

A new subspecies of *Daraninagama robinsonii* (Boulenger, 1908) from the Cameron Highlands, Malaysia (Squamata: Sauria: Agamidae) and a critical review of a critical review.

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ABSTRACT

The species *Daraninagama robinsonii* (Boulenger, 1908), known before 2014 as *Gonocephalus robinsonii*, or more recently under the invalid generic name *Malayodracon* Denzer *et al.*, 2015, has until now been treated as a single taxon (Hoser 2014b). However it has long been suspected that the western population is taxonomically distinct from the nominate form.

This paper formalizes that position by naming the new taxon *Daraninagama robinsonii cliveevatti* subsp. nov. on the basis of different morphology and an apparently disjunct distribution.

Also addressed is a series of highly defamatory lies and gross misrepresentations conducted in a very unscientific manner in a paper by Denzer *et al.* (2016). Presented in a form that breaches of all established rules of ethics and scientific methods, Denzer *et al.* (2016) is used as a pretext to justify existing and planned illegal acts of taxonomic vandalism by these authors and fellow members of the so-called Wüster gang.

The group seeks to act outside the rules of the ICZN and usurp the authority of the ICZN.

Alternatively they seek to hijack the ICZN in order to carry on their nefarious agenda of unscientific taxonomic and nomenclatural hegemony as stated in Rhodin *et al.* (2015).

Keywords: Taxonomy; Lizards; nomenclature; Hoser; Manthey; Denzer; Kaiser; Wüster; plagiarization; fraud; theft; illegal act; new genus; *Daraninagama*; 2014; synonym; *Malayodracon*; 2015; new subspecies; *cliveevatti*; PRINO; peer reviewed in name only; journals; ICZN; *International Code of Zoological Nomenclature*; taxonomic vandalism; priority; homonymy; name authority; data mining.

INTRODUCTION

Hoser (2014b) divided the Asian lizard family Draconinae into new and existing genera based on obvious phylogenetic relationships and morphology of species. Among the new genera erected was *Daraninagama* Hoser, 2014 to accommodate the divergent species *Gonocephalus robinsonii* Boulenger, 1908. The generic placement of the species into the new genus *Daraninagama* had a firm basis of evidence, including phylogenetic and morphological as cited in the paper of Hoser (2014b) and does not need to be repeated here, noting that Hoser (2014b) has been online since a month after publication and distribution is not constrained by any form of paywall.

While until now the genus *Daraninagama* has been treated as monotypic, the purpose of this paper is to formally name a western population as a new subspecies, as explained below.

That description also contains the most obvious morphological differences separating both forms.

In 2015, Denzer *et al.* published a paper largely rehashing the materials and sources cited by Hoser (2014b), and while ignoring the Hoser paper, they chose to engage in an act of taxonomic vandalism by remanufacturing this data as "new" and then illegally coining a new genus name "*Malayodracon*" for the

same taxon. The *International Code of Zoological Nomenclature* (Ride *et al.* 1999), which is a legal document, expressly forbids the reckless coining of names for taxa that have already been properly named via the rules of homonymy and priority.

As the coined name "*Malayodracon*" is a junior synonym of the legally correct *Daraninagama* Hoser, 2014, "*Malayodracon*" should never be used, except for the purpose of wasting space in synonyms lists.

The same individuals (Denzer *et al.* 2016) have also been recently (2015) closely associated with members of the so-called Wüster gang in a campaign to dishonestly steal the works of others to illegally rename taxa in direct breach and contempt of the rules of the *International Code of Zoological Nomenclature* (Ride *et al.* 1999).

They have detailed their plans in their manifesto known as Kaiser *et al.* (2013), but perhaps more appropriately known as and called "Wüster 2013", because Kaiser had earlier identified him as the lead author. The same evil plot is detailed in the documents Kaiser (2012a, 2012b, 2013, 2014a and 2014b) as well as Rhodin *et al.* (2015) and countless hate posts on sites as diverse as "Twitter", "Facebook" and many "Wikipedia" pages, which they protect with so-called robots to prevent people

correcting the obvious lies contained on the pages, as would otherwise happen on "Wikipedia" pages.

AN OSTENSIBLY CRITICAL REVIEW THAT IS NOTHING MORE THAN A COLLECTION OF LIES TO JUSTIFY ETHICALLY REPUGNANT ACTIONS.

For Denzer *et al.* (2015) to attempt to justify their overt act of attempted theft of name authority, via their improper act of trying to overwrite the legal name *Daraninagama* with their illegally coined name *Malaydracon*, three of the four authors of Denzer *et al.* (2015) and another (Philipp Wagner), did with the stated assistance of one Hinrich Kaiser, publish their justification in a so-called paper, known as Denzer *et al.* (2016).

It was titled "A critical review of Hoser's writings on draconinae, Amphibolurinae, *Laudakia* and Uromastycinae (Squamata: Agamidae)" and published in the ostensibly "peer reviewed" *Bonn Zoological Bulletin*.

The lead authors gave their institutional affiliation as being with the "Society for Southeast Asian Herpetology".

However the only evidence of any such entity on the internet as of July 2016 was the very paper I am dealing with here and three others by the same group of authors who similarly gave such address details.

There is no evidence of any functioning "Society for Southeast Asian Herpetology" in any accepted sense of the term "Society", (e.g. newsletter, bulletin, journal, society meetings or the like) which coincidentally reflects the absence of evidence of credible peer review for the paper they published as well.

Their paper commenced as follows:

"Abstract. We analyzed four papers on agamid lizards by self-proclaimed Australian herpetologist Raymond Hoser with respect to the presentation of diagnostic characters as well as their taxonomic and nomenclatural merits. In most cases the taxonomic concepts were lifted from earlier phylogenetic publications and the diagnoses were copied from other authors.

Copied text in Hoser's diagnostic section within the analyzed papers amounts to a staggering 83% for Draconinae, 82% for Amphibolurinae, 77% for *Laudakia* and 78% for Uromastycinae, respectively. We found a number of plagiarized paragraphs, sometimes half a page long. Hoser hardly ever makes any effort to attribute statements to the original author and in some cases he even omitted to cite the relevant source. With respect to nomenclature, we found that Hoser proposed names that were preoccupied or unavailable, that a nomen oblitum was resurrected incorrectly, nomina nuda were produced, a type locality was restricted incorrectly and a questionable holotype was designated for a new species.

With respect to taxonomy, we found examples of wrong diagnoses, falsely attributed species, omission of taxa and a lack of understanding or misinterpretation of previously published taxonomic studies on agamid lizards. Furthermore relevant literature on taxonomy and nomenclature has been overlooked or disregarded.

Key words. Plagiarism, IZCN rules, nomina nuda, questionable type specimen designation, ambiguous diagnoses."

However a cross referencing of the specific claims made against the Hoser papers invariably found that all were wrong or unsupported from the original sources; these being the four Hoser papers.

Many of the claims against the Hoser papers were also demonstrably false, or alternatively almost always misleading or out of context and the so-called method of determining amounts of text lifted from other papers was fundamentally flawed and therefore as represented was completely false and misleading and of absolutely no value whatsoever.

The alleged copying of text by percentages as alleged as analysed was merely confirmation of the diagnostic features of given taxa, which as a matter of course would not significantly change, no matter which author wrote about them; this simple

observation confirming the apparent similarities between diagnoses between the Hoser papers and those that preceded them (which were all cited in the proper way!).

There are dozens of examples of false and misleading claims in the Denzer *et al.* (2016) paper, many of which are repeated at various points in the nearly 20,000 word diatribe.

It is clear that as with Kaiser *et al.* (2013) and incarnations before and since (already cited), that Denzer *et al.* (2016) have run with the mantra that a lie repeated often enough will eventually be believed by a majority of people.

The entire paper of Denzer *et al.* (2016) is replete with lies and misinformation, all easily shown as such by simple cross-reference with the complained about Hoser papers, so it is strictly speaking not even necessary for me to give credibility to the rant by systematically refuting each and every claim herein.

However I mention a few of these false and misleading statements here to give an example of the unscientific claims made by the authors and the tenor of what they wrote.

At page 135 under the heading "Conflict of interest", they complained that they had not been consulted before I cited their works in my papers. The comment was both ridiculous and hypocritical. Firstly there is no legal or scientific requirement for a publishing scientist to contact the authors of papers they cite. In the case of deceased authors this would be impossible in any event! I also need not mention that the first I became aware of the Denzer *et al.* (2016) rant was when it was SPAM posted across "Facebook", "Twitter" and elsewhere on the internet and not because any of the authors had the decency to contact me or ask for my opinion of their demonstrably false claims, which is standard practice for authors who seek to publish adverse claims against others.

One of many claims of (alleged) plagiarism by myself was written thus:

"The taxonomic basis for Hoser's proposals on *Laudakia* can be found in their entirety in Macey *et al.* (1998, 2000b, 2006). Most of Hoser's proposed classification additionally reflects nodes in the phylogeny published by Pyron *et al.* (2013).", leading to the claim I had plagiarized Pyron's work because I did not cite that paper.

The problem with this is that the *Laudakia* paper subject of the criticism was published on 30 June 2012 (Hoser 2012a) and receipted by Museums, *Zoological Record* and others at the time, whereas Pyron's paper was published on 29 April 2013, or nearly a year later!

Now unless I am able to engage in such things as reading someone's mind a year hence and from the far side of the planet, it would not have been possible for me to plagiarize the works of Pyron!

This glaring evidence is just one of many such examples as to why the journal that Denzer *et al.* (2016) was published in, namely the *Bonn Zoological Bulletin* is either not "peer reviewed" or otherwise "PRINO" (peer reviewed in name only) as defined by Hoser (2015e).

That my taxonomic proposals had a basis from earlier works is not a crime either. That is provided I had properly cited them and credited the relevant authors and their works. This was done, with the three papers Macey *et al.* (1998, 2000b, 2006) and others by the same authors cited in the text of the paper and at the end in full as per standard scientific procedure.

Denzer *et al.* (2016) wrote: "Plagiarism is generally defined as passing off ideas or text from other publications as one's own", which is something I agree with, but when one actually cross checks my papers with their own claims against them, each and every claim of plagiarism fails!

Interestingly in their criticism of my Draconinae reclassification Denzer *et al.* (2016) at page 126 allege I cited too many sources!

Then there is the associated claim from the abstract of Denzer *et al.* (2016) and repeated throughout the rant "In most cases

the taxonomic concepts were lifted from earlier phylogenetic publications and the diagnoses were copied from other authors." Fact is that there is nothing wrong with either activity!

This is provided the original sources were properly acknowledged and cited, as was the case in each of the Hoser papers referred to by Denzer *et al.* (2016), namely Hoser (2012b, 2012c, 2013b, 2014a, 2014b, 2014c and 2015).

However in the roughly 20,000 word rant by Denzer *et al.* (2016), the authors failed to explicitly state the single obvious difference between the relevant Hoser papers and those earlier papers from where the Hoser papers had "lifted" data. This was that the Hoser papers assigned valid names according to the *International Code of Zoological Nomenclature* to previously unnamed clades.

This has been standard practice in Zoology for years and in the case of the relevant earlier papers, the clear error of failing to name unnamed clades was picked up and corrected in the Hoser papers!

Rhodin *et al.* (2015) made a similar complaint that I has been able to publish my papers and resulting descriptions of new taxa by the scientific method they called "data mining", which I note is not illegal and in the context alleged, eminently sensible!

Now in terms of any theft claims, the *International Code of Zoological Nomenclature* makes it clear what is deemed ethical and what is not. The time limit of a year is set on authors seeking to monopolize given taxa for making themselves "name authority" in terms of publishing a formal code-compliant description.

As Denzer *et al.* (2016), alleged in terms of the Hoser *Laudakia* paper, they said the data that formed the basis of the phylogenetic arrangement "can be found in their entirety in Macey *et al.* (1998, 2000b, 2006)."

The last of this trio pre-dates the Hoser paper by no less than 6 years meaning that any alleged "right to name" the relevant generic groups by these authors expired five years earlier!

In other words, far from stealing the work of others, Hoser (2012a) has ethically and properly corrected a series of mistakes made in earlier papers, these being assigning one or more names to unnamed clades.

Denzer *et al.* (2016) is replete with statements that assume fact and are instead simply false or derogatory, examples of which include the following:

1/ Use of the term "self-proclaimed Australian herpetologist Raymond Hoser" is derogatory and lacks explanation. However the claim I am a "self-proclaimed Australian herpetologist" has been refuted by no less than ten Victorian Judges in legal proceedings spanning the past decade, including for example the three judges who in 2014, found I was by measurable criteria, easily Australia's leading reptile expert (Court of Appeal, 2014); or the same result in VCAT (2015), the relevant judgements of which are widely published on Australian government websites.

It is significant that in both cases, evidence from some or all of Wüster (2001), Kaiser *et al.* (2013) and Zug (2014) was rejected by the courts as rants from unscientific men whose agenda was to unlawfully steal the benefits of the work and intellectual property (IP) of others. This was via trying to steal the "name authority" for taxa not owned by them that had been correctly obtained via the rules of the *International Code of Zoological Nomenclature*.

It is also significant that Denzer *et al.* (2016) in their paper said "We are grateful to George Zug and Hinrich Kaiser for a prereview of the manuscript and for their comments, corrections and suggestions.", noting that both Zug and Kaiser's writings, namely Kaiser *et al.* (2013) and Zug (2014) had been formally rejected as unscientific rants by a Judge at VCAT a year earlier (VCAT 2015).

2/ In their paper, Denzer *et al.* (2016) told numerous lies that they simply hoped that their readers would never investigate or

find out. One such example is this: They wrote:

"Hoser (2013) on Amphibolurinae

We note that the manuscript on Amphibolurinae was received by *AJH* on 20 July 2013, accepted for publication on 4 October 2013, and published on 20 October 2013. However, a tax invoice printed at the end of the publication (p. 36) states that the journal was printed on 3 October 2013, implying printed copies may have existed before the paper was accepted."

The intent of the statement is to show that the entire publication process of *AJH* is fraudulent and that the editor (myself/ Raymond Hoser) has engaged in fraud. After all, how can a paper be accepted for publication after a publication date? Now if one were to accept the claim as written and on face value and without taking the time to go to the relevant issue of *AJH* to check the claim, it would have to be accepted by the ill-informed reader as being true!

This belief would irreparably damage the reputation of myself (Raymond Hoser).

Clearly any peer reviewers or editors of this paper by Denzer *et al.* (2016) did not bother to check the original source, or if they did, they chose to recklessly ignore what they found, seeking the claim to cause maximum damage.

This is because if one goes to the source publication, namely the relevant issue of *AJH* (issue 21) one finds that the tax invoice published on p.36 carries an invoice date, but does not carry any date of publication or printing whatsoever.

Put simply, Denzer *et al.* (2016) have lied in claiming that the tax invoice on p.36 of Hoser (2013b) carried a date of printing or publication. It did not!

Hence their entire paragraph is yet another deliberate and scandalous lie by the authors.

To make things worse, the authors have continued this vein of dishonesty throughout their paper in at least three other widely spread places.

Elsewhere they wrote:

"A) Hoser (2014b) on draconinae.

As printed in the header of the paper, the Draconinae manuscript was received by the journal on 10 November 2013, accepted on 1 June 2014 and published on 1 July 2014.

According to the tax invoice, Issue 22 of the *AJH*, which includes the Draconinae paper, appears to have been planned before October 2013, which is the date of the invoice (Hoser 2013: 36, Hoser 2014a: 5; invoice date 3 October 2013, several weeks before the publisher initially received the manuscript). This could indicate that Hoser pays in advance for the printing of issues, which would imply that manuscripts may already be in hand, or that some of the publication dates are otherwise manipulated." Speculating what an invoice date "could indicate" in terms of attempting to create some kind of dishonest or criminal conspiracy on my part by Denzer *et al.* (2016) gets into the realm of the wildest conspiracy theories.

Who knows, next Denzer *et al.* may allege that my trip to the United States in 1993 was a preliminary excursion to plan the destruction of the World Trade Centre buildings 11 September 2011?

Their evidence could be that I arrived in the USA by plane and this was the weapon of choice used to destroy the relevant structures.

Of course, the date of the tax invoices as published in *AJH* indicate exactly what it says and nothing more. This is that date of issuing of the invoices for payment to the printing house for publishing of the journals. As for any connection between the issue date of the invoices, payment dates, for which Denzer *et al.* (2016) clearly do not have a clue, or the ultimate printing/ publication dates, all their speculation is purely that ... speculation. If one looks at the invoices in every issue of *AJH*, the only common thread is that they are issued before the journals are published, as one would expect!

Like most businesses, printers like to be paid and preferably before they spend their own time and money doing the work! Speaking of wild conspiracy theories, it is hard to go past the one by Denzer *et al.* (2016) at page 123 that alleges I hacked into a computer owned by the Pakistani government to steal the work of one of his gang members.

Now if I had such skills at computer hacking, I am sure that the American CIA would have recruited me to help them find Osama Bin Laden who managed to hide in Pakistan from them for a full ten years!

But the purpose of this paper is not to give a long-winded rebuttal of the false claims of Denzer *et al.* (2016).

As mentioned already, the authors rebadged the same lies more than once throughout the paper, in some kind of rotating fashion, in order to pad it out to be a nearly 20,000 word diatribe.

However the preceding is to note that they represent yet more of the lies and falsehoods of the Wüster gang as a pretext to their illegal act of stealing the works and name authorities of other authors who have properly named valid taxa before they lurched onto the scene and decided to look at the same animals critically.

Their intent to steal "name authority" for valid taxa is the entire basis of their collection of lies.

These people and their regular collections of lies and hatred have been discredited many times before (e.g. Cogger 2013, 2014a, 2014b; Court of Appeal Victoria 2012, 2014; Dubois 2014; Eipper 2013; Hoser 2012b, 2013b, 2015a-f; Mutton 2014a, 2014b; Shea 2013a-d, 2014a-b; Thorpe 2013, 2014a-c, 2015; Wellington 2013, 2014a-b, 2015 and Wells 2013, 2014a-b) and history will judge them appropriately.

However as they have published on the genus in question subject to the taxonomic act in this paper and sought to illegally rename the genus, it is appropriate that mention be made of the relevant papers, Denzer *et al.* (2015) and Denzer *et al.* (2016).

Put simply, the correct name for the genus is *Darainagama* and not the junior synonym *Malayodracon*!

No amount of lies by Denzer *et al.* or others in the Wüster gang will change this fact!

Now in fairness to Denzer *et al.* (2016), I should mention that in the nearly 20,000 word diatribe, the only correct claim against the Hoser papers was the inadvertent use of a pre-occupied name for a genus of Agamids in one of the papers, that name being *Tiaris Duméril* and Bibron, 1837. That however had no impact whatsoever on the taxonomy in the papers or the logical (legal) nomenclatural acts that followed as published within the papers.

They all remain untarnished in any way and on the basis of available evidence, still remain correct!

A QUESTION OF ETHICS IN THE PUBLISHING PROCESS

Among the more scandalous claims by Denzer *et al.* (2016) is that I had somehow stolen the work of their gang (by hacking the Pakistani government computer) and scooped them by publishing my *Laudakia* paper just days before theirs (known as Baig *et al.* 2012). They wrote:

"Baig *et al.* (2012) was published in print on 18 July 2012 and Hoser (2012a) was published in print 30 June 2012. Both papers were accepted for publication by the respective journals in April 2012. We also note that Baig *et al.* (2012) was made available in advance online on the publisher's website on 6 July 2012, appearing a week after Hoser's publication."

However a simple check finds that it would appear that the claim "Baig *et al.* (2012) was published in print on 18 July 2012" and that I had fortuitously scooped their name authority by merely a few days, is pure fantasy.

A check with *Zoological Record* online (and archived) shows that the Hoser Journal posted from Australia with the relevant paper, arrived in the UK office of *Zoological Record* and was receipted on 9 July 2012.

That equates with a fortnight to get there, which sits in line with actual printing being a few days prior to the cover date.

This is significant noting the ethical considerations involved with the rules of the *International Code of Zoological Nomenclature* and the specific rule of priority, in that to backdate a publication date in order to try to wrongly assert name priority is both illegal and very unethical.

However we find that according to *Zoological Record* their first copy of Baig *et al.* did not arrive at their UK office until 29 November 2012.

In other words, far from being published on 18 July 2012 as alleged by Denzer *et al.*, it appears that their own paper did not actually get published until five months after the Hoser paper.

For them, it was definitely not a case of just missing out on claiming "name authority" by a few days!

Evidence therefore shows that either authors, journal publishers or both have been guilty of the morally repugnant act of illegally backdating their publication dates!

WHEN THE OPPORTUNITY TO STEAL AND GET AWAY WITH IT BECOMES A REASON TO DISCARD MORALS AND THE RULES

One of the coauthors of Denzer *et al.* (2015 and 2016) is none other than Wolfgang Böhme. Until approached by the Wüster gang preceding the publication of Denzer *et al.* (2015), which accepted the call to arms by Kaiser *et al.* (2013) to step outside the rules of the *International Code of Zoological Nomenclature*, and steal name authority from others, Böhme sat on the side of ethics and against taxonomic vandalism.

In 1998, he successfully argued to the ICZN against allowing any taxonomic vandalism to be used to attack the rules of the *International Code of Zoological Nomenclature* to allow thieves to steal name authority for species or genera from earlier authors.

The case in question involved another member of the Wüster gang, namely Robert George Sprackland, who sought to steal "name authority" from Richard Wells and Ross Wellington for a Monitor species they had named as "*Odatria keithhornei*" in 1985.

An attempt to suppress the name by Richard Shine and other members of the Wüster gang (The president, Australian Society of Herpetologists. 1987) failed in 1991 (ICZN 1991) with all but one commissioner voting against the thieves.

The related case argued by Böhme in 1998 arose when Sprackland improperly sought to illegally rename the species after his wife! (Böhme and Ziegler 1998).

The arguments advanced by Böhme and Ziegler (1998), agreed by the ICZN again by near unanimous vote shortly thereafter (ICZN 2001) remain unchanged and are a direct rebuttal of the stated central aims of Denzer *et al.* (2016) from the mouth of one of the four co-authors!

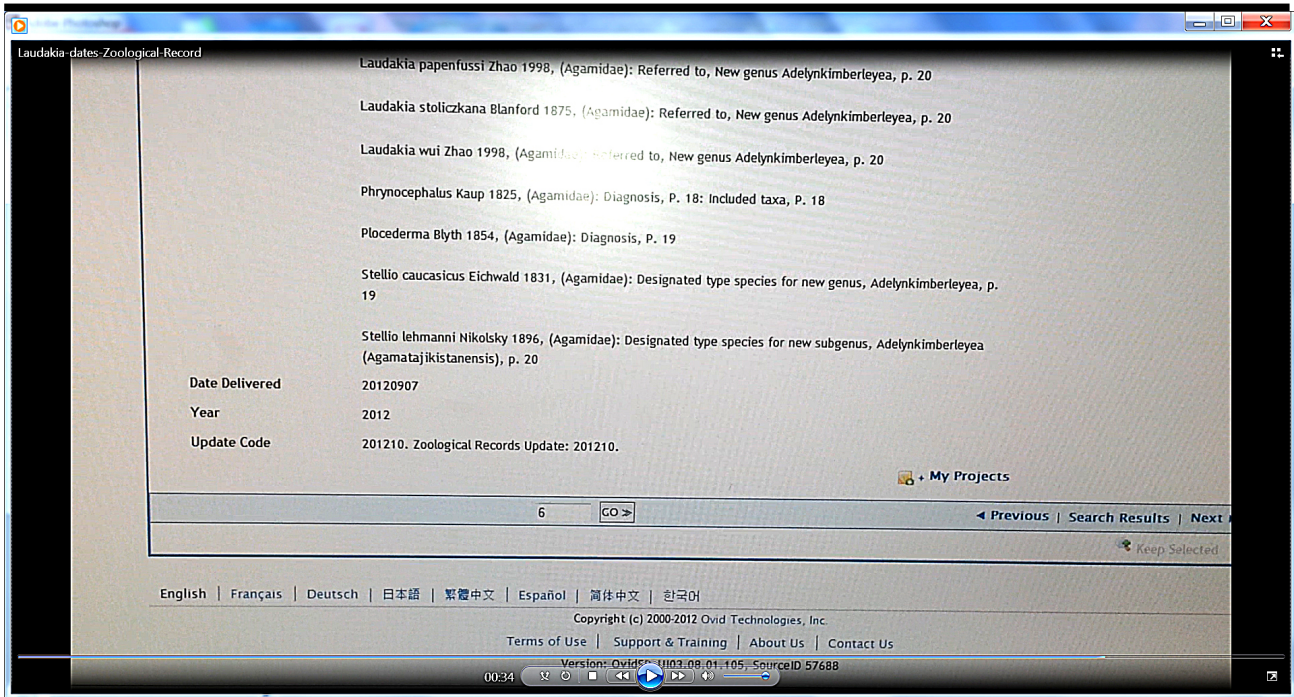
Notwithstanding this, the more recent published submission to the ICZN by Rhodin *et al.* (2015), confirms that the gang seek nothing less than to impose their own illegal hegemony on herpetological science, taxonomy and nomenclature and that they are also aggressively attempting to hijack the ICZN Commissioners themselves to carry on their own nefarious agenda.

If they succeed, taxonomy and nomenclature would descend into chaos, all for the purpose of affording the Wüster gang the self flagellation of being able to claim to have "discovered" new taxa.

Of course part of this would be their improper attempts to rewrite the history of zoological discovery in a manner no different to the way Nazis and other dictators have sought to glorify themselves in the history books they have published.

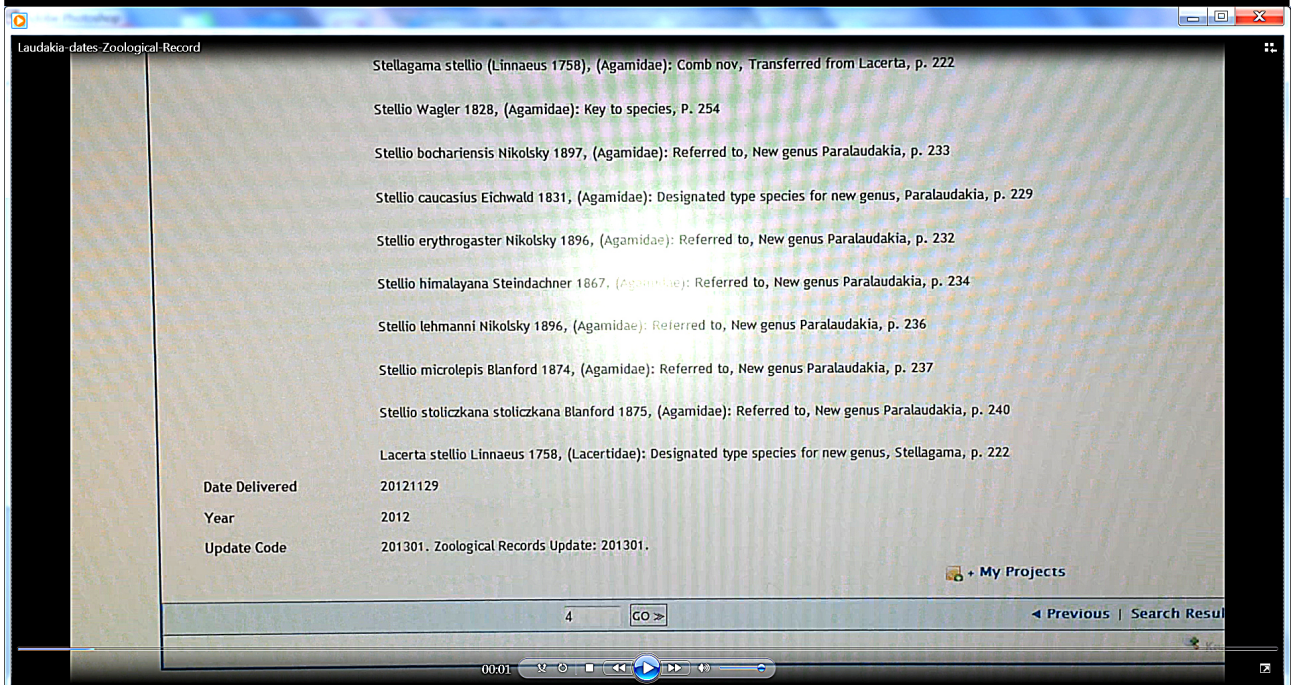
A "THANK YOU!" TO DENZER ET AL. (2016)

Denzer *et al.* (2016) was undeniably written to attack myself (Hoser), my publications and cause maximum damage to myself



No, they didn't get scooped for a "name authority" by just few days as falsely alleged by Denzer *et al.* (2016). The independent evidence suggests that Baig *et al.* (2012) was in fact published some four and a half months after Hoser (2012). Shown here are screen dumps photographed from the *Zoological Record* website in 2013 that show a delivery receipt date of 9 July 2012 for the Hoser paper and 29 November 2012 for the Baig *et al.* paper.

Hoser 2016 - Australasian Journal of Herpetology 32:53-60.



(as a pretext to the planned theft of my hard earned intellectual property, in the form of "name authority" for taxa). Denzer *et al.* (2016) continue that outdated white Eurocentric view of entitlement to steal from people elsewhere on the planet via the fabrication of false claims and in breach of all accepted rules and protocols. They must not be allowed to impose the Wüster gang's ISIS-like mob-rule on the scientific community.

However the attack by Denzer *et al.* (2016) not only failed to refute the taxonomy and nomenclature of the papers in question (which is really all that matters), but they noted that I was correct in most cases, because they alleged I had stolen other people's research work, including for example in the *Laudakia* paper.

By running the central theme that all the contents of the Hoser papers was derived from stealing the evidence and works of earlier authors (the main plagiarism claim), or what Rhodin *et al.* (2015) called my "data mining", these authors have in effect refuted the central claims of the Wüster gang made over most of the period from 1998 to 2012 (e.g. Wüster 2001 and Wüster *et al.* 2001). This was that the taxonomy of myself was ridiculous and "evidence free" and should therefore be rejected and not used on that basis.

Now that Denzer *et al.* (2016) in addition to Rhodin *et al.* (2015) have shown that the Hoser papers do have a sound scientific basis (even if they make the fanciful claim it was all stolen from hacked government computers and the like), the acceptance and use of the taxonomic concepts within these papers and the legal nomenclature arising should be a mere formality.

Therefore, I would like to publicly thank Denzer *et al.* (2016) for laying out the evidentiary basis for the relevant papers, the taxonomy within and therefore await the rest of the Wüster gang to comply with the rules of the *International Code of Zoological Nomenclature* to use the relevant names.

SUBSPECIES OF *DARANINAGAMA ROBINSONII*

The species *Daraninagama robinsonii* (Boulenger, 1908) has until now been treated as a single species with a disjunct range across the highlands of Peninsula Malaysia.

Data obtained by myself on this and other species from across south-east Asia, accumulated over some decades was stolen from my facility in an illegal armed raid by government officers on 17 August 2011.

It is significant that in the years prior to this illegal shut-down of our successful conservation, education and research business, Wüster *et al.* had run an illegal online petition calling for the government of Australia to illegally shut down our business. Their petition was posted online on a website controlled by known criminal Shane Hunter (Hunter *et al.* 2006).

In contempt of court orders to return the materials taken at gunpoint on 17 August 2011, the relevant material was either not returned or if on disks, degraded so as to be unretrievable and effectively lost.

This adversely impacted the imminent publications on numerous reptiles including nominate *Daraninagama robinsonii*. However noting the ongoing conservation risks to all populations of *Daraninagama robinsonii* due to habitat destruction, introduced pests, infectious diseases and/or parasites and other factors, I view it as important that the currently unnamed subspecies taxon be named sooner rather than later. This is because "later" may be at a time before governments recognize this potential management unit and otherwise let it expire.

In terms of the two main populations of the species, Denzer *et al.* (2015) wrote:

"Variation. Hitherto known specimens from the type locality (Gunung Tahan) do not show enlarged dorsal

scales arranged in oblique rows (Boulenger 1908, Sly 1976) as it can be seen in specimens from the Cameron Highland region. It is conceivable that these two populations have been separated for a long time and constitute subspecies. However, in order to establish consistency of this character more material from the remote mountain ranges of central Malaysia is needed.

Additionally there exists a photographic record of a specimen from the Cameron Highlands without apparent enlarged scales across the dorsum rendering the above observation doubtful."

From this paragraph it is clear that like myself these authors have viewed the potentiality that the two known populations are taxonomically distinct, but their comments with respect to the "enlarged dorsal scales arranged in oblique rows" is evidently wrong.

The holotype specimen as depicted in their paper does in fact possess such "enlarged dorsal scales arranged in oblique rows", as does the specimen of the western form as depicted in the same paper. Hence absence or presence of such rows is not in itself a means to differentiate populations.

However what is significant is that these rows are obvious in the western specimens and relatively indistinct (but still present) in the type form, giving one means to separate the two.

Inspection of specimens from each area also shows other subtle differences as outlined in the formal description of the subspecies below.

Diagnosis of *D. robinsonii* (Boulenger, 1980) can be found in both Hoser (2014) and Denzer *et al.* (2015) as well as some of the sources cited therein and is therefore not repeated here.

DARANINAGAMA ROBINSONII CLIVEEVATTI SUBSP. NOV.

Holotype: A preserved specimen at the The University of Texas at Austin, (Texas Natural History Collections), USA, specimen number: TNHC Herpetology 56648, collected from the Cameron Highlands, Pahang, (Peninsula) Malaysia. This facility allows access to its holdings.

Paratype: A preserved specimen at the The University of Texas at Austin, (Texas Natural History Collections), USA, specimen number: TNHC Herpetology 57717, collected from the Cameron Highlands, Pahang, (Peninsula) Malaysia.

Diagnosis: *Daraninagama robinsonii cliveevatti subsp. nov.* are separated from *D. robinsonii robinsonii* by the presence of enlarged dorsal scales arranged in oblique rows, versus the presence of indistinct enlarged dorsal scales arranged in oblique rows. Behind and below the eye and before the ear there is a series of enlarged white scales with black at the borders. In *D. robinsonii cliveevatti subsp. nov.* the black is thickened, whereas this is not the case in *D. robinsonii robinsonii*.

In *D. robinsonii robinsonii* the upper part of the nasal darkens, which is not the case in *D. robinsonii cliveevatti subsp. nov.*

Diagnostic information for the species *Daraninagama robinsonii* (Boulenger, 1908), including both species can be found in Hoser (2014) and Denzer *et al.* (2015).

Distribution: Known only from the general area of the Cameron Highlands, West Malaysia.

Etymology: Named in honour of barrister Clive Andreas Evatt from Turrumurra, North Shore of Sydney, NSW, Australia. Unlike most lawyers who do nothing more than lie, cheat and thieve, Clive is a man of ethics and honour. He has taken on a number of important public interest cases at huge personal cost that otherwise may not have been litigated.

Over many decades he has as a defamation lawyer successfully defended weak and vulnerable individuals from powerful interests in the media and government who have either sought to suppress the truth or do so by unlawfully slandering whistleblowers to destroy their previously good reputations.

Of particular relevance to private reptile keepers and herpetologists everywhere is that in 1996 Evatt and fellow lawyer, Michael Rollinson successfully fought the NSW National Parks and Wildlife Service (NPWS) and allies in three cases in the NSW Supreme Court to ban the newly published book, *Smuggled-2: Wildlife Trafficking, Crime and Corruption in Australia* (Hoser 1996).

False claims (similar to those of Denzer *et al.* 2016 against Hoser papers) were made against the Hoser book. Evatt systematically refuted each and every one of these false

claims and went further and showed that it was the accusers who were guilty of the very misconduct they were alleging (as is the case with Denzer *et al.* 2016).

As a result of the work of Evatt and Rollinson in making sure the public got to read the truth about the wildlife trade in Australia, the attempts to ban the book failed.

The last case was finalized on 24 December that year and widely reported in the media at the time.

As a result of the publicity and the fact that the book was now legally being sold Australia-wide, the book became a best-seller. As a direct result of the publication of the book, governments across Australia were then forced to remove more than 20 year-old bans on legal private ownership of reptiles, which came to fruition the following year (1997) in NSW and shortly thereafter elsewhere.

Some states in Australia had lifted bans on private ownership of reptiles following publication of the book *Smuggled: The Underground Trade in Australia's Wildlife* (Hoser, 1993), which was also initially banned by the NSW Government who illegally got police across Australia to seize copies from booksellers.

The 1993 ban was lifted following a major campaign by the tabloid media at the time (Hoser 1996).

A court action to ban that book also failed and the publisher Charles Pierson ultimately secured a sizeable payout arising from the illegal actions to enforce the ban in 1993.

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CONFLICT OF INTEREST

The author has no known conflicts of interest in terms of this paper and conclusions within other than as name authority for the legally correct nomen *Darainagama* Hoser, 2012.

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