hard thing to argue against!

It was only if one read the detail of the document known as Kaiser *et al.* (2013) that one saw that there were no serious workable proposals to improve quality of science, but rather a "ridiculous and unworkable" scheme to dismantle the established Zoological Code to give the Wüster gang total hegemony over the science of herpetology and the right to steal the work of others and rename taxa described by others as they saw fit (Shea 2013a).

I should also add, that their hit list of taxa and authors, was extensive and went back to major names in herpetology from the 1800's.

More recently Kaiser (2014b) proposed in an 11 page blog published in *Herpetological Review* that the ICZN should use their plenary powers to rule all Hoser names ever published (dating from 1998) as unavailable under the Zoological Code but in the absence of any proper formal submission.

However I note that in March 2015, he was listed as a co-author of a submission to the ICZN (Rhodin *et al.* 2015), published in their *BZN* seeking suppression of just the Hoser-controlled journal *Australasian Journal of Herpetology (AJH)* (Hoser 2009-2014) and nothing more, and that:

"These requests supersede those contained in three prior comments published by our co-authors (Thomson, BZN 71: 133; Wüster *et al.*, BZN 71: 37–38; Kaiser, BZN 71: 30–35)."

This is a significant step as it implies he now

supports Hoser names like *Broghammerus* Hoser, 2014 and *Leiopython hoserae* Hoser, 2000, (sometimes placed in the genus *Bothrochilus*) both of which have been recently overwritten with newly coined names by members of his gang (Reynolds *et al.* 2013a, 2013b, 2014, and Schleip 2014).

What he has called for in these blogs and comments is in fact impossible under the present version of the Zoological Code (Ride *et al.* 1999) and in law is outside of the plenary powers of the ICZN and its commissioners!

It is also illegal under IP law as ruled in similar cases, noting that even the anti-Hoser ICZN Commissioner Doug Yanega, a close friend of Wüster did on 16 January 2015, admit that legally acquired name authority for taxa was Intellectual Property (IP) (Yanega 2015).

Therefore they would be with all the attached legal rights for the "owner", or in this case the "name authority".

Cogger (2013, 2014a, 2014b), Dubois (2014), Eipper (2013), Mutton (2014a, 2014b), Shea (2013a-d), Thorpe (2013, 2014a, 2014b), Wellington (2013), Wells (2013), and many others had already condemned the Kaiser recommendations making a mockery of the claim by Kaiser (2014b) that he has broad agreement within herpetology for his (Wüster's) plans.

Notably, Wüster in late 2013 distanced himself from the document Kaiser *et al.* Wüster (2013a, 2013b), noted the:

"errors that slipped through in that paper"

(Wüster (2013a) and:

"mistakes slipped into the Kaiser *et al.* paper - big deal, that was hardly the point of the paper."

(Wüster (2013b).

This led Ross Wellington (2014a) to state he thought Wüster was preparing Kaiser to be the fall-guy when their wacky scheme failed.

Of relevance is that names proposed by myself are widely used in thousands of other people's publications. To formally or informally suppress them (as being proposed by Kaiser and his gang) and in direct breach of the rules of the Zoological Code (as well as IP laws applicable within their own countries) would cause immense instability in herpetological nomenclature.

In terms of venomous species, Kaiser's actions would potentially put human lives at risk as detailed by Hoser (2013b).

Because Kaiser *et al.* (2013) and the earlier documents have been discredited already, there is no need to do this here.

However in summary, the allegations made against

myself were found to be false. But the same charges were found to be true for Kaiser *et al.*, better known as the Wüster gang, including the following:

“evidence free taxonomy”, fraud, “unscientific taxonomic publications”, “taxonomic terrorism”, plagiarism, “unscientific taxonomy”, “unscientific practices”, “unscientific incursions” and “deliberate acts of intellectual kleptoparasitism”.

Furthermore the claims in Hoser (2013b) that Kaiser *et al.* (2013) engaged in reckless taxonomic vandalism have been vindicated by others, most notably including the very conservative former ICZN Commissioner, Dr. Harold G. Cogger (Cogger 2014a), Dr. Glenn Shea several times (Shea 2013a-d) and most recently “Dr.” Hinrich Kaiser himself in his large 11-page “Errata” published in Kaiser (2014b) as printed in *Herpetological Review*.

Kaiser and partner in crime Robert Twombly, both work from a facility calling itself “Victor Valley College” which is best described as a pseudo-university located on the outskirts of Los Angeles, California. On the website of Victor Valley College, the facility claims expertise on Creationist science and “intelligent design” with no less than 101 webpages on the subject according to a Google search on 5 July 2014. Twombly’s self-created webpage profile claims he is an expert in “Cryptozoology” (Twombly 2013).

As of 24 December 2013, Cryptozoology was defined by the Wüster edited Wikipedia (at: <http://en.wikipedia.org/wiki/Cryptozoology>) as follows:

“Cryptozoology is a pseudoscience involving the search for animals whose existence has not been proven. The animals cryptozoologists study are often referred to as cryptids, a term coined by John Wall in 1983. This includes looking for living examples of animals that are considered extinct, such as non-avian dinosaurs; animals whose existence lacks physical evidence but which appear in myths, legends, or are reported, such as Bigfoot and Chupacabra; and wild animals dramatically outside their normal geographic ranges, such as phantom cats (also known as Alien Big Cats). Cryptozoology is not a recognized branch of zoology or a discipline of science. It is an example of pseudoscience because it relies heavily upon anecdotal evidence, stories and alleged sightings.” (Various authors 2013).

For those unaware, *Herpetological Review* is a PRINO (peer reviewed in name only) journal edited by Mr. Robert Hansen, a close friend of Hinrich Kaiser, Wolfgang Wüster, Mark O’Shea and Wulf Schleip, better known as the Wüster gang, named in honour of their “leader”.

Private conversations on the Facebook walls of Schleip and Hansen have included posts showing that they intended using *Herpetological Review* as a platform to launch attacks on myself (Hoser), other eminent herpetologists and the Zoological Code itself. They also have repeatedly stated that they will under no circumstances allow the publication of any material that in any way contradicts or dissents from their own warped view on things.

A point form rebuttal of 22 alleged errors in Hoser papers as published by Kaiser (2014b) was sent to Hansen by email for printing in his journal on 29 June 2014 and was rejected by him by email within seconds of receipt and clearly before he had read the corrections.

Based on his private Facebook posts, which he was probably unaware of the fact I had accessed and read, this immediate rejection of properly submitted material was expected. However it was important that I observed correct procedure and protocols so that it would (in theory) prevent the claim being made that I had refused to submit material to them.

The document Kaiser (2014b) is rebutted in another paper published in *AJH* at the same time as this.

This paper seeks to deal with consequences arising from the Wüster plans to rename species via the use of ostensibly peer reviewed journals.

I do this by going through each of the relevant papers and pointing out obvious errors that would have been detected in any reasonable peer review process.

This leads to the unavoidable finding that the peer review process has been avoided, subverted or otherwise degraded to be worthless. Hence the use of the term PRINO (peer review in name only) to define these journals.

PEER REVIEWED JOURNALS AND PRINO JOURNALS

By definition “Peer-reviewed articles have been evaluated by several researchers or subject specialists in the academic community prior to the journal accepting it for publication.” The peer-reviewed journals are where these peer reviewed papers are published.

The industry standard minimum appears to be two “peer reviewers”.

While in numerous internet posts the Wüster gang have used the case of their papers being published in allegedly peer reviewed journals as defining some kind of guarantee that all within their papers is correct, no proper scientist would accept such a notion.

Even properly reviewed and accepted material published in the best of peer reviewed journals runs

the risk of being debunked or found to be in error, if and when further scientific evidence comes to light. This is exactly how science progresses.

As one who has acted as a peer reviewer for many scientific papers in various journals and edited two peer reviewed journals myself, I know how easy it is for errors to slip into a publication and in spite of the best protocols and intentions of both reviewers and editors.

Bonhannon (2013) details a host of editorial failures in many peer reviewed journals and shows how even the most defective of papers can be published if shopped to a sufficient number of journals.

Hence the argument that peer review forms some kind of barrier to bad quality science is questionable at best and this has been made clear countless times by other scientists.

The evidence of Bonhannon (2013) is that quality peer reviewed journals owe as much to the quality of submissions prior to review as to the review process itself. This concurs with my own observations as well, as demonstrated shortly.

Most scientists I deal with, including myself have their publications reviewed prior to submission rather than relying on the peer review process to iron out potential errors. In the case of *Australasian Journal of Herpetology* (AJH), every paper is reviewed by four experts in the relevant area as well being reviewed after this by two non-experts. As both author and editor of relevant material, I have kept myself at arms length from key parts of the process, but will accept arguments of conflict of interest do have traction.

In our case, the idea is also to ensure that the material is not just factually correct and makes sense, but also to ensure that this remains the case to those without detailed expertise in the relevant area.

Some but not all journals have guidelines as to what peer reviewers should and should not do and many major journals now have a highly automated peer review process.

Where peer review most commonly seems to fail in the real world is when reviewers fail to check the cited material to ensure that it actually says what is alleged in the given paper. Authors seeking to get substandard papers making wrong of false conclusions published will often cite material supporting their case which when actually read, doesn't in fact do this.

For example, in the allegedly peer reviewed *Herpetological Review* Kaiser (2014b) wrote "Following the publication of Kaiser *et al.* (2013) in March of 2013, Hoser worked the online social media circuit to discredit the authors and supporters

of that paper, along with the journal and its editor, as well as the scientific societies that voted in support of the presented ideas (e.g., Hoser 2013n)."

On the surface, the statement makes sense and most people would assume it to be an undisputed statement of fact.

However the statement is a deliberate lie on the part of Kaiser, not identified (or deliberately overlooked) in the peer review process.

You see the publication "Hoser 2013n" as cited by Kaiser in the same paper was in fact:

"2013n. The science of herpetology is built on evidence, ethics, quality publications and strict compliance with the rules of nomenclature. *Australasian J. Herpetol.* 18:2-79."

That is a hard-copy printed journal and not anything like evidence of having "worked the online social media circuit".

It goes without saying that Kaiser has deliberately printed a false statement in the hope his readers look no look further than the words as presented there and then, and hoping that they don't even do the simple exercise of cross-matching cited references with text, due to the excessive verbosity of his rant.

The hard copy paper (Hoser 2013b) did however discredit Kaiser *et al.* (2013).

Without dissecting the rest of Kaiser (2014b) and the countless errors within the rant, as this is done elsewhere, it is fair to say that such an error, while an obvious breakdown of peer review and editorial control is not in itself enough to define *Herpetological Review* as a PRINO journal.

However what does make *Herpetological Review* a PRINO journal is what the authors and editors have done in the wake of disclosure of this and the other significant errors in the "paper".

Ethical journals would as a matter of course publish corrections to the misinformation at the first possible opportunity. One need look no further than the blog at <http://retractionwatch.com/> to get an idea of the number of retractions and corrections published by eminent peer reviewed journals.

I should also mention that one bad paper in a journal does not necessarily mean all published there are bad.

If an author submits a good paper in the first instance, even a total absence of peer review probably won't change that.

But returning to the mistakes in Kaiser's "paper" that got through the review process at *Herpetological Review* and how Robert Hansen dealt with it is why we know he runs a PRINO journal.

On 29 June 2014, I emailed him an itemized list of

over 20 errors and corrections in that one “paper”. None of this was opinion. It was simply a case of cross-checking Kaiser’s statements with the sources he himself cited to demonstrate he’d made false statements; like in the example just given.

While one false statement in a “peer reviewed” paper is bad, more than 20 is apocalyptic!

Hansen replied to my email within seconds and clearly without ever having an interest in reading it or to correct potential errors in his journal. He said there was no way he’d publish any corrections!

It was this action alone that with total certainty defined *Herpetological Review* as a PRINO rag!

I need not mention an identical email exchange in terms of the earlier Kaiser *et al.* (2013) “paper” published in *Herpetological Review*. In spite of being aware of hundreds of cases of statements being made without evidence in the paper Kaiser *et al.* (2013), Hansen refused to publish corrections to the mistakes (Hoser 2013b).

Finally while talking about having “worked the online social media circuit”, I should report that Kaiser’s creationist and cyrtozoologist colleague at Victor Valley College, the esteemed Mr Robert Twombly did a sterling job of doing exactly that for his most recent offering in the form of Kaiser (2014b).

According to a Google search on 2 July 2014, Twombly had posted links to the paper no less than 245 times on various Facebook pages and no less than 240 times on various online forums!

At least he’d complied with “Recommendation 8A” of the Zoological Code!

THE RACE TO STEAL THE WORK OF OTHERS

The following instances of the failure of peer review as a basic quality control in publications that are heavily marketed as peer reviewed all arise as a result of the Wüster gang campaign.

Every case involves scientists or pseudoscientists acting in a method that is both unethical and unscientific to publish false and misleading information for the express purpose of claiming “name rights” or “name authority” over given species or genera that have been properly described previously by others.

In summary a collection of creative excuses are used to effectively steal my earlier works and those of others to rename species and genera in direct breach of the Zoological Code (Ride *et al.* 1999).

This is in particular the result of the publication of the false statements by Kaiser *et al.* (2013) in *Herpetological Review* which then call for other herpetologists to steal my own works and those of others and rename the very same taxa.

Hoser (2013b) already showed that Kaiser *et al.* (2013) deliberately misquoted the Zoological Code on page 20 of their paper to allege that it was possible for others to rename taxa and then have the ICZN validate their decision by plenary ruling, even though the relevant section of the code actually prohibited this, because it only applied to pre-1900 names.

Also in a classic act of Orwellian doublespeak Kaiser *et al.* (2013) wrote:

“These recommendations are not formal nomenclatural proposals according to articles of the Code”,

so as to give them an escape from censure if in fact people followed their directives elsewhere in the same paper (viz renaming taxa).

At no stage in this paper do I seriously need to justify the science behind my taxonomic proposals. This is because in each case below the relevant authors (better labelled as thieves) have agreed with my thesis and merely stolen the work!

REYNOLDS ET AL. AND THE JOURNAL MOLECULAR PHYLOGENY AND EVOLUTION

In 2003/4 I, published a paper on the taxonomy of the pythons in *Crocodylian* 4(3) and 4(4) (Hoser 2004).

In it (pp. 21-23), the new genus *Broghammerus* Hoser, 2004 was proposed for the Reticulated Python group (type species *Boa reticulata* Schneider, 1801). There was never any dispute raised that the original publication was compliant with the Zoological Code (Ride *et al.* 1999). It was also indexed in *Zoological Record* Volume 140 in accordance with the wide dissemination requirements of the Zoological Code, this action being done for all Hoser taxonomy papers.

The original description had a sound scientific basis, based on the deep and consistent morphological differences between the type species and the type for the genus *Python*, including as previously described in detail by McDowell (1975) and further characteristics identified by myself including such conservative traits as the unique pits in the upper labials unique to this genus of python.

Notwithstanding this fact, Wolfgang Wüster and others associated with him ran a strong and generally successful pseudoscientific campaign advocating non-use of the name *Broghammerus* over the following four years (Wüster 2004).

A tactical mistake made by Wüster, a man with no expertise whatsoever in terms of pythons, was to form the view that *Broghammerus* (or for that matter *Lenhoserus*) wasn’t a valid genus.

Wüster (2004) wrote:

“It’s Python. Hoser has provided no new evidence to

support his arrangement, and there are absolutely no reasons whatsoever to follow it. If future research shows that a new genus is warranted, then the name *Broghammerus* will be available, but at present, there are no grounds for changing the status quo."

While the claim of no evidence was false and I retorted the following (cut here):

"He says "no evidence". There is evidence in the paper and the sources cited therein.

Sam McDowall, a man of higher standing than WW long ago put the *reticulatus* pythons as a separate group from the *molurus* group (pity the McDowall paper was out long before WW lurched onto Kingsnake, so he's probably never read it).

...Put another way, if you (any reader) view *reticulatus* as being in a different genus to *molurus* (try the labial pits as an indicator), then there is no choice but to call them *Broghammerus*."

But the most important statement by Wüster in 2004 was his admission that *Broghammerus* was the proper code-compliant name!

Recall he said:

"If future research shows that a new genus is warranted, then the name *Broghammerus* will be available".

In 2008, Wüster's anti-*Broghammerus* campaign had unforeseen difficulties after Rawlings *et al.* (2008) published a strong molecular basis to support the placement of the type species in the genus *Broghammerus* (having allegedly diverged from *Python* more than 40 million years ago) and she distributed her paper widely online.

Beyond that date and in spite of the best efforts of Wüster and his gang to stop usage of the name (e.g. Wüster 2010), *Broghammerus* has moved into general usage in zoology as the genus name for an icon group including the world's longest known living snakes.

This usage includes in numerous books, online and countless scientific papers. See for example De Lang (2011), Grismer (2011), Schleip and O'Shea (2010), O'Shea (2013a) or alternatively view any of the 14,700 publications reported by Google on 25 December 2013.

Pyrón *et al.* (2011 and 2013) also produced phylogenies with Genbank data using *Broghammerus* with compelling evidence for its ongoing usage.

Kaiser (2012) and Kaiser *et al.* (2012 and 2013) called for people to step outside of the rules of the Zoological Code (see for example p.20 of Kaiser *et al.* 2013) and to rename all species and genera named by Hoser and Wells in the period backdated to year 2000 as well as taxa named by others

outside herpetology deemed appropriate targets (p. 20 Kaiser *et al.* 2013, Cermak *et al.* 2013).

Kaiser (2012) and Kaiser *et al.* (2012 and 2013) were discredited and shown to be documents replete with factual errors by Hoser (2012c and 2013b), as later admitted by co-author Wüster (2013a, 2013b).

Notwithstanding their own prior acceptance of and use of *Broghammerus* in the period immediately preceding the hatching of the Kaiser *et al.* (2013) document in 2012 (Kaiser 2012a, 2012b), coauthors Wüster, O'Shea and Schleip waged an intense online campaign seeking to get others to rename *Broghammerus* (O'Shea *et al.* 2013b, 2013c). This group also on 20 March 2013 advocated a white-list of authors and journals allowed to publish taxonomic papers to be controlled by themselves (O'Shea *et al.* 2013b).

In late 2013 Reynolds *et al.* drafted a paper with yet another molecular phylogeny supporting the placement of the species *reticulatus* in the genus *Broghammerus*, based on the same Genbank data used by Rawlings *et al.* (2008) and Pyron *et al.* (2011), listing the species *reticulatus* and *timoriensis* in the same genus, namely *Broghammerus*. This was posted online at <http://www.sciencedirect.com/science/article/pii/S1055790313004284>, cited here as Reynolds *et al.* (2013a).

On 6 December 2013, Bryan Fry posted a more recent draft of the same paper online on his own website at http://www.venomdoc.com/downloads/MPE_pythons.pdf Reynolds *et al.* (2013b).

this time with the authors removing the use of the name *Broghammerus* and renaming the genus *Malayopython*, with a five word "definition and diagnosis".

The same paper by Reynolds *et al.* cited Kaiser *et al.* (2013) as a justification or reason to overwrite, as in re-name the well-established Hoser-named genus. Above the diagnosis for the genus was written "*Broghammerus* Hoser 2004 *nomen nudum*".

On 13 December 2013, Shea (2013) confirmed that *Broghammerus* Hoser, 2014 was definitely not *nomen nudum* as defined in the Zoological Code (Ride *et al.* 1999).

Wells (2013) also noted the following:

"the genus *Broghammerus* cannot be treated as a *nomen nudum* by the opinions of Kaiser *et al.*, so it just

can't be replaced with the new genus name *Malayopython* on that basis. The paper where *Broghammerus* was published (*Crocodylian*) wasn't controlled or edited by Hoser. It complied fully with the Code of Zoological Nomenclature and so must be considered as an "Available Name" under the

<http://forums.kingsnake.com/view.php?id=543430,543430>

Broghammerus reticulatus or Python Reticulatus????

[Login] [User Prefs] [Search Forums] [Back to Main Page]

[Back to Pythons: Reticulated] [Reply To This Message]

Posted by: serpentinedreams at Fri Aug 13 20:37:16 2004 [Report Abuse] [Email Message]

<http://forums.kingsnake.com/view.php?id=520074,520074>

<http://www.smuggled.com/pytrev2.htm>

Ive already have an oppinon formed about this, but what does everyone else think???

-Shaun D

[Reply To This Message] [Show Entire Thread]

>> Next Message: RE: Broghammerus reticulatus or Python Reticulatus???? - WW, Sat Aug 14 03:13:23 2004

<http://forums.kingsnake.com/view.php?id=543430,543617>

RE: Broghammerus reticulatus or Python Reticulatus????

[Login] [User Prefs] [Search Forums] [Back to Main Page]

[Back to Pythons: Reticulated] [Reply To This Message]

Posted by: WW at Sat Aug 14 03:13:23 2004 [Report Abuse] [Email Message]

>><http://forums.kingsnake.com/view.php?id=520074,520074>

>>

>><http://www.smuggled.com/pytrev2.htm>

>>

>>Ive already have an oppinon formed about this, but what does everyone else think???

It's Python. Hoser has provided no new evidence to support his arrangement, and there are absolutely no reasons whatsoever to follow it. If future research shows that a new genus is warranted, then the name Broghammerus will be available, but at present, there are no grounds for changing the status quo.

Cheers,

WW

WW Home

Code. Consequently, *Malayopython* will be a junior synonym of *Broghammerus* if it is ever formally published, so get used to it."

Eipper (2013) told Wüster, Schleip and others in the Wüster gang directly "I cannot see how *Malayopython* will ever be valid" and "Dual nomenclature is NOT going to help anyone."

In spite of this rapid notification of the fact on an online forum controlled by Kaiser, Wüster and associates (Shea 2013), the group have continued since that date to post as widely as possible online that *Malayopython* should be used and that *Broghammerus* is no longer a valid name (O'Shea *et al.* 2013c).

As at 25 December 2013, less than a month after first publication of Reynolds *et al.* (2013b), there were no less than 500 webpages using *Malayopython* according to a Google search for the word.

The mass usage of the term was due to immense effort by the Wüster gang in posting it everywhere, altering Wikipedia pages and the like.

In summary they were acting in contempt of the Zoological Code by seeking to erase all evidence of the name *Broghammerus*. This conduct was later described by Thorpe (2015) as an:

"agenda of trying to rewrite the history of reptile taxonomy."

These actions by Reynolds *et al.* (2013b) and those now aggressively promoting the taxonomic and nomenclatural changes within that paper are in breach of numerous parts of the Zoological Code both in letter and spirit, 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) and other sections.

What however made *Molecular Phylogeny and Evolution (MPE)* a PRINO Journal was the actions of the editors after they were first made aware of the errors in the Reynolds *et al.* paper.

Immediately upon the posting of the online version on the web, I sent emails to all the editors advising them of the errors, including that the name *Broghammerus* was valid according to the code and that the allegation within the paper of Reynolds *et al.* that the name was *nomen nudem*, was patently false.

The editors ignored my emails (even though they were phoned and said they'd got them) shortly after they were first sent.

Three months later, a third version of the paper with no substantive changes was posted on the internet (Reynolds *et al.* 2014). However it differed from Fry's version 2 in that it had page numbers placed where there had previously been "XX".

Besides the fact that the Reynolds *et al.* paper unnecessarily cluttered zoology with another unwanted junior synonym for a genus of snakes, the paper had other more serious fundamental defects.

Because the authors and publishers created three quite obviously different versions of the same paper, the newly proposed names within the paper (in this case just the one) were not even available according to the Zoological Code (Ride *et al.* 1999).

In the code it is written that for a name to be available for use:

"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."

Hence the Reynolds *et al.* name "*Malayopython*" is simply not even available under the code!

Now noting that *MPE* is a supposedly peer reviewed journal that regularly publishes on taxonomic matters you would expect that the editors and alleged peer reviewers would have understood this most simple and basic requirement of the Zoological Code and at least complied with that!

PRINO therefore becomes the only term available to describe that journal.

Another point of relevance is:

Nomen nudem is defined in the Zoological Code as a description that fails to be "accompanied by a description or definition that states in words characters that are purported to differentiate the taxon"

The original 2004 description of *Broghammerus* has a diagnosis and description of the genus in excess of 1,000 words and so must by any objective manner not be a *nomen nudem*. By comparison, the 5 word Reynolds *et al.* (2013a, 2013b and 2014) description merely referred the reader of his paper to material used as the basis for my original 2004 description!

BAIG ET AL. AND THE JOURNAL VERTEBRATE ZOOLOGY

In 2012, Baig *et al.* published a paper in the journal *Vertebrate Zoology*, naming a number of agamid species from central Asia. On the surface there was nothing untoward about the paper. It merely presented a phylogeny of the *Laudakia* Gray, 1845 group of lizards and on the basis of this, named unnamed clades apparently in accordance with the Zoological Code.

It turned out that one of these had already been

named in a paper by myself (Hoser, 2012b), several months earlier.

While it could have been possible for the relevant authors not to have known of my earlier paper, this was not in fact the case. The documents Kaiser (2012a and 2012b) had been sent to herpetologists globally including at least some of the authors of Baig *et al.*, one of whom was Wolfgang Böhme, who was listed as a signatory of the relevant document when finally published in *Herpetological Review* in 2013.

Therefore it is clear that Baig *et al.* was knowingly published in the knowledge that its final form would be producing an invalid junior synonym for an earlier Hoser name, namely *Adelynkimberlea* Hoser, 2012. Notwithstanding this fault with at least one of the listed authors, there is no evidence presented herein to suggest foul play or misconduct in terms of the peer review process at this journal in this case, even though a serious error slipped through in the publication process. At this point in time, I cannot assert that the journal is PRINO.

There are however a few key points worth noting in terms of the authors of Baig *et al.* (2012).

Their breakup 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 as I did that was available from earlier published studies on which to base their decisions. They did however remanufacture theirs as “new” data, which in itself was fraudulent. Reynolds *et al.* (detailed already) did the same remanufacture of old data as “new” to allege their renaming of my taxon was on the basis of new research and evidence, which it was not.

However notable is that Baig *et al.* did not use my earlier name *Adelynkimberlea* Hoser, 2012 for the so-called *caucasia* group, which they renamed in violation of the Zoological Code.

Now I have noted that 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.

This means the authors of Baig *et al.* (2012), when shown to be 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 would disappear into synonymy very quickly. That is how the rules of the Zoological Code work (no homonyms and the rule of priority, which gives stability).

But notable is that Böhme (who is listed twice) as a supporter to Kaiser *et al.* (2013) in that document,

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, and who has actively campaigned to have private keeping of reptiles as pets outlawed (Uetz 2014a), has at Uetz (2013a), noted his use of the Baig *et al.* (2012) 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.

Recall they wrote:

“These recommendations are not formal nomenclatural proposals according to articles of the Code”.

In spite of this statement Uetz is blindly following the Kaiser *et al.* call to arms to step outside the Code and rename validly named taxa.

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 has made clear elsewhere he knows about. This is in spite of him marketing his website as “The Reptile Database”.

Further evidence of this is in his *stellio* page (Uetz 2013b), which mentions the Hoser *Laudakia* paper of 2012 (Hoser 2012b).

I also note the conflict in this case with Böhme's stated support for the Zoological Code and the rule of priority in particular 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).

Also of note is the alleged date of the publication of Baig *et al.*. The cover date only post-dates the Hoser *Laudakia* paper by a few weeks. However the actual difference in dates was about 4 months as evidenced by the dates of receipt listed by *Zoological Record*.

An issue that has caused problems in the past has been publication dates of taxonomy papers and the backdating of cover dates of journals.

Due to the ethical considerations involved if backdating a journal to claim nomenclatural priority over an author who may have published earlier elsewhere, it is critical that journal editors not engage in this nefarious practice.

If anything, a cover date should post-date the actual

publication date.

In the case of *AJH*, publication date is always set a few days to a week after the date of receipt of printed copies from the printers.

Online versions of papers are not published for a month after this date (plus or minus 7 days).

Raymond Hoser and associated publishing entities will never be guilty of the crime of backdating taxonomic papers to claim priority for names.

WALLACH, WÜSTER AND BROADLEY AND THE JOURNAL ZOOTAXA

Zootaxa is owned by a New Zealand-based public servant and markets itself as "A mega-journal for zoological taxonomists in the world".

In a very short space of time it has become the 1,000 kilo elephant when it comes to zoological taxonomy publications, easily eclipsing all other like journals in terms of volume of material published and new scientific names coined.

Zootaxa claims to be peer reviewed, but the regular break down of quality control at this journal clearly indicates either otherwise or severe defects in their quality control processes.

Editors of the journal have been forced to publish papers asking authors to improve quality of submissions and at the same time limiting the blame of their own reviewers for defects that slip through the editorial net.

At least once a species description for an Australian taxon was published minus any holotype details.

How such a fundamental taxonomic error can slip through the quality control net defies belief.

In late 2009, Wallach, Wüster and Broadley published a paper in *Zootaxa* making demonstrably false claims against myself in order to rename a genus of Cobra (*Spracklandus* Hoser, 2009).

The claim of note was that *Australasian Journal of Herpetology* had never been published in hard copy, was therefore not code compliant and therefore this allowed the trio of authors to steal my work and rename the species.

At the time this publication appeared in *Zootaxa*, a second paper by Wüster appeared describing a new species of snake. It appears they were submitted as a pair so that the paper renaming *Spracklandus* would slip through the peer review net more easily.

Notwithstanding the fact that the Wallach *et al.* paper creating their junior synonym "*Afronaja*" was published with errors, *Zootaxa* became known as a PRINO paper after the editors refused to retract the information they were made aware was false, notably the central claim by Wallach *et al.* that *AJH* had not been published according to the Zoological Code.

Even following the publication of Hoser (2012a) which included library receipts for hard copies of *AJH* Issue 7 in 2009 prior to the publication of the Wallach *et al.* paper later in 2009, thereby confirming the priority of the Hoser name, I did not get any published retraction by *Zootaxa*.

This is significant as the Wallach *et al.* paper also made the demonstrably false claim that my earlier dated paper had attempted to steal name rights from the later authors and the *Zootaxa* editors were also made aware of the falsity of the claim and yet refused to publicly retract it or apologise for it.

The claim also repeated by Wüster *et al.* (2014) in *BZN* that Hoser had unethically scooped their own allegedly pending work by naming *Spracklanus* was rebutted by Wüster himself in Wüster (2009a) where just six days after the publication of Hoser (2009b), he condemned the taxonomy in that paper to a global audience and added:

"The case for keeping it (*Naja*) as a single genus was made by Wüster *et al.* 2007."

His mate Bryan Fry followed this on the same date with:

"Wolfgang's 2007 paper already considered the higher order taxonomy of cobras and quite rightly lumped them into a single genus."

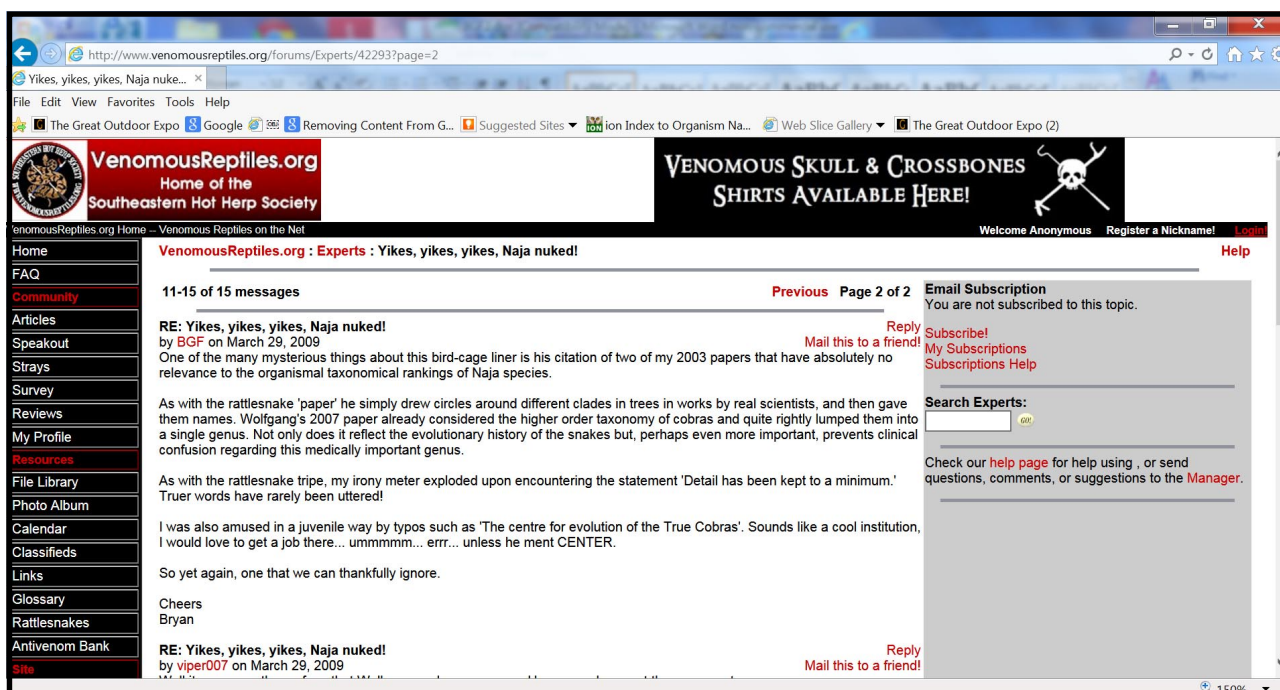
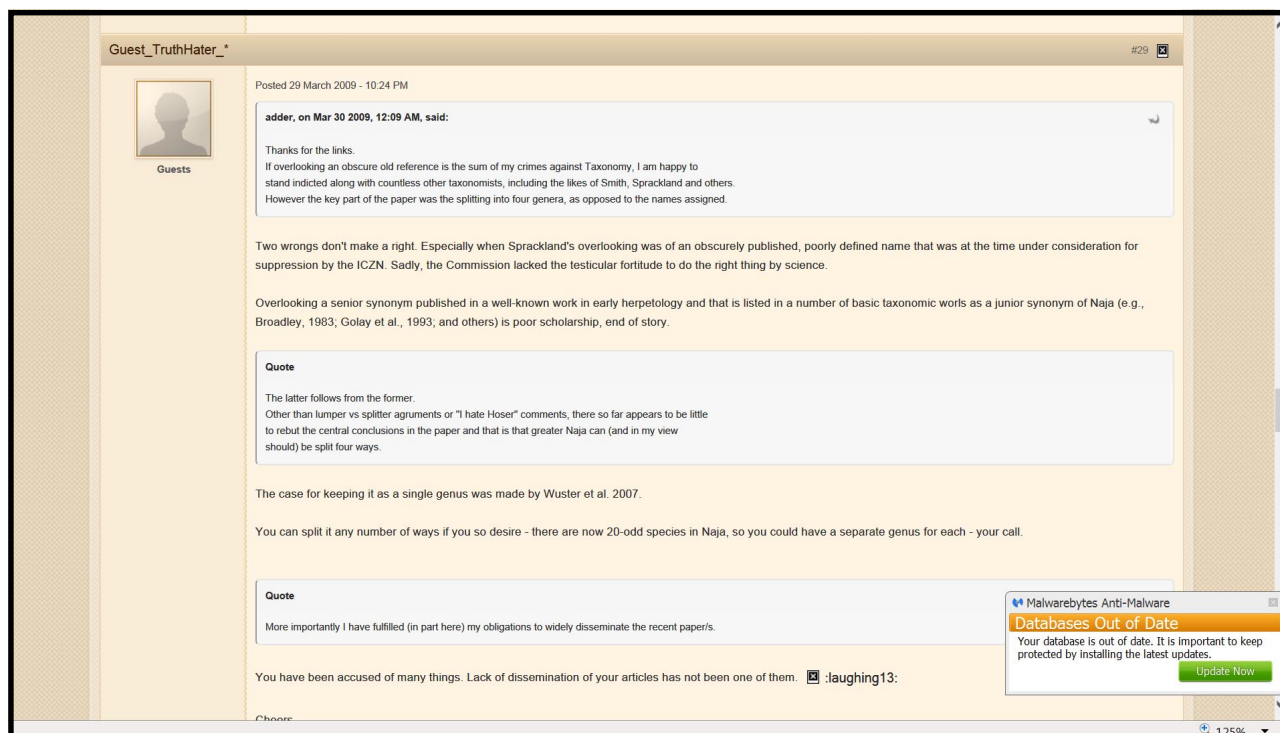
(Fry 2009).

Hoser (2009) had clearly rejected Wüster's own published taxonomy and the appropriate code-compliant nomenclature of Hoser (2009) namely *Spracklandus* Hoser, 2009, followed from this.

From the content of Wallach *et al.* (2009) it is clear that Wüster *et al.* amended their own taxonomic views to align with those of Hoser, well after the publication of Hoser (2009). This meant it was not possible for Hoser to have improperly knowingly "scooped" any work or ideas of Wüster and Wallach at the time Hoser (2009) was published, because the public record shows they were opposite at the time.

An approach by myself to the ICZN to formally rule in favour of the genus name *Spracklandus* was initially resisted by most commissioners as unnecessary, noting that the paper Hoser (2012a) clearly showed the Hoser name to be valid and have date priority and that the ICZN do not as a rule make judgements about the obvious and what as a matter of course should not be a nomenclatural dispute.

After independently verifying the proper publication of the *Spracklandus* description in issue 7 of *AJH*, and that it was fully code compliant in 2009 and in the face of the reckless publication of Kaiser *et al.* (2013), ICZN Commissioners as a group agreed that they should issue a ruling condemning the taxonomic vandalism of the Wüster gang as



encapsulated in their coining of the junior synonym *Afronaja*.

Hence the ICZN published case 3601 in the *Bulletin of Zoological Nomenclature* in 2013 (ICZN 2013).

At the time this case was first submitted to the ICZN in early 2012, Wüster and his gang had not attempted to rename any other taxa.

However in the period late 2012 to mid 2014 this changed dramatically.

BATES ET AL. AND THE JOURNAL ZOOTAXA

In 2013, I published a paper redefining and naming new genera within the African Gerrhosauridae lizards. The basis of this included well-known phylogenetic data and obvious morphological differences between taxa (Hoser 2013c).

Initially this paper was condemned by the Wüster gang.

Bill Branch loudly condemned the newly published Hoser (2013c) erection of the genera *Funkisaurus* and *Swilesaurus* as:

“futile names in his latest offering on the Cordylidae and Gerrhosauridae”,

saying herpetologists should

“reject his work in its entirety”,

(Branch 2013).

However a few months later he had obviously reconsidered his position in terms of the science and taxonomy.

This is because he literally stole the same work and repackaged it, renaming the very same genera on the basis of the very same evidence as a co-author of a paper published in *Zootaxa* known as Bates *et al.* (2013)!

The authors wrote:

“We concur with the opinions, recommendations and proposals of Kaiser *et al.* (2013) regarding best taxonomic practice, and therefore reject the unscientific taxonomy of Hoser (2013). We consider the nomenclatural changes to the families Gerrhosauridae and Cordylidae proposed therein (Hoser 2013) to be ill-conceived and unethical, and thus unavailable. We note specifically that the privately-published and personally-edited work of Hoser (2013) is in direct violation of the spirit and intention of the Code (ICZN 1999) as indicated by, inter alia, Recommendation 8A which explicitly encourages publication in “appropriate scientific journals or well-known monographic series”.

They also cited the earlier Hoser paper in full in their bibliography.

The comments above are ridiculous for several reasons, not the least being that Kaiser *et al.* (2013) specifically ruled out their comments as being code

compliant directives.

They had written:

“These recommendations are not formal nomenclatural proposals according to articles of the Code”.

The claims against myself and *AJH* are dubious at best and even at their highest level get nowhere as “Recommendation 8A” of the Zoological Code is just that; a recommendation!

As noted by many, this was just a crude attempt to steal my earlier work!

In this case, there is no hard evidence to say that there was no peer review done on the journal *Zootaxa*. Although I could easily argue that there is no evidence to suggest there was any peer review either!

However it is clear yet again that this process (peer review), if it has in fact occurred, has failed at *Zootaxa*!

Noting the code-compliant wide dissemination of my own papers and the even wider condemnation of them by the Wüster gang (including by co-author Branch himself), it defies belief that any qualified peer reviewer at any herpetological journal would have been unaware of the relevant earlier Hoser paper.

I need not mention that the Hoser paper is explicitly mentioned in the *Zootaxa* paper being “peer reviewed” so it would have been impossible for a reviewer not to have known about it.

Of course the reviewer should have perused both the relevant Hoser paper and if unaware of the relevant parts of “The Code”, it as well.

While this does not mandate anyone to agree with or use the taxonomy proposed in that Hoser paper, the rules of the Zoological Code do mandate use of the relevant names if and when the taxonomy is agreed (as it quite logically was by Bates *et al.*).

The failure of the editors at *Zootaxa* to get it right in Hoser-related matters twice clearly indicates that peer review at that journal is in name only!

Now before things here are taken out of context, I note from *Zootaxa*'s website as of 2 July 2014 said “Zootaxa published 24% of all new taxa and nomenclatural acts in the last five years, according to *Zoological Record*”. Taking this claim on face value, that is an enormous quantity in anyone's language and includes many thousands of species descriptions and papers by many thousands of authors.

The overwhelming bulk of these papers are of impeccable scientific merit and quality and this paper should not be seen as an attack on the whole.

However where there is an obvious break down in

the peer review process of quality control it must be highlighted as a first step towards removing the problem in the future.

HEDGES *ET AL.* AND THE JOURNAL *CARIBBEAN HERPETOLOGY*

In 2012, I published a large monograph reclassifying the entirety of the world's extant scolecophidians better known as blindsnakes (Hoser 2012d).

The division of families, tribes, genera and subgenera was on the basis of clear molecular and morphological criteria based on numerous studies spanning over 100 years.

It was conservative in the extreme but still resulted in the erection of dozens of new genera and subgenera for a group that had been clearly neglected by herpetologists throughout the previous 200 years.

Two years later an inveterate namer of taxa, Blair Hedges and others published a paper in *Caribbean Herpetology* (Hedges *et al.* 2014) using much the same data and renaming a nine new genera, of which eight had been previously identified and named by myself.

In this paper was no reference to my earlier work (Hoser 2012) or for that matter Kaiser *et al.* (2013), which on the face of it could lead a reader to believe the authors had made an inadvertent error and overlooked the earlier paper.

That we know this wasn't the case is shown by reading Kaiser *et al.* (2013) in which one of the authors of the Hedges *et al.* paper (Nic Vidal), is listed as a cosignatory and supporter of that very document. As the Blind snake paper (Hoser 2012) was cited by Kaiser *et al.* (2013) along with all the relevant erected genera, Vidal must have been well aware that he was over-writing valid genera with junior synonyms when publishing his own paper two years later.

I should also note here that one of the 8 genera I erected that was overwritten with a coined name by Hedges *et al.* was none other than *Argyrophis* Gray, 1845, which I had ethically resurrected from synonymy in my 2012 paper.

So in other words, the Kaiser gang had now effectively decided any and all other author's names were fair game to be overwritten with their coined names, including code compliant names erected more than a century and a half earlier!

Comments by the coauthors of this paper and other members of the Wüster gang on social media (mainly Facebook) clearly confirm that the authors knew they were trying to rename genera previously named by myself in the ridiculous belief that the Wüster gang would be able to secure an ICZN ruling to suppress the proper scientific names.

Notwithstanding the author misconduct in this case, it is clear that peer review failed to detect the critical errors in the paper, that being that all but one genus being named was in fact already named ... and not just by Raymond Hoser!

As a matter of scientific protocol and procedural integrity, relevant earlier works need to be cited by authors.

The failure of Hedges *et al.* to cite the most significant publication in history relating to the extant scolecophidians is a serious oversight indeed!

What is possible of course is that Hedges *et al.* were doing what the Wüster gang had been seeking for some time and that was to forcibly erase any record of Raymond Hoser from the zoological record (O'Shea 2013b).

PYRON AND WALLACH AND THE JOURNAL *ZOOTAXA*

Building on the taxonomic dog's breakfast of Hedges *et al.* (2014), Alexander Pyron and Van Wallach, published their own revision of the extant Blind Snakes on 8 July 2014.

As he has done previously, Wallach used the PRINO Journal *Zootaxa* as a vehicle to recklessly publish his work.

Like Hedges *et al.*, the bulk of the paper consisted of a remanufacture of old data and material that was dishonestly presented as "new" research.

Notably, the authors cited Kaiser *et al.* (2013) as the basis for their choosing to ignore and overwrite valid names first proposed in the 2012 Hoser Blindsnake paper.

This included creating yet another junior synonym for a Hoser-named genus of Madagascan Blindsnakes *Elliotttyphlopea* Hoser, 2012 in the form of their own newly coined name "*Lemuriatyphlops*" using the very same type species.

Otherwise the bulk of the paper was a rehash of Hedges *et al.* (2014) with the same obvious defects as that paper.

Of course, I need not mention that Kaiser *et al.* (2013) wrote:

"These recommendations are not formal nomenclatural proposals according to articles of the Code".

Hence the use of Kaiser *et al.* to overwrite properly formed scientific names by either set of authors was something no proper peer reviewer or editor should have allowed.

That is unless the journal is a PRINO rag!

Or as Scott Eipper (Eipper 2013) said:

"You cannot use a viewpoint (Kaiser *et al.* 2013) - to act as a veto- to disregard the use of the code."

THOMAS *ET AL.* AND THE JOURNAL ZOOTAXA ... AGAIN

In 2013, I formally described two Alligator Snapping Turtles (*Macrochelys* Gray, 1856), one as a species and the other as a subspecies (Hoser 2013a), although the latter taxon sat on the cusp between being placed as a species as well so I took the more conservative position.

In 2014, Thomas *et al.* published a paper renaming the same taxa, both as full species and using very creative means to commit what initially appeared to be the perfect crime to get away with their plan.

Unlike other papers seeking to steal my work, this one did not cite Kaiser *et al.* (2013) as the basis for their attempt to rename my taxa. Although I note that in their acknowledgements, members of the Wüster gang and co-authors of Kaiser *et al.* were thanked for their comments and advice.

Instead Thomas *et al.* mounted what on the surface seemed to be a perfectly reasonable argument in favour of their actions.

Hoser (2013a) presented perfectly normal code-compliant descriptions of the two taxa, citing holotype specimens at the University of Florida as is the usual practice.

Thomas *et al.* (2014) overwrote the Hoser descriptions as if they didn't even exist.

Near the end of the paper, the authors made the following justification:

"Hoser (2013) attempted to describe a new species, *Macrochelys maxhoseri*, and subspecies, *M. temminckii* (sic) *muscati*, in his self-published, non peer-reviewed "journal," but he erred in his methods. In designating holotypes using an online database in lieu of actually examining specimens, Hoser declared "specimens" UF 155266 and UF 165801 as primary types. However, the curator of herpetology at the FLMNH indicated that physical specimens bearing either of these numbers have never existed among their holdings; the corresponding records in the FLMNH database refer to unvouchered field sightings of *Macrochelys* (M.A. Nickerson, Pers. Comm. 2013). Hoser's holotypes are therefore designated in violation of ICZN Code Article 16.4 (they are not based on specimens; ICZN, 1999), and his names for *Macrochelys* are rendered unavailable."

I immediately commenced an investigation into what had happened and found that records at the University of Florida had been altered including the two online databases, changing the original specimen records from "preserved specimen" to that of "occurrence".

Knowing that the authors of Thomas *et al.* (2014) included staff at the relevant facility it was obvious

that the authors would want to stick with their story of my specimens not existing and therefore my entire description being invalid.

That this was to be their course of action was indicated by other unethical actions by the authors.

The authors put out several widely reported media releases claiming they had discovered the two species.

Nothing was further from the truth!

Besides the fact that I had published descriptions of the two taxa a year before them, the differences between the relevant living forms of *Macrochelys* had been known since at least 1993, when I first started working with the taxa in Florida.

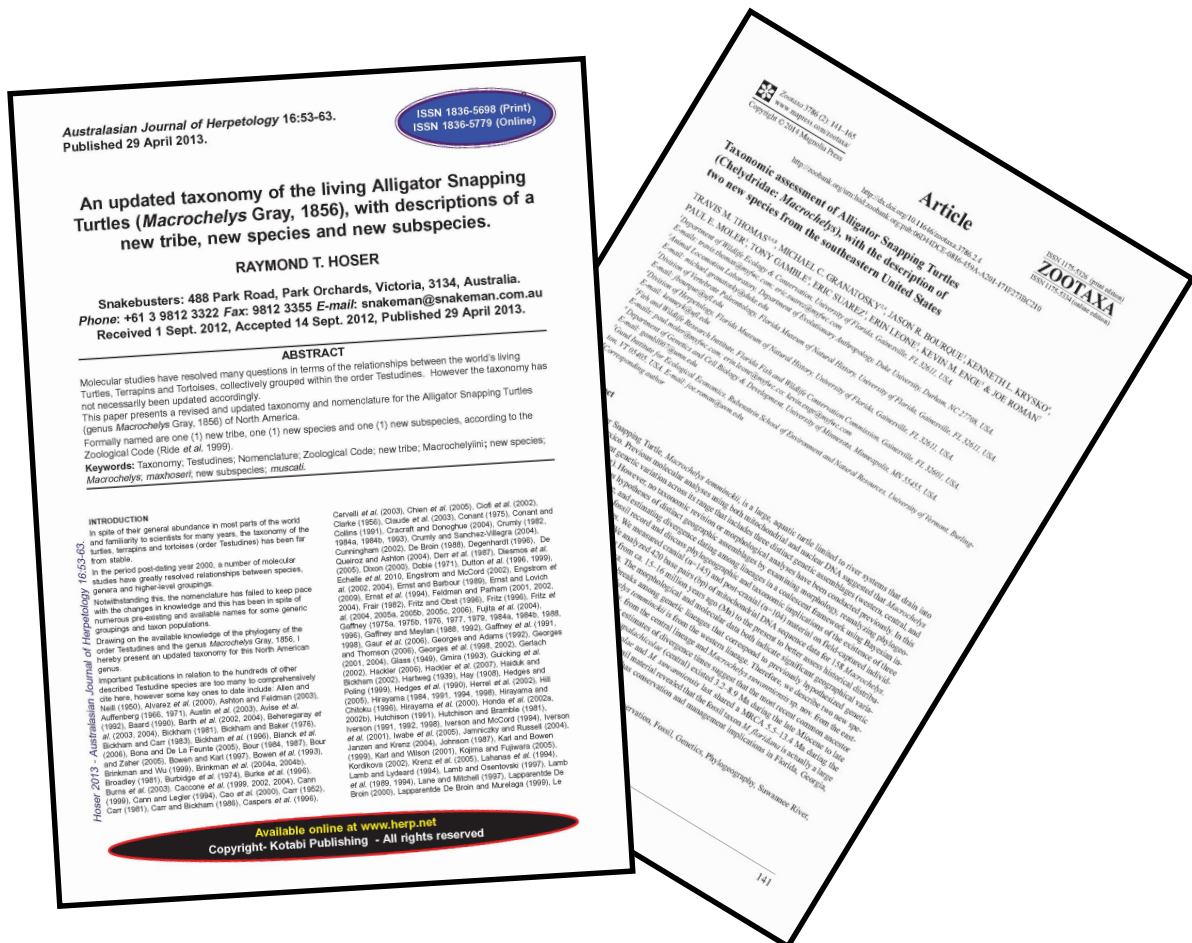
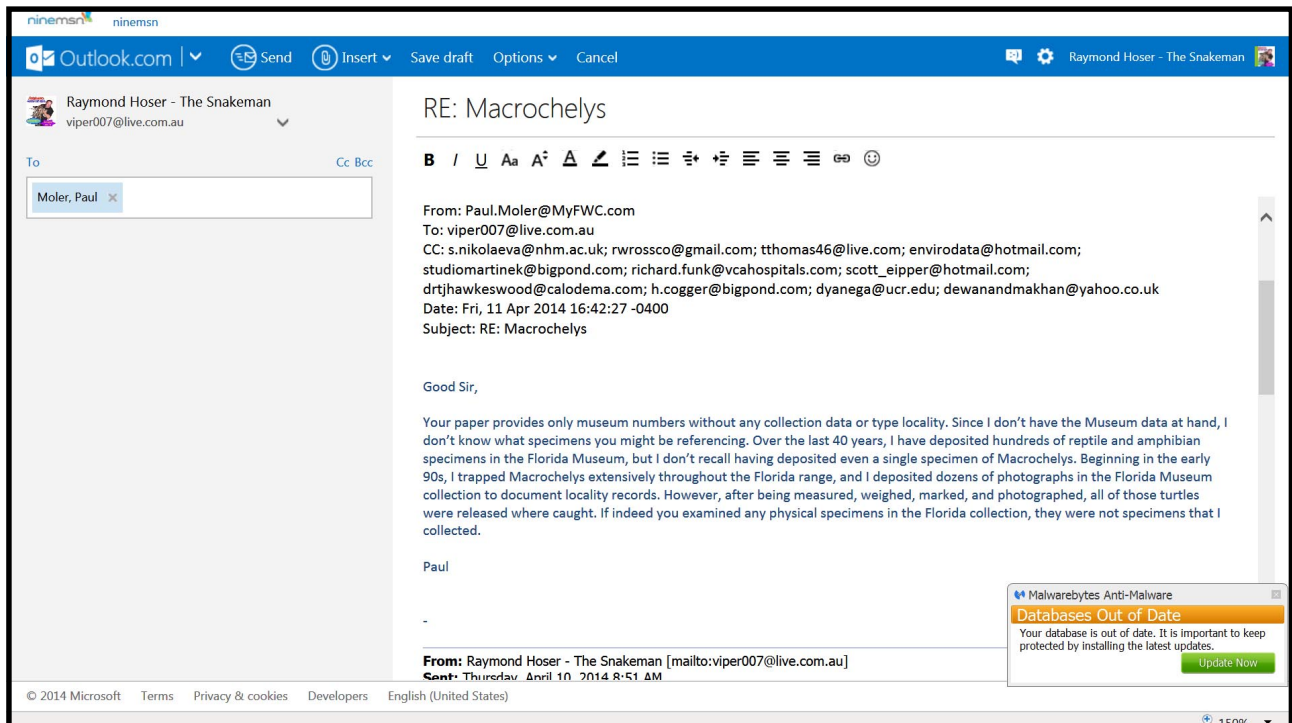
Furthermore the details of the differences between the three (newly) described forms had been in the published literature in the intervening two decades, so the claims by Thomas *et al.* of discovering the two newly described species was both false and a brazen attempt to gain favourable publicity for their "work".

In line with the papers by the authors described above, namely Wallach *et al.* (2009), Baig *et al.* (2012), Bates *et al.* (2013), Hedges *et al.* (2014) and Reynolds *et al.* (2013a, 2013b and 2014), the allegedly new data published in the paper by Thomas *et al.* to support the idea that there were (allegedly) undescribed species was simply remanufactured old data that had been on the public record for some years.

On the publicly available databases, the collector of my holotypes was listed as none other than one of the co-authors, Mr. Paul E. Moler. As part of my investigation as to whether or not my published descriptions were code-compliant, I sought his explanation as to the original (at that stage unaltered) Museum records that had formed the basis of my holotype designation.

In his reply of 11 April 2014, Moler wrote: "Good Sir,

Your paper provides only museum numbers without any collection data or type locality. Since I don't have the Museum data at hand, I don't know what specimens you might be referencing. Over the last 40 years, I have deposited hundreds of reptile and amphibian specimens in the Florida Museum, but I don't recall having deposited even a single specimen of *Macrochelys*. Beginning in the early 90s, I trapped *Macrochelys* extensively throughout the Florida range, and I deposited dozens of photographs in the Florida Museum collection to document locality records. However, after being measured, weighed, marked, and photographed, all of those turtles were released where caught. If indeed you examined any physical specimens in the



Florida collection, they were not specimens that I collected.

Paul"

Obviously not realizing the ramifications of what he had written, Moler had in fact confirmed the validity of my *Macrochelys* names by confirming that the holotype designations were in fact based on specimens, even if they were no longer at, or ever held at, the University of Florida.

This became clear during a heated debate on the online Taxacom and ICZN list forums in April 2014.

Initially ICZN commissioner Doug Yanega upheld the published view of Thomas *et al.* on the alleged basis that my names weren't valid as my holotypes did not exist and never had.

At the same time, he made baseless allegations of fraud and misconduct against myself (which were quickly refuted), but these aren't even relevant here.

What is relevant is the following comment made by Mr Yanega on 13 April when he said:

"The Code DOES require that a holotype specimen exist, and, when designating that holotype, the ONLY thing that can *ever* be used in lieu of the physical specimen itself *for purposes of explicit designation* is an illustration or photograph OF THAT EXACT SPECIMEN."

The capitals above are his.

Following my posting of the details of the email from co-author Moler, the legal status of my published descriptions changed quite dramatically as it fitted within what Yanega had said would make the descriptions code compliant, this being the existence of:

"an illustration or photograph OF THAT EXACT SPECIMEN."

This point was affirmed by fellow Commissioner, Frank Krell.

The relevant section of the code relied upon by Thomas *et al.* (2014) was 16.1 which reads:

"16.4. Species-group names: fixation of name-bearing types to be explicit. Every new specific and subspecific name published after 1999, except a new replacement name (a nomen novum), for which the name-bearing type of the nominal taxon it denotes is fixed automatically [Art. 72.7], must be accompanied in the original publication

16.4.1. by the explicit fixation of a holotype, or syntypes, for the nominal taxon [Arts. 72.2, 72.3, 73.1.1, 73.2 and Recs. 73A and 73C], and,

16.4.2. where the holotype or syntypes are extant specimens, by a statement of intent that they will be (or are) deposited in a collection and a statement indicating the name and location of that collection."

While there had originally been a claim made that

the specimens I had listed as a holotypes were non-existent as in the animals themselves had never existed (as per Yanega's above quoted statement) and an associated claim by Thomas *et al.* that I had made the whole thing up, their claim had now been altered by co-author Moler to one of that the specimens did exist as reported on the original database entries, but were not retained at the Museum, but rather were re-released back into the rivers of Florida after their photos and details were lodged and logged into the Museum's records.

This salient fact made the relevant Hoser names "available" under the Zoological Code and therefore the Thomas *et al.* names junior synonyms of them.

That in effect made the Thomas *et al.* paper a redescription of the two Hoser-named taxa.

While this material has been published on the internet forums Taxacom and the ICZN List, online sources are transient at best and so it is important that this material be published here in permanent and code-compliant form to confirm that the correct names for the relevant taxa are *Macrochelys maxhoseri* Hoser, 2013 and *Macrochelys muscati* (assuming the latter taxon is treated as full species) as opposed to merely a subspecies of the wide-ranging *M. temminckii*.

Subsequent to the disclosure that the Hoser holotypes did in fact exist and had been photographed and recorded by the University of Florida Museum via Paul Moler, Stephen Thorpe stated the obvious on Taxacom:

"Observational records or not, there is nothing in the Code which clearly invalidates Hoser's holotype designations! People are reading into the Code what suits their agenda, and not what is actually written!"

So from the above, we know that Thomas *et al.* had acted unethically.

I might also add that if there was a genuine grievance about the validity of the Hoser names from 2013, Thomas *et al.* should have done the ethical thing and advised me of their concerns so that as first author, I could deal with them appropriately.

That they did not do this proves the bad intent on their part; this being an improper desire to scoop naming rights over the two relevant species-level taxa.

The email from co-author Moler and Thorpe's quite appropriate comments in light of this, shows that with one simple question of just one listed co-author, Thomas *et al.* could have worked out that the Hoser descriptions were valid under the Zoological Code. They could have thereby avoided the reckless renaming of the two taxa!

Turning to the issue of alleged peer review, it is clear

that any credible scientist would have raised similar concerns to the authors of the paper in light of the paragraph alleging another recent paper had messed up their scientific descriptions.

Again, I note that with one simple question of one relevant person (that being the man who allegedly caught the Hoser holotypes), who also happened to be a listed co-author, a peer reviewer would have ascertained that by any reasonable interpretation of the code, the Hoser descriptions were valid, leading to either the wholesale rejection of the Thomas *et al.* paper, or a serious reworking using the correct nomenclature.

While there is no strict evidence of editorial misconduct in this case at *Zootaxa*, this case does demonstrate yet another example of serious failure in the (alleged) peer review process for this journal.

However I should also make it clear that the repeated citing of failures at *Zootaxa* are as much a reflection of the sheer quantity of publications there as opposed to the standard there being substantially worse than at other major journals.

However the consistent failure of peer review at *Zootaxa* specifically in terms of Hoser relevant papers is clearly a serious issue indicative of editorial misconduct.

The issue of poor quality publications sneaking through peer review is raised constantly and in recent times, electronic publication has been cited as part of the cause, this being the ease and speed at which authors and journals can publish.

Zootaxa is the holotype example of a publication that has exploited the technology of electronic publication and online access to skate around the spirit of the Zoological Rules of the Zoological Code, while appearing to be complying with the letter of the articles.

Hard copy *Zootaxa* are effectively non-existent and the publishers of the journal have exploited the Zoological Code's articles to skate around the requirements of Article 8, although this is somewhat redundant now that the ICZN amended the code (in 2013) to allow online only publication.

This is noted in as much as those who allege various dangers arising from the prospect of online publications need look no further than the model afforded by *Zootaxa*. If they are happy with the overall result, then any potential dangers can probably be managed without gross destabilization of taxonomy and nomenclature.

For my own part, I was opposed to the concept of electronic only publication of nomenclatural works on the basis that it is too easy to alter online works after original publication.

The best example of the manifestations of my

concerns were the two altered versions of the paper "Williams and Starkey (1998)", first published in that year, altered by the listed senior author, David John Williams, twice in year 2000, in order to allow him to make false allegations against myself.

That case is detailed by Hoser (2001) and the three versions of that paper can still be accessed on the web as of May 2015, having been posted on the server at www.smuggled.com in 2001 by myself.

More recently, the debacle of the three online versions of Reynolds *et al.* (2013a, 2013b and 2014), cited already shows how online publications frequently step outside the current version of the code.

Taxonomy list servers in 2014 and 2015 (Taxacom and ICZN list) carried numerous complaints by taxonomists of major journals and publishing houses publishing various electronic versions of what were meant to be the "same paper" and also operating outside the ICZN's recent amendments that gave requirements for code-compliant online publications.

CONSERVATIVE, RADICAL OR ON THE MONEY?

It is a gross misconception to assert that a person who formally names a lot of taxa is a so-called "splitter".

Taxonomists worth their salt do not try to create species or "split", where they are not justified.

Taxonomic vandals on the other hand do just that.

They split where splits are not justified and to qualify for the title of taxonomic vandal, they must do so without any credible evidence.

An obvious example of what I mean is seen in the dissection of the genus *Leiopython* as popularly recognized throughout most of the 20th century.

I conservatively split the black specimens from the south of New Guinea from the brown ones in the north and named the southern species.

That was the end of my "splitting" of the genus.

The split was obvious and in spite of the Wüster gang's best efforts, my taxon was widely recognized within a short time.

By contrast, Schleip "attacked" the same genus of snakes 8 years later with his unique brand of evidence free taxonomy and found no less than three "new" species which he said I had overlooked and he promptly named them.

These were rapidly relegated to the synonymy bin by myself within months in Hoser (2009a), but disregarding what would be alleged to a bias by myself, the same treatment of these non-taxa was meted out by most herpetologists almost immediately (e.g. Natusch and Lyons 2012), those authors pointing out the obvious fact that there was

[Taxacom] [iczn-list] provisions to the Code

Neal Evenhuis [neale at bishopmuseum.org](mailto:neale@bishopmuseum.org)

Sun Apr 13 18:09:04 CDT 2014

- Previous message: [\[Taxacom\] \[iczn-list\] provisions to the Code](#)
- Next message: [\[Taxacom\] \[iczn-list\] provisions to the Code](#)
- Messages sorted by: [\[date \]](#) [\[thread \]](#) [\[subject \]](#) [\[author \]](#)

I unfortunately have to agree with Stephen here.

I've again read the relevant Articles dealing with holotype and "type series" and Hoser has met the conditions required, although, granted, merely on a technicality because there are no restrictions in the current Code to what he has done, and this type of methodology of selecting a holotype will definitely not make it to the examples in the "Best practices in taxonomy" handbook (although he probably had no choice since I doubt anyone will ever loan him specimens).

The critical thing is he gave a specimen number for a holotype, which was based on an actual specimen. There is nothing in the FLMNH database to indicate that the specimen number he gave was NOT based on a specimen -- it even specifies that there was 1 specimen, not 0. The Herpnet database gives no numbers of specimens for those specimen numbers, but lists the word "Occurrence" for those same numbers (and some on this list have said they all refer to observations). Yet Herpnet also gives the word "Occurrence" for the specimen numbers in the appendix in the Thomas et al. paper that they say is "Material examined!" (how could they have examined an observation?). Again there is nothing in either database to indicate that a "Specimen number" would be associated with anything other than a specimen. Unvouchered or not, if these pertain to observations, why then give them specimen numbers? If each observation by those people listed as collector(s) for those entries was NOT based on a specimen, then what were they based on? A ghost? A hallucination?

In the midst of all this, no one has pointed out the irony here: Hoser has been consistently labeled a taxonomic "vandal" for producing questionable new taxa and people want to suppress or synonymize his names. Well, here is one case where the Thomas et al. team actually agree that Hoser was correct! But rather than alert Hoser to a potential problem that could be rectified by Hoser or a co-authored article -- because he is so despised, they took it upon themselves to name the creatures themselves.

You herp guys are indeed an amusing bunch.

.. and to follow on with what others have said: YES! The next edition of the Code needs to be more clear on what does and does not fulfill the requirements of a type "specimen". But whatever changes are made, it cannot be done retroactively.

-Neal

From: [taxacom-bounces at mailman.nhm.ku.edu](mailto:taxacom-bounces@mailman.nhm.ku.edu) [[taxacom-bounces at mailman.nhm.ku.edu](mailto:taxacom-bounces@mailman.nhm.ku.edu)] On Behalf Of Stephen Thorpe [[stephen.thorpe at yahoo.co.nz](mailto:stephen.thorpe@yahoo.co.nz)]

Sent: Sunday, April 13, 2014 11:05 AM

To: Doug Yanega; Raymond Hoser - The Snakeman; iczn-list; [taxacom at mailman.nhm.ku.edu](mailto:taxacom@mailman.nhm.ku.edu); [rwrossco at gmail.com](mailto:rwrossco@gmail.com); [envirodata at hotmail.com](mailto:envirodata@hotmail.com); [drtjhawkeswood at calodema.com](mailto:drtjhawkeswood@calodema.com)

Subject: Re: [Taxacom] [iczn-list] provisions to the Code

None of this is relevant! Observational records or not, there is nothing in the Code which clearly invalidates Hoser's holotype designations! People are reading into the Code what suits their agenda, and not what is actually written!

Specifically, the Code states (basically) that a holotype specimen must be designated. That means that a particular specimen must be specified, but it doesn't say in what ways it may be specified. Therefore, specifying a specimen by way of an observational record of that specimen is OK. The Code makes it clear that an author doesn't actually have to have seen or examined the holotype itself.

There is NO problem, Houston!

Stephen

no way to separate the three Schleip species from nominate *L. albertisi*.

Just so there is no doubt as to who's who in the zoo, so to speak, Natusch and Lyons (2012) like everyone else did accept the obvious and that was that *L. hoseri* was a unique and valid species, quite different from *L. albertisi*, the species from which I "split" it a decade later.

When Hoser (2013a) published the descriptions of two *Macrochelys* taxa, I was met with the usual howls of protest from the Wüster gang, including with an online paper by Van Dijk *et al.* (2014) telling people not to use the names *M. temminckii muscati* or *M. maxhoseri*, and to instead treat them as synonyms of *Macrochelys*.

Without any scientific explanation Van Dijk *et al.* (2014) wrote:

"treat his names, *Macrochelys temminckii muscati* and *Macrochelys maxhoseri*, as unavailable synonyms of *Macrochelys temminckii*."

Of course this position was nothing more than the denial of reality ratbaggy that has been a hallmark of the Wüster gang since year 1998.

After all the differences between the relevant forms was well known and had been in the popular literature for many years as noted by Hoser (2013b). However when the Wüster gang finally got around to checking out my paper and deciding to steal my work and improperly rename the taxa as detailed already, they went further than myself and elevated *M. temminckii muscati* to be a full species, but instead with their own improperly coined name.

As for Schleip and his newly found *Leiopython* species, created by him after reviewing my paper of 8 years prior, my review of the relevant group had been shown by him at least to be way too conservative (Schleip 2008)!

Evidence of any taxonomic vandalism in terms of the Hoser taxonomy is simply absent.

Folt and Guyer on 27 March 2015 published a paper titled, "Evaluating recent taxonomic changes for alligator snapping turtles (Testudines: Chelydridae)", in *Zootaxa* 3947(3):447-450.

In line with the paper by Barker *et al.* (2015) dealt with elsewhere in this paper, the authors of this brief paper ignore the paper of Hoser (2013a) and instead use the names of Thomas *et al.* (2014).

It questionable if the authors and journal editors and alleged peer reviewers at *Zootaxa* should be given the benefit of the doubt in terms of their apparent lack of knowledge of either the Hoser (2013a) descriptions of the *Macrochelys* taxa (bearing in mind they'd been referred to in an earlier *Zootaxa* paper (Thomas *et al.* 2014) or that they were in fact

valid, but we'll ignore these things for the moment.

Significantly however the authors of the 2015 paper in fact argue in favour of my exact taxonomic position in not recognising, *M. temminckii muscati* as a full species.

The end point in all this, is that after two fraudulent papers published in the same PRINO journal *Zootaxa*, that dishonestly remanufacture other people's earlier research as their own "new" research, the authors of these apparently PRINO papers have collectively arrived at the same taxonomic position as Hoser (2013a) did two years prior!

As argued elsewhere by Ross Wellington (of Wells and Wellington 1983 and 1985 fame), time alone will show who is in fact the taxonomic vandals and who in fact was the first to have made correct scientific taxonomic judgements.

ZOOTAXA, ZOOTAXA, ZOOTAXA!

Here in Australia a number of Museum curators and other scientific authors have deridingly referred to *Zootaxa* as "the McDonalds of taxonomic publications" on the basis they publish almost anything and without any semblance of checks or quality control.

Some of these people publish there themselves and when I questioned this, they replied along the lines that like fast food, it is quick and convenient. "You write, they publish!" and "...no waiting".

Yes it is a good business model!

However, the number of papers with inexcusable errors that seem to have appeared in *Zootaxa* is immense and clearly shows a breakdown in terms of editorial control and by definition, quality peer review as well.

For example, Thomson and Georges (2009) was published in *Zootaxa* and it made some outrageous allegations against another scientist Richard Wells. The paper alleged that a paper of Wells published two years prior (Wells 2007), which named a genus of Australian freshwater terrapins had not been published according to Article 8 of the Zoological Code.

Noting that Wells had been publishing taxonomic papers since at least 1983 (more than two decades) and this was well-known noting that the ICZN had themselves had several sets of proceedings involving his papers (ICZN 1991, 2001) one would presume he was aware of the most basic rules of the code, including the need to produce multiple identical printed hard copies.

So if a claim to the contrary were made, you would expect a search to be made to ensure that the claim was factually correct.

The scam perpetrated by Thomson and Georges

From: [taxacom-bounces at mailman.nhm.ku.edu](mailto:taxacom-bounces@mailman.nhm.ku.edu) <[taxacom-bounces at mailman.nhm.ku.edu](mailto:taxacom-bounces@mailman.nhm.ku.edu)> on behalf of Michael A. Ivie <[mivie at montana.edu](mailto:mivie@montana.edu)>
 Sent: 16 April 2014 19:06
 To: [taxacom at mailman.nhm.ku.edu](mailto:taxacom@mailman.nhm.ku.edu)
 Subject: Re: [Taxacom] [iczn-list] provisions to the Code

OK, everyone step back and breath. This is not that simple. How much worse is AJH than Calodema? How much worse is Calodema than Mélanges Exotico-Entomologiques? Than L'Echange? Those last 2 journals, privately published by the author of tens of thousands of poorly documented species were proposed for suppression decades ago, and that was voted down. What about the allegedly back-dated "Cicindelidae: Bulletin of Worldwide Research"? If I remember correctly, some Scandinavian acarologist who published a large number of very poor descriptions ordered his types all destroyed upon his death. Herpetologists have probably never heard of these other journals/cases and don't care about them, but the same applies by non-herpetologists about AJH. Once you decide that AJH is so bad it has to be placed on the list of rejected works in its entirety, what about the next-worst? And, the next worst? Every sub-discipline has a journal or person that they think is the worst. This proposal will never pass, with good, practical reasons.

Today, declaring an entire journal with a living editor to be placed on the list of rejected publications will do nothing. Next week a newly named journal will appear and revalidate all the names. Rejecting the names one by one would be a better solution, although I do not see anything to keep them from being repeated next week as well. As for extending the ban into the future, to invalidate works not yet published is indeed censorship. We call it a gag rule. I am wondering if any work by a living author has ever been placed on the rejected list contrary to their wishes? Has it ever been done that names proposed by a certain person are placed on the rejected list for simply being named by that person, both those already published and those that may be published in the future? When a work is placed on the rejected list, a reason is given, Geoffroy for not being consistently binomial, etc. What reason will be given for AJH? "Unpopular Editor" would be pretty weak in a 100 years.

If coleopterists can survive 38,000 Pic names, herpetologists can survive a mere 600 Hoser names. And yes, the Macrochelys is valid. Live with it.

Mike (who swore he was not getting into this)

(2009) was no different to that of Wallach *et al.* (2009) detailed above who similarly made the same claim against myself the same year in their paper, notably also published by *Zootaxa*!

Thomson and Georges (2009) said they looked for and couldn't find the relevant paper from Wells in 2007 in hard copy anywhere.

Sitting here in Melbourne, Australia I knew that the Thomson and Georges (2009) claim was a lie, as Wells had sent me unsolicited ten copies of his 2007 paper to hand out to others. But in fairness to Thomson and Georges they would not be expected to call me!

However the code does stipulate that copies should be sent to *Zoological Record* and so I made enquiries of them for the purposes of this paper in 2014 and found that, yes, they had received the Wells paper in hard copy and indexed it as appropriately published in volume 143 of the journal.

I checked the Australian National Library in Canberra, the home town for Scott Thomson himself and a short walk from where he had lived. I found that they too had a hard copy, which they even offered to loan me! The Australian National Library is an obvious place to check because in Australia there is a legal deposit law that means that if Wells didn't send them a hard copy of his journal paper, then he'd face criminal charges and potentially jail!

So without too much difficulty, I found hard copies of Wells (2007) published on 3 March that year in what were in effect the most likely of places. That Thomson and Georges (2009) did not look in these places shows quite clearly that they chose to not find hard copies of the Wells paper so that they could manufacture a lie to say it hadn't been published.

This was to green-light (justify) their own reckless coining of a new name for the genus Wells had named two years prior.

In summary Thomson and Georges (2009) published their paper for the sole purpose of overwriting the Wells (2007) genus *Wollumbinia* with their own coined name *Myuchelys*!

That no professional peer reviewer or editor at *Zootaxa* went through the simple checking exercise I was able to do within minutes and then allowed such a taxonomically destabilizing paper to be published is indeed a serious indictment of the editorial practices at *Zootaxa*.

In line with other so-called predatory journals, *Zootaxa* as a publication, appears to have subjugated proper peer review, compliance with the rules of zoology and even ethics in their insatiable quest to become the dominant taxon naming journal on the planet.

SCHLEIP AND THE JOURNAL OF HERPETOLOGY

In December 2008, amateur snake handler, Wulf Schleip, a listed co-author of Kaiser *et al.* (2013) and key member of the Wüster gang published a paper in *Journal of Herpetology* (Schleip 2008).

The paper allegedly described three new species of *Leiopython*, all previously identified as the single species, the northern Brown White-lipped Python (*L. albertisi*).

A detailed review of the genus by myself in year 2000 (Hoser 2000), published in *Ophidia Review* only recognized the one species, although that paper did describe the larger black specimens from southern New Guinea as a new species, namely *Leiopython hoserae*.

In view of the fact that my review in 2000 failed to identify any hidden species among the northern population and also noting that Kaiser *et al.* (2013) (which had included Schleip as a listed coauthor) had accused me of over-splitting taxa, the concept that at least three hidden species existed within such a small geographical area and in the absence of known major physical barriers was to say the least, radical!

The abstract of Schleip's (2008) paper stated that the descriptions were based on robust molecular evidence. Molecular data indicating phylogenetic divergence of allopatric populations has become the gold pass for the description of new species and is rarely argued with, so on the basis of Schleip's abstract, he had certainly made a case for recognition of his new taxa.

However a reading of the paper (notably hidden behind a paywall, meaning very few people would actually get to see the detail of the paper) showed that there was no molecular data whatsoever to support his claim of three new species.

In summary he had committed a fraud.

That any peer reviewer or editor would not notice such an anomaly is beyond belief.

Therefore the only conclusion available in terms of this paper and *Journal of Herpetology*, is that it was a PRINO Journal!

Of further relevance is that the editors of this journal have repeatedly been made aware of this and other serious defects in the Schleip paper, but have never published a retraction or corrections.

A full dissection of the 2008 Schleip paper and numerous earlier acts of fraud and misconduct by Schleip in the period 2001 to 2008 was covered by Hoser (2009a) and is not repeated here. The bulk of that paper (about 2/3 of the text) deals with Schleip's activities and not taxonomy itself and with the two parts of the paper being well delineated

(Schleip's activities first), I strongly urge people to read that section of the paper. I should also note that almost none of the content in that paper is repeated here, but in terms of the context of discussing Schleip's activities in the relevant period 2001-2008 it is an important account.

In 2014, Kaiser (2014) made the ridiculous claim that no Hoser papers complied with the Zoological Code because they were not "issued for the purpose of providing a public and permanent scientific record".

Besides the fact that every issue of *AJH* states that material within is provided for that purpose, the content of each paper clearly indicates this is the case.

Thorpe (2014) made the apt remark:

"People are reading into the Code what suits their agenda, and not what is actually written!"

To justify Kaiser's bogus claim in Kaiser (2014b) he cited a paper "Schleip (2014)", not published as of June 2014, but cited at the rear of his blog as "Schleip, W. D. 2014. Two new species of *Leiopython* Hubrecht, 1879 (Pythonidae: Serpentes): Non-compliance with the International Code of Zoological Nomenclature leads to unavailable names in zoological nomenclature. *J. Herpetol.* In press."

Noting that in his 2008 paper, Schleip elevated a Hoser-named subspecies *Leiopython albertisi bennetti* to full species and recognized *L. hoserae* as described in 2008, it was evident that the as yet unpublished paper would be seeking to invoke Kaiser's bogus claims of non-compliance with the code to rename these validly named taxa with his own coined names.

No one in the previous 14 years had alleged that Hoser (2000) was not a code compliant publication. For most of the eight years to 2008, Schleip had merely alleged my taxa didn't exist.

He also widely posted his claim on various internet sites including his own at "leiopython.de" and herpbreeder.com as well as "Wikipedia", which he obsessively edited. However he reversed this view without explanation when he published Schleip (2008), as detailed in Hoser (2009) and recognized the Hoser-named taxa as valid.

As it happens, contrary to the false claims of Kaiser (2013), published in the ICZN's *BZN*, the journal *Ophidia Review*, in which the paper was published, was not owned or controlled by me in any way, but as is commonly said, truth is dispensed with in war and this is what Kaiser is waging.

The names proposed in that paper (Hoser 2000), including *Leiopython hoserae* Hoser, 2000 have

been widely recognized and used including in the post 2008 period by Wüster gang members Schleip and O'Shea (Schleip 2008, Schleip and O'Shea 2010)!

So there can be absolutely no doubt at all that Schleip was (as of June 2014) about to embark on what may well be his most reckless case of taxonomic vandalism and nomenclatural fraud perpetrated to date.

That Schleip was well aware of the fraud he was about to commit, this being the renaming of *Leiopython hoserae* in breach of the Zoological Code, the evidence can be found all over the place, including from none other than Schleip himself.

In 2004, Schleip wrote: "his papers are valid under the rules of the code." (Schleip 2004).

In 2005 his good friend Wüster said "Hoser has been pretty careful to adhere to the rather minimalist requirements of the Code, and most of his names are available." (Wüster 2005).

Upon being made aware of Kaiser (2014b) and the citation of the as yet unpublished Schleip paper, I emailed the editors of *Journal of Herpetology*.

They received the email but chose not to reply, leading me to phone one of them (Paul Bartelt). In the phone call that fortuitously was recorded, Bartelt said the paper had been pre-published on the *Journal of Herpetology* website but "pulled" due to the obvious defects in it.

I sent him copies of the relevant Hoser (2000) and Schleip (2008) papers as well as an image depicting a printed hard copy of Hoser (2000) in case a false claim may be made that it was only published online; as done by Wallach *et al.* in 2009, for the paper Hoser (2009b).

Bartelt had pled ignorance of my papers, saying his expertise was in other areas of herpetology hence seeking me to email them to him, as well as the earlier Schleip (2008) paper.

As of 3 July 2014, the Schleip paper had not been published and I had hoped that was to be the case.

However on the morning of 5 July 2014, I was alerted to hundreds of posts on the internet from Schleip and Wüster announcing the publication of the paper.

The online paper (Schleip 2014b) renamed *Leiopython hoserae* Hoser, 2000, with his own coined name "*L. meridionalis*" and also renamed the Hoser (2000) subspecies, *L. albertisi bennetti*. Schleip made the false claim Hoser (2000) did not comply with Article 8.1.1 of the Zoological Code (invoking Kaiser *et al.* 2013 and Kaiser 2014b), described by Eipper (2013) as the Kaiser *et al.* "veto".

Schleip had justified his theft of my work with the

following bizarre statement:

“Regardless of the requirements of Article 8.1.1 of the Code, nomenclature is not a scientific discipline, and works that contain nomenclatural acts (i.e., establishing new taxon names) should not automatically be eligible for the scientific record”.

I need not explain that there is nothing whatsoever in the Zoological Code (Ride *et al.* 1999) to support this bizarre assertion, but this is exactly what Schleip relied upon!

This newfound non-recognition of the nomenclatural availability of the Hoser names from year 2000 and others, also reversed Schleip's own position in Schleip (2008) and Schleip and O'Shea (2010) both of whom accepted and used the correct Hoser (2000) name *Leiopython hoseae*, as well as countless other posts he'd made on the internet lamenting the fact that names proposed by myself for taxa were available and he, as well as others would be forced to use them (e.g. Schleip 2004). Significantly, Schleip (2014b) was published in the face of advice by two separate expert reviewers that his paper's claims against Hoser (2000) were false and that he would be acting in contempt of the Zoological Code (Shea 2014, Raw, 2014).

Dr Glenn Shea, one of Australia's pre-eminent herpetological taxonomists formally recommended REJECTION of the Schleip paper to the editors of *Journal of Herpetology*. Lynn Raw did likewise in direct correspondence to Schleip where Schleip was then trying to raise the claim that the words in the Hoser descriptions themselves were not code-compliant, which was a proposition also rejected by Raw (Raw 2014).

I should also note that Schleip's redescription of *Leiopython hoseae* in his 2008 and 2014 papers was little more than a direct “cut and paste” of my original Hoser (2000) description. He had the same holotype, diagnostic information and so on.

This direct theft of my work without attribution in the relevant part of the paper is better known as plagiarization and a serious misdemeanour for a scientist to be caught out for.

However this “cut and paste” and reliance on my own (2000) work for his own two papers (in 2008 and again in 2014, the latter of which falsely referred to his 2008 paper as the original source of the data) didn't stop Schleip in 2008 from committing yet other serious of acts of fraud.

He then recklessly edited the Wikipedia “*Leiopython*” page to allege that my original description was “vague and questionable” and that he had tidied up the taxonomic mess I had allegedly created!

I should however note a brief chronology of the

history of the publication of the name *Leiopython hoseae* and the conduct of Schleip (and to a lesser extent Wüster) in the 14 year period between that paper and the publication of Schleip (2014b).

The original Hoser (2000) paper recognized a significantly new and different species of python that even a five year old could distinguish from the species it was formerly referred to. One species is black on top. The other is brown. One has a medium sized head and the other is large. One is found south and east of the New Guinea highlands and the other north and west.

Their scales are different and they are ecologically different as well.

You do not need to be a molecular biologist to see the obvious!

In 2001 on a website he controlled, namely herpbreeder.com, Schleip recognized *L. hoseae* as a valid taxon.

In 2002, on advice from Wüster, Schleip reversed this and alleged that *L. hoseae* was nothing more than *L. albertisi* (Schleip 2002). This position he generally maintained until end 2008, and he peddled it widely, although at times he did drop his guard and have to admit the obvious to other herpetologists (Schleip 2003a, 2003b).

Many of his reasons given to other herpetologists for denying the existence of *L. hoseae* bordered on the ridiculous. Schleip (2003c) said the Hoser (2000) species only looked different because:

“these “changes” may be caused by mutagenous chemicals or something alike.”, notwithstanding that a month earlier he'd posted: “it's a pain in my a** I do accept *L. hoseae* as a available name and a valid species.”

(Schleip 2003b) and:

“Indeed these names are valid under the rules of the ICZN code but I prefer not to use them as Hoser gives only poor diagnosis”

(Schleip 2003a).

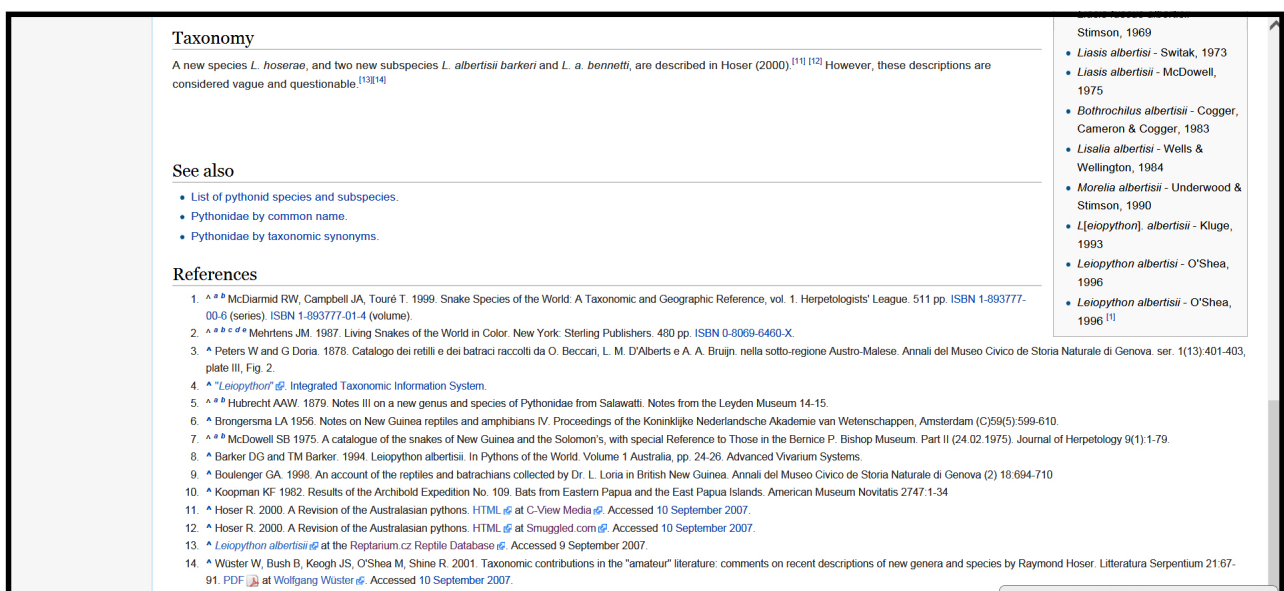
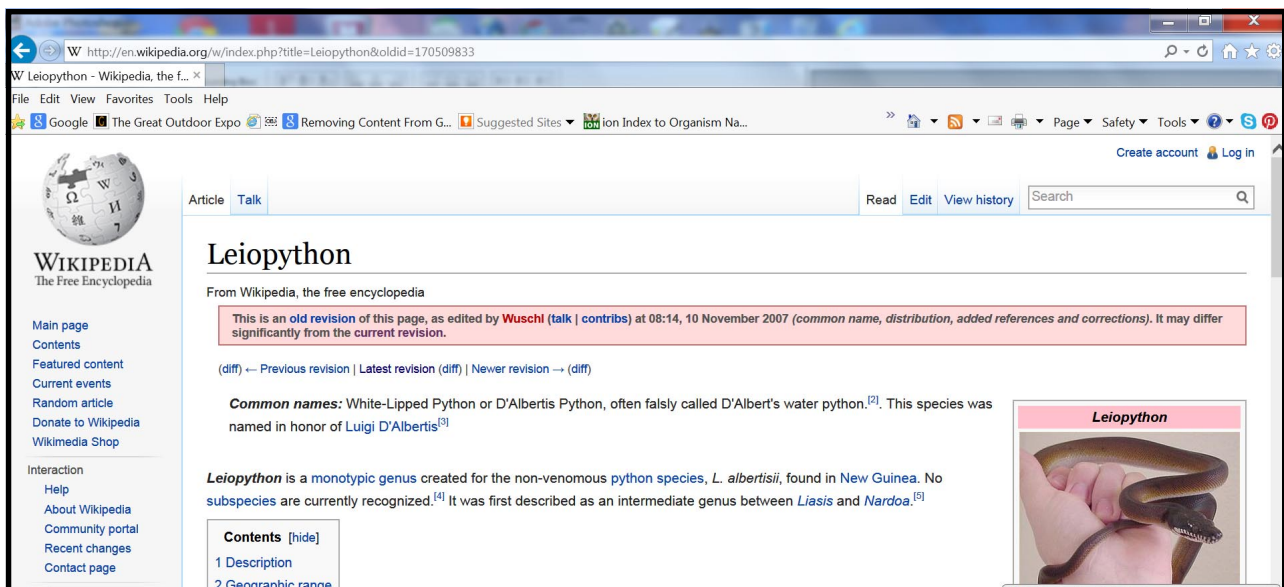
As noted in Hoser (2009a), Schleip's denial of the obvious for most of the period to end 2008 did not stop herpetologists globally recognizing *L. hoseae*.

At end, 2008, Schleip reversed his ever changing position (again!) and recognized *L. hoseae* as a valid species and described as such in accordance with the code, but to justify his antics made up the lie that my description had been “vague and controversial” (as stated in Schleip 2014b), thus in his words allegedly requiring him to tidy the taxonomic mess I had created.

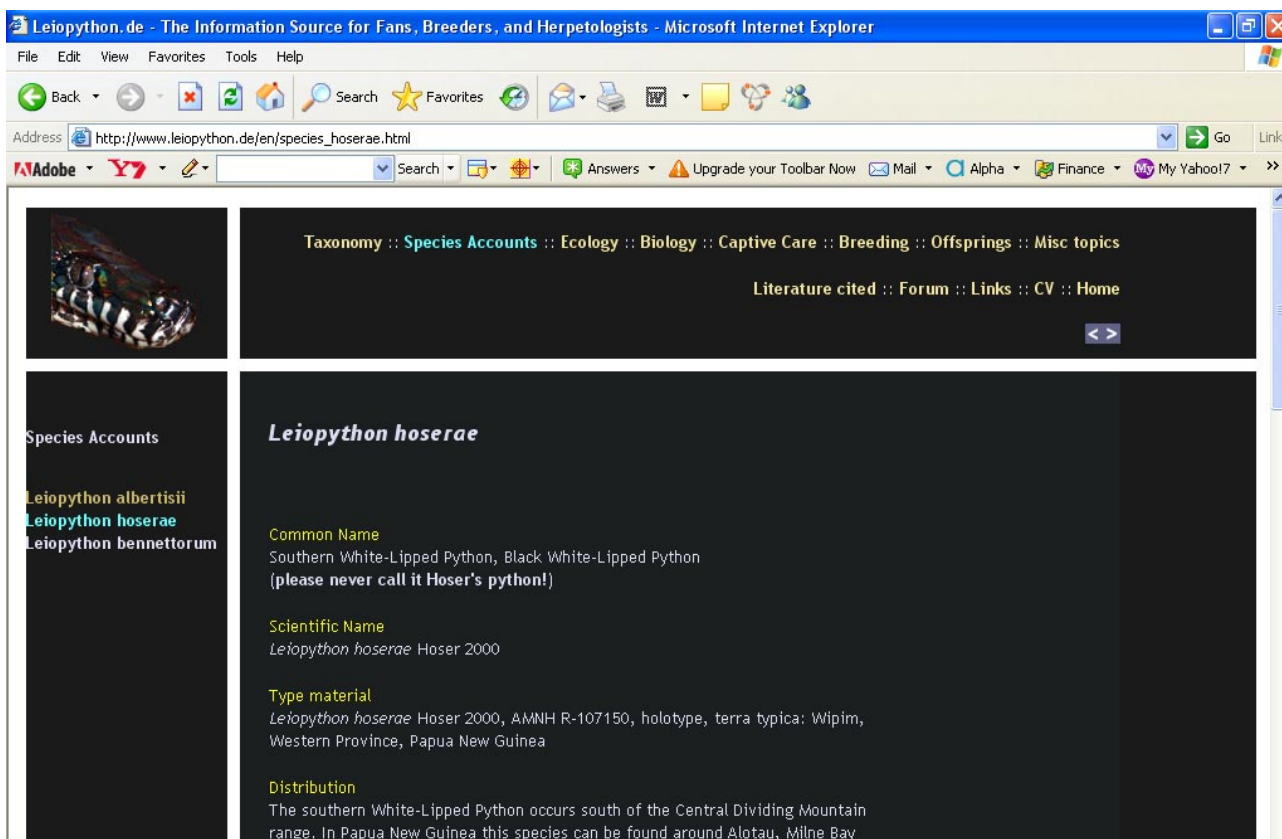
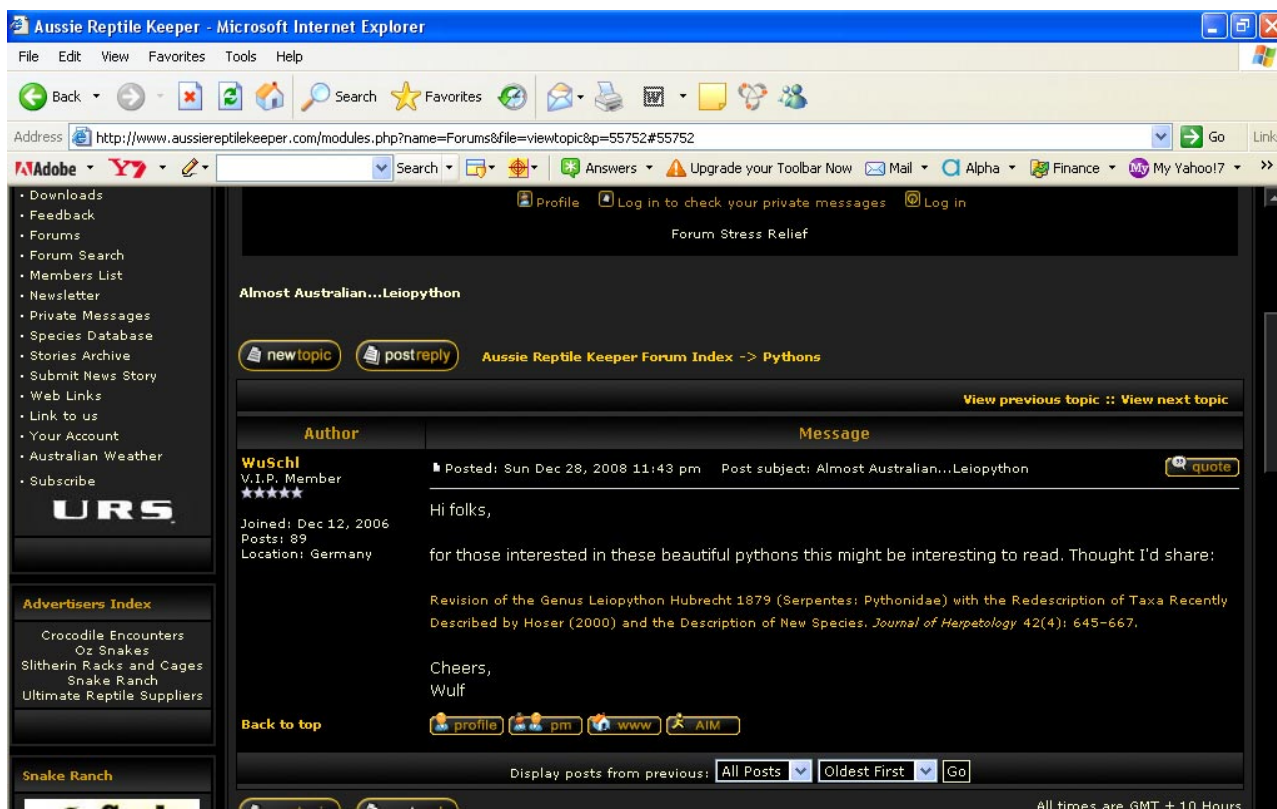
That this was a lie is shown by a direct comparison of my original year 2000 description with his 2008 one, which is in effect a cut and paste of the 2000

Leiopython hoserae is not a valid species! ... According to Wulf Schleip in 2007.

The digital trail, kindly provided by “Wikipedia” shows Wulf Schleip’s obsessive editing of a Wikipedia page for *Leiopython* asserting that there is only one species (as of his 2007 edit shown here) and that *L. hoserae* is merely a variant of it! We now have a situation whereby after at least 7 years of denying the existence of the taxon to 2008, then an admission of the obvious (that it is valid), Schleip has now audaciously attempted to steal “name authority” for the taxon in 2014! Others in the gang of thieves also foist their warped world and taxonomic views on others via their despotic editing of Wikipedia pages.



No apology but ...



one!

In year 2013, Schleip listed himself as a coauthor of Kaiser *et al.* (2013), which in spite of the outrageous demands in the document, recognized *L. hoseri* as a valid name for a valid species and to be used by herpetologists according to their own Taxon Filter (Kaiser 2013) on the basis that listed co-author of Kaiser *et al.* (2013) Schleip had previously used the name as valid in his 2008 and 2010 papers.

Hence in year 2014, Schleip has effectively jettisoned all his previous statements to rename the species in breach of the code.

Previous (and changing) statements by Schleip that he effectively jettisoned in publishing Schleip (2014b) and the false claims within included:

1/ Schleip's 2001 recognition of *L. hoseri*, Hoser, 2000 as valid according to taxonomy and the Zoological Code ; 2/ His repeated denials from 2001 to 2008 of *L. hoseri* as a valid taxon; 3/ His statements in 2004, 2008 and 2010 that the Hoser names for taxa were valid and code compliant. 4/ His recognition of *L. hoseri* in 2008 and 2010 being valid both in taxonomy and the code, which was a reversal of his denials from 2001-2008.

Then finally 5/ Most recently Schleip (2014) which accepted the validity of *L. hoseri* as a taxon, but invented a false claim (he knew as false as indicated by Schleip 2004), that *L. hoseri* wasn't described according to the rules of the Zoological Code.

That we know Schleip knew all along that *L. hoseri* was valid and validly named is revealed by his original acceptance of the obvious in 2001, again in 2008 and more importantly his statement in Schleip 2014b, where he said "Despite the ostensible monotypy of *Leiopython*, two separate lineages became recognized in the international pet trade, and for over 30 yr specimens were referred to as the northern and southern "races" of White-Lipped Pythons."

In other words, at all materially relevant times post 2001, Schleip had been recklessly destabilizing taxonomy and nomenclature in breach of both the spirit and the letter of the Zoological Code.

By contrast, I had done nothing of the sort. I had published just one scientific description of the relevant taxon in 2000 (Hoser 2000) in a completely normal code-compliant manner and in the absence of evidence to the contrary, I had maintained that position ever since.

No renaming of a species previously named (like Schleip 2014) and no false denials of the obvious! In other words. Schleip and associates have been treating the science of herpetological taxonomy and the Zoological Code with gross contempt!

The publication of Schleip (2014b) was also condemned by former ICZN Commissioner Hal Cogger within hours of its appearance online (Cogger 2014b) as well as numerous others including Shea (2014b, Wells 2014b, Wellington 2014b, Raw 2014, Thorpe 2014c) and even from his own camp (Uetz, 2014b), who said of Schleip's 2014 paper:

"How can this go past a reviewer or editor?"

No "paper" in the history of herpetology has ever had such widespread condemnation and for good reason!

In spite of this, within 24 hours of online publication of Schleip (2014b), Wüster and Schleip had managed to cross-post links to the paper on Facebook and elsewhere online more than 200 times!

Notable also is that Kaiser *et al.* (2013) when itemising their alleged "line in the sand" to delineate which Hoser names they were to overwrite with their own coined synonyms, specifically excluded *Leiopython hoseri* Hoser, 2000 from their hit list on the basis that Schleip had recognized it as valid and used the name previously.

Noting that Schleip was a signed co-author of Kaiser *et al.* (2013) it is significant that a year later he has overstepped his own arbitrary "line in the sand".

The significance of this is that the Kaiser "veto" as employed by the Wüster gang to declare a paper outside of Article 8.1.1 of the Zoological Code (invoking Kaiser *et al.* 2013 and Kaiser 2014b), can now (as far as the Wüster gang are concerned) be applied to any paper by any author that they see fit to steal the work from!

The Schleip 2014 position if adopted by others would also put under threat such widely used and recognized Hoser names as *Pseudechis (Pailsus) pailsei* Hoser, 1998, *P. rosignollii* Hoser, 2000 and *Acanthophis wellsei* Hoser, 1998, widely referred to in texts such as Cogger (2014), Eipper (2012) and pretty much every other relevant contemporary herpetology book or scientific paper.

As already mentioned, the central claim of Schleip (2014b) is the creative one that all Hoser publications fail Article 8.1.1 of the Zoological Code. This ridiculous claim was refuted by the ICZN in 1991 and again in 2001 in terms of the Wells and Wellington publications (Wells and Wellington 1984, 1985, ICZN 1991, ICZN 2001).

In fact Schleip's close friend Wolfgang Wüster in a private post said:

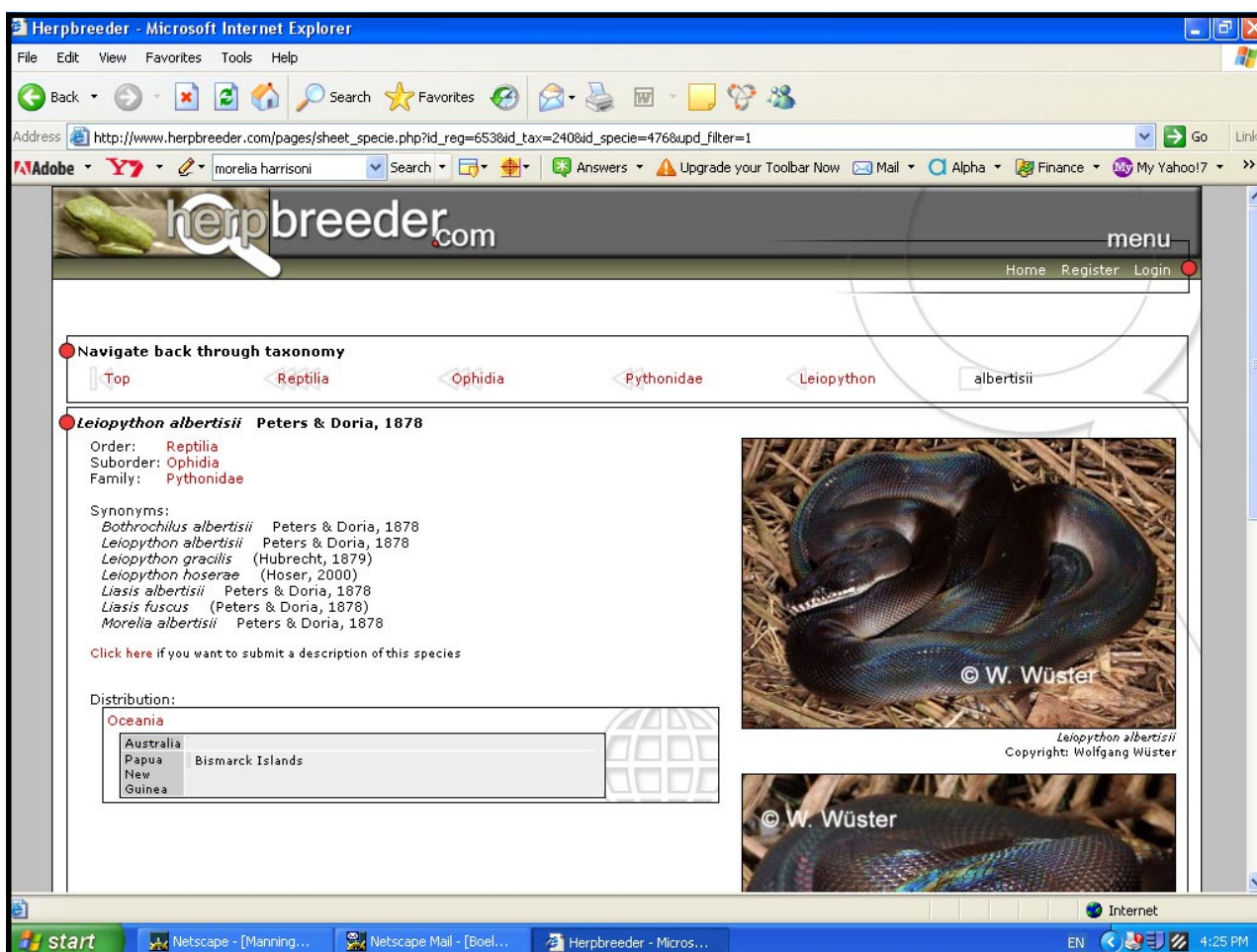
"Hoser has been pretty careful to adhere to the rather minimalist requirements of the Code, and most of his names are available." (Wüster 2005). As a result of this obvious fact and the related fact

Below: Wulf Schleip's webpage in 2002, refusing to recognise *Leiopython hoserae* Hoser, 2000 as being different from *L. albertisi*.

In 2014, he publicly accused myself via Hoser (2000) of being a thief (lower image), claiming I had "stolen" the concept from McDowell (1975) to justify his renaming the taxon as *Leiopython meridionalis* in his "paper" of 2014.

The page below shows Schleip is a liar as in his 2014 paper he stated that the taxon *L. hoserae* had been recognized within herpetology for "over 30 years".

This meant he knew in 2002 he was lying in claiming it was merely *L. albertisi*.



Wulf Schleip

- 1) Nobody other than Hoser himself would call him a "God" 😊
- 2) Since Cameron Brady is not a member of the ICZN she cannot read what was said there. And off course, one of the members (not a commissioner of the ICZN but just a single user!) stated that my paper was a waste of space. He has his opinion, other have theirs 😊
- 3) ...by stealing a the work of scientist who had named the species two decades back.

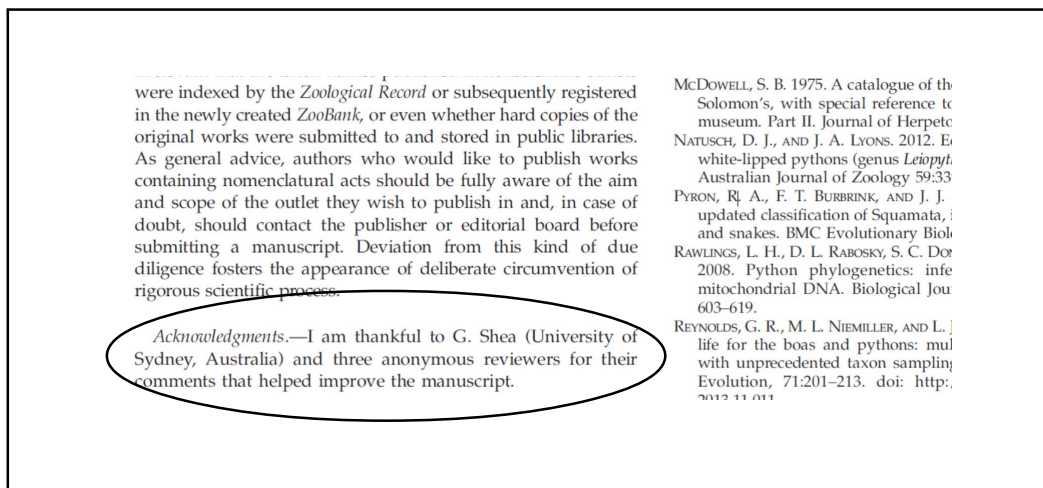
You did nothing more than to steal the findings of McDowell (1975). You did not provide ANY new evidence or new information yourself. I did in that I managed to separate the southern from the northern form by mtDNA analysis! So, who is the thief? Not me, but Hoser!

Misleading and deceptive conduct by Wulf Schleip!

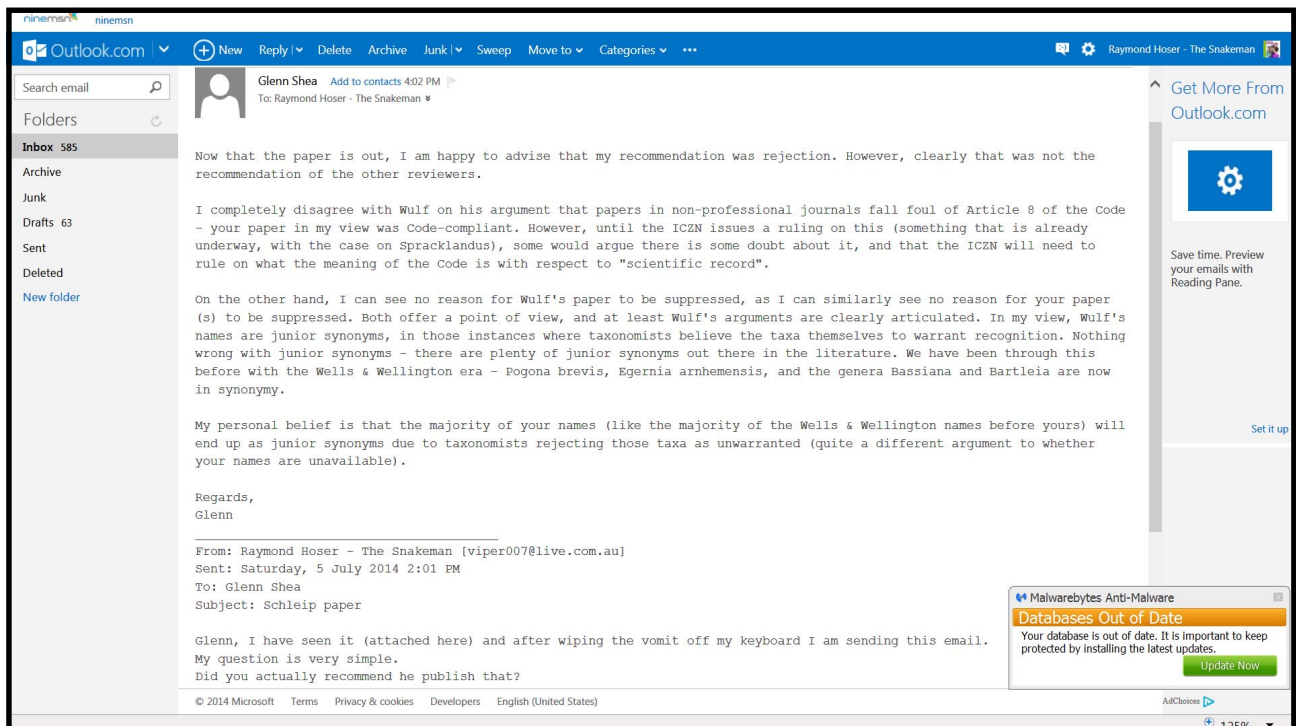
The paper of Schleip 2014, infers that Dr. Glenn Shea, a widely respected herpetologist and taxonomist supported his actions in renaming *Leiopython hoseri* Hoser, 2000, with his own coined name 14 years later.

However the email of Glenn Shea published below exposes Schleip's lie.

Because Schleip has regularly complained publicly of statements being taken out of context by myself (Hoser), his paper from 2014 (widely posted on the web as "open access") is reprinted on the following pages unedited and in full.



Hoser 2015 - Australasian Journal of Herpetology 26:3-64.



that the only purpose of Schleip's 2014 paper was to steal the naming rights to the Hoser taxa described in year 2000, it is clear that I need not bother addressing the other parts of the Schleip paper, which in effect are mere "padding" to give his views an air of more authority.

It could even be argued that by spending more time detailing other errors in Schleip's paper, I am in effect giving the whole publication greater credibility. However the document itself is so replete with serious errors, many of which amount to a case of outright fraud it would be remiss of me not to deal with them. This is especially as the editors of the journal and Schleip himself have spent considerable effort alleging that theirs is a "peer reviewed" journal and therefore has some kind of authority over and above the material I have published, which they consistently and erroneously state is not peer reviewed.

As this paper deals with deliberate failings in the alleged peer review system, I itemise further serious errors in the Schleip (2014b) paper below.

However before continuing, I should make a point of stressing it is my considered belief that Schleip (2014b) was not actually written by him and/or a major part of the work was by someone else, almost certainly that person being none other than Wolfgang Wüster himself.

While this would be denied by the pair, there are too many reasons indicating support for my contention. Schleip (2000) stated in a post:

"By the way. Sorry for my bad english writing. I'm not really used to it....".

In almost all his online posts in the intervening 14 years, Schleip has demonstrated an extremely poor grasp of English, herpetology, taxonomy and the Zoological Code. This is evidenced quite eloquently in his posts on taxacom and elsewhere in year 2014.

The wording of Schleip (2014b) is little different from the things Wüster has posted at various times and the verbal garbage as presented in Schleip (2014b) typifies the bureaucratic double-speak and likes that Wüster is now well known for.

So while the comments in terms of the Schleip (2014b) paper are directed at Schleip himself, this is made on the basis he is the author, or claims to be and may have an element of error in as much another person (probably Wüster) may have in fact been the main author.

I should also mention that erstwhile Wüster ally and a very close friend of Wüster, Mr. Peter Uetz even decided that Schleip's most recent attempt to overwrite the 14-year-old Hoser name "*Leiopython hoserae*" was taking the whole Wüster campaign to

new depths and on his own website <http://www.reptile-database.org> as of 8 August 2014, he maintained usage of the correct Hoser (2000) name and relegated the Schleip coined name to synonymy (Uetz 2014c).

FRAUDULENT ACTIONS WITHIN THE SCHLEIP 2014 PYTHON PAPER

The central claim underpinning the paper that Hoser papers from years 1998-2014 are not Zoological Code compliant has been refuted above (including by Schleip 2004, Schleip 2008, Schleip and O'shea 2010, Wüster 2005) and others from within his own gang. Hence there is no need to repeat the fact that Schleip knew he was wrong at all materially relevant times.

I noted above that Dr Glenn Shea, one of Australia's pre-eminent herpetological taxonomists formally recommended REJECTION of the Schleip paper to the editors of *Journal of Herpetology*.

I also noted that Lynn Raw did likewise in direct correspondence to Schleip where Schleip was then trying to raise the claim that the words in the Hoser descriptions themselves were not code-compliant, which was a proposition also rejected by Raw (Raw 2014).

The recommendation to reject the Schleip paper by Shea is significant in another very important way.

At the end of the Schleip paper he has written: "Acknowledgments.—I am thankful to G. Shea (University of Sydney, Australia) and three anonymous reviewers for their comments that helped improve the manuscript."

There is no mention at all that Shea recommended REJECTION of the said paper.

However from reading the "Acknowledgments" the only inference that can be drawn from them is that Shea and the unnamed others all accepted the paper and endorsed its comments.

Well aware of the fact that Schleip's paper was not only acting against the articles of the Zoological Code, but more seriously represented a direct attack on the authority of the code and the ICZN Commissioners, I didn't believe that Shea would ever allow such a paper to be published.

This is especially noting that he has been lobbying for some years to be nominated to become a Commissioner of the ICZN.

So I emailed Shea asking him the simple question: "Did you actually recommend he publish that?",

to which Shea (2014b) replied:

"Now that the paper is out, I am happy to advise that my recommendation was rejection. However, clearly that was not the recommendation of the other reviewers. I completely disagree with Wulf on his

argument that papers in non-professional journals fall foul of Article 8 of the Code - your paper in my view was Code-compliant."

Shea's response was confirmation that Schleip had deliberately misled his readers to believe that Shea had endorsed his ludicrous position with regards to the Zoological Code.

The fact was that Schleip had concealed from his readers at all times was that he knew Shea and others were very opposed to what he intended publishing!

More significantly, this gets to the PRINO (Peer review in name only) aspect of the journal.

The wording of the "Acknowledgments" should have thrown a red flag to any qualified reviewer and most certainly the journal editor. That the misrepresentation of the truth was allowed to bypass the quality control process is complete confirmation that the *Journal of Herpetology*, is nothing more than a PRINO rag!

Further substantiation of the fact that *Journal of Herpetology* is PRINO is seen on the final document posted online.

The very last four words reads as follows:

"Accepted: 27 January 2014."

This statement implies that the paper had been checked and reviewed prior to that date for publication and then "Accepted" on that date.

However, I can state with total authority that this statement is a lie and that as of "27 January 2014," the paper Schleip (2014b) hadn't even been written! Now if it hadn't been written as of that date, how could it have possibly been peer reviewed and accepted?

As to how I know that the paper hadn't been written as of 27 January 2014, I had to look no further than the paper itself.

Looking at the references, I found the following:

"C-VIEW MEDIA GROUP. 2000. Content of the magazine *Ophidia Review*. Available at <http://www.cviewmedia.com/Contents/or01.html>. Archived by WebCite at <http://www.webcitation.org/6Lw0xbV7K>. Accessed 15 March 2014."

and

"MANTELLA PUBLISHING. 2000. News from Mantella Publishing Limited. Archived by WebCite at <http://www.webcitation.org/6Gq00zgbM>. Accessed 15 March 2014."

and

"VICTORIAN ASSOCIATION OF AMATEUR HERPETOLOGISTS (VAAH). 2004.

Victorian Association of Amateur Herpetologists Welcome Page. Archived by WebCite at <http://web.archive.org/web/20040609111603/http://>

www.vaah.org.au/index.htm. Accessed 15 March 2014."

It was not possible for a paper still being written on 15 March 2014 to have passed any peer review process in January the same year!

Clearly therefore there was no peer review at *Journal of Herpetology* as alleged by them and the conduct of both Schleip and the editors has been fraudulent and shambolic at all stages of the publishing process.

Journal of Herpetology is therefore a holotype PRINO journal!

As for the "three anonymous reviewers" cited by Schleip at the end of his paper, one can only assume they are non-existent as no one has come forward and said that they reviewed the paper and cleared it for publication.

Significant is Shea's outing himself from Schleip (2014b) was within 24 hours of the publication appearing online.

Significant is that Schleip (2014b) identified Shea as a reviewer of his paper (even though he hid the fact that Shea rejected it).

Significant is that neither Schleip or anyone else have identified any other alleged peer reviewer of the paper, leading to the inescapable conclusion that none actually existed!

Of course further evidence of a lack of proper peer review or quality control at *Journal of Herpetology* and Schleip (2014b) in particular are found throughout the paper.

In fact there are way too many errors for me to detail here.

However one need look no further than the title of the (online) printed paper itself which reads:

"Two New Species of *Leiopython* Hubecht, 1879 (Pythonidae: Serpentes): Non-Compliance with the International Code of Zoological Nomenclature Leads to Unavailable Names in Zoological Nomenclature".

There is in fact no such person as "Hubecht" who described the genus *Leiopython*!

The person Ambrosius Arnold Willem Hubrecht (2 March 1853, Rotterdam - 21 March 1915, Utrecht) was a Dutch zoologist who published widely and is known to all modern herpetologists for his work.

That neither Schleip (who markets himself widely as an expert on *Leiopython*), or any of the three alleged expert "reviewers" who allegedly green-lighted the paper for publication in *Journal of Herpetology* would not have detected the obvious typographical error in the title of the paper defies belief.

The only plausible explanation for the error is that 1/ Schleip has no relevant expertise on the genus

Leiopython beyond what he steals from my own papers and 2/ There were no appropriately qualified anonymous reviewers involved either!

I haven't even addressed the other relevant facts being that if the editors of the journal were unable to pick this obvious error, they shouldn't be editing a herpetological journal in the first instance, or the other relevant fact being that Wüster claims expertise in venomous snakes and that pythons sit well outside of his domain.

The defect in the title of the Schleip (2014b) paper sets the tune for the rest of the paper as well.

I won't give the details of all the "cut and paste" Schleip did from my 2000 paper into both the 2014 and 2008 papers as I have referred to this already. Among the collection of obvious errors in the paper are the following:

"The proposal (*Leiopython hoseae* Hoser, 2000) was not widely followed because subsequent workers were either unaware of this effort or because the descriptions presented were considered vague and controversial."

The statement is factually incorrect on several grounds (as Schleip himself would know).

The claim *Leiopython hoseae* Hoser, 2000 "was not widely followed" is clearly false. Schleip himself used it in 2008 and with the many thousands of posts online he had made promoting his 2008 paper (Schleip 2008), it'd be virtually impossible for any herpetologist by 2014 not to know of the name!

On 10 July 2014, Google search revealed no less than 11,100 pages recognizing and using the taxon name, including significantly, Wikipedia, which hadn't yet been vandalized by Schleip and infected with his latest brand of taxonomic vandalism.

Recall prior to the publication of his 2008 paper, Schleip regularly edited Wikipedia sites to make sure that *Leiopython hoseae* wasn't "recognized" and after that date he edited the page to take credit for resolving the taxonomy of the genus.

Of course Schleip's own controlled websites in late 2000 and early 2001 also recognized and used *Leiopython hoseae*, so even back then, the name was well-known and widely used.

Schleip's justification for his obviously false claim that the name *L. hoseae* "was not widely follows", is supposedly substantiated by the next part of his statement which read "because subsequent workers were either unaware of this effort or because the descriptions presented were considered vague and controversial."

That the first part of the statement was false was confirmed by Schleip's own usage of the name to mid 2001 on his own websites, as well as Wüster (2009b) who made it clear that I had always widely

disseminated my papers. I might add that in his attack on me, Kaiser (2014b) went further than Wüster (2005) and accused me of over-disseminating my papers!

Of course none of these authors have mentioned the relevant part of what the Zoological Code (Ride *et al.* 1999) says about the need to widely disseminate papers, and this is quoted here in full: "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."

It goes without saying that Hoser (2000) was sent to *Zoological Record* in year 2000 and detailed in volume 138 of the journal, a fact readily available to herpetologists everywhere including Schleip and Wüster, via their free online database at: <http://www.organismnames.com/>

In terms of Schleip's unsubstantiated claim "the descriptions (of Hoser 2000) presented were considered vague and controversial", well one need look no further than either the original year 2000 descriptions or the usage of the names since.

The descriptions are simple and logical and fortuitously identify taxa so different to one another that even a five-year old could tell them apart from one another.

Hence, even in the face of a vague description as alleged by Schleip (2014b), no one in herpetology would have trouble telling a large black python from a large brown one!

However when a search was done to find if anyone else had the view that the descriptions in Hoser (2000) were in fact "vague and controversial" the only source I could find was Wikipedia. Of course the statement had been placed there by none other than Schleip himself.

As for the descriptions of Hoser (2000) being controversial, nothing could be further from the truth.

Other than Schleip's numerous online denials of *Leiopython hoseae* from 2001 to 2008, no one ever published a single paper anywhere in the 14 year period from 2000 to 2014 denying the existence of *L. hoseae* or providing a shred of evidence to refute that it was a separate species to *L. albertsi*.

In other words, the entire paragraph as quoted above is a collection of false statements!

I should also add that if my year 2000 descriptions

Journal of Herpetology, Vol. 48, No. 2, 272–275, 2014
Copyright 2014 Society for the Study of Amphibians and Reptiles

Two New Species of *Leiopython* Hubrecht, 1879 (Pythonidae: Serpentes): Non-Compliance with the International Code of Zoological Nomenclature Leads to Unavailable Names in Zoological Nomenclature

WULF D. SCHLEIP

Hanrathstrasse 39, 53332 Bornheim, Germany; E-mail: webmaster@leiopython.de

ABSTRACT.—The *International Code of Zoological Nomenclature* (ICZN; hereafter, the *Code*) governs not only the availability and application of scientific zoological names once they enter the realm of nomenclature but also what is to be considered published work for the purpose of zoological nomenclature. Article 8.1.1 of the *Code* clearly demands that to be considered published in the meaning of the *Code*, works "...must be issued for the purpose of providing a permanent public and scientific record." This requirement is often unfulfilled with the publication of nomenclatural acts in hobbyist magazines and amateur literature. Nevertheless, some names published in such outlets are in use today although, under strict application of the *Code*, these names could be de facto nonexistent for the purpose of nomenclature and cannot be made available simply by subsequent usage (ICZN, 1999: Articles 11.5.2, 16.1). In this paper, I discuss the application of Article 8.1.1 to the nonscientific literature and, as a consequence, resolve a nomenclatural problem posed by two populations of snakes in the genus *Leiopython* Hubrecht, 1879 that have been recognized as valid species but that do not have valid names under the requirements of the *Code*.

Hubrecht (1879) introduced the genus *Leiopython* for a single species of White-Lipped Python from New Guinea, *Leiopython gracilis*, shortly after the description of *Liasis albertisii* (Northern White-lipped Python) by Peters and Doria in 1878. Boulenger (1893) synonymized Hubrecht's species with *L. albertisii* and, therefore, the genus *Leiopython* became a synonym of *Liasis* Gray, 1842. This situation remained until Kluge (1993) resurrected *Leiopython*. Despite the ostensible monotypy of *Leiopython*, two separate lineages became recognized in the international pet trade, and for over 30 yr specimens were referred to as the northern and southern "races" of White-Lipped Pythons. Hoser (2000) attempted to formally separate these forms and introduced two new subspecies, one from Wau (Morobe Province, Papua New Guinea), called *L. albertisii bennetti* (Wau White-lipped Python), and the other from the remote St. Matthias Island group of the Bismarck Archipelago, called *L. albertisii barkeri* (Barker's White-lipped Python). The proposal was not widely followed because subsequent workers were either unaware of this effort or because the descriptions presented were considered vague and controversial: the designated type material had not been examined but had been merely selected from specimens listed in the published literature (e.g., McDowell, 1975). Furthermore, the two subspecific names required emendation, with the subspecific name *barkeri* considered a nomen nudum (Wüster et al., 2001; Schleip, 2008). Regardless of this, it should be noted that a recent study by Reynolds et al. (2013) synonymizes the generic name *Leiopython* with *Bothrochilus* Fitzinger, 1843 because they were found to be sister taxa. However, the genetic data are inconclusive and, until further data are available and for the purpose of this work, I will retain the genus *Leiopython*.

Schleip (2008) eventually provided the genetic evidence for the separation of the northern and southern forms of *Leiopython* along with morphological evidence for the taxonomic validity of one of Hoser's (2000) subspecies. While Schleip (2008) was able to solve the taxonomic problems created by Hoser (2000), the nomenclatural problems remain and need to be resolved.

Issues of unresolved nomenclature require careful attention to preserve the stability of taxon names used in broader applications of taxonomy. Confusing nomenclature may have

a negative impact on biodiversity estimates and conservation issues. Almost 12 yr after Hoser (2000) and 5 yr after Schleip (2008), the Convention on International Trade in Endangered Species of Wild Fauna and Flora (CITES) still recognizes only *Leiopython albertisii* in its export quotas (Natusch and Lyons, 2012).

Leiopython meridionalis sp. nov. (Fig. 1A, C)

Suggested English name: Southern White-lipped Python.

Holotype.—AMNH R-107150, an adult male from Wipim, Western Province, Papua New Guinea (PNG), 8.791°S 142.869°E, collected by F. Parker in August 1969.

Diagnosis.—A detailed description and diagnosis of this species was presented by Schleip (2008:656–658 under the heading "*Leiopython hoseri*" [Southern White-lipped Python]). *Leiopython meridionalis* sp. nov. is separated from *L. albertisii* Peters and Doria, 1878, *Leiopython biakensis* (Biak White-lipped Python) Schleip, 2008, and *Leiopython fredparkeri* (Fred Parker's White-lipped Python) Schleip, 2008 by larger average size in hatchlings and adults along with a generally darker, greyish-black color with white or off-white flanks compared with the yellowish coloration seen in *L. albertisii*. It can further be distinguished in having only one pair of parietal scales that form a characteristic pattern, by a lower number of dorsal midbody scale rows, and by the absence of the whitish spot behind the eye. Additionally, mitochondrial DNA analysis (see Schleip, 2008) supports the distinction of *L. meridionalis* from *L. albertisii*, with a genetic distance of up to 9.3% (for details see Schleip, 2008). It can be separated from *Leiopython montanus* sp. nov. (see below) in having a lower number of loreals, prefrontals, and dorsal midbody scale rows.

Etymology.—The specific name *meridionalis* derives from the Latin for 'southern,' in reference to the fact that this species was often referred to as the southern form of White-Lipped Pythons.

Leiopython montanus sp. nov. (Fig. 1B, D)

Suggested English name: Wau White-lipped Python

Holotype.—BPBM 5452 F, a juvenile female specimen from the Bishop Museum Field Station near Wau, Morobe Province,

DOI: 10.1670/13-157

TWO NEW SPECIES OF *LEIOPYTHON*

273

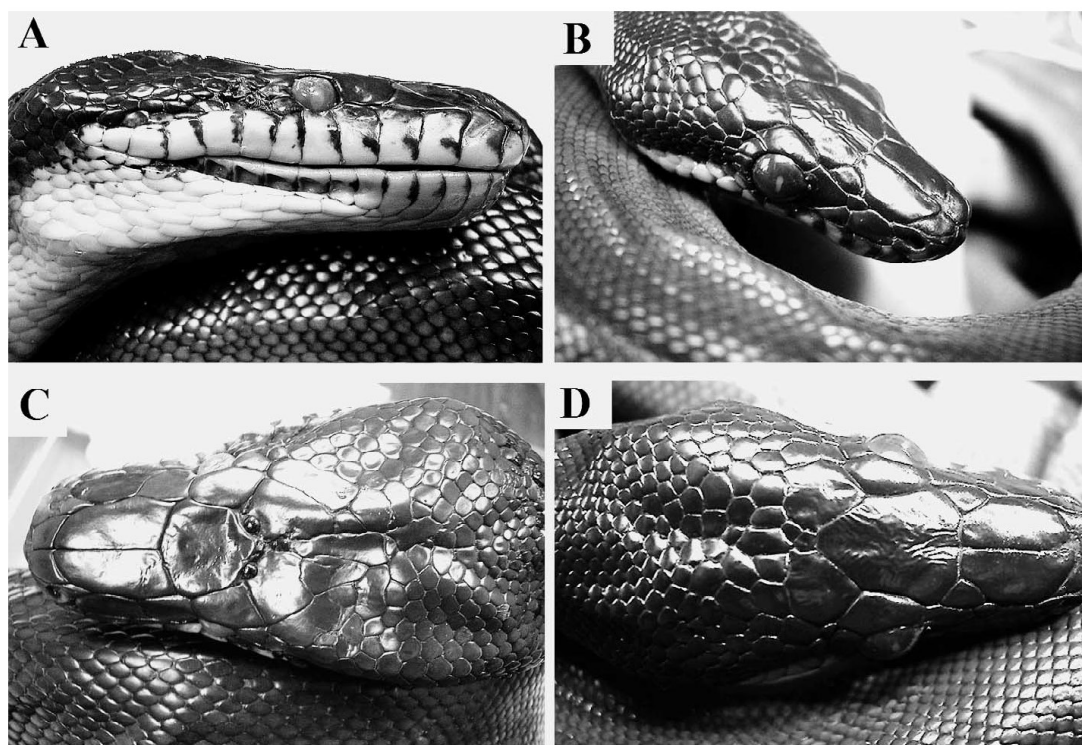


FIG. 1. Lateral (A) and dorsal (C) view of the head of the holotype of *Leiopython meridionalis* sp. nov. Dorsolateral (B) and dorsal (D) view of the head of the holotype of *Leiopython montanus* sp. nov. Modified from Schleip (2008:fig. 6).

PNG, elevation approximately 1,250 m, 7.341°S 146.705°E, collected by A. C. Ziegler on 13 June 1967.

Diagnosis.—A detailed diagnosis and description can be found in Schleip (2008:658–659 under the heading “*Leiopython benettorum*” (Wau White-lipped Python). *Leiopython montanus* sp. nov. can be distinguished easily from all other members of the genus *Leiopython* by a higher loreal scale count and by a second pair of small lateral prefrontals. Furthermore, higher average midbody scale row and postocular scale counts separate this species from *L. meridionalis*, *Leiopython huonensis* (Huon White-lipped Python), *L. fredparkeri*, and *L. albertisii*.

Etymology.—The specific name *montanus* derives from the Latin adjective for ‘living on a hill,’ in reference to the topography near the town of Wau.

DISCUSSION

Publishing According to the Code.—To be considered available for the purpose of zoological nomenclature, a taxon name must have been published in accordance with the *Code*. The *Code* not only governs the availability and application of such names once they have entered the realm of nomenclature but also provides a series of mandatory requirements against which the publication of works that include nomenclatural acts can be judged. If a work is published in line with these requirements, then the taxon names contained therein will be deemed published and available for the purpose of nomenclature. If it is not, then the proposed

taxon names have no standing of any kind in nomenclature; they are nonexistent for nomenclatural purposes.

Publishing for the Permanent Scientific Record.—Although the wording of the *Code* generally leaves space for interpretation, Article 8.1.1 is very precise and specific (Knapp and Wright, 2010:85) in its demand that works considered published, “must be issued for the purpose of providing a public and permanent scientific record” (Art. 8.1.1; ICZN, 1999). In terms of creating a permanent scientific record, periodicals such as herpetoculture magazines and scientific journals are generally issued by a publisher and, therefore, the process of issuing the publication is normally beyond an author’s responsibility. On the other hand, it is clearly an author’s responsibility, prior to submission of a manuscript, to consider carefully the aim and scope of the outlet so that their work has proper context and reaches the appropriate audience. Given the fundamental difference in the target audience, aim, and scope between hobbyist magazines and scientific journals, it is reasonable to assume that authors who submit manuscripts to the former consciously decide to address a nonscientific audience and, hence, cannot expect to provide a permanent scientific record. This interpretation of the publishing side of nomenclature and taxonomy is anchored in the spirit of the *Code*, according to which “Authors, editors, and publishers have a responsibility to ensure that works containing new names, nomenclatural acts, or information likely to affect nomenclature are self-evidently published within the meaning of the *Code*” (Recommendation 8D; ICZN, 1999).

One may argue that since the advent of the binomial system of nomenclature, and even since the establishment of the first edition of the *Code* in 1961, a considerable number of names in zoology have been published in what might be called the 'grey literature,' and many of these are fully accepted, have been validated, and are being used by the scientific community. While this may be the case for names published in outlets with an acceptable quality standard and a long history, it is doubtful that scientific merit should be extended to a newly established and therefore unknown herpetoculture magazine. It is in situations like these that the *Code* is unable to insure that taxon names are properly vetted and thus assigned scientific merit (Kaiser, 2013). It is one of the general recommendations of the *Code* that works containing nomenclatural acts should be published in outlets that have "a wide circulation, and which zoologists would not regard as unlikely to contain new names in the taxonomic field concerned" (Appendix B.8, General Recommendations; ICZN, 1999); alas, this Appendix to the *Code* is not mandatory.

Science and Nomenclature.—The scientific record builds the foundation of our scientific knowledge and requires works to meet certain standards in scientific quality and rigor to ensure sufficient detail for others to reproduce the research (Carraway, 2009; Kaiser et al., 2013). Hence, the accuracy and reproducibility of research are two of the major pillars in science whereas inaccurate or error-prone works compromise the integrity of the scientific record. Scientists and publishers of scientific literature, therefore, carry a responsibility and make great efforts to uphold the integrity of this process (e.g., Hoppeler et al., 2008; Carraway, 2009). Regardless of the requirements of Article 8.1.1 of the *Code*, nomenclature is not a scientific discipline, and works that contain nomenclatural acts (i.e., establishing new taxon names) should not automatically be eligible for the scientific record (Dayrat, 2005; Kaiser, 2013).

The Problem with Hoser (2000).—The herpetoculture magazine *Ophidia Review* was announced in 2000 as a "new magazine dedicated to snake keeping," to be published by the British company Mantella Publishing (Mantella Publishing, 2000); its specified audience was snake keepers and not scientists working in snake taxonomy. This distinction is clear from the layout and style of the magazine along with the advertisements therein (C-View Media, 2000). The first and only published issue consists of 36 pages with three articles and advertisements. Two articles were herpetocultural in nature, dealing with the care and maintenance of viperid snakes, and the third (Hoser, 2000) proposed new names for some Australian pythonid snakes. This latter work does not meet generally accepted standards for scientific writing (Wüster et al., 2001), and the publication *Ophidia Review* does not comply with Article 8.1.1 of the *Code*. Therefore, I contend that the article published by Hoser (2000) was not "published for the permanent scientific record" (emphasis added), and that several names coined by Hoser (2000) are nonexistent for the purpose of zoological nomenclature.

Hoser's efforts in taxonomy are considered "taxonomic vandalism" (Pyron et al., 2013) and their use has been formally abandoned by scientists (Kaiser, 2013; Kaiser et al., 2013). Therefore, most of the names coined by Hoser (2000) are not recognized or used by either professional herpetologists or herpetoculturists. However, a few names have come into use because subsequent authors of bona fide scientific publications (e.g., Schleip, 2008; Schleip and O'Shea, 2010; Natusch and Lyons, 2012) assumed Hoser taxon names to be nomenclaturally available. However, these authors did not analyze the original

descriptions with reference to the *Code*. Although I erroneously used Hoser's *Leiopython* names myself (Schleip, 2008), and provided a detailed description of the groups, their names remained unpublished for the purpose of nomenclature because I did not explicitly denote them as 'new,' a requirement of Article 16.1 (ICZN, 1999). This has led to the undesirable situation that authors use nomenclaturally nonexistent names while there are no nomenclaturally available names for these organisms. Another example of this is the use of *Brogghammerus* Hoser, 2004 by Rawlings et al. (2008) for a group of pythons that only received the name *Malayopython* in 2013 (Reynolds et al., 2013). To return the taxonomy of the genus *Leiopython* to nomenclatural stability, it became necessary to propose new names for the two above-named taxa. All taxon names coined by Hoser (2000) are nomenclaturally nonexistent, and the fate of groups carrying such names requires scientific attention. For example, in the case of the New Guinea Carpet Python, whose name was given as "*Morelia harrisoni*" by Hoser (2000), it remains unclear if this is a valid taxon because morphological and genetic data have yet to be examined properly.

The Trouble with Article 8.1.1.—The strict application of article 8.1.1 presented here may not meet with everyone's approval. Critics may argue, for example, that the border between suitable and unsuitable media for the publication of nomenclatural works and acts is blurred and that there are plenty of names published in the nonscientific literature that are perfectly fine. However, under the strict application of the criteria of the *Code* (Articles 8, 9; ICZN, 1999), perhaps none of those names coined this century should be considered available for the purpose of nomenclature. It is incomprehensible why, for instance, the principle of priority (Article 23; ICZN, 1999) must always be applied strictly while other mandatory rules may be applied more freely. The strict application of the *Code* in regard to nonscientific literature is also compulsory because it has a stabilizing effect on nomenclature. Nowadays, determining whether or not works are to be considered published in the meaning of the *Code* is a simple and clear-cut 'yes' or 'no' decision on the question of if they were deliberately "...issued for the purpose of providing a public and permanent scientific record" as required by the *Code* (Art. 8.1.1; ICZN, 1999). This requirement implies that work itself should qualify for that purpose. Likewise, an author of a science fiction novel could claim to provide a scientific record. In other words, would the public expect that a publication contains scientific material relevant to taxonomy and nomenclature and that such a publication provides an appropriate vehicle for that information to be entered into the permanent scientific record? If this question cannot be answered unequivocally with 'yes,' or the purpose is not self-evidently clear from the outlet (Recommendation 8D; ICZN, 1999), then the work must be regarded as unpublished for the purpose of zoological nomenclature. This is also true for other works in nonscientific literature (see Kullander, 2011).

Another Case Resolved.—Recently, a discussion arose regarding the status of the generic name *Brogghammerus* Hoser, 2004 and whether it was published validly under the *Code* (Kaiser et al., 2013; Reynolds et al., 2013). This taxon name was originally published by a small local Australian reptile club called the "Victorian Association of Amateur Herpetologists" (VAAAH) in a magazine-like newsletter called "*The Crocodilian*." The newsletter was sent out only to the club's members, and nonmembers were unable to obtain hard copies from the club's website (VAAAH, 2004). Print runs were only slightly higher than the number of club members, which counted approximately 50 at the time the information was published. Other than internal club news, the

TWO NEW SPECIES OF *LEIOPYTHON*

275

issue contained advertisements from local retail shops (e.g., hotels, computers, reptiles) and reptile-related newspaper clippings, along with four articles about regional herpetocultural topics. Mixed into this was an article by Hoser dealing with the taxonomy of pythonid snakes, which was published in two parts in separate issues (Hoser, 2003, 2004). Hobby magazines and club newsletters such as *Ophidia Review* and *The Crocodilian* are not issued for the purpose of providing a permanent scientific record (Art. 8.1.1; ICZN 1999). Additionally, *The Crocodilian* was not published to provide a public record, and the status of the works therein must be considered the same as that of doctoral theses and conference abstract volumes that are issued for a small circle of people directly involved with the process. Such works are not recognized as published for the purpose of zoological nomenclature (Article 9.9; ICZN, 1999). Nonetheless, Rawlings et al. (2008) were urged by the reviewers and journal editors to use this name as a possible senior synonym, although without denoting it as 'new' (Articles 11.5.2, 16.1; ICZN, 1999). Reynolds et al. (2013) finally resolved the issue by publishing the name *Malayopython* to replace the de facto nonexistent generic name *Brogghammerus*.

The Issue of Stability.—The equally strict application of all rules of the *Code* avoids uncertainty as to the existence and availability of scientific names in the future yet does not restrict taxonomic freedom. In the cases of *Leiopython* and *Malayopython* mentioned above, it is now possible to connect these names with a transparent scientific process and reliable evidence, something that was not possible with the nonscientific works of Hoser (2000, 2003, 2004). For the purpose of zoological nomenclature, it is irrelevant that the taxon names published in nonscientific outlets were indexed by the *Zoological Record* or subsequently registered in the newly created *ZooBank*, or even whether hard copies of the original works were submitted to and stored in public libraries. As general advice, authors who would like to publish works containing nomenclatural acts should be fully aware of the aim and scope of the outlet they wish to publish in and, in case of doubt, should contact the publisher or editorial board before submitting a manuscript. Deviation from this kind of due diligence fosters the appearance of deliberate circumvention of rigorous scientific process.

Acknowledgments.—I am thankful to G. Shea (University of Sydney, Australia) and three anonymous reviewers for their comments that helped improve the manuscript.

LITERATURE CITED

- BOULENGER, G. A. 1893. Catalogue of the Snakes in the British Museum (Natural History). Vol. I. British Museum (Natural History), London, UK.
- CARRAWAY, L. N. 2009. Ethics for and responsibilities of authors, reviewers and editors in science. *American Midland Naturalist* 161: 146–164.
- C-VIEW MEDIA GROUP. 2000. Content of the magazine *Ophidia Review*. Available at <http://www.cviewmedia.com/Contents/or01.html>. Archived by WebCite at <http://www.webcitation.org/6Lw0xbV7K>. Accessed 15 March 2014.
- DAYRAT, B. 2005. Towards integrative taxonomy. *Biological Journal of the Linnean Society* 85:407–415.
- HOPPELER, H., M. HANDEL, AND O. C. MOULTON. 2008. Editorial: Maintaining the integrity of the scientific record. *The Journal of Experimental Biology* 211:3651.
- HOSER, R. T. 2000. A revision of the Australasian pythons. *Ophidia Review* 1:7–27.
- . 2003. A reclassification of the pythoninae including the descriptions of two new genera, two new species and nine new subspecies. *Crocodilian* 4(3):31–37.
- . 2004. A reclassification of the pythoninae including the descriptions of two new genera, two new species and nine new subspecies. *Crocodilian* 4(4):21–40.
- HUBRECHT, A. A. W. 1879. Notes III. On a new genus and species of Pythonidae from Salawatti. Notes from the Leyden Museum 1:14–15.
- INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE (ICZN). 1999. International Code of Zoological Nomenclature. 4th Ed. The International Trust for Zoological Nomenclature, London, UK.
- KAISER, H. 2013. The taxon filter, a novel mechanism designed to facilitate the relationship between taxonomy and nomenclature, vis-à-vis the utility of the *Code's* Article 81 (the Commission's plenary power). *Bulletin of Zoological Nomenclature* 70:293–302.
- KAISER, H., B. I. CROTHER, C. M. R. KELLY, L. LUISSELL, M. O'SHEA, H. OTA, P. PASSOS, W. D. SCHLEIP, AND W. WÜSTER. 2013. 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* 44:8–23.
- KLUGE, A. G. 1993. *Aspidites* and the phylogeny of the pythonine snakes. *Records of the Australian Museum* 19(Suppl.):1–77.
- KNAPP, S., AND D. WRIGHT. 2010. E-Publish or Perish? In A. Polaszek (ed.), *Systema Naturae 250 – The Linnaean Ark*, pp. 83–94. CRC Press, Taylor & Francis Group, USA.
- KULLANDER, S. O. 2011. Nomenclatural availability of putative scientific generic names applied to the South American cichlid fish *Apistogramma ramirezi* Myers & Harry, 1948 (Teleostei: Cichlidae). *Zootaxa* 3131:35–51.
- MANTELLA PUBLISHING. 2000. News from Mantella Publishing Limited. Archived by WebCite at <http://www.webcitation.org/6Gq00zgbM>. Accessed 15 March 2014.
- MCDOWELL, S. B. 1975. A catalogue of the snakes of New Guinea and the Solomon's, with special reference to those in the Bernice P. Bishop museum. Part II. *Journal of Herpetology* 9:1–79.
- NATUSCH, D. J., AND J. A. LYONS. 2012. Ecological attributes and trade of white-lipped pythons (genus *Leiopython*) in Indonesian New Guinea. *Australian Journal of Zoology* 59:339–343.
- PYRON, R. A., F. T. BURBRINK, AND J. J. WIENS. 2013. A phylogeny and updated classification of Squamata, including 4161 species of lizards and snakes. *BMC Evolutionary Biology* 13:93.
- RAWLINGS, L. H., D. L. RABOSKY, S. C. DONNELLAN, AND M. N. HUTCHINSON. 2008. Python phylogenetics: inference from morphology and mitochondrial DNA. *Biological Journal of the Linnean Society* 93: 603–619.
- REYNOLDS, G. R., M. L. NIEMILLER, AND L. J. REVELL. 2014. Toward a tree-of-life for the boas and pythons: multilocus species-level phylogeny with unprecedented taxon sampling. *Molecular Phylogenetics and Evolution*, 71:201–213. doi: <http://dx.doi.org/10.1016/j.ympev.2013.11.011>.
- SCHLEIP, W. D. 2008. Revision of the Genus *Leiopython* Hubrecht, 1879 (Serpentes: Pythonidae) with the re-description of taxa recently described by Hoser (2000) and the description of new species. *Journal of Herpetology* 42:645–667.
- SCHLEIP, W. D., AND M. O'SHEA. 2010. Annotated checklist of the recent and extinct pythons (Serpentes, Pythonidae), with notes on nomenclature, taxonomy, and distribution. *ZooKeys* 66:29–79.
- VICTORIAN ASSOCIATION OF AMATEUR HERPETOLOGISTS (VAAH). 2004. Victorian Association of Amateur Herpetologists Welcome Page. Archived by WebCite at <http://web.archive.org/web/20040609111603/http://www.vaah.org.au/index.htm>. Accessed 15 March 2014.
- WÜSTER, W., B. BUSH, J. S. KEOGH, M. O'SHEA, AND R. SHINE. 2001. Taxonomic contributions in the "amateur" literature: comments on recent descriptions of new genera and species by Raymond Hoser. *Litteratura Serpentina* 21:67–79.

Accepted: 27 January 2014.

were “vague and controversial” as contended by Schleip (2014b), this would make Schleip’s 2014 descriptions just as “vague and controversial”, considering they amounted to little more than a mere lifting of my data and reuse of it!

The same applies for another of his many insults, like “Hoser’s efforts in taxonomy are considered “taxonomic vandalism””, which if actually true, would mean his redescription of taxa I had named in year 2000 must similarly be taxonomic vandalism as he lifted and used without significant change or alteration my very data!

The same applies to the Schleip (2014) claim that my year 2000 paper was “unscientific”!

Although Schleip repeats himself many times, as if to make out that the more he repeats a lie, the more people may believe him, his case against Hoser (2000) and the code-compliant names within the paper is summed up thus:

“that Article 8.1.1 of the Code says that to be valid under the Zoological Code the description must be “issued for the purpose of providing a public and permanent scientific record””

and that

“I contend that the article published by Hoser (2000) was not “published for the permanent scientific record” (emphasis added), and that several names coined by Hoser (2000) are nonexistent for the purpose of zoological nomenclature.”

What Schleip has not addressed are the other relevant parts of the code which actually define what constitutes valid publication under the code, as in the rest of Article 8.

Not only does Hoser (2000) fall within this, but the ICZN has qualified this many times in the past including in 1991 and 2001 (ICZN 1999, 2001), meaning that Schleip has knowingly misled his readers.

If one were to use the Australian phrase “baffle them with bullshit”, Schleip makes a series of false and derogatory remarks to attack the Hoser (2000) paper including that it was published in a “herpetoculture magazine” is “non-scientific literature” (undefined) and not a proper scientific journal, which he has not defined either, but can be assumed here to be something in which his group maintains editorial control.

Schleip claims that because of the above, *Ophidia Review* is in breach of his interpretation of “(Recommendation 8D; ICZN, 1999)”.

What he fails to recognize is that 8D is an optional and not mandatory part of the code and so failure to comply with it (if accepted) would not invalidate a scientific description.

As a result, his whole thesis falls apart.

By the way 8D of the code (deliberately unquoted by Schleip (2014b)) reads:

“Recommendation 8D. Responsibilities of authors, editors and publishers. Authors, editors and publishers have a responsibility to ensure that works containing new names, nomenclatural acts, or information likely to affect nomenclature are self-evidently published within the meaning of the Code. Editors and publishers should ensure that works contain the date of publication, and information about where they may be obtained.”

This of course refers to layout of the publication, style of descriptions (e.g. Holotype details under a heading) and so on.

A quick check of Hoser (2000), available online as a pdf copy shows that “Recommendation 8D” is in fact complied with by Hoser (2000).

In summary Schleip has quoted by section a part of the Zoological Code and falsely alleged I breached it in Hoser (2000), when a check of the code and the original Hoser (2000) paper shows the claim to be false. Worse still, Schleip goes on to state:

“This is also true for other works in nonscientific literature”

(meaning journals his group don’t control), and “perhaps none of those names coined this century should be considered available for the purpose of nomenclature.”,

in effect widening the potential for his group to recklessly overwrite names of others to an almost unlimited extent!

More alarming yet was a comment by Schleip in response to criticisms of his Schleip (2014b) paper by correspondents on the Taxacom list server.

He said:

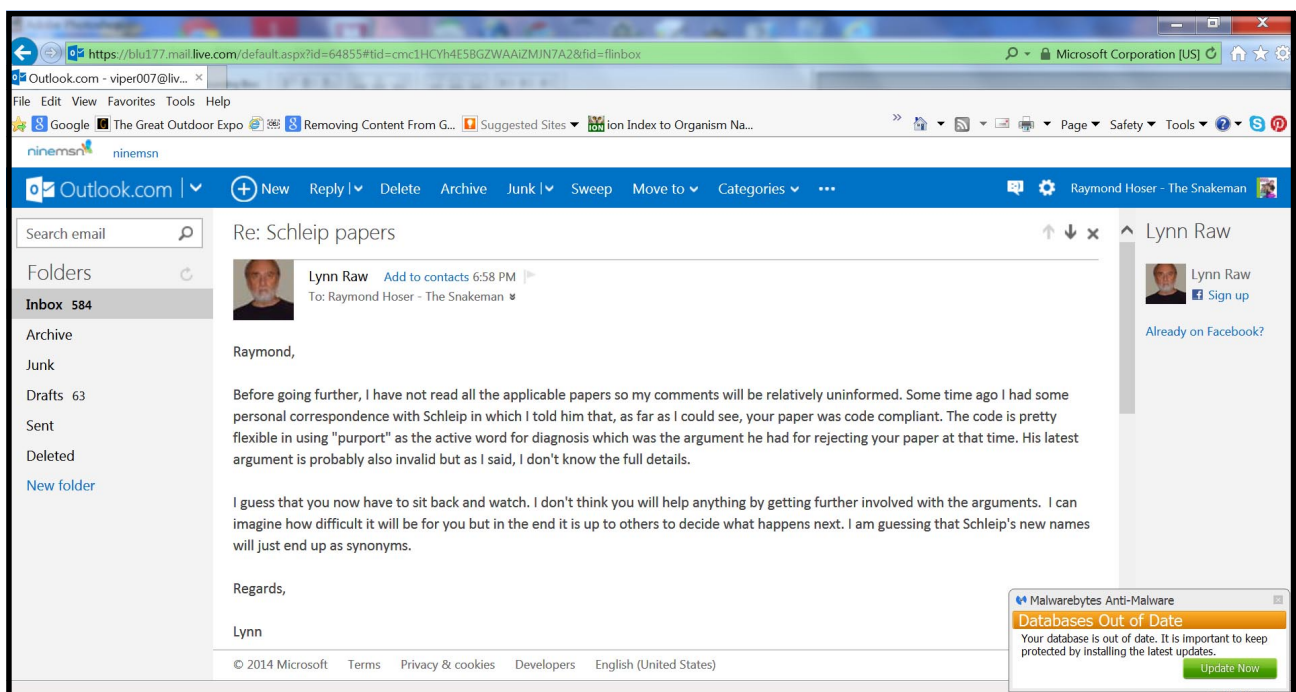
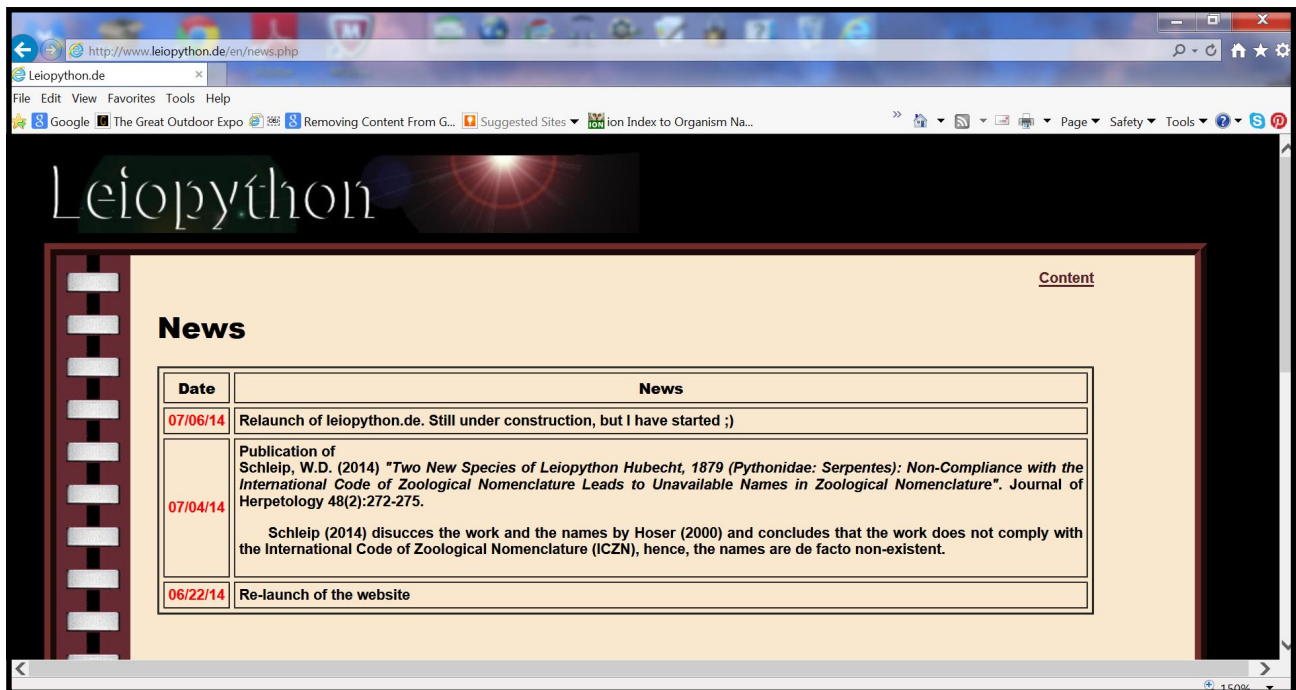
“Critics of Kaiser et al.: Kaiser et al. (which includes myself a co-author) were criticized for stepping outside the Code by calling to boycott Hoser’s work and names. ... I tend to no (sic) long (sic) stick to the Code at any time!

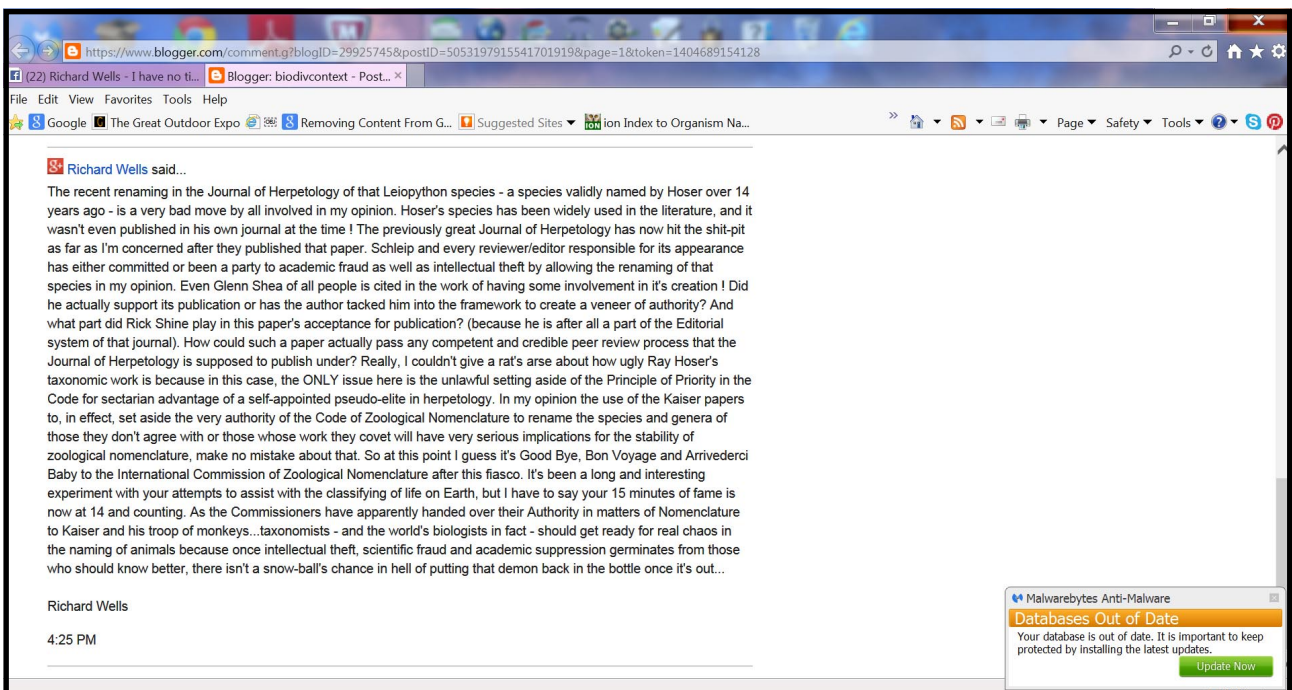
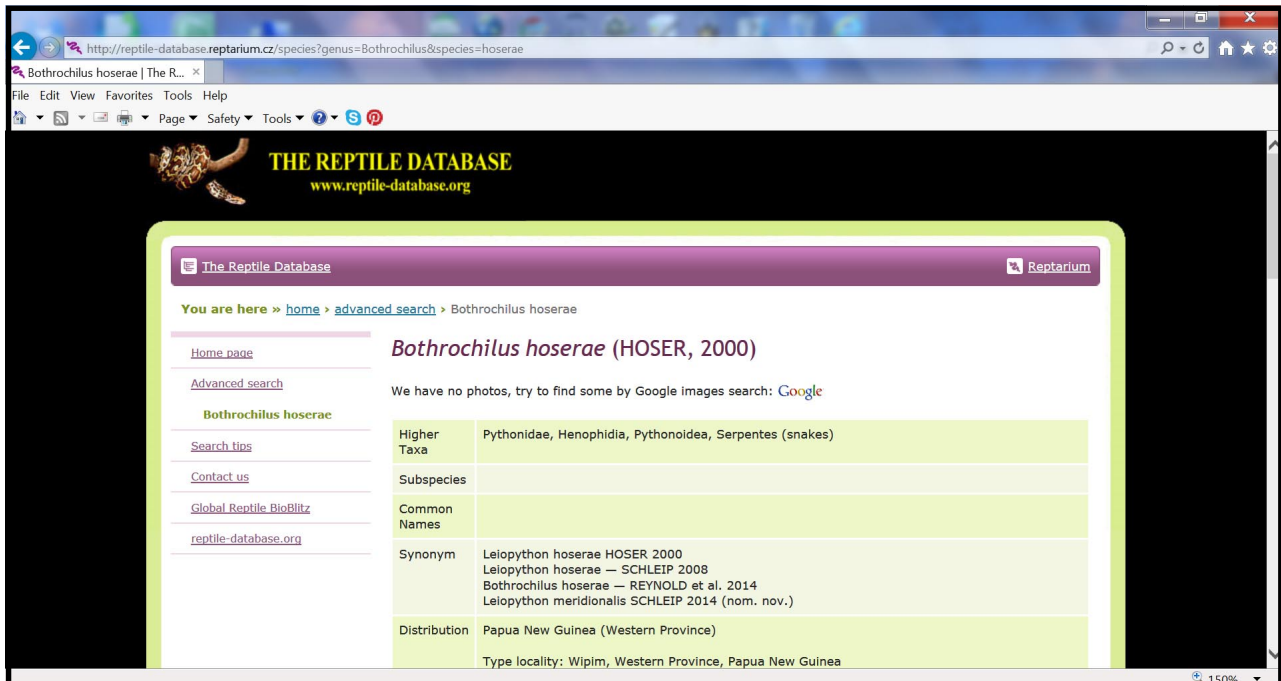
... The names were problematic, not the science behind. If the names were without any doubt ok, I wouldn’t have published that paper at all. ...”

In summary Schleip is a rule-breaking taxonomic vandal of the worst form.

He has hijacked a PRINO journal through his close friend, editor of associated journal *Herpetological Review*, Rob Hansen.

In this “journal” he has published destabilizing rubbish which if allowed to go unchecked threatens to unleash unprecedented nomenclatural chaos, not just in herpetology, but zoology in general.





MID PAPER ADDENDUM: (2015) MORE OF THE SAME ... DAVID BARKER LIES IN A PRINO PAPER!

David G. Barker of Texas, USA, published as lead author the following online pdf "paper" in early 2015:

Barker, D. G., Barker, T. M., Davis, M. A. and Schuett, G. W. 2015. A review of the systematics and taxonomy of Pythonidae: an ancient serpent lineage. *Zoological Journal of the Linnean Society*, 2015. Online:19 pp.

While no new names are proposed for taxa, the paper is yet another significant part of the Kaiser plan in their attempt to fraudulently rewrite the history of the science involving these snakes.

These authors draw on the stated input of celebrated taxonomic vandal, Wulf Schleip, and other members of the Wüster gang that they name in their acknowledgements as assisting them.

The authors in the paper attempt to present a veneer of a review of the literature of the past 25 years with regards to python taxonomy.

This is in order to present at the end of the paper their list of "the binomials of python species of the world with standard common names."

Clearly their hope is that everyone else uses the names of their choice.

Continuing with the taxonomic vandalism and nomenclatural theft of the Wüster gang the authors improperly use the invalid names *Malayopython* Reynolds et al. 2013a, 2013b and 2014 instead of the correct *Broghammerus* Hoser, 2004 and *Leiopython meridionalis* Schleip, 2014 instead of the correct *Leiopython hoseri* Hoser, 2000.

There is no justification for this action in the paper. In fact neither the original papers of Hoser are cited or for that matter the Kaiser "veto" in the form of Kaiser et al. (2013) or any other alternative.

In fact there are no citations of the Hoser works at all and any references to them in the text of his paper are oblique and improper and deliberately avoid citing the papers by name or references.

Now this is not merely a case of a rival taxonomist getting his nose out of joint because his work isn't cited, but rather something far more serious as shown herein.

In terms of the genus *Broghammerus*, the closest they got to referring to it was when they wrote: "In sum, Rawlings et al. (2008) support the Laurasian origin of pythons, identify a paraphyletic division of *Python* that is hypothesized in all three analyses of combined morphological and molecular characters, and propose *Broghammerus* (*nomen dubium*) as a new genus for the (*reticulatus* + *timoriensis*) clade.

There is no other mention of either *Broghammerus* or "Hoser" in terms of these snakes within the paper, except in a table where the name is inadvertently used four times.

Even where Barker et al. present a formal redescription of the genus

"*Malayopython* REYNOLDS ET AL. (2014)", they do not even list *Broghammerus* as a synonym!

That this wasn't just a case of sloppy taxonomy is shown below.

In terms of the taxon, *Leiopython hoseri*, the closest they got to referring to it or "Hoser" was when they wrote:

"Rawlings et al. (2008; as above) which maintains *Aspidites* composed of (*ramsayi* + *melanocephalus*), and *Bothrochilus*, composed of (*hoseri* [*nomen dubium* corrected to *L. meridionalis* (Schleip, 2014)] + (*boa* + *albertisii*))."

The deliberate exclusion of these proper Hoser taxon names in any proper way from their account, or the original descriptions makes a mockery of any veneer that the Barker et al. paper is a proper review of python taxonomy over the last quarter century.

This is even more apparent when one revisits the author's alleged justification for the forcible suppression of the names and authorship for the junior synonyms improperly coined by their friends.

For both *Broghammerus* and *Leiopython hoseri*, Barker et al. listed them as being "*nomen dubium*", which is a term I am sure they hoped most readers did not fully understand.

Fortunately it is properly defined in the *International Code of Zoological Nomenclature*, which states: "*nomen dubium* (pl. *nomina dubia*), n.

A Latin term meaning "a name of unknown or doubtful application".

If one then reads the original scientific descriptions of both *Broghammerus* and *Leiopython hoseri*, one finds that both names are not of unknown or doubtful application. We know this for a fact, because the original descriptions speak for themselves in that each define their taxa by reference to holotype or type species and are then followed by an extensive diagnosis, just to make sure of the fact.

Furthermore Schleip (2014) and Reynolds et al. (2014), both cited as the correct authorities by Barker et al. (2015) knew exactly which species and genera each name applied to.

They had merely invoked the Kaiser "veto" to override the proper names and not because the names were *nomina dubia*.

That the lead author of Barker et al. (2015) had

knowingly made a fraudulent claim that both *Broghammerus* and *Leiopython hoserae* were both *nomen dubium* is found from the earlier writings of Mr. Barker himself!

In his paper:

The Corrected Lengths of Two Well-known Giant Pythons and the Establishment of a

New Maximum Length Record for Burmese Pythons, *Python bivittatus*; by David G. Barker, Stephen L. Barten, DVM, Jonas P. Ehrsam and Louis Daddono, published in the *Bulletin of the Chicago Herpetological Society* 47(1):1-6 in 2012, and online as of 1 May 2015 at:

[http://www.chicagoherp.org/bulletin/47\(1\).pdf](http://www.chicagoherp.org/bulletin/47(1).pdf) he refers to Reticulated Pythons by their correct name *Broghammerus reticulatus*, citing both common and scientific name at the same time and place.

On his own website at:

<http://vpi.com/2013/04/05/040513-call-data-pythons-indonesia-philippines-papua-new-guinea-and-continental-asia>

in a post dated 4 May 2013 and still online as of 1 May 2015, he refers to the Southern White Lipped Python as "Southern whitelip python *Leiopython hoserae*".

Yes he uses those exact words.

The url for that one is at:

<http://vpi.com/2013/04/05/040513-call-data-pythons-indonesia-philippines-papua-new-guinea-and-continental-asia>

This means that as recently as 2012 and 2013, David G. Barker himself was well aware of the correct application for each of these taxon names.

This means they could not possibly be *nomen dubium* and he knew that all along!

Put bluntly he had lied!

What Barker *et al.* had in fact done was made a series of fraudulent claims in their 2015 paper to ignore the correct nomenclature in order to use what they well knew as the incorrect names.

This fraudulent science by Mr. David G. Barker is of particular concern as he has been a vocal critic of other reptile scientists (e.g. Rhodda and Reed) via papers he has published in the *Bulletin of the Chicago Herpetological Society* via an editor who is a close personal friend and gives him an effective free reign to write whatever he wants and without significant editorial control.

These critiques were online as of 1 May 2015 on his own website at:

https://usark.org/wp-content/uploads/2012/05/Barkers_DataSetCritique-002.pdf

and

http://vpi.com/sites/default/files/Barkers_2ClimateSpaceModels-for_Burmese.pdf and

http://vpi.com/sites/default/files/ReedRodda_Review_BarkerBarker2010_4.pdf

and more than ten others as listed at:

<http://vpi.com/publications>

David Barker has been a serial critic of others and alleged defects in their scientific methods.

He has accused US Government scientists of ethical misconduct in their papers and yet in the paper Barker *et al.* (2015) he has committed a series of dishonest acts of scientific fraud that easily eclipses those of others he has similarly accused.

Then there's the issue of evidence free taxonomic vandalism, the holotype example being the improper creation of three non-existent *Leiopython* species by Schleip in 2008 (all in fact synonyms of *L. albertisi*).

Recall that Schleip claimed to have DNA evidence to support his "new" species in the preamble of his paper, but that the paper itself contained no such evidence.

Hoser (2009a), pointed out this obvious fact and other critical defects with Schleip's work. Noting that no one in the pet trade or outside the immediate circle of the Wüster gang of which Schleip is a part pretends that these taxa exist and that even as of 2015, there is not a shred of DNA or other evidence to support the idea that the three alleged species exist, it was scandalous for Barker *et al.* (2015) to list them as being valid and widely recognised species in the absence of a shred of new evidence.

However they did exactly that!

Does all this make *Zoological Journal of the Linnean Society* a PRINO journal?

The answer must be a "yes".

After all, Pythons are not unknown to most herpetologists. *Broghammerus* has been around for more than a decade and being attached to the world's longest species of snake, is well-known to every herpetologist (including as we know David Barker)!

That an expert reviewer would not be aware of the fact and not question a previously unheard of allegation that the name of more than ten years of age was suddenly a *nomen dubium* defies credible belief.

The same applies for the icon-species *Leiopython hoserae*, widely known and recognized for the previous 15 years (including as we know, David Barker)!

Barker *et al.* refer extensively in their paper to "*Morelia azurea*", a taxon resurrected from

The historical record has showed Schleip was lying again!

Wulf Schleip

Page 1 of 3

Wulf Schleip

Home

Timeline About Photos Friends More

Do you know Wulf?

If you know Wulf, send him a message.

Photos · 72

Likes · 164

Groups · 8

Notes · 3

Wulf Schleip shared a link via Snakebusters Australian Snakeman. 3 hours ago · Edited

HAHA And this is from his snakebusters site... So why should either Mark or I or anybody else we know rename his available shit?

Here's his text:

Well, well, well, the world's worst secret is out! Serial thieves, Mark O'Shea, Wulf Schleip and others in the Wuster gang seek to rename the genus Broghammerus, Hoser 2004 with their own wacky name in breach of the 200 year old zoological rules. They are doing this via a planned so-called paper in a PRINO journal they have editorial control and influence, (PRINO being an acronym for "peer reviewed in name only"). These serial law breakers and rule breakers have already been condemned by commissioners of the ICZN (International Commission for Zoological Nomenclature), but of course even this doesn't stop the gang of Immoral thieves. By the way, any attempt to rename an existing genus, will merely make the new name relegated to the bowels of synonymy. More alarming is their reckless ongoing attempts to destabilize the taxonomy and nomenclature of our best known reptiles. For details of what they are about go here <http://www.smuggled.com/scientific-fraud-wulf-schleip.htm>

Scientific fraud, Wulf Schleip, three non-existent Leiopython species
www.smuggled.com

In 2008, an amateur snake handler, Wulf Schleip invented three species of python in a paper he published in a journal published by a

Share

2 people like this.

Kristen Wiley Poser really loves himself. What a loser. 3 hours ago via mobile

Wulf Schleip Now and again from me to you: Neither the "truth haters", "Wuster gang" or "serial thieves" or however you may call us or anybody we know intends or is in progress of renaming Broghammerus so Hoser, please SHUT THE FUCK UP! and stop spreading lies! 3 hours ago · 3

Phillip Wagner prino journal find ich wichtig ... See Translation 2 hours ago via mobile · 2

David Kelley He seriously has a mental problem....sad almost. 2 hours ago via mobile · 1

Wulf Schleip But only almost 2 hours ago · 1

David Kelley Exactly. 2 hours ago via mobile · 1

Phillip Wagner The problem is that he is a narcissist. So it would be the best for us and worst for him to ignore him. But thats hard. 2 hours ago via mobile · 2

Wulf Schleip shared Raymond Hoser's photo. December 7

Mathew Booth shared Australian Conservation Foundation's photo.

David Dieri commented on Legislation USA's activity in The High Republic... Money Invest

Nick Staszewski likes Angel photo.

Monica Downalymonroe commented on her own photo: "as K..."

Yocan Hantham likes Olivia said she or Start

Olivia H. Hawkeswood likes Michelle Baker's photo.

Joey Kerr commented on her own photo: "they are amazing. I am..."

Michelle Richardson likes Dariusz reptiles.

James Strawbz Dudley commented on Police Vs Hoon's photo: "hahahaha"

Martin Driessen I would rather spend my money on ideas and dreams than alcohol and fantasies

Ben Moore commented on his own photo: "The wet ground is my wee as I pissed myself whilst 5 stories up! Haha"

Jessica Jeanette Yuwono likes Ange Smith's photo.

Eddy Clerc commented on Orlene Fournier's link: "Ah bon mais quel est ce lien?"

Scott Grant Mobile

Terrill Anne Banks Web

Stuart Hampton Mobile

Lauren Polwarth Mobile

Alex Staszewski Mobile

James Potter Web

Adrian Gedlak Web

MORE FRIENDS (25)

Ajriky Katkar Mobile

Ball Python Royal Mobile

Search

<https://www.facebook.com/wulf.schleip?fref=ts>

09/12/2013

synonymy with "*Morelia viridis*" by myself in Hoser (2009a) for the first time in 135 years, but fraudulently refuse to cite this important point. They instead fraudulently imply that they are to be the future cited name authority for this "new" taxonomic act.

This critically important fact is however noted by another member of the Wüster gang, namely Peter Uetz, on his "The Reptile Database", as downloaded from:

[http://reptile-database.reptarium.cz/](http://reptile-database.reptarium.cz/species?genus=Morelia&species=viridis)

species?genus=Morelia&species=viridis

as recently as 1 May 2015, where he wrote:

"*M. azureus* was resurrected from the synonymy of *M. viridis* by Hoser (2009)."

Uetz, as a loyal lieutenant to the Wüster gang has never been in the business of correctly citing my work or recognizing sensible taxonomic acts by myself, so I should note here that his preceding statement was made in the context that he was in turn re-synonymising the taxon with "*M. viridis*".

No doubt when the obvious fact that "*M. azureus*" is a different taxon to "*M. viridis*" is widely recognized, as will inevitably be the case (and seen for example by the statement of the obvious in Barker *et al.* 2015), Uetz will alter his page to exclude any note that I had been the first to recognise the taxon as distinct in 135 years and that my judgement (opposed by Kaiser *et al.* 2013) had been shown to be correct.

Two other author names are also synonymous with python taxonomy, namely Wells and Wellington, most notably via their papers from 1983 and 1985, which among other things properly named the widely recognized genus *Antaresia*. While relying on the works of Wells and Wellington extensively throughout their paper, Barker *et al.* (2015) did not cite their names once!

This is known as plagiarism, that being the theft of the work of another person without proper attribution of the source.

The same plagiarism of work occurred when the authors of Barker *et al.* (2015) reused my diagnostic material for the genus *Broghammerus* (calling it "*Malayopython*") without citing the original source, noting that the same ethically repugnant act was committed by Reynolds *et al.* (2013a, 2013b and 2014) as well..

This obvious deliberate non-citation of the most significant name authorities for pythons in the past quarter century would have raised the eyebrows of any professional peer reviewer.

Of course the taxon names of Wells and Wellington have also been over-written in violation of the code by members of the Wüster gang (but rejected by

many including Cogger, 2014a) and hence Barker *et al.* have chosen to try to erase their names from the history of herpetology as well.

To get an idea of the level of improper suppression of citation of the works of those outside the Wüster gang of which Barker is clearly a part, one need look no further than his 1994 book *Pythons of the World: Australia*, which was published in a period before the Kaiser *et al.* (2013) scheme was hatched.

In that book, the most cited authority in the bibliography was none other than Raymond Hoser. Within the book proper, Wells and Wellington were cited throughout and also appeared in the bibliography.

Thorpe (2015) described the improper actions of the Wüster gang's "papers" as "trying to rewrite the history of reptile taxonomy..

The paper by Barker *et al.* (2015) was one most of the most fraudulent attempts at Nazi-style rewriting of herpetological history attempted so far!

They have engaged in a modern day digital "burning of books" that would have made Hitler's Nazis proud.

TAXONOMIC VANDALISM DEFINED

Taxonomic vandalism is a term widely used by the Wüster gang and others to decry any taxonomy papers they either don't agree with or have been published by people they deem enemies.

The term is used with such frequency as to be almost meaningless, is invariably never defined or defined in context and often used to describe publications the accusers haven't even read!

However a paper that makes a taxonomic proposal of any sort is not in fact vandalism!

Vandalism by definition is the wilful destruction of something.

Alternatively as defined by a Google search for the words "definition of vandalism", the definition is given as "action involving deliberate destruction of or damage to public or private property."

Hence by definition taxonomic vandalism can only ever be "the wilful destruction of or damage to an existing taxonomic and nomenclatural framework."

Now as I have never stated a desire to cause destruction or damage to anything taxonomic, I cannot by definition ever be realistically accused of taxonomic vandalism.

The point is made here because this is the improper accusation repeatedly laid against me by the Wüster gang including in Kaiser *et al.* (2013).

However it is clear from the statements of Kaiser *et al.* (2013), page 20, that they are asking others to do exactly that ... engaging in taxonomic vandalism.

They are asking other authors to defy the rules of the code, ... to destroy and vandalise it!

It is also clear that the authors, Wallach *et al.* (2009), Baig *et al.* (2013), Bates *et al.* (2013), Reynolds *et al.* (2013a, 2013b and 2014), Hedges *et al.* (2014), Thomas *et al.* (2014) and Schleip (2014b) as cited by Kaiser (2014b) have all engaged in the most reckless taxonomic vandalism possible.

All have wilfully sought to attack and destroy a code-complaint nomenclature that fits within a taxonomic framework they agree with to deliberately create taxonomic and nomenclatural instability.

These people have definitely engaged in "taxonomic vandalism".

As an addenda to the above, I must note that Rhodin *et al.* (2015), published in *BZN* in March 2015, which lists Schleip as a co-author, explicitly excludes *Leiopython hoseri* from the hitlist of taxon names they seek to overwrite.

Has Schleip really changed his mind?

Has he in 2015, decided to reverse his desire to overwrite *Leiopython hoseri* with his own coined name?

Or has he merely signed an anti-Hoser rant published in *BZN* without actually reading the document and seeing the relevance to his own acts of taxonomic vandalism?

No matter how one views the relevant publications to which Schleip has added his name, either as author or co-author, it is clear that his inconsistent actions alone have done more to destabilize zoological nomenclature than any papers by people such as myself.

JOURNAL HIJACKING!

This can take several forms and has several definitions.

In this case, I define "journal hijacking" as when a group with a vested interest take control of a what may have previously been a highly regarded scientific publication and use it to peddle their own agenda.

This includes by only publishing material that fits within their pre-determined position. Included within this definition is a breakdown of proper peer review and overt censorship of scientific material that properly rebuts the position of those who have hijacked the publication.

Hijackers may either totally debase a journal for their objectives, or alternatively only those aspects relevant to their agenda, allowing other parts or manifestations of the publication to continue unaffected.

It is clear that the Wüster gang have been engaged in this practice.

Hoser (2013b) detailed how Mark O'Shea hijacked the International Herpetological Society Journal, *The Herptile* in the period post-dating 2004 to peddle his own warped anti-Hoser agenda.

This included via his own articles that published demonstrably false claims about venomoid snakes regenerating venom, ripping off my registered trademarks (e.g. Snakebusters) and using them to cause damage to my lawful business within the same journal and also engaging in the serious scientific crime of plagiarising earlier papers of mine. That is use of my work without proper attribution.

More recently, it has become quite clear that the Wüster gang, through editor Robert Hansen have hijacked *Herpetological Review* in order to peddle their warped and reckless agenda.

Both journals have repeatedly refused to publish material that dissents from their position or print corrections to demonstrably false statements within their publications.

I have been able to publish material exposing their misconduct in a journal that they cannot control, this being *Australasian Journal of Herpetology (AJH)*. They will also claim that I have in effect hijacked my own publication and to a limited degree the claim would have a factual basis.

However the claims would fall down for two key reasons.

Firstly the journal *AJH* was set up more than a decade after the Wüster gang had been harassing journal editors globally not to publish my material. While their success was limited, it was causing problems in that my papers were being delayed, I was wasting time dealing with editors receiving illegal threats and there was the ever-present risk of theft of my material by the Wüster gang.

AJH can therefore be better viewed as a defensive reaction to the Wüster gang's misconduct.

Furthermore the main reason to publish *AJH* was in fact to retain control of my intellectual property (IP), a decision that paralleled my own moves in the 1990's to publish all my later books, even though publishers were lining up to publish my material for me.

As for the repeated claims against my own publishing material by other people in *AJH*, there is one reason and that is cost.

In 2009 \$50,000 was earmarked to fund publishing *AJH* with papers by others. Numerous authors submitted material for publication and lot of papers were reviewed and ready to print.

However in mid 2010, partly as a result of agitation by the Wüster gang and allies (see Hunter *et al.* 2006), I found myself facing bogus criminal charges

and legal proceedings that have so far cost well over \$50,000 to defend and more than \$1,000,000 in lost income.

We did win various legal proceedings on 8 June 2012 and 5 September 2015 (Supreme Court of Victoria - Court of Appeal Victoria, Victoria 2012 and Supreme Court of Victoria - Court of Appeal Victoria, Victoria 2014).

The result of this was that our unlawfully shut down wildlife education business could now trade again, two previous court judgements against me for alleged criminal offences were formally quashed (set aside as wrong) and costs were awarded in our favour.

However, and in spite of a preliminary bill for \$1 million already being served on the relevant government department, we have not yet received this money or any other money from them.

As a result of the above sequence of events and the current (as of May 2015) state of play, the ability to fund the publication of papers by others in *AJH* was lost and all papers by others were returned to the authors unprinted in 2011.

Of relevance here is that by definition, a hijacked journal is usually a PRINO journal if it masquerades as having peer review but lacks it.

Wellington (2013) alleged that the ICZN Journal, *Bulletin of Zoological Nomenclature* (*BZN*) has also been hijacked as it printed a reckless "paper" by Kaiser (Kaiser 2013) effectively calling for a dismantling of the Zoological Code and replete with numerous errors.

The significance of the publication was that Kaiser's paper was an attack on the code in the very publication that should be defending it!

This claim was strengthened by the fact that in March 2014, three more sets of "comments" were printed with demonstrably false information within (backed up with another round of false comments in the June 2014 issue and more subsequent to that date), all effectively launching an attack on the Zoological Code and calling for a departure from it.

A "free speech" justification fails as this only applies when people stick to the facts.

None of Kaiser (2013), Kaiser (2014), Schleich (2014), Wüster *et al.* (2014), Thomson (2014), or Rhodin *et al.* (2015) have done this!

Furthermore a further paper by Harvey and Yanega in the December 2013 issue of *BZN* called for comments to be made online in relation to the ridiculous propositions of Kaiser (2013) (Kaiser's "Taxon Filter" being his perverse form of taxonomic and nomenclatural anarchy and censorship at the one time).

However when comments in response to Harvey and Yanega (2013) were posted online, the editors of the relevant webpage refused to publish corrections to false statements made by Kaiser and associates, and furthermore did nothing to either correct or remove what they knew to be wrong.

In summary the so-called debate was evidently being managed to present a veneer of a viewpoint opposite to the majority view or alternatively based on arguments lacking a factual basis.

In fairness to the editors of *BZN*, while they have yet to publish corrections to many of the reckless comments of Kaiser (2013), Kaiser (2014), Schleich (2014a), Wüster *et al.* (2014), and Thomson (2014) they may well do so, bearing in mind the sizeable number of responses opposing the views of the Wüster gang submitted so far.

Worrying however is that all of Kaiser (2014), Schleich (2014a) and Wüster *et al.* (2014), were submitted after my own formal response to Kaiser (2013), with my response not being published in the next issue of *BZN*, but the other three sets of "comments", replete with obvious factual errors, were as was the Thomson submission three months after the first round!

In partial defence of the ICZN, the secretary for the ICZN Bulletin advised me that it was not unusual for them to allow negative comments before favourable responses in terms of submissions, so before making a final judgement on the *BZN* with regards to Case 3601 and associated issues, it may be best to "watch that space" for the next couple of years.

Also *BZN* is not a peer reviewed scientific journal in the normal sense of the word and so attempting to compare the publication with these may not be fair to the ICZN either.

But I should however note that Wellington's outrage was not the first time there have been allegations made in terms of the ICZN Bulletin.

Dr Alain Dubois, of France, an advocate for strict adherence to the rules of the code, made similar claims.

Dubois (2005) said:

"In the recent years, in order to support its action against a strict respect of the Rules, the ICZN Secretariat has deliberately biased the international public discussion which should normally have developed freely in its Bulletin: it has done so both through active support to some interpretations of the cases and repeated censorship of adverse opinions submitted to *BZN* (see Note 2 above). A recent practice, quite foreign to the intellectual tradition of science but closer to political, economic or other social activities, has been the support brought by ICZN to clear "lobby" actions undertaken by some

researchers in order for various colleagues to speak for “their” nomenclature, despite its clear invalidity according to the Rules (e.g., Jennings *et al.* 1994; Webb *et al.* 1994). That such suddenly numerous comments cannot have been “spontaneous” but were duly organized is made clear by the usual complete or almost complete absence of comments in similar cases in the same zoological group and at the same period, many votes of ICZN having been made in the absence of even a single published comment in *BZN*.”

Footnote 2 included the following:

“most colleagues who ever published a text in *BZN* know how their text can be authoritatively shortened or modified before publication, not rarely in a sense quite different from that of the originally submitted manuscript. Indisputably, in the recent decades *BZN* has worked as the “private journal” of a small number of persons, not as the official journal of an international body supposedly at the service of zoology and zoologists worldwide.”

What I should also note is that the ICZN secretariat of 2014 is different to that of year 2005 or thereabouts and so the earlier accusations (even if upheld) cannot by default be levelled against the current editors.

However, accepting the claims of Dubois to have validity (he provides extensive case details in his paper) it is clear that even well-regarded publications thought to be beyond reproach may be targeted by journal hijackers and everyone should be vigilant of this risk.

This also means that evidence of misuse of *BZN* by the Wüster gang should be taken seriously.

I note their use of the publication at end 2013 to mid 2015 as a vehicle to peddle lies as parts of their campaign is outlined by him on various private Facebook pages.

Even if Wüster’s claims of widespread support for his position were true in part, it would only have been built on the basis of lies and misrepresentations as seen on their own edited “Wikipedia” pages (Wüster, O’Shea *et al.* 2000-2014).

In this regard, I note that Wüster *et al.* and their proxies attended herpetological society meetings and sought resolutions from members against Hoser papers, without affording members an opportunity to even view and read the papers they were being asked to pass motions against (ASH 2013) or when members of societies were at drunken gatherings (Rowley 2014).

Wüster *et al.* tried to get the prestigious herpetological society (Australasian Society of Herpetologists), to sign their crusade manifesto

when the limited remaining membership present at a meeting were highly intoxicated after guzzling no less than 100 cases of beer! (ASH 2013, Rowley 2014).

Put another way, this group were not in a sober position to be voting on dozens of papers they hadn’t seen or read (ASH 2013, Rowley 2014)!

THE REAL AND LIMITED SUPPORT OF THE WÜSTER GANG

In submissions to the ICZN against Wells and Wellington in 1987 and the immediately following years, the proponents against using the nomenclature of the pair claimed a majority support against the pair as detailed in the original submission against them (The President, Australian Society of Herpetologists, 1987) and later comments, including those cited by Anonymous (1988). That was derived from a signed petition by an alleged 81 signatories who stated that failure to suppress the Wells and Wellington papers of 1983 and 1985 would:

“cause massive and long lasting instability and confusion in the nomenclature of Australasian herpetofauna”.

By the way, the unnamed “President, Australian Society of Herpetologists” at the time in 1987 was none other than Richard Shine, based at the University of Sydney.

He was evidently afraid to put his name to the document filed with the ICZN as he presumably knew many of the claims within it were fraudulent and didn’t want it to backfire on him.

Kaiser’s threat of mass disobedience against the code by the majority of herpetologists in the event of a judgement in favour of *Spracklandus* (Kaiser 2014a), which he also said was the test case for his plan to suppress all Hoser names, was repeated by his good friends Wüster *et al.* (2014), Rhodin *et al.* (2015) and Schleip (2014a) who said “If the Commission, however, were to vote in favor of Case 3601 and declare the name *Spracklandus* Hoser, 2009 available ... I predict that the majority of herpetologists will follow the recommendations of Kaiser *et al.* (2013) and continue to ignore *AJH* as a reliable source for nomenclatural and taxonomic information.”

This is the same threat made in *BZN* in relation to the Wells and Wellington papers and names proposed within them made by Stone (1988) and others.

Stone (1988) wrote:

“If the Commission takes no action with respect to the nomenclature proposed in these publications other scientists may of course choose to ignore that obligation.”

King (1988) made identical comments to Kaiser *et al.* (2013) when he said:
 "If they (the ICZN) fail to do so (suppress the works of Wells and Wellington) they will jeopardise the survival of the system of nomenclature which we all use."

Following the ICZN's judgement in favour of the alleged taxonomic vandals (Wells and Wellington) (ICZN 1991) there was no mass disobedience against the code as foreshadowed by Stone (1988) or King (1988) and in the fullness of time the original code-compliant names were accepted and widely used (Shea 2013, Cogger 2014a) and the code survived intact. This usage included the original code-compliant names being used by authors in favour of the junior synonyms coined by the protesters who had hoped the ICZN would formally suppress the earlier code-compliant papers (Shea 2014a, Cogger 2014a).

Kaiser's claim to represent "the herpetological community" (as also made by Wüster *et al.* 2014) is false as demonstrated by Wellington (2013), Wells (2014a) and others, but again no different to the claims made by those seeking to suppress the Wells and Wellington papers (Australian Society of Herpetologists 1987, ICZN 1991) so claims made by Kaiser in this alleged respect must also be rejected.

There are two other telling bits of evidence that make a mockery of claims that the Wüster gang and the group before them in the 1980's have majority support for their views, even in the face of their reckless and dishonest campaigns, and obvious overt censorship of debate to stifle views opposite to their own in journals and online forums they control

ruthlessly.

On 7 March 2014, Dr. Glenn Shea, one who made submissions to the ICZN in relation to the Wells and Wellington case and certainly abreast of it all, made a very telling commentary on an online forum. The relevant part of Shea (2014a) is in the brackets. He wrote:

"So in summary, yes, there was a period between 1985 and when the ICZN ruled not to rule on the two W&W papers when people in herpetology were hoping that the new names and other nomenclatural acts in your two papers would be suppressed, in part based on the suggestions of certain ICZN members.

While that case was in progress, certain people (and it was a small number of taxonomists involved in reality) created a group of new names in the hope that once your two papers were suppressed for nomenclatural purposes, the new names would automatically take over.

Once the ICZN refused to rule and it became clear that the two W&W papers remained nomenclaturally valid, most of the names that were considered by taxonomists to apply to distinct taxa have been used!"

Or if one wants contemporary proof of the lie of widespread support for the Wüster gang, one need look no further than the favourite online vehicle for the Wüster gang's actions, namely Facebook.

Their main facebook page is one they call herpetological taxonomy, at:
<http://www.facebook.com/groups/Herpetofauna.taxonomy/> (Twombly *et al.* 2014)

Hoser 2015 - Australasian Journal of Herpetology 26:3-64.



As of 9 July 2014, it had exactly 290 “likes”. By contrast my own Facebook page at: <https://www.facebook.com/pages/Snakebusters-Australian-Snakeman/112806842145321> (Hoser *et al.* 2014) carried 2,199 likes, this being several times their number!

On 1 May 2015, their number of “likes” remained at 290, while <https://www.facebook.com/pages/Snakebusters-Australian-Snakeman/112806842145321> had added 200 to be at 2,333.

In other words, when one strips the lies and rhetoric from the Wüster gang, as well as the “multiple personalities” they operate under, you find they have very little support from herpetologists in general, the majority of whom can see their lies and deception for what they are.

BEFORE THEY BECAME THIEVES

Above, we’ve seen the details of papers published in PRINO journals from Scott Thomson (the man who seeks to steal the work of others to become the name authority on Australian terrapins) and Wulf Schleip (the man who wants to steal work of others to become the name authority for *Leiopython* species). Their papers cited here seek to do exactly what the first version of Kaiser *et al.* (Kaiser 2012b) asked them to do, which was to start “working outside acceptable rules of science and taxonomy”.

This is to get others to use their improperly coined names in favour of the correct code-compliant names, scientifically proposed of myself, Wells, Gray, Wellington and others even though we did the relevant research and publications decades or even centuries earlier.

But in their earlier days in herpetology, these men, Thomson and Schleip, didn’t have delusions of grandeur and seek to steal the work of others. In fact they even supported the rules of the Zoological Code.

This I know from checking some of their old posts from Kingsnake.com

Some of what they said in 2003 made sense and having just taken a look at their archived posts on kingsnake.com forums in 2014 (when Wuster was still alleging my papers described non-taxa, instead of expressing a desire to rename them all), they did make sense.

Here’s some quotes:

<http://forums.kingsnake.com/view.php?id=279134,292763>

Scott Thomson said:

“Ignoring names that meet the requirements of the ICZN is not an option. This cannot be done under strict application of the rules as the names if valid are Available and must be used. The alternative is to

refute them. So disagree with the names all you like, “Refute or Accept”. That is publish a valid refutation of the taxon to which the name applies.”

and

“I consider the ignorance of valid names by so called “professionals” to be as big a crime in taxonomy as those who published the names in the first place. As I said “refute it or accept it” if anyone has that much of an opinion then they can publish a refutation. If not use the name. It is refusal by some, for whatever reason, and use by others that causes so much confusion.”

These statements do of course rebut the central tenet of the Kaiser *et al.* documents he now claims to be a signatory to!

Thomson also wrote:

“Nomenclature is pretty black and white. There are a set of rules. Apply them, if the name is valid, use it, if not reject it. If you don’t like it.... well I don’t recall that being in the rules.

Cheers, Scott

Carettochelys.com”

Once again, a direct rebuttal of the entire Wuster gang campaign, which when stripped bare becomes, “we don’t like Hoser, so we won’t use his names”

Schleip’s pearl of wisdom:

<http://forums.kingsnake.com/view.php?id=279134,283007>

“If the paper (even hardly) meets the recommendations of the code of the ICZN the paper must be seen as “published” and therefore the names are available. Though it is still questionable if these taxa are biological valid entities or not. You can put them to synonymy, you can revist the taxa and come up with other conclusions, but you can not just ignore them.”

Well put Wulf, I couldn’t have done better myself!

THE THREE PHASES OF TRUTH

According to Arthur Schopenhauer, German philosopher (1788-1860), truth goes through three stages.

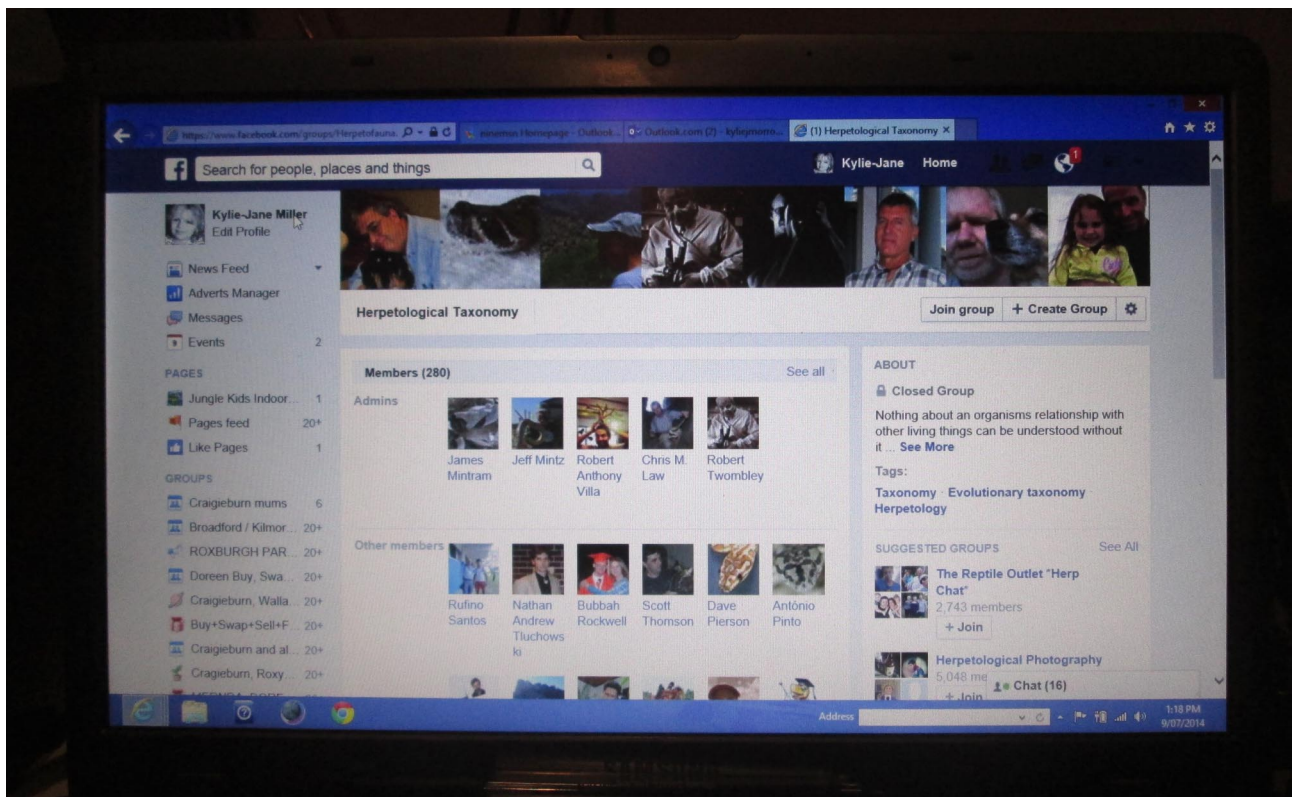
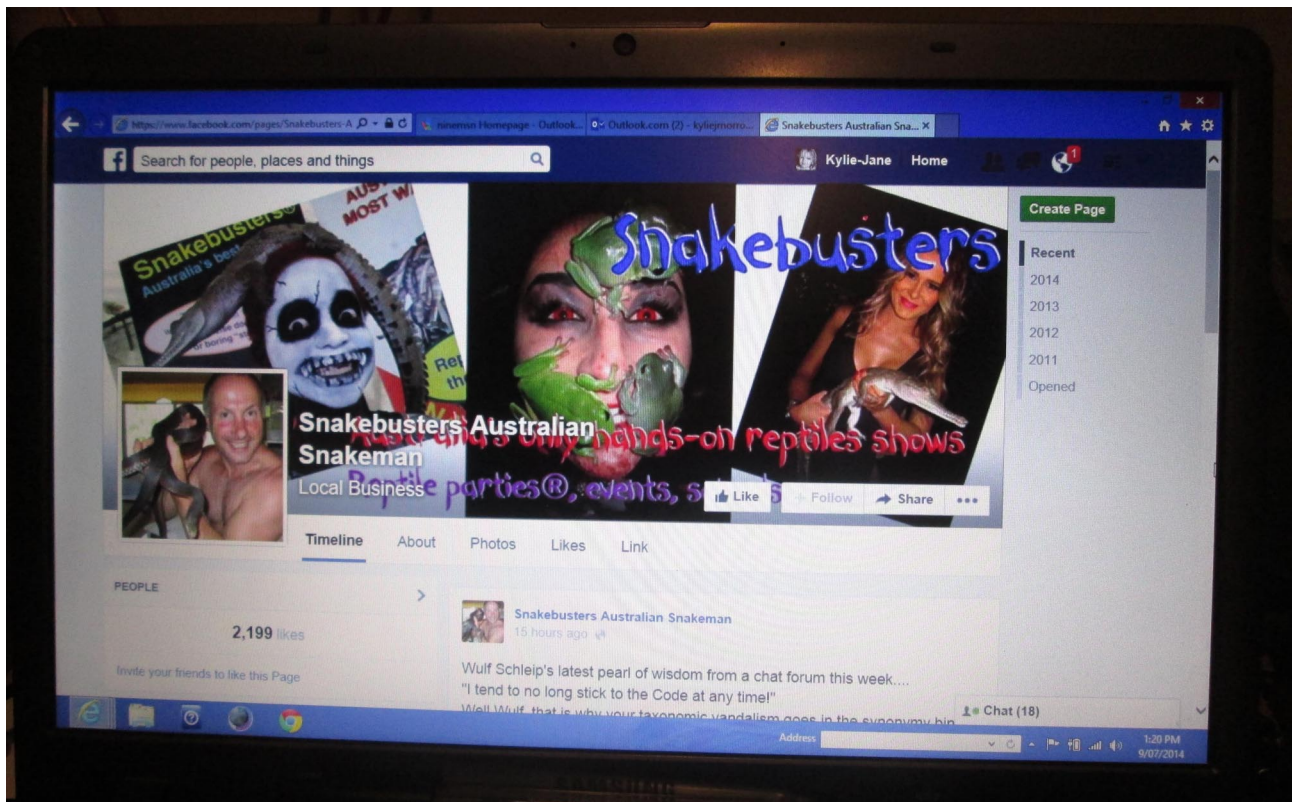
He said:

“All truth passes through three stages. First, it is ridiculed. Second, it is violently opposed. Third, it is accepted as being self-evident.”

In herpetology at the present time we can amend this to read:

“All truth passes through four stages. First, it is ridiculed. Second, it is violently opposed. Third, it is accepted as being self-evident. Fourth, it is stolen!”

This is the situation seen repeatedly in terms of the Wüster gang and their actions in terms of



Hoser 2015 - Australasian Journal of Herpetology 26:3-64.

herpetological taxonomy.

For the African Spitting Cobras, my 2009 description in Hoser (2009b) was ridiculed by Wüster and Fry just six days after the original description was published. Later realising that the diagnosis of *Spracklandus* Hoser, 2009 was obviously self evident, Wüster and associates (Wallach *et al.* 2009) published a bogus paper in a PRINO journal seeking to steal name credit for the genus.

For the African Gerrhosauridae genera *Funkisaurus* and *Swilesaurus* described by myself in 2013, (Hoser 2013c), Branch (2013) launched into an online tirade ridiculing the descriptions. As with the Cobra genus *Spracklandus*, the morphological and molecular evidence supporting my descriptions was self evident. Later that year Branch as a co-author, effectively stole my work by using the Kaiser "veto" (Eipper 2013), to rename the same genera using the same self evident data.

Over a longer time frame the same has occurred in terms of the species described by Nick Mutton in 2014 as a "no brainer valid species", this being *Leiopython hoserae* Hoser, 2000.

Denied as being valid for about 7 years by Schleip and others, he derided my taxonomy as obvious in the period from 2008 to 2014.

As seen herein, he published a collection of lies in his friend's PRINO journal to try to steal the name rights for the taxon in Schleip (2014b).

In terms of this paper, all cases relate to myself, Raymond Hoser, as the scientist whose work is being effectively stolen by unethical pseudo-scientists.

The public record speaks for itself.

If the ICZN allows these actions to go unchecked as sought by Kaiser *et al.* (2013) as republished by others including Kaiser (2012a, 2012b, 2013, 2014a, 2014b), Schleip (2014b) and others, there is no doubt that the value of the Zoological Code as a means of scientific communication will be damaged severely and potentially beyond repair.

CORRECTIONS OR DISSENT: USUALLY ABSENT IN PRINO JOURNALS

Science is best defined as a search for the truth. PRINO Journals can alternatively be defined as those journals that claim to be doing just that; searching for the truth with good quality control, but who for one reason or other, fail and then after the fact, continue to do so by choice.

Notwithstanding all that's written so far, there can and may be perfectly reasonable explanations for many of the serious errors making their way into print in the various journals identified herein.

Any journal that publishes a sufficient volume of

material will publish errors, some of which are so significant as to warrant immediate correction.

It is how a journal deals with correction of errors of fact that can also be used to identify if it is PRINO or not.

A journal editor (and/or author) that refuses to correct obvious errors after the error is brought to their attention is engaging in fraudulent behaviour.

This sort of behaviour is in many ways more reckless than the original publication of false, misleading or erroneous material in the first place.

Identified in this paper are numerous cases at various ostensibly peer reviewed journals where the editors have had serious fundamental flaws in papers drawn to their attention and yet in spite of this, they have flatly refused to correct the errors.

In 2009, I advised the editors of *Zootaxa* of the fundamental factual error that fatally disabled the central thesis of the paper by Wallach *et al.* (2009).

The reason for that paper was to rename the Cobra genus *Spracklandus*. But there was no scientific basis to do so!

Five years later, there has been no correction or retraction from the journal *Zootaxa* in any way, shape or form.

In order to neutralize the instability caused by the reckless publication of that paper, in 2012, I had to go to the ICZN to seek their intervention to stop the destabilization of the nomenclature of the Spitting Cobras, via Case 3601 at the ICZN.

By contrast, at *AJH*, when errors have been identified in papers, including even by Wüster himself, corrections and retractions have been made without hesitation and at the first possible opportunity.

In 2009, in *AJH* I published a description of a Cobra genus *Wellsus* Hoser, 2009 (Hoser 2009b).

Within a fortnight of the publication of the paper, Wüster took joy in announcing to the world via online posts on chat forums that I had made an error and that *Wellsus* was in fact a junior synonym for the name *Uraeus* Wagler, 1830.

After making checks to confirm the validity of his claims, I personally renounced the name online within hours of first being advised.

Furthermore at the first available opportunity, the name was formally renounced in *AJH* (Hoser 2012e)!

While peer review had evidently failed to detect the mistake in the paper pre-publication, as editor, I was not going to allow an error to stand once detected!

Any damage potentially caused in terms of destabilizing zoological nomenclature was to be minimized.

My own conduct in terms of Cobra taxonomy should be contrasted with that of the editors at *Zootaxa*, who five years after being advised of a serious and critical error in one of their papers (Wallach *et al.* 2009), they have not in any way retracted or corrected the mistake/s.

I should also mention that in the period 2009-2014, the time frame that *AJH* has been published, a number of issues have carried corrections, including some that could be described as minor and insignificant and not worth correcting.

The cases above are also significant as they also show another means to detect PRINO publications. PRINO publications are usually those that are intolerant of dissent and present a single and distorted view of the world.

Robert Hansen's *Herpetological Review* is a classic example of a publication that refuses to print dissenting views and/or corrections to obvious factual errors.

Even as journal editor, I have allowed views to be published in my journals that are the anti-thesis of what I believe and at a personal level, I'd prefer not to see in print.

As editor of *Monitor: Journal of the Victorian Herpetological Society* in 1998 and 1999, I published submissions totally opposed to a taxonomic paper (Hoser 1998) that I had published in a previous issue (Aplin 1999, Wells and Wellington 1999).

Wüster *et al.* regularly cite Aplin (1999) as a relevant criticism of my own works (e.g. Wüster *et al.* 2001), but choose to ignore that it was myself as editor of a properly run peer reviewed journal that allowed this very intense attack on myself to be published.

I did not engage in Wüster-style censorship at the journals I managed.

There has never been such judicial editorial management at any journals controlled by the Wüster gang allowing criticisms of their own works to ever be published.

Not one has done so!

Of note therefore is that all the publications associated with the Wüster gang and their reckless factually incorrect publications can only be defined as PRINO!

SUMMARY

The term PRINO, an abbreviation of "peer review in name only" is formally coined to identify those journals where the peer review process is so debased or shambolic as to effectively be absent in terms of any real benefit.

PRINO publications in the form of both papers and entire journals include material in all branches of

science, although the examples here are confined to herpetology, where PRINO is clearly commonplace.

In another example of apparent PRINO publication, Bennett (2014), provides an example of failure of alleged peer review at the journal *Sauria* in 2006 (Lutz 2006). He said:

"peer reviewers should require unambiguous proof of captive breeding success before accepting such claims in the future."

Standard practice for journal editors should be to provide reviewers with all cited literature and/or at least relevant cited parts (usually in the form of pdf's or jpegs), to ensure that what is alleged in these publications is in fact what is said, as well as in context.

Research data should be checkable and as a rule copyable by others. Reviewers should be asked to literally join the dots to make sure that the paper is factual and accurate and that any conclusions drawn, are able to be drawn on the basis of the evidence provided.

Of relevance here is that in the Supreme Court of Appeal of Victoria on 8 June 2012, Judges Nettle and Buchanan reversed an earlier legal judgement against me on the basis that the earlier judge had made a factual finding against me that was physically impossible on the basis of the evidence agreed by both sides. This was repeated by three other judges on 5 September 2014 when Judges Redlich, Tate and Santamaria squashed convictions and judgements previously upheld on the basis of claims that were scientifically physically impossible, conclusions that could not possibly be drawn on evidence available and therefore not available in law either (Supreme Court of Victoria - Court of Appeal Victoria, Victoria 2012 and Supreme Court of Victoria - Court of Appeal Victoria, Victoria 2014).

Science and peer review must be treated the same way. Reviewers and editors, must not simply review papers for "form" or "format" or accept author's claims without checking the evidence. Science must be based on facts and any conclusions or theories must sit within the realms of being physically possible.

Basic rules of communication, these being nomenclatural for taxonomists must be adhered to, not just in spirit, but in the detail of the code of zoological nomenclature itself.

The Zoological Code is not a "tricky" document as alleged by Kaiser (2012a, 2012b) and Kaiser *et al.* (2013). It is an easily read document, publicly available at no cost from the ICZN's website and should be consulted as need be by all publishing taxonomists and reviewers.

If all this slows down the publication process, then

so be it!

With publishing lead times now being measured in days, as opposed to years, just a few decades ago, a delay of a few more days due to a more effective peer review process is a small price to pay in return for better quality scientific publications. This is especially the case in terms of taxonomy, where reckless publication of junior synonyms creates unnecessary work for scientists for many years.

As to what will happen to the reckless taxonomic vandalism of Wallach *et al.* (2009), Baig *et al.* (2013), Bates *et al.* (2013), Reynolds *et al.* (2013a, 2013b and 2014), Hedges *et al.* (2014), Pyron and Wallach (2014), Thomas *et al.* (2014) and Schleip (2014) the historical record gives us a clue.

Shea (2014) in his online conversation with Richard Wells summed it up quite succinctly.

His statement was in the context of what happened when a similar group of taxonomic vandals hoped to rename taxa named by Wells and Wellington in the 1980's but the message is no different now.

Shea wrote:

"there was a period between 1985 and when the ICZN ruled not to rule on the two W&W papers (1991) when people in herpetology were hoping that the new names and other nomenclatural acts in your two papers would be suppressed, in part based on the suggestions of certain ICZN members.

While that case was in progress, certain people (and it was a small number of taxonomists involved in reality) created a group of new names in the hope that once your two papers were suppressed for nomenclatural purposes, the new names would automatically take over. Once the ICZN refused to rule and it became clear that the two W&W papers remained nomenclaturally valid, most of the names that were considered by taxonomists to apply to distinct taxa have been used!"

What Shea didn't mention but has been made clear by others, is that the renamers of validly named taxa have stamped themselves a place in history is unethical taxonomic vandals and would-be thieves, engaging in reckless and unscientific conduct and nothing more.

Or as Scott Eipper (Eipper 2013) said:

"You cannot use a viewpoint (Kaiser *et al.* 2013) - to act as a veto- to disregard the use of the code."

POSTSCRIPT: MORE THAN ZOOTAXA!

At the time this paper was being conceived, prepared and written, I was hampered by the ongoing destructive actions of the Wüster gang, in particular their non-stop over-writing of correct Hoser-names for reptile taxa.

On 7 July 2014, PRINO Journal *Zootaxa* published

yet another instalment of nomenclatural misconduct by the Wüster gang. The paper published was as follows:

"A Taxonomic Revision of Boas (Serpentes: Boidae)", (Pyron *et al.* 2014), authored by R. Alexander Pyron, R. Graham Reynolds and Frank T. Burbrink, all three being very close friends of Wüster himself.

Readers here will recall that Reynolds was the lead author of the non-code compliant paper that attempted to rename *Broghammerus* Hoser, 2004, with their own coined name "*Malayopython*" at end 2013 and early 2014 as detailed above.

This paper was a similar exercise in repackaging old information as "new" as a pretext to stealing my own earlier work and asserting name rights over taxa formally named by myself previously.

However in detailing the content of this most recent paper in proper context, I need to step back in time to the creation of two earlier papers.

In the 1990's when doing research on wildlife smuggling, I first inspected specimens of Boas in the genus *Candoia* being held at Sydney's government-owned Taronga Park Zoo. After two decades of research on Boas in general and *Candoia* in particular, I published a major paper on the group in 2013 (Hoser 2013d).

The most significant taxonomic act in the paper was the erection of a new family to accommodate the species within the genus *Candoia* (as generally recognized), namely Candoiidae Hoser, 2013.

The paper carried a publication date of 29 April 2013, but was in fact published about a fortnight earlier and hard copies were widely disseminated at the time, received by *Zoological Record* and the like. Shortly after this (on 1 May to be exact) another paper appeared online authored by Pyron, Burbrink and Weins (Pyron *et al.* 2013).

The paper's publication date had been backdated to 29 April 2013 and not coincidentally carried a description of a new subfamily of boas, namely Candoiinae.

While it is self-evident that the online paper was rushed out to try to steal my own "name authority" for the family level classification of those snakes, I did not at the time have any hard evidence for this fact.

After all, it is not uncommon for published papers to have their publication dates backdated (a common result of delayed publishing processes) and it is also not uncommon for unconnected authors to coincidentally describe the same taxa at the same time, with one or other obviously getting date priority over the other.

Even allowing for the fact that the authors of this

paper, Pyron and Burbrink in particular have been vocal in their attacks on myself in the 2013/2014 period, there was still at the time nothing in my possession to compellingly prove bad intent on the authors of the later paper that effectively renamed my boa family.

For that reason, I did not at the time raise the issue anywhere and simply let it slide.

After all, if and when other herpetologists sought to establish which of the two alleged 29 April 2013 papers had in fact been published first, they'd soon find that mine had priority on the basis of receipt by museums, *Zoological Record* (published in issue 149) and the like.

Furthermore, the names Candoiinae and Candoiidae both were derived from the genus group and so regardless of where others placed these snakes in terms of familial level, the names would be unchanged, regardless of which author/s took name authority.

Notable is that on 1 May 2014, Burbrink posted on his Facebook wall that the online paper of Pyron *et al.* (2013) just posted by himself had problems with it. He wrote: "Ok, there are still some minor edits that BMC didn't make. Alex is trying to get them to correct these problems now." Later the same day he wrote "I am not sure when they will publish the final version. There are a number of corrections to be made."

These posts did of course confirm beyond any doubt that their paper was as of 1 May 2013 not published and therefore, they did not have name authority for Candoiidae by any interpretation of the rules of the Zoological Code.

However in spite of all this, there was still no direct and irrefutable evidence that the authors had sought to deliberately or recklessly rename a Hoser-named taxon group, with the possibility of coincidental publication of the same findings being within reason.

The final amended version of Pyron *et al.* (2013) was published later in 2013 on 23 September.

However based on the above and in terms of the issues within this paper, they had been effectively forgotten by me as significant in any way in terms of the Wüster gang campaign until the publication of "A Taxonomic Revision of Boas (Serpentes: Boidae)", (Pyron *et al.* 2014), in *Zootaxa*.

It was that paper which confirmed the original intent of the authors in the earlier paper (that being to steal name authority rights) and also confirm other serious defects in their earlier 2013 paper.

Pyron *et al.* (2014) wrote:

"the study was published in an online-only open-access journal (BMC Evolutionary Biology). Thus, the creation of new, valid taxonomic names in the

work is governed by the recent amendment to the Code recognizing electronic publication (see Dubois *et al.* 2013). A key requirement of this amendment is that the work be registered with a Life Sciences ID at the ICZN's official online repository of names (ZooBank.org). Unfortunately, registration to ZooBank did not occur in Pyron *et al.* (2013), and Candoiinae is thus unavailable from that work. In addition, a grammatical ambiguity in the definition gave the impression to some readers that diagnostic characters were not shared by all species in the subfamily (genus *Candoia*). We resolve these issues here by re-describing the taxon with an expanded diagnosis, and provide comments that will produce a clearer and more consistent taxonomy within Booidea."

The authors then describe the group as a family thus:

"Candoiidae *fam. nov.*"

Significant also is that this time, the authors have in effect accepted that the original Hoser taxonomy and nomenclature of a year earlier is in fact correct, even though on their Facebook pages they had repeatedly claimed the very same act of erecting the family had been "taxonomic vandalism".

Of note however is that the authors Pyron *et al.* (2014) make no mention in this paper of Hoser, Wüster or Kaiser in any way, so one could at first glance be forgiven for thinking that the authors of this paper had merely inadvertently overlooked the earlier Hoser (2013) paper naming Candoiidae.

That we know this is not a possibility is seen by several means.

From the paper Pyron *et al.* (2014), they wrote: "We thank J. Boundy and A. Dubois for pointing out the nomenclatural issues addressed here, and Z. Nagy, W. Schleip, and one anonymous reviewer for comments on this MS."

Schleip of course was a listed co-author of Kaiser *et al.* (2013) and a vocal critic of all Hoser papers. He should have been aware of the content of the Hoser paper of 2013 (although I note Schleip has said regularly that he hadn't bothered to read the Hoser papers ... which incidentally I believe is true for the majority).

More significantly however, one of the authors, Burbrink has been vocal in his criticism of issues of *Australasian Journal of Herpetology* when published, via his Facebook page news feed, so there is absolutely no doubt that he was well aware of Candoiidae Hoser, 2013.

Or alternatively one must ask, how is it possible for so-called scientists to overlook the name Candoiidae Hoser, 2013 when it is listed in both *Zoological Record* and the ICZN's own repository

Zoobank.

Note further that the paper Pyron *et al.* (2014) was even listed by them in Zoobank, so surely you'd have expected them, the editors or the publishers to make the obvious name priority check there!

Talking PRINO, this gets us to the editors at *Zootaxa* as well.

Even if one were to accept the impossible scenario of ignorance by Pyron *et al.* (2014) of Candoiidae Hoser, 2013, this does not exclude the editors of *Zootaxa* and the supposed peer reviewers.

To believe that none were either aware of Candoiidae Hoser, 2013 or failed to make a cursory check for the previous designation of the family is simply not tenable and merely reinforces the status of *Zootaxa* as a predatory PRINO journal.

DEEPER PROBLEMS WITH SO-CALLED SCIENTISTS

But even when ignoring the reckless overwriting of a name authority for family Candoiidae Hoser, 2013, the papers by Python *et al.* (2013) and (2014) yield an even more disturbing picture in terms of quality of science, scientific publication and alleged peer review at scientific journals.

It goes to the ethics of so-called scientists and the very right of the authors of the relevant paper to hold either an undergraduate or graduate science degree.

BMC Evolutionary Biology is supposedly a peer reviewed scientific journal.

Found online at:

<http://www.biomedcentral.com/bmcevolbiol>

the journal's sales pitch (as of 8 August 2014) writes the following:

"BMC Evolutionary Biology is an open access, peer-reviewed journal that considers articles on all aspects of molecular and non-molecular evolution of all organisms, as well as phylogenetics and palaeontology.

BMC Evolutionary Biology is part of the BMC series which publishes subject-specific journals focused on the needs of individual research communities across all areas of biology and medicine. We offer an efficient, fair and friendly peer review service, and are committed to publishing all sound science, provided that there is some advance in knowledge presented by the work.

BMC series - open, inclusive and trusted."

However based on what Pyron *et al.* (2014) said (above), it is clear that the peer review process failed in that *BMC Evolutionary Biology* published in 2013 an alleged description of a boa subfamily that did not even comply with the most basic formal requirements of the Zoological Code!

In this case we have a failure of peer review, but I note also of no irrefutable evidence of PRINO beyond an indication of this.

More alarming is that the three authors of the paper Pyron *et al.* (2013) are university tenured scientists with PHD's in the zoological sciences and taxonomy and in receipt of very large government grants!

In spite of this very relevant fact, not one of them was able to get a very simple paper and description into order to have their subfamily description compliant with the minimal standards of the Zoological Code.

That such incompetence can exist among so many so-called scientists is outrageous.

However it is not unusual in this age of internet and access to anything online.

Students can now sail through undergraduate degrees by outsourcing assignments and the like to willing people in places like India and graduate with little if any knowledge of the subjects they can now claim formal qualifications for.

PHD's can be gained in much the same way, by plagiarising work of others, or reworking the data of others with a new computer program downloaded for free from a server somewhere.

In summary we have a situation of so-called scientists with degrees obtained in circumstances that can only be described as questionable, who clearly are not properly qualified to write and publish even the most basic of scientific papers.

Pyron *et al.* (2014), have in fact indicted Pyron *et al.* (2013). They did this by stating that the authors of the first paper (2 out of 3 the same people) were not competent to publish a code-compliant scientific description of a taxon group in a scientific journal.

Richard Wells said that the trio who authored Pyron *et al.* (2013) should be asked to hand back their degrees. However because they receive large dollops of taxpayer's funds on the basis of their possession of these degrees and have regular holidays at exotic locations around the world at the largesse of the US taxpayer, this certainly won't be happening in a hurry.

However in the interim, people should be warned not to be so trusting of those who may claim academic qualifications and expertise.

Publications should instead be judged solely on content and not who wrote them or where they have been published.

REFERENCES CITED

Anonymous (editor of BZN) 1988. Comments: Three works by Richard W. Wells and C. Ross Wellington: proposed suppression for nomenclatural purposes. *Bulletin of Zoological Nomenclature* 45(2):153.

- Aplin, K. 1999. Amateur taxonomy in Australian herpetology: Help or hindrance? *Monitor: Journal of the Victorian Herpetological Society* 10(2/3):104-109.
- ASH (Australian Society of Herpetologists) 2013. Minutes of the 37th AGM of the Australian Society of Herpetologists Inc. 8 pp. Online at: <http://www.australiansocietyofherpetologists.org/>
- Baig *et al.* 2012. A morphology-based taxonomic revision of *Laudakia* Gray, 1845 (Squamata: Agamidae). *Vertebrate Zoology* 62(2):213-260 (6 July 2012) (Received by *Zoological Record* on 29 November 2012).
- Barker, D. G. and Barker, T. M. 2004. *Pythons of the World: Australia*. AVS:USA.
- Barker, D. G. and Barker, T. M. 2010. Review: Giant Constrictors: Biological and Management Profiles and an Establishment Risk Assessment for Nine Large Species of Pythons, Anacondas, and the Boa Constrictor by Robert N. Reed and Gordon H. Rodda 2009. U.S. Geological Survey Open-File Report 2009-1202, xviii + 302 pp. *Bulletin of the Chicago Herpetological Society* 45(1) unpaginated and posted online at: http://vpi.com/sites/default/files/ReedRodda_Review_BarkerBarker2010_4.pdf as of 1 May 2015.
- Barker, D. G. and Barker, T. M. 2010. A Critique of the Analysis Used to Predict the Climate Space of the Burmese Python in the United States by Rodda *et al.* (2008, 2009) and Reed and Rodda (2009). *Bulletin of the Chicago Herpetological Society* 45(6):97-10.
- Barker, D. G. and Barker, T. M. 2012. A Discussion of Two Methods of Modeling Suitable Climate for the Burmese Python, *Python bivittatus*, with Comments on Rodda, Jarnevič and Reed (2011). *Bulletin of the Chicago Herpetological Society* 47(6):69-76.
- Barker, D. G. and Barker, T. M. 2013. 04/05/13 CALL FOR DATA!!! Pythons of Indonesia, Philippines, Papua New Guinea, and Continental Asia. Post at: <http://vpi.com/2013/04/05/040513-call-data-pythons-indonesia-philippines-papua-new-guinea-and-continental-asia>
- Barker, D. G., Barten, S. L., Ehrt, J. P. and Daddono, L. 2012. The Corrected Lengths of Two Well-known Giant Pythons and the Establishment of a New Maximum Length Record for Burmese Pythons, *Python bivittatus*. *Bulletin of the Chicago Herpetological Society* 47(1):1-6. online as of 1 May 2015 at: [http://www.chicagoherp.org/bulletin/47\(1\).pdf](http://www.chicagoherp.org/bulletin/47(1).pdf)
- Barker, D. G., Barker, T. M., Davis, M. A. and Schuett, G. W. 2015. A review of the systematics and taxonomy of Pythonidae: an ancient serpent lineage. *Zoological Journal of the Linnean Society*, 2015. Online:19 pp.
- Bates, M. F. *et al.* 2013. A molecular phylogeny of the African plated lizards, genus *Gerrhosaurus* Wiegmann, 1828 (Squamata: Gerrhosauridae), with the description of two new genera. *Zootaxa* 3750 (5): 465-493 (23 December 2013).
- Bennett, D. 2014. A Dubious Account of Breeding *Varanus olivaceus* in Captivity at the Paradise Reptile Zoo in Mindoro, Philippines. *Biawak*, 8(1):12-14.
- Bohannon, J. 2013. Who's afraid of peer review? *Science* 342:60-64.
- Böhme, W. and Ziegler, T. 1988. Comments on the proposed specific name of *Varanus teriae* Sprackland, 1991 (Reptilia, Squamata). *Bulletin of Zoological Nomenclature* 55(2):111-113.
- Branch, B. 2013. Post at <https://www.facebook.com/groups/Arabianherps/> on 26 October 10.15 PM.
- Cogger, H. G. 2013. Email to Raymond Hoser, 17 August 2013.
- Cogger, H. G. 2014a. *Reptiles and Amphibians of Australia*, Seventh Edition, CSIRO Publishing, Sydney, NSW, Australia.
- Cogger, H. G. 2014b. Email to Raymond Hoser, 5 July.
- Coritz, A. 2008. Youtube video placed online at: <http://au.youtube.com/watch?v=QzgluS-tIKc> (Downloaded 1 Dec 2008).
- De Lang, R. 2011. *The Snakes of the Lesser Sunda Islands (Nusa Tenggara), Indonesia: A Field Guide to the Terrestrial and Semi-aquatic Snakes with Identification Key*. Edition Chimaira, Frankfurt:359 pp.
- Dubois, A. 2005. Proposed Rules for the incorporation of nomina of higher-ranked zoological taxa in the International Code of Zoological Nomenclature. 1. Some general questions, concepts and terms of biological nomenclature. *Zoosystema* 27(2):365-426.
- Dubois, A. 2014. Email to Raymond Hoser, 14 May.
- Eipper, S. 2012. *A guide to Australian snakes in captivity: Elapids and Colubrids*. Reptile Publications, Burleigh, Queensland, Australia:280 pp.
- Eipper, S. 2013. Post on Facebook 16 December 2013.
- Folt, B. and Guyer, C. 2015. Evaluating recent taxonomic changes for alligator snapping turtles (Testudines: Chelydridae). *Zootaxa* 3947(3):447-450.
- Fry, B. G. 2009. Re: Yikes, yikes, yikes, Naja nuked! Post on 29 March at: <http://>

www.venomousreptiles.org/forums/Experts/42293?page=2

Grismer, L. 2011. *Amphibians and reptiles of the Seribu Archipelago. Peninsula Malaysia*. Edition Chimaira, Germany:239 pp.

Harvey, M. S. and Yanega, D. A. 2013. Call for comments: Taxonomic practice and the code. *Bulletin of Zoological Nomenclature* 70(4):216-217.

Hedges, S. B., Marion, A. B., Lipp, K. M., Marin, J. and Vidal, N. 2014. A taxonomic framework for typhlopoid snakes from the Caribbean and other regions (Reptilia, Squamata). *Caribbean Herpetology* 49:1-61.

Hoser, R. T. 1980. Further records of aggregations of various species of Australian Snake, *Herpetofauna*, 12(1):16-22.

Hoser, R. T. 2000. A revision of the Australasian Pythons. *Ophidia Review* 1:7-27.

Hoser, R. T. 2001. *Pailsus* - a story of herpetology, science, politics, pseudoscience, more politics and scientific fraud. *Crocodylian* 2(10):4-31.

Hoser, R. T. 2004. A reclassification of the pythoninae including the descriptions of two new genera, two new species and nine new subspecies. *Crocodylian* 4(3):(November 2003):31-37 and 4(4):(June 2004):21-40.

Hoser, R. T. 2009a. Creationism and contrived science: A review of recent python systematics papers and the resolution of issues of taxonomy and nomenclature. *Australasian Journal of Herpetology* 2:1-34. (3 February).

Hoser, R. T. 2009b. A reclassification of the True Cobras; species formerly referred to the genera *Naja*, *Boulengerina* and *Paranaja*. *Australasian Journal of Herpetology* 7 (2009):1-15. (23 March).

Hoser, R. T. 2012a. Exposing a fraud! *Afronaja* Wallach, Wüster and Broadley 2009, is a junior synonym of *Spracklandus* Hoser 2009! *Australasian Journal of Herpetology* 9 (3 April 2012):1-64.

Hoser, R. T. 2012b. A five-way division of the agamid genus *Laudakia* Gray, 1845 (Squamata: Sauria: Agamidae). *Australasian Journal of Herpetology* 14:17-23.

Hoser, R. T. 2012c. 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, R. T. 2012d. A review of the extant scolecophidians ("blindsnakes") including the formal naming and diagnosis of new tribes, genera, subgenera, species and subspecies for divergent taxa. *Australasian Journal of Herpetology* 15:1-64.

Hoser, R. T. 2012e. A reassessment of the higher

taxonomy of the elapidae. *Australasian Journal of Herpetology* 10:49-63.

Hoser, R. T. 2013a. An updated taxonomy of the living Alligator Snapping Turtles (*Macrochelys* Gray, 1856), with descriptions of a new tribe, new species and new subspecies. *Australasian Journal of Herpetology* 16:53-63.

Hoser, R. T. 2013b. The science of herpetology is built on evidence, ethics, quality publications and strict compliance with the rules of nomenclature. *Australasian Journal of Herpetology* 18:2-79.

Hoser, R. T. 2013c. A revised taxonomy for the lizard families Gerrhosauridae and Cordylidae. *Australasian Journal of Herpetology* 21:2-32.

Hoser, R. T. 2013d. Tidying up the taxonomy of the extant Booidea, including the erection and naming of two new families, the description of *Acrantophis sloppi* sp. nov., a new species of Ground Boa from Madagascar and *Candoia aspera iansimpsoni*, subsp. nov., a new subspecies of Boa from Papua New Guinea. *Australasian Journal of Herpetology* 16:3-8.

Hoser, R. T. 2009-2014. All papers within Issues 1-24. Issues 1-24 *Australasian Journal of Herpetology*. Kotabi Pty Ltd: Doncaster, Victoria, Australia.

Hoser, R. T. 2015a. Dealing with the "truth haters" ... a summary! Introduction to Issues 25 and 26 of *Australasian Journal of Herpetology*. Including "A timeline of relevant key publishing and other events relevant to Wolfgang Wüster and his gang of thieves." and a "Synonyms list". *Australasian Journal of Herpetology* 25:3-13.

Hoser, R. T. 2015b. The Wüster gang and their proposed "Taxon Filter": How they are knowingly publishing false information, recklessly engaging in taxonomic vandalism and directly attacking the rules and stability of zoological nomenclature. *Australasian Journal of Herpetology* 25:14-38.

Hoser, R. T. 2015c. Best Practices in herpetology: Hinrich Kaiser's claims are unsubstantiated. *Australasian Journal of Herpetology* 25:39-52.

Hoser, R. T. 2015d. Comments on *Spracklandus* Hoser, 2009 (Reptilia, Serpentes, ELAPIDAE): request for confirmation of the availability of the generic name and for the nomenclatural validation of the journal in which it was published (Case 3601; see *BZN* 70: 234-237; comments *BZN* 71:30-38, 133-135). (unedited version) *Australasian Journal of Herpetology* 27:37-42.

Hoser, R. T. 2015e. Rhodin *et al.* 2015, Yet more lies, misrepresentations and falsehoods by a band of thieves intent on stealing credit for the scientific works of others. *Australasian Journal of Herpetology* 27:3-64.

Hoser 2015 - Australasian Journal of Herpetology 26:3-64.

- Hoser, R. T. *et al.* 2014. Snakebusters Australian Snakeman facebook page at: <https://www.facebook.com/pages/Snakebusters-Australian-Snakeman/112806842145321> (downloaded 10 July).
- Hoser, R. T. *et al.* 2015. Snakebusters Australian Snakeman facebook page at: <https://www.facebook.com/pages/Snakebusters-Australian-Snakeman/112806842145321> (downloaded 1 May).
- Hunter, S. *et al.* 2006. Online petition to shut down Raymond Hoser, published at: <http://www.aussiereptileclassifieds.com/phpPETITION>
- ICZN 1991. Case 2531, Decision of the Commission: Three works by Richard W. Wells and C. Ross Wellington: proposed suppression for nomenclatural purposes. *Bulletin of Zoological Nomenclature* 48(4):337-338.
- ICZN 2001. Opinion 1970 *Odatia keithhornei* Wells & Wellington, 1985 (Reptilia, Squamata): specific name placed on the Official List. *Bulletin of Zoological Nomenclature* 58(1):74.
- ICZN 2013. Case 3601. *Spracklandus* Hoser, 2009 (Reptilia, Serpentes, ELAPIDAE): request for confirmation of the availability of the generic name and for the nomenclatural validation of the journal in which it was published. *Bulletin of Zoological Nomenclature* 70(4):234-237.
- Kaiser, H. 2012a. SPAM email sent out to numerous recipients on 5 June 2012.
- Kaiser, H. 2012b. (or Kaiser *et al.* 2012) Point of view. Hate article sent as attachment with SPAM email sent out on 5 June 2012.
- Kaiser, H. 2013. The Taxon Filter, a novel mechanism designed to facilitate the relationship between taxonomy and nomenclature, vis-à-vis the utility of the Code's Article 81 (the Commission's plenary power). *Bulletin of Zoological Nomenclature* 70(4) December 2013:293-302.
- Kaiser, H. 2014a. Comments on *Spracklandus* Hoser, 2009 (Reptilia, Serpentes, ELAPIDAE): request for confirmation of the availability of the generic name and for the nomenclatural validation of the journal in which it was published. *Bulletin of Zoological Nomenclature*, 71(1):30-35.
- Kaiser H. 2014b. Best Practices in Herpetological Taxonomy: Errata and Addenda. *Herpetological Review*, 45(2):257-268.
- Kaiser, H., Crother, B. L., Kelly, C. M. R., Luiselli, L., O'Shea, M., Ota, H., Passos, P., Schleip, W. D. and Wüster, W. 2013. 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.
- King, M. 1988. Comments on the proposed suppression for nomenclature of three works by R. W. Wells and C. R. Wellington (Case 2531: see BZN 44: 116-121; 257-261 and 45: 52-54). *Bulletin of Zoological Nomenclature* 45(2):150-151.
- Lutz, M. 2006. Der Butaan (*Varanus olivaceus*), Hallowell 1856, Haltung und erste erfolgreiche Nachzucht im Terrarium. *Sauria* 28(4):5-13.
- Moler, P. 2014. Email to Raymond Hoser, 11 April.
- Mutton, N. 2014a. Private email (via Facebook) to Raymond Hoser. 6.31 AM, 30 May.
- Mutton, N. 2014b. Two emails to Raymond Hoser, 9 June.
- O'Shea, M. 2013a. Amazon snake mystery. Web page article posted at: http://www.markoshea.info/oba4-4_peru03.php downloaded 9 December 2013.
- Natusch, D. J. S. and Lyons, J. A. 2012. Ecological attributes and trade of white-lipped pythons (Genus *Leiopython*) in Indonesian New Guinea. *Australian Journal of Zoology* 59(5):339-343. <http://dx.doi.org/10.1071/ZO12017> 16 May.
- Newman, C. 2000. Editorial. *Ophidia Review*, C-View publishing, UK:3.
- O'Shea, M. 2013b. Facebook post on wall of Wulf Schleip dated 25 March.
- Pyrone, R. A. and Wallach, V. 2014. Systematics of the blindsnakes (Serpentes: Scolecophidia: Typhlopoidea) based on molecular and morphological evidence. *Zootaxa* 3829 (1):1-81.
- Pyrone, R. A., Burbrink, F. T., Colli, G. R., de Oca, A. N., Vitt, L. J., Kuczynski, C. A. and Weins, J. J. 2011. The phylogeny of advanced snakes (Colubroidea), with discovery of a new subfamily and comparison of support methods for likelihood trees. *Molecular Phylogenetics and Evolution* 58:329-342.
- Pyrone, R. A., Burbrink, F. T. and Wiens, J. J. 2013. A phylogeny and revised classification of Squamata, including 4151 species of lizards and snakes. *BMC Evolutionary Biology* 13:93. [doi:10.1186/1471-2148-13-93]. (Version 1, published 1 May 2013, and version 2 published on 23 September 2013, both published only online).
- Pyrone, R. A., Reynolds, R. G. and Burbrink, F. T. 2014. A Taxonomic Revision of Boas (Serpentes: Boidae). *Zootaxa* 3846 (2): 249-260 (online).
- Raw, L. 2014. Email to Raymond Hoser, 5 July.
- Rawlings, L. H., Rabosky, D., Donnellan, S. C. and Hutchinson, M. N. 2008. Python phylogenetics: inference from morphology and mitochondrial DNA. *Biological Journal of the Linnean Society* 93(3): 603-619.
- Reynolds, R. G., Niemiller, M. L. and Revella, L. J.

- 2013a. Toward a Tree-of-Life for the boas and pythons: Multilocus species-level phylogeny with unprecedented taxon sampling. *Molecular Phylogenetics and Evolution*, Uncorrected proof uploaded on 6 December 2013 to <http://www.sciencedirect.com/science/article/pii/S1055790313004284>
- Reynolds, R. G., Niemiller, M. L. and Revella, L. J. 2013b. Toward a Tree-of-Life for the boas and pythons: Multilocus species-level phylogeny with unprecedented taxon sampling. *Molecular Phylogenetics and Evolution*, Uncorrected proof uploaded on 6 December 2013 to http://www.venomdoc.com/downloads/MPE_pythons.pdf
- Reynolds, R. G., Niemiller, M. L. and Revella, L. J. 2014. Toward a Tree-of-Life for the boas and pythons: Multilocus species-level phylogeny with unprecedented taxon sampling. *Molecular Phylogenetics and Evolution*, 71:201-203 (posted online at: <http://www.sciencedirect.com/science/article/pii/S1055790313004284>)
- Rhodin, A. *et al.* 2015. Comment on *Spracklandus* Hoser, 2009 (Reptilia, Serpentes, ELAPIDAE): request for confirmation of the availability of the generic name and for the nomenclatural validation of the journal in which it was published (Case 3601; see *BZN* 70: 234-237; 71: 30-38, 133-135, 181-182, 252-253). *Bulletin of Zoological Nomenclature* 72(1):65-78.
- Ride, W. D. L. (ed.) *et al.* (on behalf of the International Commission on Zoological Nomenclature) 1999. *International code of Zoological Nomenclature*. The Natural History Museum - Cromwell Road, London SW7 5BD, UK (also commonly cited as "ICZN 1999").
- Rowley, J. 2014. Over 100 cases of beer drunk in 3 nights at the Australian Society of Herpetologists meeting! & talks were amazing! Twitter post, Jodi Rowley, 31 January.
- Schleip, W. D. 2000. taxon of australian pythons ?! Post on yahoo groups on 25 June at: <https://groups.yahoo.com/neo/groups/australianherps/conversations/topics/5075>
- Schleip, W. D. 2002. Webpage at: http://www.herpbreeder.com/pages/sheet_specie.php?id_reg=653&id_tax=240&id_specie=476&upd_filter=1
- Schleip, W. D. 2003a. Forum post on 4 September at: <http://forums.kingsnake.com/view.php?id=157725,157725>
- Schleip, W. D. 2003b. Forum post on 24 November at: <http://forums.kingsnake.com/view.php?id=255955,256748>
- Schleip, W. D. 2003c. Forum post on 2 December at: <http://www.venomdoc.com/forums/viewtopic.php?t=199>
- Schleip, W. D. 2004. Post on www.kingsnake.com dated 20 August at: <http://forums.kingsnake.com/view.php?id=546835,548880>
- Schleip, W. D. 2008. Revision of the Genus *Leiopython* Hubrecht 1879 (Serpentes: Pythonidae) with the Redescription of Taxa Recently Described by Hoser (2000) and the Description of New Species. *Journal of Herpetology* 42(4):645-667.
- Schleip, W. 2014a. Comments on *Spracklandus* Hoser, 2009 (Reptilia, Serpentes, ELAPIDAE): request for confirmation of the availability of the generic name and for the nomenclatural validation of the journal in which it was published (Case 3601; see *BZN* 70: 234-237). *Bulletin of Zoological Nomenclature* 7(1):37-38.
- Schleip, W. D. 2014b. Two new species of *Leiopython* Hubecht (sic), 1879 (Pythonidae: Serpentes): Non-compliance with the International Code of Zoological Nomenclature leads to unavailable names in zoological nomenclature. *Journal of Herpetology* 48(2):272-275.
- Schleip, W. D. 2014c. Post on Taxacom list server on 5 July at: edu.ku.nhm.mailman.taxacom
- Schleip, W. D. and O'Shea, M. 2010. Annotated checklist of the recent and extinct pythons (Serpentes, Pythonidae), with notes on nomenclature, taxonomy, and distribution. *ZooKeys* 66:29-79.
- Shea, G. 2013a. Email to Raymond Hoser dated Fri, 8 Mar 2013 04:29:39 +0000.
- Shea, G. 2013b. Post on facebook at: <http://www.facebook.com/glenn.shea.73?ref=ts&fref=ts> on 8 March at 7.51 AM.
- Shea, G. 2013c. Post on facebook on 20 March at: <http://www.facebook.com/glenn.shea.73?ref=ts&fref=ts#!/bryangrieg.fry?fref=ts>
- Shea, G. 2013d. Post on facebook on 20 March at: <http://www.facebook.com/glenn.shea.73?ref=ts&fref=ts#!/bryangrieg.fry?fref=ts>
- Shea, G. 2014a. Goodbye *Niveoscincus* and Hello *Carinascincus*...Cogger (2014). Facebook post on wall of Richard Wells, 7 March.
- Shea, G. 2014b. Email to Raymond Hoser, 5 July.
- Stone, J. 1988. Comments on the proposed suppression for nomenclature of three works by R. W. Wells and C. R. Wellington (Case 2531: see *BZN* 44: 116-121 and 257-261). *Bulletin of Zoological Nomenclature* 45(1):53-54.
- Supreme Court of Victoria - Court of Appeal Victoria, Victoria 2012. Transcript and Judgement, Raymond

Hoser versus the Department of Sustainability and Environment, Victoria. 8 June.

Supreme Court of Victoria - Court of Appeal Victoria 2014. Judgement, Raymond Hoser versus the Department of Sustainability and Environment, Victoria. 5 September.

The president, Australian Society of Herpetologists 1987. Three works by Richard W. Wells and C. Ross Wellington: proposed suppression for nomenclatural purposes. *Bulletin of Zoological Nomenclature* 46(2):116-122.

Thomas, T. M., Granatosky, M. C., Bourque, J. R., Krysko, K. L., Moler, P. E., Gambel, T., Suarez, E., Enge, K. M. and Roman, J. 2014. Taxonomic assessment of Alligator Snapping Turtles (Chelydridae: *Macrochelys*), with the description of two new species from the southeastern United States. *Zootaxa* 3786(2):141-165.

Thomson, S. 2003. Post at: <http://forums.kingsnake.com/view.php?id=279134,292763> dated Dec 29 03:59:46 downloaded 1 May 2015.

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

Thomson, S. and Georges, A. 2009. *Myuchelys* gen. nov.: a new genus for *Elseya latisternum* and related forms of Australian freshwater turtle (Testudines: Pleurodira: Chelidae). *Zootaxa* 2053:32-42.

Thorpe, S. 2013. Post to the Taxacom listserver, 21 May 2014.

Thorpe, S. 2014a. Post on Taxacom forum dated 13 April.

Thorpe, S. 2014b. Email to ICZN. 29 April.

Thorpe S. 2014c. Email to Doug Yanega and ICZN List. 6 July.

Thorpe, S. 2015. Post on Taxacom list server on 30 April at 01:19.

Twombly, R. 2013. Webpage at: <http://www.iherp.com/Public/ShowUser.aspx?UserId=75ebfdab-bd2b-4261-9146-e479aa261fc8> (downloaded 30 December 2013).

Twombly, R. et al. 2014. Facebook page herpetological taxonomy at <http://www.facebook.com/groups/Herpetofauna.taxonomy> (downloaded 10 July 2014).

Twombly, R. et al. 2015. Facebook page herpetological taxonomy at <http://www.facebook.com/groups/Herpetofauna.taxonomy> (downloaded 1 May 2015).

Uetz, P. 2013a The reptile database, URL at: <http://www.reptile-database.org/db-info/news.html>

Uetz, P. 2013b The reptile database, URL at: <http://reptile-database.reptarium.cz/species?genus=Stellagama&species=stellio> (downloaded on 29 Dec 2013).

Uetz, P. 2014a. Snakes as pets. Url at: http://www.reptile-database.org/db-info/snakes_as_pets.html (downloaded on 29 June).

Uetz, P. 2014b. Comment on taxacom forum on 5 July at: edu.ku.nhm.mailman.taxacom

Uetz, P. 2014c. Webpage at: http://reptile-database.reptarium.cz/pecies?genus=Bothrochilus&species=hoserae&search_param=%28%28search%3D%27bothrochilus%27%29%29 downloaded on 8 August 2014.

Van Aken, G. 2001. Editorial. *Litteratura Serpentina* 21(3):66.

Van Dijk, P. P., Iverson, J. B., Rhodin, A. G. J., Bradley Shaffer, H. and Bour, R. 2014. Turtles of the world, 7th edition: Annotated Checklist of Taxonomy, Synonymy, Distribution with Maps and Conservation Status. Turtle Taxonomy Working Group. Chelonian Research Foundation, 16 June.

Various authors 2013. Wikipedia page at: <http://en.wikipedia.org/wiki/Cryptozoology> downloaded on 24 December 2013.

Various authors 2014. ICZN List archives (January-June 2014), hosted at:

<http://list.afriherp.org/mailman/private/iczn-list/>

Wallach, V., Wüster, W. and Broadley, D. 2009. In praise of subgenera: taxonomic status of Cobras of the genus *Naja* Laurenti (Serpentes: Elapidae). *Zootaxa* 2236:26-36 (online paper).

Wellington, C. R. 2013. Post on Facebook 26 December 2013.

Wellington, C. R. 2014a. Post on Facebook wall of Scott Eipper 6 April.

Wellington, C. R. 2014b. Email to ICZN List and others on 9 July 2014.

Wells, R. W. 2007. Some taxonomic and nomenclatural considerations on the class Reptilia in Australia. A new genus of the family Chelidae from eastern Australia. *Australian Biodiversity Record* (3):1-13.

Wells, R. W. 2013. Post on Facebook dated 18 December.

Wells, R. W. 2014a. Post on Facebook wall of Scott Eipper 6 April.

Wells, R. W. 2014b. Post on blogger.com on 7 July.

Wells, R. W. and Wellington, C. R. 1999. A response to Ken Aplin's article on herpetology in Australia. *Monitor: Journal of the Victorian Herpetological Society* 10(2/3):110-112.

Williams, D. J. 2008. Two posts dated 14 February

2008 at: <http://www.reptileforums.co.uk/snakes/87176-support-new-guinea-snakebite-research-2.html> and also posted at the same time at "aussiepythons.com" and numerous other internet chat forums.

Wüster, W. 2001. Post on Kingsnake.com at: January 22, 2001 at 11:29:07 (same document as Wüster, W., Bush, B., Scott Keogh, J., O'Shea, M. and Shine, R. 2001. Cited below except Wüster was listed here as the sole author).

Wüster, W. 2004. RE: Broghammerus reticulatus or Python Reticulatus???? Post on 14 August at: <http://forums.kingsnake.com/view.php?id=543430,543430>

Wüster, W. 2005. Post on 21 October at: <http://forums.kingsnake.com/view.php?id=927616,928298>

Wüster, W. 2008. Posts at: <http://www.reptileforums.co.uk/snakes/87176-supportnew-guinea-snakebite-research.html>. Dated 20 Jan and later.

Wüster, W. 2009a. Posts on internet chat forum at: <http://www.venomlist.com/forums/index.php?showtopic=24325&st=20>, 29 March 2009.

Wüster, W. 2009b. Post on Venomdoc forum. 20 July.

Wüster, W. 2010. Post on 23 December on Wikipedia talk page at: http://en.wikipedia.org/wiki/Talk:Python_reticulatus

Wüster, W. 2013a. Post at <http://pleiotropy.fieldofscience.com/2009/03/taxonomic-war.html> dated 9 May 2013.

Wüster, W. 2013b. Post at <http://www.fieldherpforum.com/forum/>

viewtopic.php?f=13&t=15823&p=190713 on 27 April, 2013, 9:08 pm.

Wüster, W., Broadley, D. G. and Wallach, V. 2014. Comments on *Spracklandus* Hoser, 2009 (Reptilia, Serpentes, ELAPIDAE): request for confirmation of the availability of the generic name and for the nomenclatural validation of the journal in which it was published (Case 3601; see BZN 70: 234–237). *Bulletin of Zoological Nomenclature* 7(1):37–38.

Wüster, W., Bush, B., Keogh, J. S., O'Shea, M. and Shine, R. 2001. Taxonomic contributions in the "amateur" literature: comments on recent descriptions of new genera and species by Raymond Hoser. *Litteratura Serpentina* 21:67–79, 86–91. (Previously published by Wüster in 2001 as sole author on www.kingsnake.com).

Yanega, D. 2014. Post on Taxacom forum dated 13 April.

Yanega, D. 2015. Post on ICZN List server 16 Jan at: 10:01:01 -0800.

TAX INVOICE
Kwik Kopy Printing Box Hill
3/1031 Whitehorse Road
Box Hill VIC 3128
t: 039 899 0833 f: 039 899 0536
ABN: 65 740 679 782

CONTACT
Australasian Journal of Herpetology

INVOICE DETAILS
Issue Date: 18/05/2015
Ship Via:

QUANTITY	DESCRIPTION	VALUE Ex GST
50	Australasian Journal of Herpetology issue 28 - 64 page document printed black and white on 120gsm gloss and 250gsm gloss cover in colour	\$227.27
50	Australasian Journal of Herpetology issue 28 - 64 page document printed black and white on 120gsm gloss and 250gsm gloss cover in colour	\$227.27
50	Australasian Journal of Herpetology issue 27 - 64 page document printed black and white on 120gsm gloss and 250gsm gloss cover in colour	\$227.27
50	Australasian Journal of Herpetology issue 27 - 64 page document printed black and white on 120gsm gloss and 250gsm gloss cover in colour	\$227.27
	TOTAL	\$999.99

RECEIVED BY:
Terms: Please refer to terms & conditions of trade. Account customers, please pay in full, on pickup. We accept Payment via EFT POS, Mastercard, Visa & Amex. All card transactions will incur a 2% surcharge. You can deposit directly into our bank account. E & O.E. All claims and returned goods are to be accompanied by this invoice, and made within 7 days of receipt of goods.

CUSTOMER: Australasian Journal of Herp
INVOICE No.: 21560
INVOICE Tot.: \$999.99

REMITTANCE
SUBTOTAL \$999.99
GST \$99.99
TOTAL \$999.99
AMOUNT DUE \$999.99
PAYMENT
OTHER:

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.

Details at: <http://www.herp.net>