Australasian Journal of Herpetology 2 (2009):1-34.

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

CREATIONISM AND CONTRIVED SCIENCE: A REVIEW OF RECENT PYTHON SYSTEMATICS PAPERS AND THE RESOLUTION OF ISSUES OF TAXONOMY AND NOMENCLATURE.

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Received 5 January 2009, Accepted 25 January 2009, Published 3 February 2009.

ABSTRACT

This paper revisits the systematics of pythons as detailed in post 2000 papers and finds the taxonomy at the species level to be generally sound.

The undisputed data showed need for the stabilization of the taxonomy and nomenclature at other levels, the result being this paper formally names for the first time two new subgenera and a new subspecies, (*Chondropython viridis adelynhoserae* subsp. nov.) as well as listing names of recently diagnosed species taxa with available names that are not widely known among herpetologists.

Furthermore, two subspecies within the genus *Aspidites* are named as is a *Leiopython* subspecies. However, much of this paper deals with a 2008 division of the species "*Leiopython albertisi*" in 2008 by a self confessed "amateur herpetologist" to create a total of what he now claims is many species (at least five). This is demonstrated to be incorrect on the basis of the evidence presented.

An assessment shows no evidence supporting division of the genus into any more than the two long recognised taxa, *Leiopython albertisi* Peters and Doria, 1878 (the brown *Leiopython* from north of the central New Guinea cordillera) and *Leiopython hoserae* Hoser 2000 (the larger, black *Leiopython* from south of the central New Guinea cordillera) at the species level.

Dishonest practices, cynical alteration or omission of "facts", data and "findings", statistical manipulation with questionable parameters, gross double standards, fraud, and plagiarization by the author of the 2008 paper and/or his immediate (self-identified) co-workers are identified.

Keywords: snake; reptile; *Jackypython*; *adelynhoserae*; *rawlingspython*; *Leiopython*; *hoserae*; Schleip; Wüster; David John Williams; smuggling, animal cruelty; *albertisi*; taxonomy; fraud; wikipedia; *biakensis*; *barkerorum*; *bennettorum*; *huonensis*; *fredparkeri*; *antaresia*; *chondropython*; *Australiasis*; *Aspidites*; *neildavieii*; *rickjonesii*, python

INTRODUCTION

The snake, known generally as the White-lipped Python, *Liasis albertisii* Peters and Doria, (1878), was inadvertently redescribed the following year as *Leiopython gracilis* by Hubrecht (1879), who also created the monotypic genus for the taxon, namely *Leiopython*.

Over the intervening 120 years the taxonomy of the genus remained stable at the species level, but the genus assignment varied in line with general trends in python taxonomy, with various authors assigning the taxon to the genera *Liasis* (e.g. Stull, 1935, Stimson, 1969, McDowell, 1975), *Bothrochilus* (e.g. Cogger, Cameron and Cogger, 1983, Hoser, 1989, Rawlings, et. al. 2008), *Lisalia* (e.g. Wells and Wellington, 1984) and *Morelia* (e.g. Underwood and Stimson, 1990).

Kluge 1993, and most authors since then, including O'Shea (1996), Hoser (2000b and later papers), O'Shea (2007a, 2007b) have referred the taxon to the genus *Leiopython*, with the nomenclature at the genus level remaining that way to at least 2008 with very few exceptions.

While this paper continues to place the White-lipped Pythons in the genus *Leiopython*, the genus placement of these snakes is not of importance in the context of this paper.

For many years, it's been known that there were two distinct variants being identified as "White-lipped

Python", this taxon, lumped generically as "*L. albertisi*", including the northern "brown race" and the southern "black race" (see for example O'Shea 2007a).

The differences, included size, behavioural, including temperament and scalation. The two variants were also separated by distribution, in the form of the central New Guinea Range and that both may be different species was known by many herpetologists for many years, including throughout the latter part of the twentieth century (O'Shea 2007a). Hence the formal naming of the taxon as *Leiopython hoserae* Hoser 2000, by Hoser 2000b was relatively uncontroversial and widely accepted by herpetologists and appearing in numerous relevant publications including for example Allison (2006) and O'Shea (2008).

The division of serpent taxa separated by the main central range of New Guinea, was not just done with regards to the genus *Leiopython*.

Hoser 1998 did a similar split for the snakes of the genus *Acanthophis*, erecting a new species (*A. barnetti*) for those found north of the main range, that until then were undescribed and also long recognised as a distinct species (see O'Shea 1996, p. 158).

Rawlings and Donnellan (2003) did the same for the Green Pythons (*Chondropython viridis*) placed by them in the genus "*Morelia*", with species names already available for north and south populations.

Harvey et. al. looked at MtDNA of the Scrub Pythons (the "*amethistina*" species complex, which they placed in "*Morelia*") and in line with the published results for this species complex in Hoser 2000b decided that there were grounds to declare those north of the range a different species level taxon to those from the south.

(As stated in Hoser 2000b, Hoser 2000b, deferred naming these snakes pending the paper being published by Harvey et. al., stating "This author had assigned names to forms previously regarded as *A. amethistina* that is found in Islands to the north-west and south-west of New Guinea, however they were withdrawn from this paper after it became apparent that David Barker and others were similarly subdividing the genus *Austroliasis* as it is described here.")

Notwithstanding this statement and the fact that both this paper and Harvey et. al. (including David Barker) were published in 2000, this didn't stop a habitual liar and long-term adversary of Raymond Hoser, Mr. John Weigel from posting on the "australianherps" list server in 2001 the false statement that Raymond Hoser was plotting to "steal" naming rights for those taxa from Barker at. al.. Those posts remain on the internet as of 2008.

In line with Harvey et. al.'s results for "*Morelia amethistina*" in terms of northern New Guinea specimens being assignable to a single species level taxon (p. 171 their paper, see figs A, B, and C) , Hoser, 2000b independently did the same for the pythons assigned to the taxon *L. albertisi* in that all from the mirroring region were assigned to the taxon *albertisi*.

Noting that the geographical and physical barriers affecting the genera *Acanthophis* (see Hoser 1998), "*Morelia viridis*" (see Rawlings and Donnellan 2003), "*Morelia amethistina*" (see Harvey et. al. 2000) and *Leiopython* (see Hoser 2000b) were apparently one and the same, it was totally expected that all four studies, across two (mainly) terrestrial snake families, demonstrated species splits broadly along the same regions (and movement barriers), even though all studies were on different taxa and used quite different materials and methods to arrive at essentially the same results.

These results are notable in that while it'd be reasonable to infer that the python taxon "*amethistina*" may be larger and perhaps more mobile than "*L. albertisi*" (see Harvey et. al.'s comments for their new "*amethistina*" taxon), studies across Australia for *Acanthophis* (NSW and WA studies by Hoser, summarised in Hoser 2002), have indicated these snakes to be considerably less mobile and therefore more prone to speciation than for other pythons (which would by all known measures include *Leiopython*), and yet to date, no island specific population of *Acanthophis* for Biak has been described or named.

The two most outlying populations of *Leiopython albertisi*, namely those from the (south) eastern extreme and those from the northern extreme, both away from the (far west located) type localities for both *albertisi* and *gracilis*, were designated subspecies status on the basis of minor head scalation and other differences by Hoser 2000b (as redefined and agreed by Schleip 2008) and with limited sample sizes, and on the basis of a continuous distribution at the present time and/or very recent past (within the last 12,000 years).

At the time (2000) and even now, Hoser (this author) was of the view that not enough evidence warranted splitting of those populations to the species level.

Until end 2008, the Hoser 2000b divisions of the genus *Leiopython* were generally recognised by herpetologists with an interest in *Leiopython*.

Leiopython hoserae Hoser 2000 was readily adopted by authors (e.g. Allison, 2007, O'Shea 2007b and others), while no publishing authors regarded any regional races of *L. albertisi* (the two named subspecies) as being of distinct species and not one single publication appeared in print recognising or naming those variants as species, even to the extent of identifying different populations.

THE TRUTH HATERS

Dissent in terms of the Hoser taxonomy was only voiced by a group known as the "truth haters" or "the

Hoser critics", centred on two men, namely a serial wildlife smuggler David John Williams and his close friend Wolfgang Wüster a Wales based "academic" at Bangor University with a history of publishing sloppy work.

Wulf Schleip, author of Schleip 2008 is a close friend of the above pair.

Relevant here is that like them, he's also had too much spare time on his hands and through his own website created in 2001 and others he visits and posts on via webforums, he has joined in the anti-truth and hate campaign by means of non-stop posts adverse of Hoser wherever he felt his arguments would gain traction.

None of their continual barrage of criticisms has had a grain of merit. However using their excess amounts of spare time and the near limitless resources of the internet, these man have managed to wage a campaign against Hoser of a scale and magnitude that is truly amazing. Recruiting a small-band of misfits, with the ability to repeatedly post under false names and to censor and edit internet sites they control, these men have at times created a veneer and perception that there is widespread disagreement with the various Hoser taxonomy papers (and anything else to do with "Hoser", including the extremely popular venomoid (surgically devenomized) snakes) when the reality among qualified practicing herpetologists has been very different (Hoser 2004c).

By and large the Hoser taxonomy has been very routine and uncontroversial and the divisions at species level generally been of well-defined taxa, conservatively definable via a myriad of criteria.

No one has petitioned the ICZN in relation to any of the Hoser papers, indicating a general lack of concern in terms of the taxonomy and nomenclature.

Outside the group of people to be described in more detail below, the general perception of "Hoser taxonomy" has been that it is at times too conservative, befitting the position of a "lumper" as opposed to a "splitter", as voiced by Wells (2002), the result being further splits of taxa examined by Hoser being proposed by other authors, including one may suppose Schleip (2008), although as this paper shows shortly, the professed views of Schleip cannot be found to be consistent, credible or for that matter even honestly held by Schleip himself.

THE ORIGINAL "TRUTH HATER" THE CONVICTED REPTILE SMUGGLER DAVID JOHN WILLIAMS

In his view his reason to hate "Hoser" was justified on the basis that he was adversely named in both "*Smuggled*" books, (Hoser 1993 and Hoser 1996) and that forms the original basis of his ongoing hatred since then, which has over time expanded.

While the material in the book was true and correct as easily confirmed by publicly available court records (see Magistrate's Court of Queensland 1997), Williams has held the grudge against Hoser and pursued it ruthlessly and without scruple.

David John Williams, posting on the internet as "Toxinologist" and other names is a man with numerous serious convictions for animal cruelty and wildlife smuggling (see for example a mere fraction of the number of his crimes and convictions in the full transcripts of Magistrates Court of Queensland (1997).

Williams and his close friend Wolfgang Wüster have both been guilty or party to a serious case of scientific fraud as detailed by Hoser 2001a and Hoser 2001b.

That scientific fraud revolved around an improperly altered (on at least three occasions) "online" paper that was published in the first instance as an alleged critique of the description of *Pailsus pailsi* Hoser 1998.

Since then, his alleged co-conspirator in the fraud Brian Starkey (listed by Williams as a junior co-author of the fraudulent and ever-changing online paper Williams and Starkey (1999 – three versions, listed here as "a', "b" and "c")), has stated that he had no part in the fraud and that Williams had without his permission included his name as co-author in the fraudulently altered paper and in fact printed material that both men knew was patently untrue.

The ill-fated paper did in it's first incarnations claim that the newly described species "*Pailsus pailsi* Hoser 1998" was in fact nothing more than a small or underfed Mulga Snake *Cannia* ("*Pseudechis*") *australis*. The claim was underpinned by some statistical gymnastics not unlike some of those seen in the 2008 Schleip paper.

Williams altered and reposted the paper at least three times (cited herein as Williams and Starkey 1999a, 1999b and 1999c), the varied versions being dutifully downloaded by myself and others and now archived and accessible in a single file on the internet as part of the historical record of the fraud, or alternatively separately from the website http://www.smuggled.com/Sland1.htm as links to their originally published forms.

The final altered version of the paper, that had it's publication date post-dated effectively reversed the original claims about *Pailsus* and falsely inferred Williams was set to describe the New Guinea taxon, *rossignollii*, actually described and named in Hoser 2000a with the publication Hoser 2000a, long predating the first actual uploading and posting in January 2001 of Williams and Starkey (1999), version "c".

Williams then made false claims on "www.kingsnake.com" and the internet chat forum "australianherps" along the lines that Hoser had stolen his "naming rights" to the New Guinea taxon, later changing it to the claim that Hoser had sought to do so, but inadvertently named another taxon (namely

rossignollii), with Williams still about to name yet another unnamed taxon, for which fortuitously his enemy Hoser did not have access to the specimens.

In spite of Williams making these claims in 2000 and 2001, as of 2008 he has failed to identify or name any such taxon, even though in 2005, he coauthored a paper ostensibly on the taxonomy of the "*Pailsus"/ "Pseudechis"* group of snakes (Wüster at. al. 2005). That paper did not name any new taxa anywhere!

This again shows the unreliability of statements or material published that Williams and his associates write.

As it happens, Williams had nothing whatsoever to do with the initial discovery or naming of the *Pailsus rossignollii* taxon (see Hoser 2000a), although in a book he published in 2005 (Williams et. al. 2005), he did recognise it as valid taxon that had been properly named by Hoser in Hoser 2000a (see pages 58, 59 and the distribution map in the Williams book, now identifying the *rossignollii* taxon as also occurring in PNG in the alleged region of his allegedly undescribed similar taxon).

This was significant in itself as it reversed opposing dogma as published by his close associate Wüster at. al. (2001) to the effect that *Pailsus rossignollii* was either "*nomen nudem*" (see the definition of the term in ICZN (1999), or *Pailsus rossignollii* was alternatively not a valid taxon, that had in turn been widely reposted and cited by Williams as "fact".

Notably however, while Williams chose to use the Hoser material in his book (on venomous species from New Guinea), and in spite of an extensive bibliography, Williams chose to deliberately exclude any Hoser publications from his references list in spite of several being key publications on the relevant groups of snakes and yet he chose to cite his own and Wüster's publications (post-dating the Hoser ones by some years) that had committed the morally reprehensible sin of plagiarising the key Hoser results (see below).

However in spite of the above facts relating to the description of the two Hoser *Pailsus* species in 1998 and 2000, Brian Starkey actually had no role in the false claims made in the ever-morphing paper originally published and dated from 1999 (Williams and Starkey 1999a), (AKA version 1). In 2008, the "alleged" or "stated" co-author Brian Starkey wrote in an e-mail of that ill-fated 1999 paper that:

> "I had absolutely nothing to do with time alteration and the reposting on web.
>
> If fact I was in two minds about the whole paper, without even seeing a specimen of *pailus*. I didn't want to pass judgement until I had got out there and looked for
>
> myself. I did four trips asap to the area and found a couple of specimens 40-50 km from Cloncurry. I knew as soon as I saw my first DOR, that you were right!
>
> When I showed David a few pic's and close ups he knew too! Then I got a live specimen amongst a small group of rocks, so fast I nearly lost it. I have probably
>
> seen about 3 live and 4-5 DOR specimens in 9 or more trips. I wish we didn't jump the gun.
>
> But David wrote the paper and added my name. I never actually wrote a word, although he may have quoted things I said during phone conversations.
>
> And that's the truth."

In other words, Williams had knowingly published false information and conclusions to try to convince third parties of his lie that *Pailsus pailsi* Hoser 1998 was not a valid taxon.

Williams has had an axe to grind against Hoser and used it constantly to attack my credibility, after being adversely named in both "*Smuggled*" books, (Hoser 1993, 1996).

Those books detailed numerous cases of animal cruelty and reptile smuggling involving Williams in the periods predating publication of both books. For all cases referred to in the books, he was ultimately charged, convicted and fined by the Australian or Queensland governments, the last relevant case being finalized in 1997 for extreme cruelty to live reptiles and smuggling-related matters.

Himself and his close associates, including a so-called academic named Wolfgang Wüster from a University in Wales, UK, have since spent much of their paid time stalking the internet telling people not to use so-called "Hoser names".

They have done this while simultaneously committing the ethically repugnant crime of plagiarising Hoser research papers and republishing the results in their own later publications (e.g. Williams. et. al. 2005, Wüster, et. al. 2005), while consistently failing to appropriately cite or acknowledge the original source of the "findings" (also see Williams, Wüster and Fry 2006).

A close friend of theirs, with a similar "anti-Hoser" position was a self-admitted "amateur herpetologist" (see text at: http://leiopython.de/en/vita.html downloaded on 28 December 2008, or last words page 19, Schleip 2008a) by the name of Wulf Schleip, who in the period after 2001, took a strong interest in the snakes of the genus *Leiopython* which he had as "pets, and to his obvious dislike found that one of just two named and obvious species in the genus was "*Leiopython hoserae*".

Wüster's best known critique of the "Hoser taxonomy" was a paper he shopped to various journals before it

got through a new and gullible editor at *Litteratura Serpentium* in 2001. The paper has since been posted by Wüster and all other "Hoser haters" widely over the web and elsewhere to further their cause, including by Williams, Schleip and others. However all the central arguments in the paper (Wüster et. al. 2001, and later ones repeating the same or similar lies) have long since been shown to be false (see for example Hoser 2001a, 2001b, in direct reference to the Wüster et. al. piece, or alternatively Kuch, et. al. 2005, Rawlings et. al. 2008 and others who in turn rebut the false claims by Wüster et. al. relating to the Hoser taxonomy), but that has never stopped these men from repeating, embellishing and further exaggerating their lies and false claims on internet posts and even hard-copy publications, including for example (Williams, Wüster and Fry 2006).

Williams and Wüster have a history of "shopping" their "papers" through friendly and not so friendly editors to publish material that under normal circumstances would never pass even the most basic of editorial processes in anything masquerading as "scientific literature".

Simultaneously they have phoned and written to journal editors making false claims, threats and even sending legal letters, trying to harass and intimidate editors not to publish material correcting their lies. Affected journal editors include those from *Crocodilian*, *Herptile*, *Litteratura Serpentium*, *Boydii*, *Monitor* and others as well as even the *Herald-Sun* newspaper in Melbourne.

The latter received numerous threats and then even a letter (later passed to myself) after the newspaper published a world first photo of Raymond Hoser "free handling" a large number of the world's top four deadliest snakes (*Parademansia microlepidota*, *Oxyuranus scutellatus*, *Pseudonaja textilis* and *Notechis scutatus*), that happened to be the world's first venomoids of those taxa (Hoser 2004b), even though the accompanying captions and stories had no relevance or references to Williams, Wüster or their associates or in theory gave them any reason to contact the newspaper.

This is mentioned merely to indicate the obsessiveness and extent of the campaign against Hoser interests by these men and the degree to which they actively "stalk" and try to counter any favourable mentions of "Hoser" in any context.

FURTHER FRAUDULENT ACTIVITY BY THE CONVICTED REPTILE SMUGGLER DAVID WILLIAMS

This is detailed here due to the fact that Schleip has by his own admissions in his 2008 paper worked closely with this convicted conman and that it appears he employs similar morals.

In a widely reported statement made by a PNG Pharmacist, Richard McGuiness in 2008, David Williams also stole dozens of vials of snake anti-venom from government stores when not authorised to do so.

Noting the serious ongoing shortage of such anti-venom in PNG, the obvious outcome would be an increase in the number of lives lost to snakebite in a country where annual deaths are measured in the thousands.

Williams denied re-selling the anti-venom on the black-market, instead stating that he had used the missing vials for live-saving work, as in treating bite victims.

The explanation was rubbished by McGuiness who stated that there was no evidence to back up the Williams claims.

To date Williams has not provided any detailed explanation to rebut the McGuiness claims.

Furthermore, Williams had been shown on the ABC TV programme "*Foreign Correspondent*" masquerading as "Dr", David Williams even though he was not a medical practitioner of any form. Furthermore he had no PhD or other similar qualification allowing him to use the title "Dr" to describe himself in the footage filmed at end 2007 and screened in early 2008.

Several news reports in PNG and Australia also saw Williams identified as being involved in a fraudulent act of improperly arranging the import of Indian anti-venom, ostensibly to be used to treat snakebites in New Guinea.

Such anti-venom is useless on PNG snakes and PNG, Port Moresby City Pharmacy boss Mahesh Patel condemned Williams and his agents for promoting it's use or availability in New Guinea, saying that his activities could put lives at risk (see Marshall (2008) and material cited therein and Staff Reporters 2008)).

At the time the debacle emerged of the improper importation of the wrong anti-venom emerged, David Williams justified the importation and ordering the anti-venom on the basis he was planning a trip to regions to the west where such anti-venom may work on some of the local species and hence was a better alternative to having nothing.

EARLIER INCIDENTS INVOLVING REPTILE SMUGGLER DAVID WILLIAMS

Williams was also the principal of a now defunct enterprise called "Austoxin".

Set-up in around 1994/6 ostensibly to save lives in New Guinea by supposedly supplying venom to make anti-venom, the enterprise actually turned out to be a highly organised reptile smuggling racket that intended to illegally send reptiles out of the country to supply an illegal global reptile trade.

When it collapsed, the debacle was widely reported in

the PNG and Australian tabloid media at the time and labelled potentially the largest reptile smuggling racket in PNG history with the unwitting involvement of the then Deputy PM who was also apparently duped by Williams.

Williams blamed the debacle on his business partner Wayne Lewis, who in turn blamed Williams.

Regardless of who was to blame, Williams fled the country.

In a widely circulated statement made on 17 December 2007 (Lewis 2007a), Lewis wrote:

> "My name is Wayne Lewis and I was one of the founding Directors of Austoxin
>
> P/L and a Director of Austoxin (PNG) Ltd. I ran the exhibits in shopping
>
> centre's in Australia during the 94/95 period and made ALL of Williams
>
> reptile transactions on his behalf. I then spent a year in Port Moresby in
>
> total limbo both during and after Williams fell out with Ed Jones, John
>
> Ellsworth and Chris Hiaveta the then deputy PM of PNG. A bit of research
>
> will comfirm these facts.
>
> I can attest to all of Williams illegal transactions during the period as
>
> well as drug importation from PNG to Australia by someone who I've read is
>
> now Williams business partner."

A letter by Lewis sent via e-mail and hard copy, dated 18 December 2007 (Lewis 2007b) sent to the Australian Broadcasting Corporation (ABC), not widely disseminated is printed below for the benefit of the public record.

So that I am not accused of misquoting the letter, it is reproduced in full.

> "From: Rocky Guyforfun <rockybloke@gmail.com>
>
> Date: 18 Dec 2007 22:18
>
> Subject: URGENT. Upcoming episode relating to DAVID WILLIAMS in PNG.
>
> To:
>
> foreign2@your.abc.net.au
>
> Dear Sirs.
>
> This is a rather long winded summary but I implore you to read it
>
> thoroughly.
>
> This is a basic narration of my associations with DAVID JOHN WILLIAMS that
>
> involves both conspired and direct criminal activity. I have
>
> original documentation to prove any and all claims made in this
>
> correspondence.
>
> I have been following with keen interest the activities of David John
>
> Williams and in particular the press surrounding his project in Papua New
>
> Guinea. I understand that your program is dedicating time to an episode on
>
> the above mentioned. I feel it necessary, after viewing a 60 Minutes episode
>
> recently on the same subject, to raise concerns regarding the portrayal of
>
> Williams as an all round nice guy dedicated to the salvation of the people
>
> of PNG with regard to snakebite..
>
> I was involved with Williams in a venture in the mid nineties called
>
> Austoxin Pty. Ltd and Austoxin (PNG) Ltd. The primary aim of the companies
>
> was to further the research of venom components for medical applications.
>
> Further aims were to provide educational displays to the public and schools.
>
> A partnership was entered into with Sydney University under the direction of
>
> Prof. Richard Kristopherson. (spelling error) to provide whole dried venom
>
> for research purposes. The company recruited numerous private shareholders
>
> and other stake holders. The founding directors were David Williams, Wayne
>
> Lewis and Laurie Haddrick. All of Cairns, Qld. The company started way under
>
> capitalized and things went down hill fast financially. Williams basically
>
> lived from the company bank accounts and the company premises were always up
>
> market residential rental properties. I was in charge of the travelling
>
> displays and PNG company. I was later accused of fraud by Williams and
>
> slandered in emails by him at the time with regard to the PNG company. All
>
> since proven false.
>
> The PNG company was incorporated with Williams, Lewis and John Ellsworth as
>
> directors. The aim of this company was to collect animals to produce venom
>
> for both the World Health Organization and

Sydney University. Also the export of native fauna to the USA through an American fauna dealer Ed Jones was planned and implemented. The plan was to get PNG nationals to capture large quantities of native reptiles, amphibians and mammals for dispatch to the USA via Jones. Initially this was to be done in accordance with the CITES agreement at the time and the then Deputy PM. Chris Hiaveta was the major financier and was able to pull strings when questions were asked. In the end no animals were sent to the USA as Williams fell out with Hiaveta's representative Ellsworth and Jones over the export of animals. Williams and Jones were for the export.

Hiaveta and Ellsworth were against due to mounting political pressures in PNG and the Police Superannuation Fund scandal that Hiaveta was embroiled in.

I ran the PNG company and was under the direct instruction of Williams and Jones. Initially enclosures were sent from Australia and set up in a Port Moresby warehouse. Numerous specimens were caught by the initial group of Australian expats including Williams, Lewis, Brian Starkey. Specimens included were Chondropythons, Carpet Snakes, Northern Tree Snakes, Papuan Taipans, Small Eyed Snakes, Monitors and Gekkos. All of which I still have photographs of inside the enclosures at the PNG warehouse. Only two of those species are of any use for venom research. Williams instructions to me upon departure to PNG was to get the export of the reptiles moving as fast as possible to provide funding for the Australian operation which by that stage was in dire straits financially.

The fact that PNG did not allow the export of native fauna under the CITES agreement was generally considered by Williams et al something to be overcome by Hiaveta. I was recruited by Ellsworth and Hiaveta to continue the PNG operation without the export side of it. However due to family health issues in Australia I returned to Australia leaving Austoxin and severing any and all association with Williams. I was a very naive person to be involved in such a level of business at the time and relied on Williams apparent expertise. This was found to be misplaced trust as with Williams appearances are often deceptive.

Williams may be on a noble crusade these days however his past is exceptionally blemished with criminal offences against fauna and trade in fauna in Australia. I acted as his middle man in the mid nineties and made numerous illegal reptile sales on his behalf, using his licence, to some prominent amateur herpetologist in QLD and Victoria. He swapped Dept. of Environment and Heritage implants from his captive bred animals to wild caught specimens and sold these and their wild caught offspring, though me, on numerous occasions. I was prosecuted by the DEH in 1995 for illegal movement of Williams animals, on his license, to a movie shoot for the movie All Men Are Liars. My signature is on all movement documents from 1994 to mid 1996. Williams himself was convicted of cruelty charges in 1997 relating to rotting animals found in the former Austoxin warehouse in Bolton St Cairns by a DEH raid. I was interviewed by Mike Chepp from DEH and provided my opinions on the state of the animals at the time. Williams was fined some $7500 and a conviction recorded.

David Williams is a very personable chap who exudes confidence and sincerity, however I have seen the other side of his persona and believe me, though he may well be giving his full commitment to his research in PNG, he is capable of great deception and has always been driven by his ego and need for professional recognition. This overrides all other aspects of David.

> I can be contacted on this email address
>
> Your sincerely
> Wayne Lewis."

Also obtained was a raft of supporting documents, including many from David Williams himself, which confirmed the detail of the above, including that Williams had unsuccessfully raised obscure legal arguments as an appeal defense against his convictions and fines for culpable cruelty to reptiles and smuggling (Williams 1997). The appeal failed with all fines and penalties being re-imposed.

Before the Austoxin debacle, David Williams had pled guilty in Australia to smuggling reptiles in the post in an unrelated incident.

In another incident, David Williams went to a company trading as "Network Rentals and Rent A Ute", where he hired a truck to use for a reptile demonstration. According to a statement by a debt collector,

> "The truck was reported stolen after a few weeks, the police caught Williams
>
> driving it, but did not do anything as he paid by cheque and it bounced so
>
> they said it was a civil matter now."

At the end, Williams wasn't pursued for the debt as he lacked assets and the truck itself had been recovered intact.

See Woolf (2008) for details.

The details of these and other Williams incidents are all beyond the scope of this paper, but readily accessible via court files, news clips of the relevant times.

A mere fraction of these are listed in the bibliographies in Hoser (1993) and Hoser (1996).

HOLIDAY INN COMPETITION AND VOTE RIGGING EXPOSED

At end 2007 and early 2008 David Williams decided to promote himself as some kind of unsung hero, saving people from death by Snakebite in Papua New Guinea.

He successfully got funds from the "Australian Venom Reearch Unit" (AVRU), in Melbourne for what are best described as "collecting trips" and the like.

He solicited and duped the ABC TV's "*Foreign Corespondent*" into doing a favourable story about him that was later shown to be fraudulent (see elsewhere in this paper), including what a number of herpetologists speculated was the alleged faking of a Taipan bite (the bite was not shown on camera, immediately arousing suspicions as every other part of the alleged event was shown and Williams made an apparent near "instant" recovery by the next day, and made inconsistent statements in terms of availability of anti-venom on the ABC broadcast and on internet forums including www.aussiereptilekeeper.com, the latter of which has said he had spare stored at his facility).

Then there was the already mentioned making false statements to acquire a special order of Indian snake anti-venom for resale in PNG, even though it was of no use to local species.

Peter Lloyd, an ABC work colleague of the reporter who worked with Williams in the New Guinea story, was shortly after, in July 2008 caught and prosecuted for Drug Trafficking in Singapore. He pled guilty to three drug-related offences, including possessing 0.41 grams of methamphetamine, or "ice" and was sentenced to 10 months' jail on 2 December 2008 (Meade 2008).

Also following the making of the ABC report, Williams was also exposed for improper conduct elsewhere as part of his broad campaign to masquerade as a life-saving hero from New Guinea.

In early 2008, Williams and associates, Wolfgang Wüster, Wulf Schleip, Al Coritz and Mark O'Shea spammed internet sites and most major internet reptile forums seeking people to vote for him as a so-called "Everyday hero" in a contest where the winner got a free all inclusive holiday at a hotel run by the Holiday Inn group valued by them at US$20,000.00.

Wüster posted on UK sites and others including http://www.reptileforums.co.uk inviting reptile enthusiasts to post multiple votes for Williams (see Wüster 2008) being touted as "one of us".

Williams and the same crew that usually devote their endless hours of spare time attacking myself had found a new cause to promote and as their actions didn't impinge on me, it was a useful diversion for them in my view.

My only regret is that the competition didn't run for several years!

The history of the competition is thankfully recorded on the archived posts of the many reptile forums easily searchable via the internet and not yet deleted or edited.

As the competition progressed Williams found himself being outvoted by another man from Pakistan and so Williams and others stepped up the campaign for votes continuing to call for people to register multiple votes and even encouraging people to register fake e-mail addresses solely to bolster votes for Williams.

One of Wüster's students posting under the name of "Gaboon" on http://www.reptileforums.co.uk even sought higher marks from his University teacher (Wüster) if he voted for Williams (Gaboon 2008).

The Gaboon post followed numerous repeated pleas for assistance by Wüster on the same forum and others.

On the UK forum there was a general disinterest, so Wüster repeatedly had to "bump" the thread to make it seen (in at least one case merely posting the word "bump"), or otherwise it'd have dropped off the main front page of the site, making the thread less likely to be seen by third parties.

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

Which read:

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

However it appears that the help wasn't all above board, with Williams actually being disqualified for vote rigging as identified by the Hotel Chain running the contest.

In order to beat the main competitor, Williams or someone working on his behalf had illegally inflated his vote tally near the end by improperly adding a massive 4,000 votes in order to get him over the line as alleged "winner".

Based on a separate post by Williams on http://www.reptileforums.co.uk (and many others) he implied that the fraudulent votes had come from a single computer (see Williams 2008), which seems to be patently obvious in hindsight, especially noting the skills in false and cross-posting Williams and associates has developed over the preceding ten years.

It also emerged that Williams also faced potential disqualification for making a false claim about himself on the Hotel chain's own website http://www.holidayinneverydayheroes.com/readmore.aspx?id=57&page=1 which also happened to be against the Hotel chain's guidelines

As mentioned before, working with Williams in this fraudulent debacle were his close friends, Wolfgang Wüster, Mark O'Shea, Wulf Schleip, Shane Hunter and Al Coritz.

Coritz even went to the extent of filming and posting a video on "youtube" (at: http://au.youtube.com/watch?v=QzgluS-tIKc) of him ranting on, solely for the purpose of calling on other reptile enthusiasts to vote for Williams.

Coritz is better known to herpetologists for the squalid conditions he kept a wild-caught Taipan through another video he posted on "Youtube" at: http://au.youtube.com/watch?v=ujBiDuIoYgM.

This shows a thin snake at his home covered in exo-parasites, as a result having failed to shed properly in an unventilated cage replete with an inappropriate turned up and spilt water bowl, creating a bacteriological cocktail of a nightmare as the by-product it is shown mixed with an inappropriately wet substrate and uncleaned faecal matter strewn across the cage in a room with loose electrical wires forming a potential death trap for both snake and handler!

While one may ask what the relevance of this hotel competition fraud has to do with reptile taxonomy and the like, it goes to show how this group of men will use improper means to peddle views, including to make out that they are more widespread than is actually the case.

In the case of the Hotel competition, Williams managed to garner at least 4,000 votes for himself, with the obvious aim and intention to mislead innocent persons and to form a false perception that there was a groundswell of independent people in support of him, which was never the case.

His actual support base was at best a mere handful of people.

There is absolutely no doubt at all that following publication of this paper that Williams, Schleip and Wüster will post material contrary to the facts and views that are in this paper including under fake ID's, as well as use their influence to improperly censor out balancing viewpoints on forums that they control.

This will be done in order to lead to a false perception that their views are those of the majority of herpetologists, which quite clearly they never have been.

SUCCESS BY SCHLEIP, WÜSTER AND WILLIAMS IN MISINFORMATION CAMPAIGNS

Unfortunately these men continue to run their warped campaigns because at times they do have a degree of success and it is this level of success that is cause for concern, as it relies on tactics of bullying and censorship, rather than persuasive and valid arguments.

To give an accurate appraisal and motive for their improper actions, some further instances of their actions should be related.

The major taxonomic act of Hoser 2004a was the erection of a genus for the *reticulatus* pythons, transferring them from "*Python*" to a new genus, "*Broghammerus* Hoser 2004".

Essentially adopting diagnostic characters derived from earlier authors, most notably McDowell (1975), the most notable thing about the designation was that no one had attempted it earlier, which was point raised by a number of independent commentators.

The morphological and behavioural differences between the Reticulated and Indian/Burmese pythons is stark and for them to be placed in separate genera

made eminent sense.

Following publication of Hoser 2004a, Schleip and Wüster true to usual form stalked the internet in usual fashion and bullied people into not using the name *Broghammerus*, including in places like www.kingsnake.com and elsewhere.

On 24 July 2004, I posted at http://forums.kingsnake.com/view.php?id=520074,520074 (Hoser, 2004d) advising merely of the publication in accordance with the ICZN rules.

The relevant section is:

> "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."

Immediately, Schleip, made repeated posts here and elsewhere specifically discouraging persons from using *Broghammerus*, but without providing any sensible reasons for the position. (See Schleip 2004b, 2004c).

Schleip of course was joined by Wüster on the same forum, who supported his position in favour of non-usage of *Broghammerus*, again without providing any sensible reason, but nevertheless making considerable noise, (see Wüster 2004a and 2004b as examples) with numerous similar posts on other sites made by both men, whenever reference was made either to the Hoser paper or the name *Broghammerus.*

As a result of their bullying and vigilance in stifling dissent, the name did not get widespread usage.

Google searches as of early 2008, showed that without exception, whenever the name "*Broghammerus*" was raised on any internet forum (anywhere in the world), Schleip, Williams and Wüster would descend on the thread to condemn use of the name and flame and bully anyone who supported it, including forcing supportive posts to be deleted, in order to present a false view that the use of *Broghammerus* was not generally supported.

The men would invariably refer in their posts to the online version of Wüster et. al. 2001, posted on Wüster's own university-funded website, the alleged (and long discredited) facts in the paper being justification not to use Hoser-names.

In 2008, Rawlings et. al. independently and without any input from Hoser, published their own paper that using mtDNA data, not surprisingly confirmed the Hoser 2004a position and adopted the use of *Broghammerus*, extending it to include *timorensis* (a taxon with which I have little expertise), that action being the significant taxonomic move in the paper.

Noting that Wüster and Williams have in the past been ruthless in stopping publications in favour of the Hoser positions, including harassing and intimidating journal editors, it's fair to assume that neither were aware of the paper's imminent publication or the central conclusions.

None of, Wüster, Williams, Schleip or close associates, Fry, Coritz, (Peter) Mirtschin or O'Shea are listed in any way as being consulted or assisting in the paper in the acknowledgements, which is notable, as had any been aware of the paper, they'd almost certainly have tried to stop it's publication as they have done previously.

This paper effectively undermined the Wüster et. al. claims that "Hoser' was a useless and clueless amateur (Wüster et. al. 2001), who's taxonomy should be forcibly suppressed and ignored (again see Wüster et. al. 2001), thereby leading other herpetologists to accept the Rawlings et. al. position and adopt *Broghammerus* for the *reticulatus* group.

Wüster, Schleip and Williams continued to stalk the web and "flaming" anyone who dared use the term "*Broghammerus*" including through the use of assumed names, but eventually the tide became overwhelming, as had occurred some years earlier, when Wüster had fought a losing battle against the acceptance of *Acanthophis wellsi* Hoser 1998 (see details of Wüster's campaign about this in Hoser 2001b).

The comments during this campaign were to say the least improper, like for example:

> "Raymond Hoser should be banned from EVER having a scientific description considered as valid",

posted anonymously on: http://www.albertareptilesociety.org/forum/archive/index.php?t-963.html on 23 Feb 2008, or a post by Wüster ("in person" this time) on 2 December 2008 on an obscure South African reptile forum at: http://www.sareptiles.co.za/forum/viewtopic.php?f=5&p=104864 where he said that he'd never in his life use the term "*Broghammerus*", (Wüster 2004c).

There is no doubt that as for other Hoser-named taxa that manage to gain widespread acceptance in spite of the bullying and misinformation by Wüster/Schleip/Williams, their campaign of hatred will descend to the usual mud-slinging and false claims.

These will be along the lines that Raymond Hoser stole all the research work out of someone else's filing cabinet and deliberately "scooped" them in naming the taxon/taxa before they could do so.

THE HISTORY OF THE WEBSITE WWW.LEIOPYTHON.DE

In 2001, a private snake hobbyist by the name of Wulf Schleip from Germany, created the website www.leiopython.de. Here he professed to disseminate

information on the genus *Leiopython,* which happened to be the genus/species of snake he was keeping at the time.

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

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

Unfortunately, and presumably as a result of his frequenting similar internet chat groups to the convicted smuggler David Williams, Schleip soon became a close friend and associate of him and Wüster, generally offering support to Williams whenever he "flamed' or attacked others and of course in the ill-fated Hotel Competition detailed above.

Significantly in the context of this paper, from at least 2004, and after a series of posts on webforums, including "www.kingsnake.com" by Wulfgang Wüster and convicted smuggler David Williams, Schleip amended his site to deny the legitimacy of the taxon *hoserae*, variously declaring it "*nomen nudem*" in numerous places and also stating that the southern black "race" regularly climbed the central range of New Guinea to hybridise with the Northern "race" of *L. albertisi* (Schleip 2007b).

Put simply, he joined the David Williams campaign of lies and hate against "Hoser".

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

Based on the mtDNA material in the Schleip 2008 paper, we now know his 2004 statement to be totally dishonest, which must therefore make everything else Schleip writes similarly questionable and worthy of closer assessment before accepted as "correct" as would commonly be the place after a sizeable taxonomic treatise is published.

While either of Schleip's "new" 2004 concepts are patently ridiculous, there was no means or for that matter reason for myself to try to change or remove the offending material.

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

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

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

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

and the edit history is publicly available via a link on the page.

As late as 12 December 2008, that site read as follows:

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

For the record, in terms of all the Hoser descriptions of taxa, they most certainly conformed with the relevant "Rules" as published by the ICZN (ICZN 1999).

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

Furthermore, while anything is possible, it seemed unlikely to me that a forest-dwelling python would be able to climb extremely high, sometimes snow-capped hills of the New Guinea central range to find other snakes to breed with, especially as in over 100 years no one has ever found any snake that is apparently intermediate or hybrid in any way to the taxa *L. albertisi* and *L. hoserae*.

The Schleip website and comments by Wüster, including those he published in *Litteratura Serpentium* in 2001, were in the materially relevant times clearly an attack on Hoser as opposed to any credible scientific assault on the taxonomy or nomenclature of the relevant Hoser papers.

As it happens, all major taxonomic conclusions (and following on nomenclature) of the Hoser papers, have been corroborated by independent studies of other herpetologists and generally been viewed by them as conservative.

The list runs broadly as follows:

> Hoser 1998/2002 *Acanthophis* taxonomy (confirmed by Aplin and Donnellan 1999, Wells 2002, bootlegged and agreed by Fry et. al. 2002 and Wüster et. al. 2005)(also see support from Starkey 2008 dating back many years)
>
> Hoser 2000b/2003a/2004a *Python* Taxonomy (confirmed by Rawlings and Donnellan 2003 ("*Chondropython*"), Rawlings, et. al. 2008 ("*Broghammerus*" and other genera), O'Shea 2007a, 2007b ("*Leiopython*"), Schleip 2008 ("*Leiopython hoserae*" and other), Wells 2005

("*Morelia*" Carpet Pythons))

Hoser 1998/2000a/2001 "*Pseudechis*" group taxonomy (confirmed by Kuch, et. al. 2005, bootlegged and agreed by Wüster et. al. 2005)

Hoser 2001a/2001b *Pseudonaja* taxonomy (bootlegged and agreed by Williams et. al. 2008)

For other Hoser-named taxa, e.g. *Tropidechis sadlieri* Hoser 2003 (Hoser 2003b), which are generally easily and consistently diagnosed species (there has never been public argument in terms of the original findings)(See J. Craig Venter Institute 2008).

Of significance to this paper is that as of late 2008, Schleip's website was still peddling the line that the Hoser taxonomy for the genus *Leiopython* was wrong and that all could be assigned to a single species.

Also of note is the consistent (opposing Hoser) position of Schleip (and Williams and Wüster), no matter how absurd the opposing position actually is.

All three men control websites running anti-Hoser petitions, the main one as of 2006-8 being one against Raymond Hoser being allowed to own or possess venomoid (devenomized) snakes for the purposes of being able to do educational wildlife demonstrations without putting the public at risk.

In terms of that petition and websites associated with it, the three men have peddled countless lies, including most seriously that the Hoser venomoid snakes have regenerated venom and are dangerous.

After a video of numerous world's deadliest snakes, venomoid snakes biting Hoser (with no effect) appeared on "youtube" these men and/or associates petitioned "youtube" to have the video removed, the actual reason being it made a mockery of their lies.

On 24 December 2008, when I posted material on websites calling for an end to the sale and use of "glue traps" to kill snakes in Australia, the "Hoser haters" posted material on "www.aussiereptilekeeper.com" in support of the continued use of the traps (see Hunter 2008) on the same day, which remained unchallenged (for at least a fortnight) solely on the basis that the position was opposite to the Hoser one, with Schleip being a poster on and official sponsor of the site/server/s at the materially relevant time, including on 28 December 2008!

THE SCHLEIP 2008 PAPER ON *LEIOPYTHON*

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

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

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

(cited here as Schleip 2008c).

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

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

The site's relaunch was based around the simultaneous (within days prior) publication of his 2008 paper, broadly accepting the Hoser taxonomy and in turn "creating" three new species of Brown *Leiopython* from the northern New Guinea region.

On 28 December 2008, he posted details of his newly published paper on *Leiopython* taxonomy on the website www.aussiereptilekeeper, a site moderated by the convicted reptile smuggler David John Williams, whose main reason to exist is to attack Raymond Hoser (this author) and numerous other places at advertise his new paper and new "species".

As inferred earlier, Williams cannot be sued for defamation due to his lack of assets.

A search of the internet yielded abstracts of the paper only, (at: http://www.bioone.org/perlserv/?request=get-abstract&doi=10.1670%2F06-182R5.1) see Bioone (2008), however I was able to acquire a photocopy of the publication through a Museum-based subscriber to the *Journal of herpetology.*

The abstract was quite definitive in stating it's basis for diagnosing and describing new taxa of *Leiopython*, including mtDNA, which one would reasonably assume would be for those species that may otherwise have a questionable diagnosis. However a read of the paper itself had the data revealing a different picture to that inferred in the abstract and essentially no different to that of Hoser 2000b.

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

The differences in essence were a more vitriolic attack on myself and less editorial discipline leading to his inadvertent and inconsistent statements including some on various webpages stating that all the northern white-lipped pythons are of the same species, namely "*L. albertisi*"!

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

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

He was also apparently active at wikipedia, this time

apparently making anonymous edits to webpages for *Leiopython*. This time however he was changing the pages to indicate all new taxa as recognised in his 2008 paper. That Schleip was the editor was revealed via a reverse IP address search giving the European address of his internet gateway and seeing that it matched his footprint elsewhere on the web.

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

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

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

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

Regardless of how badly either that or his webpage are written, whether or not his newly "created" species are actually valid ultimately turns on the evidence and it is this that is herein assessed and found to be lacking.

THE *LEIOPYTHON* SPECIES

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

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

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

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

None of the above so far made the Schleip paper notable in any way, or for that matter worthy of comment. However what was worthy of analysis here was the dramatic move by Schleip of creating three new species of Brown *Leiopython*, namely *L. fredparkeri* and *L. huonensis* from the mainland New Guinea population of *L. albertisi* and *L. biakensis* for the specimens from the Island of Biak.

MtDNA EVIDENCE

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

He wrote:

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

However his paper provides no DNA evidence whatsoever to separate any of his newly named northern taxa from one another or for that matter from the nominate race of *L. albertisi*.

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

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

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

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

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

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

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

substantive and obvious.

In the case of *L. hoserae*, versus *L. albertisi*, the need to look at mtDNA to confirm the validity of the species designation was slight. The two taxa are obviously very different, obviously allopatric, being split by a very substantive barrier and hence obviously different species (see for example O'Shea's comments on this in O'Shea 2007a).

By contrast all the northern *Leiopython* are essentially similar in most respects, as conceded by Schleip, not divided by any obvious and permanent barriers and hence are the obvious targets for mtDNA analysis as inferred in the widely disseminated abstract, but not delivered on in the actual paper.

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

SEPARATION OF THE THREE SCHLEIP CREATED SPECIES

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

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

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

In the 2008 paper, Schleip relied heavily on so-called "Operational Taxonomic Units" to allege what he called "geographically isolated or disjunctive populations", later also used to separate his new "species".

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

Hence, the end point as stated in his paper for defining these new "species" using his relatively newly invoked "evolutionary species concept"(or ESC) is that his species populations are genetically isolated from one another by being distributionally disjunct.

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

Those authors (Harvey et. al. 2000) found that by molecular analysis the Scrub Python snakes from Biak were effectively identical to those from nearby mainland New Guinea. Hence it'd be expected a similar situation would exist for the White-lipped Pythons (*Leiopython*). Furthermore, noting the findings of Harvey et. al. were published eight years earlier and known to Schleip, it'd have been incumbent for him to provide contrary data for his own new taxa from the same place.

Schleip has not done this!

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

Alternatively, has he chosen not to investigate where it may lead to findings contrary to that which he seeks to publish and disseminate.

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

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

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

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

The question then begs, are these minor scale differences observed in pitifully small samples of snakes sufficient grounds to split these snakes off as a separate species?

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

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

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

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

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

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

Again, Schleip has chosen to exclude snakes found in regions between these newly created "species" as they would almost certainly be clinal (again) to those he has named.

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

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

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

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

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

It's also noted here that assuming this trait to be unique to those specimens, it may not be consistent among others from Biak!

Schleip also stated:

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

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

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

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

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

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

With many hundreds of islands offshore to New Guinea, many separated for less than 12,500 years, you can see the potential for a taxonomic nightmare emerging, not just for herpetologists, but all biologists, in terms if the idea of naming all island populations full species ever takes hold.

However such an outcome will keep editors of

publications like "*Zootaxa*" busy for a long, long time!

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

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

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

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

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

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

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

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

DOES WULF SCHLEIP REALLY THINK THE BROWN *L. ALBERTISI* ARE REALLY SEVERAL SPECIES?

Frankly I doubt it.

After one analyses the statistical gymnastics of Schleip and one allows for the unaccounted for specimens from the mainly uncollected parts of island New Guinea, his excluded samples and the like, it becomes clear that it'd be difficult for a herpetologist to accurately assign a random brown *Leiopython* to any specific taxa as identified by Schleip in his 2008 paper.

Put simply, there are too many question marks.

As it happens, Schleip himself seems unable to do this for specimens analysed in his own paper for which he states he is unable to identify provenance.

Perhaps more tellingly is his website that he revamped and reloaded in December 2008.

On a number of his web-pages he talks about the husbandry of White-lipped Pythons" and on these he always splits the snakes into just two species, namely the "southern white-lipped python *Leiopython hoserae"* and the "Northern White-lipped Python *Leiopython.albertisi"*, the latter of which is discussed as a single species and never with reference to his myriad "new" taxa.

A "BIG NAME HUNTER" IS ISOLATED

Ironically it was Schleip's colleague Wüster in 2007, who spoke to an editor of the journal "*Nature*" for an article later printed and titled "Big Name Hunters" (Borrell 2007).

In the poorly written article Wüster spoke of so-called "amateur" (defined by himself as not being on the government payroll) taxonomists "naming" species in a rush so as to get a "big name" for themselves or to "scoop" competitors.

Wüster was as always attacking Hoser, describing the Hoser papers as "shoddy descriptions" and making what he knew to be the false claim that Hoser had deliberately scooped Aplin to name *Acanthophis* wellsi.

The *Nature* article was poorly written, having liberal quotes from Wüster, with myself never being interviewed or even aware of the article until the time of publication. The lack of balance in the article was perhaps best seen in the citations, which liberally referred to the Wüster criticisms of the Hoser papers, while failing to cite a single paper from myself (or for that matter many of the others by others independently supporting the Hoser position).

Notwithstanding this lack of balance, the reporter managed to state about myself that "There's no one in history that's spent so much time dealing with, looking at catching and breeding Death Adders" and that the description of myself (Hoser) as an "amateur" is "complete rubbish".

The amazing part of this attack on myself by Wüster was that at the time he was still actively bootlegging my papers and their findings in his various publications, including making numerous false claims of "firsts".

One was in a 2005 paper (Wüster et. al. 2005), where he made the audacious "discovery" that *Acanthophis praelongus* was restricted to Cape York and not across northern Australia as formerly thought.

Of course the same position had been established by Hoser 1998, and confirmed by Hoser 2002 and Wells

2002, putting Wüster et. al. third in line to have made the "discovery"!

This particular series of lies by Wüster et. al., isn't the key part of the "Big Name Hunter" story.

More significant is that at the same approximate time, it was his mate Wulf Schleip who was actually guilty of the sin of rushing to print names of taxa without sufficient evidence, or as Wüster, Schleip and Williams repeatedly claimed (for myself) descriptions lacking testable evidence.

Furthermore it seems that Wüster was aware of the fact that Schleip's new "taxa" are on questionable grounds.

In the acknowledgements, Wüster is gratefully thanked by Schleip for revising drafts of the manuscript. Yet he is not listed as co-author as one would expect. This is significant as Wüster, being an academic based in Wales, usually rushes to have himself listed as a co-author in papers of taxonomic nature (see citations here and elsewhere, including his own website at: http://biology.bangor.ac.uk/~bss166/, which incidentally has a single banner advert that links to Shane Hunter's anti-Hoser petition at: http://www.aussiereptileclassifieds.com/phpPETITION/, itself a rich source of lies and misinformation about myself (Hoser) and venomoid (devenomized) snakes produced at our facility, and also proudly boasts Schleip as an "official sponsor").

Similar occurs with the convicted reptile smuggler, David John Williams, recently fined $7,500 for animal cruelty and smuggling, who is also one who usually jumps at the chance to be listed as co-author. Schleip gratefully thanks him, but again does not list him as co-author.

Schleip's summary in his paper states that he has failed to look at intermediate and perhaps clinal populations. Schleip's summary admits to looking at mtDNA for the Black *Leiopython*, long recognised as separate from the rest, but fails to provide similar mtDNA data for his alleged new taxa and admits that such work would be necessary to confirm the taxa.

Surely this basic work should have been done before he rushed to print and put names to alleged taxa and not left to someone else to either validate or repudiate his own position.

Or was it merely a case of Wüster's mate Wulf Schleip being guilty of rushing to be a "Big Name Hunter!" as written about in the journal *Nature.*

Even more amazing is that the editor's of a journal such as the *Journal of Herpetology*, actually allowed such premature and sloppy work to be published!

One may guess that with so many lies, damned lies, or statistics, that the editor in chief may not have read the devil in the detail.

"DILUTION" OF A SPECIES – THE END POINT OF TAXONOMIC EXAGGERATION

The relative importance of a species is diluted when one becomes many. If the change is warranted, so be it. However in the case of the Brown *Leiopython* there seems to be a case of so-called "taxonomic exaggeration", whereby the significance of minor differences are being exaggerated in order to push a group of snakes over the line in terms of being more than a single species.

As seen in Orchids (Pillon and Chase 2006), we may see in snakes such as *Leiopython* excess funds and resources being devoted to the conservation of alleged taxa that don't really warrant it, such as perhaps for a regional group, that in real terms may not be significantly different to those elsewhere.

Noting the already stretched resources in terms of conserving threatened reptiles, taxonomic exaggeration by Schleip in terms of the brown *Leiopython* is not just against the sane principals of modern taxonomy, but also potentially against long term reptile and wildlife conservation efforts if such misconduct is allowed to go on uncondemned.

In Australia, we already are seeing the ill-effects of taxonomic exaggeration diverting funds away from more meaningful projects.

In Victoria for example the local wildlife department DSE, is spending vast amounts of money counting numbers of Carpet Pythons (*Morelia metcalfei*) from this state, where they are only found in a small part of the state and hence have a "rare" listing, on the alleged basis of alleged differences to specimens found north and east of Victoria where they remain generally widespread across most of NSW and nearby parts of SA and Qld.

Broadly speaking the taxon is under no threat and the efforts spent counting local Victorian snakes could be far better spent on other projects.

If the creation of *Leiopython* "species" that satisfies an innate urge by hobbyist snake keeper Wulf Schleip for self gratification becomes widely accepted and adopted, a potential outcome may be other hobbyists rushing to print with poorly constructed descriptions that end up clogging herpetological journals with dodgy statistical analysis and the like to literally "baffle readers with bullshit" in order to get gratification of pseudo-species with their names attached seen in other third party publications.

IS *LEIOPYTHON ALBERTISI BARKERORUM* "*NOMEN NUDEM*"?

Both on his revamped (December 2008) website and in his 2008 paper, Schleip states that *Leiopython albertisi barkeri* Hoser 2000 is a "*nomen nudem*".

The only positive of this argument by Schleip is that

for the first time ever, he'd put in print that the other named "Hoser taxa" are in fact validly named, contradicting earlier versions of his website, as well as his many 2007 "edits" of "wikipedia" or for that matter Schleip (2004)!

Just as Schleip has quoted out of context and misrepresented facts to get to a predetermined and preferred position with his newly created "species", it appears he has similarly done the same thing to arrive at his desired position that the Hoser name is a *nomen nudem*, even if the facts don't necessarily support his claim.

To simplify things, I shall reprint in full the original diagnosis from the original description in Hoser 2000b. The undisputed Holotype details and the like are excluded here, even though under the ICZN rules such details are mandatory.

> "DIAGNOSIS: This is the subspecies of *L. albertisi* that is endemic to Mussau Island in the Saint Matthias Group, Bismarck Archipelago. It is separated from *L. albertisi albertisi* by the mutually exclusive distribution and by analysis of mitochondrial DNA. Ventral counts for this species are near the lower limit for the range for New Guinea *L. albertisi.* The trait may be used as a potential indicator for the subspecies in the absence of other data. Other scalation counts and properties also overlap with those of the type subspecies."

Schleip 2008a claimed that the diagnosis didn't comply with Article 13.1.1 of the code, saying "yet Hoser (2000) had failed to provide evidence for these statements". He then said:

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

In other words, Schleip claimed the name was *nomen nudem* on the basis my diagnosis lacked evidence!

However article 13.1.1 of the code makes no such mention of evidence or characters.

The relevant part of the code in fact reads:

> "Article 13. Names published after 1930.
>
> 13.1. Requirements. To be available, every new name published after 1930 must satisfy the provisions of Article 11 and must 13.1.1. be accompanied by a description or definition that states in words characters that are purported to differentiate the taxon"

The significance is that the diagnosis itself does not need to be correct or have evidence to support it! The word "purport" is not defined by the code, but most dictionary definitions are similar. From the *Webster's New Twentieth Century Dictionary*, published in 1970, by the World Publishing Company, USA, (Various 1970) the definition is:

> "purport. Anglo/French
> 1/ To profess or claim it's meaning
> 2/ To give the appearance of, often falsely of being, in lending, etc."

As it happens there are countless examples in zoology of validly described taxa having diagnoses that are totally wrong or false and yet the names remain 'available" under the code, provided the other essential ingredients such as a name bearing holotype are met.

In other words, in my view even if the Hoser 2000b diagnosis for *L. albertisi barkerorum* is totally wrong and false, or pure unmitigated crap, the mere fact there is a diagnosis purporting something, makes the name available.

A similar situation happened recently with Wells and Wellington diagnoses for skinks in the genus *Cyclodomorphus*. Shea found the original diagnosis for a taxon (*C. michaeli*) to be wrong, but the Wells and Wellington name remained available and hence was used by the later author as the first available name. See Wells and Wellington 1983 and 1984 and then Shea 1995 for the detail.

There are countless similar such cases.

In other words, by my interpretation of the code, the name *Leiopython barkerorum* remains available under the code (ICZN 1999).

The rest of Schleip's diatribe about *Leiopython barkerorum* is similarly wrong and repetitive and most importantly never gets near the point referred to in the online abstract to the paper, namely the mtDNA.

Harvey et. al. 2000 found a 5% divergence in mtDNA between the New Ireland Scrub Pythons (herein identified as *Australiasis duceboracensis* Günther (1879)) and those from nearby mainland New Guinea (the northern taxon being referrable to *A. amethistina*), (the genus name *Australiasis* having been proposed for the Scrub Pythons by Wells and Wellington and for consistency purposes has been adopted and used by Hoser 2000b and is preferred here at the genus level for the species complex), going on to say that each were probably a different species. Other authors (including Barker) have relied on a 3% divergence to separate three python species from the "*curtus*" group, from western Indonesia (Keogh et. al. 2001).

Noting the known location of "*Leiopython barkerorum* Hoser 2000*"* in the same general region as *Australiasis duceboracensis* Günther (1879), one would expect a similar mtDNA divergence for these snakes as compared to the mainland New Guinea animals, due to a likelihood of the snakes being affected by the same physical barriers and more importantly a known gap in the distribution of the brown *Leiopython* in the area

generally inhabited by the species *Bothrochilus boa*.

In his bibliography, Schleip 2008a claims to have read Harvey et. al. 2000 (cited as a reference), and in spite of the undisputed facts above, Schleip has chosen not to compare mtDNA between the relevant populations of Brown *Leiopython* and yet has found an innate urge to attempt "purge" the Hoser taxon "*Leiopython barkerorum"* as a valid taxon, regardless of the underlying nomenclature and yet without any real evidence.

Again I note that the amazing thing about all this, is that a journal with the status of *Journal of Herpetology* even printed such a poorly written "paper" with such gaping holes in it's methodology, leading to obvious questions about the quality of "peer review" in this instance.

STABILIZING THE NOMENCLATURE OF *LEIOPYTHON*

Schleip and associates as named have greater time, money and other resources than myself and a far greater demonstrated ability to make "noise", the upshot being that they may continue to destabilize the nomenclature of this genus for many years. In the first instance, the only sensible means to settle such "noise" may be to petition the ICZN, the process of which may take several more years to resolve.

Regardless of the arguments for or against the Hoser 2000 description of "*Leiopython albertisi barkeri*", the stabilizing of the nomenclature is important and as an alternative to a drawn out case before the ICZN, this paper seeks to stabilize the nomenclature by simpler means.

Therefore, and without reference to the Hoser 2000 description (Hoser 2000b), that taxon is described herein below as "new" and without reference to the Hoser 2000b paper, the significant net result of this action to most other taxonomists being a citation date of 2008 for the said taxon.

The relevant section of the ICZN rules (ICZN 1999) is printed below:

> ***"nomen nudum* (*pl. nomina nuda*), *n.***
> A Latin term referring to a name that, if published before 1931, fails to conform to Article 12; or, if published after 1930, fails to conform to Article 13. A *nomen nudum* is not an available name, and therefore the same name may be made available later for the same or a different concept; in such a case it would take authorship and date [Arts. 50, 21] from that act of establishment, not from any earlier publication as a *nomen nudum*."

***LEIOPYTHON ALBERTISI BARKERORUM* SUBSP. NOV.**

HOLOTYPE: A female specimen, at the Universitetets Zoologiske Museum, Copenhagen (R5444) collected by the Noona Dan Expedition, from the Island of Mussau in the Saint Matthias Group, Bismarck Archipelago, Lat: 1° 30' Long: 149° 40'. Scalation is smooth with 267 ventrals and 72 subcaudals.

PARATYPE: A male specimen, at the Universitetets Zoologiske Museum, Copenhagen (R5445) collected by the Noona Dan Expedition, from the Island of Mussau, in the Saint Matthias Group, Bismarck Archipelago, Lat: 1° 30' 149° 40'. Scalation is smooth with 271 ventrals and 73 subcaudals.

DIAGNOSIS: This is the subspecies of *L. albertisi* that is endemic to Mussau Island in the Saint Matthias Group, Bismarck Archipelago. It is separated from *L. albertisi albertisi* and all other taxa formerly attributed to this species or genus (*Leiopython*) by the mutually exclusive distribution, which is diagnostic of this taxon either alone and/or in combination with other features/ traits.

Also diagnostic of this taxon (separate or in combination with other features) is the positioning and nature of the whitish spot behind the eye. The spot is present in an upper post-ocular, but is brownish in colour, tending to yellow in the center. The scale immediately above this (the supra-ocular) has a similar brownish marking, that tends slightly closer to the eye itself and also borders the lower part of the scale. No other *Leiopython* other than this taxon has this exact scalation trait making this taxon easily separated from all other *Leiopython*.

Also diagnostic of this taxon (separate or in combination with other features) is the white barring of the lips, which rather than being whitish in colour, as seen in all other *Leiopython*, has a distinctive yellowish hue unique to this taxon.

The distinct white (or yellowish in the case of this taxon) barring of the upper labials in this taxon, which also separates this genus from all other pythons (except *Lenhoserus boeleni*, that's separated by a dorsal pattern not seen in *Leiopython*) is more extensive than for all other *Leiopython albertisi*. In this taxon, *Leiopython albertisi barkerorum* subsp. nov. , an average of 60% of the labials are "light", whereas the ratio for other *Leiopython* is 45-50%. This alone and/ or in combination with one or more other traits is diagnostic of this taxon, namely *Leiopython albertisi barkerorum* subsp. nov.

Ventral counts for this species are near the lower limit for the range for all other *L. albertisi* and taken as average counts are also diagnostic for this taxon, either alone or in combination with other diagnostic traits.

The details are as follows:

> Average ventral count for *Leiopython albertisi barkerorum* subsp. nov. is 269
>
> Average ventral count for *Leiopython albertisi* from West Papua and Salwatti Island is 278.2

Average ventral count for *Leiopython albertisi* from PNG only is 270.9

Average ventral count for *Leiopython albertisi* from the entire known range is 275.7

Analysis of mitochondrial DNA and nuclear DNA as a matter of course shows divergence of base pairs as compared to all other *Leiopython* not attributable to this taxon and is diagnostic of this taxon either alone or in combination with other diagnostic traits.

ETYMOLOGY: Named after two people, namely David and Tracy Barker of Texas. The husband and wife team have developed one of the most sophisticated python breeding facilities in the world.

OTHER DUBIOUS WORK BY SCHLEIP

Further reading of Schleip's paper shows that he has made other false or misleading statements, often in contradiction to the position accepted by most other herpetologists.

Adding to that some of the rubbish on his website, it'd be too tedious and time-consuming to list them all.

However, Hoser 2000b stated that the reports of "*L. albertisi"* (now known as *L. hoserae*) from the Australian territorial islands near the New Guinea landmass were probably false and based on misidentified water pythons. This assertion was based on the following:

· Similarity in appearance to lay people.

· A known abundance of Water Pythons in the area, including in recent (1990's and (now) beyond collecting expeditions).

· Habitat being more suited to Water Pythons and not to *Leiopython*.

· No *Leiopython* from the said island locations being lodged in museums in spite of intensive collecting in the area.

In the absence of evidence to the contrary, this position was in turn adopted by virtually all publishing herpetologists, including Steve Wilson (Wilson 2005), based at the Museum of Queensland, who of all people would know of any legitimate records of the taxon in Australian territory and has worked closely with most (legal) collectors at Sabai Island and other relevant places, and Swan (2007), accepting the Hoser (2000b) position stating, on page 18 of the book that he excluded the taxon (from his accounts of Australian pythons) on the basis that claims from Australian territory were inconsistent.

In spite of this, Schleip misrepresented Barker and Barker 1994 to claim and describe specimens from these islands.

However as Hoser 2000b had stated or inferred, the description of "Australian" *L. albertisi* was based on southern New Guinea animals that were by presumption those likely to be found on Australian territorial islands that straddle the south New Guinea coast and have been presumed to have the same or similar fauna (e.g. *Varanus prasinus*).

Being well-aware that the basis of the old reports of *Leiopython* from Australian territory were almost certainly false, or so unreliable as to best be treated as false, I e-mailed a query letter to the cited source, Dave Barker himself.

In an e-mail reply from Dave Barker dated 14 December 2008, thus post-dating the publication of Schleip's paper, Dave Barker himself confirmed the above and that his published description of Australian *Leiopython* were based on second-hand reports and not any such specimens caught or sighted or confirmed by himself as being from Australian territory.

He stated:

"My account is based on other published reports."

It's common-knowledge that the old reports are erroneous.

By way of example, Harry Ehmann's book (Ehmann 1992), erroneously depicted a Brown (north New Guinea), *Leiopython albertisi* and described it as an Australian taxon, referring it's distribution to Torres Strait Island Australian territory.

On that basis and in the absence of any new evidence, it must remain the case that there are no *L. hoserae* (or *L. albertisi*) known from Australian territorial islands, which is of course contrary to the grossly misrepresented information in the Schleip paper, that somehow escaped judicious editorial quality control.

Hence some obvious questions arise.

Why didn't Schleip make a similar inquiry to my own December 2008 query of Barker to ascertain the facts about allegedly Australian *L. hoserae*, before printing old and questionable information?

Barker is not a hermit and is readily accessible via his website at:

www.vpi.com

To his credit Barker is generally prompt at answering all bona-fide questions from all comers.

It seems even stranger that noting Schleip's constant use of the internet, including to incessantly promote multiple votes for the convicted reptile smuggler David Williams for an ill-fated Hotel contest, he was apparently unable, unwilling, or too lazy to check his basic facts on *Leiopython* before rushing to print and disseminate what is well-known to be false and inaccurate information.

Even more odd, is a notation near the end of the paper (page 19, Schleip 2008a) that says he thanks David

G. Barker for sharing his great knowledge of the genus *Leiopython* with him, meaning perhaps Schleip either forgot to ask the logical questions of him, and/or he forgot the very simple answer, that I had to extract independently in December 2008.

CONTRARY TO THE ICZN CODE

Schleip's paper and website both repeatedly misrepresent the ICZN's code.

I can assume he hopes that not too many people actually read the relevant sections of the code and look into the detail.

However his repeated references to the code in the past 7 years on his website and the way he has constructed his paper of 2008 indicates he is familiar with the ICZN rules.

His manifestly inadequate descriptions of *Leiopython* taxa (in particular for his "*Leiopython biakensis*"), while thoroughly unconvincing as they stand and lacking in evidence to support their position, do fulfil the minimum requirements of the 1999 (effective 2000) code of the ICZN.

In other words all names are "available" within the meaning of the code.

The Schleip names are not *nomen nudem*!

One can reliably conclude that Schleip has literally thrown the names into the pool of available names with the vain hope that one day, one or two may actually be used, but on the basis of a more thorough analysis by another herpetologist.

On his website he does actually quote sections of the code, and it is evident that he has gleaned this either from a hard copy or more likely from the ICZN's website, where the rules are now posted (at: http://www.iczn.org/iczn/index.jsp).

However his continued vicious attacks and rhetoric against myself and the language used is totally in violation of the rules and the ethics within.

Under "Appendix A, Code of Ethics" one finds:

> "5. Intemperate language should not be used in any discussion or writing which involves zoological nomenclature, and all debates should be conducted in a courteous and friendly manner.
> 6. Editors and others responsible for the publication of zoological papers should avoid publishing any material which appears to them to contain a breach of the above principles."

However on his internet site (where Schleip is editor and author) you find repeated fowl language and insults, for which there are no reasonable justifications and are therefore clear breaches of the ICZN's rules. On his own website (December 2008 reloaded version), he makes a barrage of false claims about myself. There are too many to list here, as to do so would require yet more time wasted printing the truth (in the form of rebuttals of idiot claims). But the end-point of his lies is his ultimate statement being that Raymond Hoser is: "a taxonomic nerd and his actions are taxonomic vandalism!" That clearly violates both points 5 and 6 of the ICZN's code of ethics. Schleip's, Wüster's and Williams' comments on third party sites are of course far worse!

That's also before one factors in even more offensive material posted by the group under false names.

WIDER TAXONOMIC ISSUES INVOLVING THE PYTHONS

The morphological and molecular data provided by authors as cited earlier and at the rear of this paper is broadly consistent.

By and large recognized taxa have been named and at the appropriate level and hence no need to revisit this material here.

At the species level, there has been little dispute among authors, including the recent diagnosis of newly described species in the genera *Leiopython* (as identified by Hoser) 2000b, *Australiasis* (Harvey et. al. 2000), including those they didn't assign names to for which names are already available, and *Chondropython* (Rawlings and Donnellan 2003).

Because neither Harvey et. al. (2000) or Rawlings and Donellan 2003, had a close look at the nomenclature surrounding all the pythons subject of their inquiries and/or failed to make this appropriately clear in their papers, it's proper that this be done and the appropriate naming issues resolved.

In both the preceding cases, there were available names for the species-level taxa and these are given below.

For the *Chondropython*, (relegated by some authors to subgenus or ignored in favor of the wider encompassing "*Morelia*"), the two obvious species have available names, as does one of the obvious subspecies (the Australian one). However another from Normanby Island in the Milne Bay Province of PNG does not, and hence it is formally described and diagnosed below.

While the level of subgenus has not always been widely used in the Pythoninae, it is appropriate that when a genus has two or more distinct groups of species, that subgenus be employed to delineate the groups, especially if and when most taxonomists will not choose to split the genus into two, but yet recognize the obvious species groups within.

Hence for the *Morelia* group, the *carinata* species group (as a monotypic species) is herein placed in it's own subgenus, away from the smooth-scaled congeners, generally known as "Carpet Pythons" and including the similar "Diamond Python".

In the case of the *Antaresia*, the distinctive Ant-hill Python is also moved into a group of it's own in a new subgenus, away from the other named taxa in the genus.

Within the Pythoninae, there is no evidence for any other obvious splits to subgenus level and/or alternatively there are names available already in the event that obvious divisions are made.

In my view, the species most likely have more unnamed subspecies is *Broghammerus reticulatus*, whose distribution includes relatively unstudied areas between continental Asia and Australia.

SCRUB PYTHONS

Grouped by most authors within the genus "*Morelia*", some including Wells and Wellington place them within "*Australiasis*", which may ultimately be regarded by most herpetologists as a "subgenus".

Here *Australiasis* is treated as a full genus.

Resolution of the taxonomy within the group was done effectively by Harvey et. al. 2000, who also assigned names to those taxa for which names were unavailable.

Although citing the need for extra work to be done on the population centered on Island New Guinea and nearby, their paper did view it as likely that identifiable groups could be ultimately recognized as full species, although in fairness to the authors their results for these snakes was somewhat ambiguous.

Taking their findings at face value, it seems inevitable that if those populations do not get recognition as species in the long term, then subspecies may be the appropriate designation.

For the purposes of what follows, they are identified below as full species for consistency and to allow other herpetologists to accurately identify snakes they may work with or collect from known localities.

A serious problem in the past has been when ecological studies on a "single" species later are found to have included several and with the lack of identifying notes at the time of the original study, the ultimate worth of the study is devalued in the light of newer information.

One such example is that of Shine (1980) who's study on Death Adders (*Acanthophis* spp.) was later found to have included several species lumped as one, but who at the time failed to take appropriate notes of the locality data for material examined.

Based primarily on "Fig 6" from Harvey et. al. and other data within the paper, we find the Scrub Pythons classified (by distribution) as shown below. The only significant change is the removal of "*kinghorni*" as a junior synonym for "*clarki*", which comes from the "Australian" side of Torres Strait and must (in the absence of contrary evidence) therefore by deemed to be of the same taxon as "*kinghorni*". Also note that Harvey et. al. page 162, specifically identify the Murray Island scrub pythons as being of the same taxon that they refer to the more recently named "*kinghorni*", thereby again giving "*clarki*" priority under the ICZN rules.

This accords with the decision made by Hoser 2000b for the same taxon in terms of identity of the relevant snakes and priority of name.

My guess is that Harvey et. al. inadvertently overlooked the above anomaly as published in their 2000 paper.

Hence below, and for the first time ever is published a list identifying all Scrub Pythons (*Australiasis*) with a "species" name and the locations they occur.

List of *Australiasis* species by distribution

Australiasis amethistina (Schneider 1801), Northwestern New Guinea and perhaps further afield.

Australiasis clarki (Barbour 1914), Continental Australia and immediately adjacent islands.

Australiasis clastolepis (Harvey et. al. 2000), Ambon/Ceram and nearby islands.

Australiasis dipsadides (Ogilby 1891), southern half of Island New Guinea and immediately adjacent islands.

Australiasis duceboracensis (Günther 1879), Bismarck Archipelago

Australiasis nauta (Harvey et. al. 2000), Tanimbar Islands.

Australiasis tracyae (Harvey et. al. 2000), Halmahera and nearby islands.

CARPET PYTHONS

Until the description of *M. carinata* by Smith (1980) based on a recently discovered specimen from Western Australia, all Carpet Pythons were known to be essentially similar in form and smooth-scaled.

While the taxonomy at the genus level has been in a state of flux, sensu-stricto, the genus *Morelia* has included just the smooth-scaled Carpet/Diamond Pythons, treated in turn by many authors as a "super-species" or species complex.

MtDNA evidence hasn't necessarily resolved the taxonomy of the group because different results in DNA do not necessarily match observed differences in phenotype.

However within the Carpet/Diamond Pythons all species are allopatric to one another, sometimes being referred to as regional races, (often with so-called "intergrades" known as shown in Hoser 1989), with the notable exception of *M. carinata*, which is sympatric with *M. variegata* (the top-end Carpet Python).

Noting the obvious differences between *M. carinata*, not just in having keeled versus smooth-scales, but dentition and other differences between these and

other "Carpet Pythons" it makes sense to split these snakes apart.

Genus is not an appropriate division based on current understandings of the term, but species also fails to state the level of differences between this and the other taxa in the genus.

Hence there is no alternative but to assign the taxon *carinata* to a new subgenus.

***JACKYPYTHON* SUBGEN.NOV.**

Type species: *Python carinatus* Smith 1980.

Diagnosis: The only strongly keeled member of the genus *Morelia*. This alone separates the taxa.

Further and/or alternatively separated from smooth-scaled Carpet Pythons by longer and more recurved teeth (average 25% longer than for all other *Morelia*, with *Morelia* and species within the genus being defined as in Hoser 2000b).

Restricted to Western Australia's Kimberly region.

Etymology: Named after the younger daughter of this author, Jacky Hoser in honor of her valuable education work at reptile demonstrations and the like, including from the age of three safely handling venomoid versions of the world's five deadliest snake genera, namely *Parademansia*, *Oxyuranus*, *Pseudonaja*, *Notechis* and *Acanthophis* and most importantly after five years of doing so, never having had a single bite! This emphatically proves that the best way to avoid snakebites is to be nice to them.

ANTARESIA

The genus was first erected by Wells and Wellington (1984) (or 1985) and has gained widespread acceptance since the use of the name by Barker and Barker 1994.

It includes all species formerly grouped as "*childreni*" (formerly placed in the genus "*Liasis*") and later split by various authors into three taxa, namely *childreni*, *stimsoni* (for which *saxacola* Wells and Wellington 1984 has priority but may or may not be available, see below) and *maculosus*, as well as very different taxon, known as the Ant-hill Python, *A. perthensis*.

The first three taxa are all apparently allopatric and essentially similar in most respects, which is why for many years all were treated as a single variable species.

Antaresia perthensis is sympatric to *A. stimsoni/saxacola* in the Pilbara of Western Australia. It is different in terms of it's smaller adult size, more stocky build, smaller clutch size, colouration, habits and scalation (notably mid-body row count) and while clearly has affinities with the others in the genus, is apart from them. Hence it is appropriate that it be separated from it's congeners at a level above species and yet not as a full-genus. Hence the creation of a new subgenus for the taxon.

***RAWLINGSPYTHON* SUBGEN NOV.**

Type Species: *Liasis perthensis* Stull 1932

Diagnosis: Separated from all other *Antaresia* by 31-35 mid-body scale rows, versus 37-47 mid body rows for all other recognized species in the genus *Antaresia*, namely *A. stimsoni* (or *A. saxacola*), *A. childreni* and *A. maculosus*.

No other snakes are likely to be confused with *Rawlingspython* subgen. nov.

Further separated from other *Antaresia* by the generally reddish color, including blotches and background, versus a generally brownish background color for all other *Antaresia.*

If blotches in other *Antaresia* are reddish (as opposed to actually red), they will still be on a yellowish, whitish or brown background color.

Rawlingspython are smaller as adults (to 61 cm long), versus to 105 cm long or larger for all other *Antaresia*.

Etymology: Named after Adelaide-based Museum researcher, Lesley H. Rawlings in recognition of her work on python systematics.

SAXACOLA* VERSUS *STIMSONI

Until the publication of Hoser 2000b, most herpetologists in Australia identified the so-called Western Children's Python as *A. stimsoni*, as described by Smith (1985).

The name "*stimsoni*" emerged shortly after Wells and Wellington's paper naming the same taxon "*saxacola*".

As a result of a petition to the ICZN seeking suppression of the relevant Wells and Wellington paper, the Smith name gained wide usage, but the Wells and Wellington one didn't.

The petition to the ICZN failed in 1991, (see Storr, Smith and Johnstone 2002) or the ICZN's ruling as published, by which stage "*saxacola*" had been all but forgotten by most herpetologists.

Following resurrection of "*saxacola*" by Hoser 2000b, as part of an overview of python systematics, Aplin wrote the following in Storr, Smith and Johnstone (2002):

> "The rules controlling the names of animals dictate that the oldest available name be applied to any given species and that the descriptions meet certain minimum criteria to ensure identification. Although the name *saxacola* narrowly predates *Liasis stimsoni orientalis* it was proposed without any form of differential diagnosis and is thus regarded as a *nomen nudem* (literally 'naked name') and hence is unavailable (Aplin and Smith 2001). Hoser (2000) has attempted to encourage the use of *saxacola* but has failed to address the issue of non-availability."

Hoser 2000 (myself) used the name "*saxacola*" on the basis that the original Wells and Wellington description had a seven line "diagnosis" following the heading, viewing that as satisfying the ICZN code.

The relevant section of the code/rules is, Article 13.1.1, which as noted earlier here, reads:

> "13.1.1. be accompanied by a description or definition that states in words characters that are purported to differentiate the taxon"

The diagnosis, is the only part of the Wells and Wellingon description in doubt.

Richard Wells in various phone conversations alleges that his (Wells and Wellington 1984) description fits this, in that even if his diagnosis fails, it "purports" to differentiate the taxon and that is good enough.

"Purport" is defined earlier in this paper.

Aplin says that under his interpretation of the code the Wells and Wellington description fails.

My personal view is that the description of *saxacola*, does fit within the guidelines of Article 13.1.1 (on it's most liberal of interpretations) on the basis that Wells and Wellington could argue that by referring their new taxon to images of the same taxon and the other species they say are likely to be confused with it, (in their description) they have compared the new taxon with that it is likely to be confused with. Wells and Wellington could also argue that they have not considered congeners (as recognized by them at the time) as the differences are obvious and not needed to be diagnosed.

This is of course a subjective judgment, but unfortunately on which it all seems to turn.

Having said the above, most Australian herpetologists do not recognize the name "*saxacola*" and perhaps ultimate resolution of the issue will be through a specific petition to the ICZN.

This in my view should happen sooner rather than later in order to resolve any potential confusion.

So as to enable readers to properly ascertain the relative merits of Aplin's argument against *saxacola*, as compared to the potential arguments in favor of usage of the name, I have reprinted the relevant passage from Wells and Wellington 1984 in it's entirety and unedited below:

> "PYTHONIDAE
>
> *ANTARESIA* Wells and Wellington, 1984
>
> *Antaresia childreni* (Gray, 1842)
>
> *Antaresia gilberti* (Gray, 1842)
>
> *Antaresia maculosus* (Peters, 1873)
>
> *Antaresia perthensis* (Stull, 1932)
>
> *Antaresia saxacola* sp. Nov.
>
> Holotype: An adult specimen in the Australian Museum R60304. Collected at 6 km north of Barrow Creek, (on Stuart Highway) Northern Territory (21 04'S X 134 10'E) on 16 January, 1977 by Peter Rankin and Grant Husband.
>
> Diagnosis: A member of the *Antaresia childreni* complex most closely related to *Antaresia* gilberti and believed confined to central Australia. *Antaresia saxacola* is Figured in Cogger (1983:Plates 174 and 409 from Wilcannia, New South Wales). Gow (1977, Snakes of the Darwin Area) illustrates its congener *Antaresia childreni*. The holotype of Antaresia saxacola measures 102.5 cm snout-vent length and 9.6 cm to tail length.
>
> Etymology: The name refers to its essentially rock-dwelling habits."

The above was reprinted due to the general difficulty most herpetologists have in acquiring original copies of the relevant Wells and Wellington paper.

***CHONDROPYTHON* MEYER 1874**

Designation of so-called "Green Pythons" in the genus *Chondropython* has been the normal situation among taxonomists for most of the past 100 years. Having said this, in recent times a number of taxonomists lump the Green Pythons in the genus *Morelia*, noting the obvious affinities between the two groups.

Hobbyists still call the snakes "Chondro's" regardless of what scientific name they use.

Allowing for the latter placement of these snakes in the supergenus "*Morelia*", these snakes remain outside the core *Morelia* group of "Carpet Pythons", with the *Australiasis* snakes (so-called Scrub Pythons) also forming a different group.

If one doesn't accept the obvious splits to include the genera *Australiasis*, *Lenhoserus* and *Chondropython*, then all preceding names are available at the subgenus level.

For the purposes of this paper and following on from Hoser 2000b, *Chondropython* is treated here as a full genus.

Rawlings and Donellan (2003) in their Phylogeographic Analysis of the Green Python, yielded results in accordance with similar studies for other snake genera with similar cross New Guinea distributions, including *Acanthophis* (Hoser 1998), *Leiopython* (Hoser 2000) and *Australiasis* (Harvey et. al. 2000).

While Hoser 1998 and Hoser 2000b did not relate the findings with geological evidence in terms of seeking explanations for results, other authors including Harvey et. al. (2000) have.

That is that as a result of the formation and uplifting of the central New Guinea range commencing about 5 million years before present, species were split into allopatric groups which in turn speciated, giving the

present day results.

Hence the barriers affecting one species seems to have similarly affected others, giving a near mirror image distribution patterns for the various python genera (as diagnosed by Hoser 2000b) and also perhaps elapid groups as well.

That Rawlings and Donellan found evidence to support two species of Green Python was not a surprise and had been anticipated by hobbyist keepers for decades.

They wrote:

"The pattern of relationships found for mitochondrial and nuclear genes suggests the presence of two species of *M. viridis*, one present north of the central cordillera and the other present in Southern New Guinea and Australia."

Their mtDNA evidence in terms of the outlier Australian population concurred with Hoser 2000b and McDowell 1975 in that while it had clear affinities to the southern New Guinea snakes, they were derived from them in relatively recent geological time and by a migration south from the main population.

Hobbyist keepers in Europe and the USA were well aware of the different pattern morphs from different locations over many years, including differences between those from north and south of the main dividing range.

While Rawlings and Donellan 2003 didn't concern themselves with nomenclature of the regional forms of Green Python, all taxa have been named at the species level. This contradicts Rawlings et. al. 2008, p. 604, who state the northern New Guinea taxon is "unnamed", when in fact it was named in 1875 by Meyer. Furthermore one of two obvious subspecies, the Australian Green Python (*Chondropython viridis shireenae*) Hoser 2003 has also been named.

A second highly distinct form of Green Python, hitherto unnamed and from Normanby Island, Milne Bay Province, PNG, is formally described for the first time here as the subspecies *Chondropython viridis adelynhoserae* subsp. nov.

That these snakes differ from other *Chondropython* has been speculated for some time. However until recently I had not seen any specimens in life or good quality photos of specimens in life. DNA evidence as provided by Rawlings and Donnellan 2003 also supports the hypothesis that these snakes differ from other *Chondropython* and are reproductively isolated from them and have been for some time.

For the record, *Chondropython pulcher* Sauvage 1878, is a synonym of *C. azureus* Meyer 1875 being derived from the same general region as *C. azureus*.

***CHONDROPYTHON VIRIDIS ADELYNHOSERAE* SUBSP. NOV.**

Holotype: A specimen in the Australian Museum R129716, from Normanby Island, Milne Bay, Papua New Guinea.

Diagnosis: This is the form of Green Python restricted to Normanby Island, Milne Bay Province, PNG.

It is separated from all other *Chondropython* in New Guinea and Australia by it's adult dorsal pattern of (smallish) white blotches that in the main do not cover the spinal ridge, as seen in all other Australian and other PNG *Chondropython*.

Sometimes *Chondropython* from elsewhere will have similar blotches, but invariably, these snakes either also have a mid-dorsal line or dots (not seen in *adelynhoserae*), or the blotches run well over the spinal (mid dorsal) mid-line.

MtDNA for the holotype was examined by Rawlings and Donellan 2003 and compared with other *Chondropython* yielding traits broadly in line with *C. viridis viridis* (but a three per cent sequence divergence) (see p. 41 their paper). Having said that, it also shared five nucleotide substitutions that would otherwise be synapomorphies of the northern lineage, one of which is an indel.

This result in terms of mtDNA and base pair analysis, as published by Rawlings and Donellan 2003, forms an additional and/or alternative diagnostic means of identifying and separating *C. v. adelynhoserae* subsp. nov. from other *Chondropython.*

In other words, the taxon *C. v. adelynhoserae* subsp. nov. can be separated from other *C. viridis* by the degree of base pair separation/divergence of mtDNA and/or nuclear DNA as detailed by Rawlings and Donellan 2003.

This is the only *Chondropython* taxon found on Normanby Island, Milne Bay Province, PNG and is allopatric to all other *C. viridis* or *C. azureus.*

Etymology: Named after the elder daughter of this author, Adelyn (pronounced: Adder-lyn) Hoser, in honor of her valuable education work at educational reptile demonstrations and the like, including from the age of five safely handling venomoid versions of the world's five deadliest snake genera, namely *Parademansia*, *Oxyuranus*, *Pseudonaja*, *Notechis* and *Acanthophis* and most importantly after five years of doing so, never having had a single bite! This emphatically proves that the best way to avoid snakebites is to be nice to them.

***CHONDROPYTHON* SUMARY**

Based on what is now believed to be allopatric distribution and factors outlined elsewhere, the named taxa of Green Python are now as follows:

Chondropython viridis (Schlegel 1872), southern New Guinea generally and offshore Islands.

Chondropython viridis shireenae Hoser 2003, Australia only.

Chondropython viridis adelynhoserae subsp. nov. (this paper), Normanby Island, New Guinea.

Chondropython azureus Meyer 1875, New Guinea north of the central range, including offshore islands.

Maxwell (2005), gave detailed information about local "races" of *Chondropython*, including from islands. However noting the extreme phenotypic variation of the genus and the non-sampling of intermediate populations (when available), and the added variables of local adaptations to altitude and so on resulting in localized colour variants and the like, there is no evidence that any of the forms identified in the book warrant recognition beyond the taxa (to subspecies level) identified in this paper based on available evidence, most notably that of Rawlings and Donnellan 2003.

Notwithstanding this, the book remains mandatory reading for those with an interest in the genus.

ASPIDITES TAXONOMY

Various geographical races were recognized long before Hoser 2000, formally named some of them (see relevant references in Hoser 2000). According to Ken Aplin in Storr, Smith and Johnstone (2002), two names first used by Hoser (2000), *Aspidites melanocephalus daveii* and *Aspidites ramsayi richardjonesii* are "*nomen nudem*" on the basis that they only differentiate taxa on the basis of distribution and that alone is not a distinguishing character. Furthermore the argument is advanced that both names are invalid, as essentially similar diagnostic features (excluding distribution) are used for other newly named taxa, namely *A. melanocephalus adelynensis* and *A. ramsayi panoptes* both names of which are accepted as "available" for their said taxa and take precedence on the basis that they appear before the "unavailable" names in the original paper and hence have "page priority".

Unlike in the case of *Antaresia saxacola*, for which there is an alternative name available on the basis of it's (allegedly) being a "*nomen nudem*", namely *A. stimsoni* Smith 1985, there are as yet no other available names for the *Aspidites* taxa identified by Hoser by distribution (alone?) under the above "*nomen nudem*" names.

Hence in order to stabilize the taxonomy and nomenclature for the genus, the two relevant taxa, the north-west Woma and the north-west Black-headed Python are both formally named and described here.

Rather than have any further destabilization of the nomenclature, different name combinations are adopted for the relevant taxa for "new" descriptions of them.

This is to ensure stability of names used from now on.

This is important as with newly legalized collection and breeding of these pythons in the three main states of WA, NT and SA, increasing numbers are now captive and being studied and it is important that biological information obtained is correctly attributed to the correct regional taxa.

Furthermore a diagnosis of the genus as a whole is provided here, which is largely similar to that printed in Hoser (2000) however with important changes and updates.

While Aplin did not state a reliance on Article 24.2.2. of the ICZN rules (ICZN 1999), in his work in terms of determining which *Aspidites* subspecies names took precedence in terms of being the available name, if one were to accept that he in fact relied upon this article, then he spelt out those actually published first in terms of either page or position priority.

Assuming however that Aplin has not sought to be a "first reviser", which appears the reasonable position based on his writings, and so as to remove any ambiguity, I seek herein to rely on section 24.2.2. of the code to be the "first reviser" and to assign valid available names on the basis of the same page or positional priority in the original Hoser 2000 paper.

In other words the names to be hereafter regarded as available from Hoser 2000 on the basis of section 24.2.2 of the ICZN rules are, *Aspidites melanocephalus adelynensis* Hoser 2000, and *Aspidites ramsayi panoptes* Hoser 2000.

Hence the end result is the same whether or not one accepts that I am in fact the "first reviser".

GENUS _ASPIDITES_ PETERS 1876

Aspidites is a genus of large terrestrial Pythons endemic to continental Australia. These pythons are readily distinguished from all other Australian species by the apparent absence of pits on the labial or rostral scales, although in Black-headed Pythons (*A. melanocephalus*) at least, a tiny vertical slit on the rostral region is apparently equivalent. Other diagnostic traits are the absence of teeth on the premaxilla and enlarged symmetrical shields on the top of the head. Prior to now, most authorities have divided the genus into two well-defined species. These are the Black-headed Python (*A. melanocephalus*) and the Woma (*A. ramsayi*).

The former is separated from the latter by its distinct glossy black head. At best the latter only has black markings on the head. Few authors recognize subspecies or races. Those that have subdivided the above species into races or regional variants, include Barker and Barker (1994a) and Wells and Wellington (1985a). The former recognized different races without naming them, while the latter recognized *A. collaris* as described by Longman in 1913 (see below). Taxonomy of this genus has gained greater interest in

recent years with the introduction of more formalized reptile-keeper licensing systems in most Australian states combined with the high prices of specimens traded. Authorities in some states have taken a strong stand against hybridization of races of snakes, a view shared by a substantial number of private keepers.

Noting that distinct differences between races of *Aspidites* are well known and acknowledged and that for many years a substantial number of herpetologists have recognized different races as being at least different subspecies, it is somewhat surprising that up until now no one has put names to these different races. Black-headed Pythons and Womas are known to occur sympatrically in parts of Western Australia, with this author catching both species on the western edge of the Great Sandy Desert, north of Port Hedland, WA. (refer to photos published in Hoser 1989). There is presently no evidence of cross-breeding between the two species either in the wild or captivity. However Hoser (2007) demonstrated how easy it was to extract semen from snakes, including a NT specimen of *A. melanocephalus*, for the purposes of inseminating other snakes of choice, making cross-breeding of taxa far easier than had previously been the case.

Smith (1981) also found similar sympatry between both species in Western Australia. Worrell (1963) recorded sympatry between both species in the Northern Territory. To date no similar sympatry has been recorded in Queensland. That sympatry occurs between the two species of *Aspidites* is not altogether surprising as their habitat preferences are somewhat generalist, with the snakes being found in a variety of habitat, soil and vegetation types. Biological information about *Aspidites* is provided by Cogger (1996), Barker and Barker (1994a), Hoser (1981, 1989), Sonneman (1999) Storr, Smith and Johnstone (1986), Worrell (1970) and others. Excellent photos of *Aspidites* are provided by the authors named immediately above. Photos of habitats inhabited by *Aspidites* are provided by a number of authors including Hoser (1989) and Barker and Barker (1994a). Barker and Barker (1994a) provide an excellent bibliography of cited references on *Aspidites* and pythons in general including cases of captive breeding, breeding data and other useful material. Type material for all species listed below has not necessarily been inspected by this author, however this author has inspected a substantial number of specimens including from the type localities given.

SPECIES AND SUBSPECIES OF *ASPIDITES* NOW RECOGNISED

Aspidites melanocephalus (Krefft, 1864)
Aspidites melanocephalus adelynensis Hoser 2000
Aspidites melanocephalus rickjonesii subsp. nov. (this paper)
Aspidites ramsayi (Macleay, 1882)
Aspidites ramsayi panoptes Hoser 2000
Aspidites ramsayi neildaveii subp. nov. (this paper)
Total of 2 species comprising six subspecies.

***ASPIDITES MELANOCEPHALUS* (KREFFT, 1864)**

Type locality is Port Dennison (Bowen) in North-east Queensland. The holotype is held in the British Museum of Natural History (UK). *Aspidites melanocephalus melanocephalus*, the nominate subspecies, is herein restricted to Queensland and most parts of the top third of the Northern Territory. Most Black-headed Pythons in captivity are of this form.

***ASPIDITES MELANOCEPHALUS ADELYNENSIS* HOSER 2000.**

HOLOTYPE: A specimen at the Western Australian Museum, number 51208 from Wyndham, WA Lat: 15° 28' Long:128° 06'

PARATYPE: A specimen at the Western Australian Museum, number 17115 from 8 km south of Wyndham, WA. Lat: 15° 28' Long:128° 07'

DIAGNOSIS: Known only from Kimberley region of WA, this population of Black-headed Pythons appears to be isolated from the population to the south in the Pilbara. It is uncertain as to how much gene flow occurs between this population and that to the east in the adjacent parts of WA and the NT. *Aspidites melanocephalus adelynensis* like *A. m. rickjonesii* (see below) is separated from other Black-headed Pythons by usually having one loreal, no suboculars and a single pair of large parietals, while most NT and Queensland Black-headed Pythons have 2-4 loreals, 1-2 suboculars and 2-4 pairs of parietals. *A. m. adelynensis* is separated from *A. m. rickjonesii* by the possession of yellowish lighter bands as opposed to whitish lighter bands in *A. m. rickjonesii*.

It is also separated from *A. rickjonesii* by distribution. It is separated by part of the western flank of the Great Sandy Desert where it meets the WA coast. There are no unusually "high light" specimens of *A. m. adelynensis* known (as occurs in *A. m. rickjonesii*). *Aspidites melanocephalus rickjonesii* subsp. nov. is further separated from other *A. melanocephalus* by it's smaller adult size (average 180 cm total length in measured specimens), versus average of 200 cm total length in measured specimens of *A. melanocephalus adelynensis* from further north in WA (The Kimberly region) and 210 cm for *A. melanocephalus melanocephalus* the taxon from north-east Australia. Analysis of the mitochondrial DNA of *A. m. adelynensis* will further ascertain the differences between this and the other Black-headed Pythons, in particular, how much genetic interaction has occurred between this population and those to the east.

ETYMOLOGY: Named after Adelyn Hoser, the author's daughter. See elsewhere this paper for more

details. .

***ASPIDITES MELANOCEPHALUS RICKJONESII* SUBSP. NOV.**

HOLOTYPE: A specimen at the Western Australian Museum, number 46170 from Tom Price WA Lat:22° 39 Long:117° 40'.

PARATYPE: A specimen at the Western Australian Museum, number 12268 from near Port Hedland, WA. Lat: 20° 19' Long: 118° 34'.

DIAGNOSIS: Known only from Pilbara region of WA, this population of Black-headed Pythons appears to be isolated from the population to the north in the Kimberley Ranges. Some but not all specimens of *A. m. rickjonesii* are of a distinctly lighter than usual ground colour. However this is not a general diagnostic characteristic on it's own.

What is diagnostic of this taxon as opposed to all other *A. melanocephalus* is that the lighter cross-bands have a distinctive whitish hue, as opposed to the yellowish hue in the lighter cross-bands of all other *A. melanocephalus*.

Aspidites melanocephalus rickjonesii like *A. m. adelynensis* (see above) is separated from other Black-headed Pythons by usually having one loreal, no suboculars and a single pair of large parietals, while most NT and Queensland Black-headed Pythons (*A. m. melanocephalus*) have 2-4 loreals, 1-2 suboculars and 2-4 pairs of parietals.

In this taxon *Aspidites melanocephalus rickjonesii* subsp. nov. the large parietals are more circular in shape than those seen in both the other subspecies, which are either jagged in shape) (as seen in *A. m. adelynensis*) or as distinctly smallish circular with irregular edging/irregular shape in *A. melanocephalus melanocephalus* from eastern parts of Australia. *Aspidites melanocephalus rickjonesii* subsp. nov. is further separated from other *A. melanocephalus* by it's smaller adult size (average 180 cm total length in measured specimens), versus average of 200 cm total length in measured specimens of *A. melanocephalus adelynensis* from further north in WA (The Kimberly region) and 210 cm for *A. melanocephalus melanocephalus* the taxon from north-east Australia. This author has caught both lighter and more 'normal' coloured specimens in the Goldsworthy/Shay Gap areas of WA. In the northern part of the Pilbara region, the Black-headed Pythons seem to be more common in the hillier areas, while the Womas (*A. ramsayi*) appear to be found more in the red sand-dune habitats. *A. m. rickjonesii* is separated from *A. m. adelynensis* by distribution, being separated by part of the western flank of the Great Sandy Desert where it meets the WA coast. These same differences were identified by Barker and Barker (1994a).

Wild caught specimens of *A. m. rickjonesii* caught have also demonstrated behavioral differences that diagnose it as separate from other *A. melanocephalus.* Unlike the other subspecies that tend to rear up and hiss when caught, this trait is only seen in this taxon when harassed, as opposed to merely encountered. Analysis of the mitochondrial DNA of *A. m. rickjonesii* will further ascertain the differences between this and the other Black-headed Pythons.

ETYMOLOGY: Named after a NSW Member of Parliament, Richard Jones, also known as "Rick Jones" for his ongoing contributions towards wildlife conservation, integrity in government and other matters. An honest and decent parliamentarian such as Richard Jones is a rare thing in Australia. That is also why he isn't with a major party.

***ASPIDITES RAMSAYI* MACLEAY, 1882**

The type locality is Fort Bourke in NSW. The snake later described by Longman in 1913 as *Aspidites collaris* from near Cunnamulla, Queensland, is believed to be the same race as the nominate form and is treated here is synonymous. The distance between Bourke and Cunnamulla is not substantial. Habitats, including soils and vegetation regimes and herpetofaunas in the two areas are essentially similar. Thus the type form of Woma is in fact the Eastern Australian form. It is distinctly more grey in dorsal colour (as opposed to yellowish brown) than both the western subspecies and has far more prominent dark markings over the eyes as compared to more western specimens which may or may not have such markings. While distributional information for Womas in Australia is patchy, partly in reflection of the relatively remote areas that they occur in, most herpetologists believe that it is not continuous throughout the arid parts of Australia. For the purposes of this paper, and until information to the contrary is received, the nominate subspecies, *Aspidites ramsayi ramsayi* is herein restricted to inland parts of NSW and adjacent Queensland, essentially confined to the upper Darling River basin.

All three subspecies of *Aspidites ramsayi* are believed to be allopatric.

***ASPIDITES RAMSAYI PANOPTES* HOSER 2000**

HOLOTYPE: A specimen at the Western Australian Museum, number 43459 from Burracoppin, WA Lat: 31° 24' Long:118° 29'.

PARATYPE: A specimen at the Western Australian Museum, number 17662 from Merredin, WA. Lat: 31° 31' Long:118° 14'

DIAGNOSIS: This race of Womas has a lower average ventral and subcaudal count than the main race (Barker and Barker 1984). Unlike the nominate form, *A. r. panoptes* does not usually retain the juvenile darkening over the eyes in adults. This latter trait is a trait shared with *A. r. richardjonesii*, also of WA, (see

below). This is the south-western Woma. It is separated from all other Womas by distribution (Smith 1981).

Hoser 2000 stated that the population is believed to be isolated from the main centralian population by a belt of heavy soils between Karalee and Zanthus, WA (Smith, 1981). In the absence of evidence to the contrary, this author accepts Smith's proposition (see below). In south-western Australia at least, this south-western population appears to be in terminal decline (Brian Bush, pers. comm.). The probable causes include introduced predators such as foxes and cats, habitat destruction and perhaps other unknown causes.

Aplin in Storr, Smith and Johnstone (2002) noted that habitat in south-west WA had changed dramatically within the last 16,000 years and as a result, it'd be reasonable to expect that the present isolation of the south-west WA population from the nearby Nullabor population is recent in geological terms and therefore the snakes there should be attributed to the same taxon.

In terms of physical traits, the southern Australian population, as in that found in South Australia, but not including red-soiled areas in the state's furthest north, should also be attributed to this subspecies in the absence of evidence to the contrary.

The basis of this assertion is the physical similarity of the snakes as mentioned elsewhere in this paper, but including physical size, morphology, scalation, pattern, demeanor and known biology.

The specimens from red-soil areas of the NT and north-west are of a distinctly smaller and more gracile race and are described as a separate subspecies below.

All three subspecies of *Aspidites ramsayi* are believed to be allopatric.

ETYMOLOGY: The subspecies was named *panoptes* due to popularity of the scientific name for a species of monitor lizard among some Australians. Therefore I have bowed to their wishes and legitimately named another reptile by this name. ICZN rules allow species from different family and genus to carry the same species name.

***ASPIDITES RAMSAYI NEILDAVIEII* SUBSP. NOV.**

HOLOTYPE: A specimen at the Western Australian Museum, number 34070 from near Port Hedland, WA Lat: 20° 19' Long:118° 34'.

DIAGNOSIS: Unlike *A. r. ramsayi*, this form loses the juvenile pattern (of darkening) around the eyes at maturity and separates these taxa. This is diagnostic for the subspecies. While this trait is also diagnostic for *A. r. panoptes* (see description above), the two forms are separated by a vast distance, including most of the Pilbara region. *A. r. neildavieii* is also separated from *A. r. ramsayi* by distribution.

Previously it was thought that the form was only known from the Western edge of the Great Sandy Desert in WA. This population (and subspecies) is actually thought to extend into central Australia, being common throughout most of the "red center". *A. ramsayi neildavieii* subsp. nov. is separated from *A. r. panoptes* by it's distinctive yellowish hue in it's base colour as opposed to brownish in *A. r. panoptes.*

A. r. panoptes also has cross-bands that are indistinct as compared to *A. ramsayi neildavieii* subsp. nov. Each of the previous characters alone and/or in combination separate these two subspecies taxa. *A. ramsayi neildavieii* is also separated from *A. r. panoptes* and all other *A. ramsayi* by it's considerably more placid behavior.

Wild caught specimens of other *A. ramsayi* will tend to rear up and even strike when first caught. This is not the case for *A. ramsayi neildavieii* subsp. nov., which rarely if ever rears up or attempts to bite when first caught.

It was thought until recently that Centralian populations appeared to have characteristics intermediate between the Easternmost and Westernmost populations of *A. ramsayi*, that is now thought not to be the case and those specimens are generally attributable to this taxon and not the nominate *A. ramsayi ramsayi.*

A. ramsayi ramsayi is separated from other *A. ramsayi* subspecies by more grey in dorsal colour (as opposed to yellowish brown) than both the western subspecies and has far more prominent dark markings over the eyes as compared to more western specimens which may or may not have such markings and if they do, to a markedly lesser extent than seen in *A. ramsayi ramsayi*.

Of the various *Aspidites ramsayi* subspecies, *A. ramsayi neildavieii* subsp. nov. has the smallest average adult size, being 150 cm total length in adults, versus 160-180 cm total length for all other named subspecies measured, or for which specimen/group measurements were available.

These average size measurements have been corroborated against the now sizeable number of *Aspidites ramsayi* of all regional subspecies from across Australia now in captivity. While some *Aspidites ramsayi neildavieii* may have a limited amount of darkening around the eyes (especially in juveniles), this is never to the standard extent seen in *A. ramsayi ramsayi* from inland Eastern Australia.

Captive *A. ramsayi neildavieii* average 8-10 eggs per clutch versus 12-16 for reported cases of the subspecies from mid-south Australia, herein also referred to as *A. r. panoptes*.

The smaller clutch sizes seems to be a direct reflection of smaller adult sizes, with individual egg masses apparently being generally similar.

The demarcation between *A. ramsayi neildavieii subsp. nov. and A. ramsayi ramsayi* is believed to be in the region of Western Queensland, south of Mount Isa, through a wide area known to have Black-headed Pythons (*A. melanocephalus*), but not *A. ramsayi*.

All three subspecies of *Aspidites ramsayi* are believed to be allopatric.

ETYMOLOGY: Named after Neil Davie, founder of the Victorian Association of Amateur Herpetologists (VAAH) for ongoing services to herpetology, including the period beyond his involvement in that society, including in his work for conservation and his education related work with reptiles.

SUMMARY

The papers Hoser 2000b, 2003a, 2004a and this paper, including updates and changes indicated within each, do between them give an accurate overview of the systematics of the pythons of Australasia and elsewhere.

Based on what's known about the population dynamics and taxonomy of all pythons in the Australasian region in the last five million years, combined with the published results of Schleip 2008a, it makes sense to continue to recognise the genus *Leiopython* in the format given by Hoser 2000b, even if one were to shift the two species, *albertisi* and *hoserae* to the genus *Bothrochilus* as done by Rawlings et. al. (2008). Schleip has provided evidence for the continued recognition of the two subspecies of *L. albertisi* named by Hoser in 2000b and at the same time provided limited evidence for the potential recognition of at least three other subspecies as named by Schleip in his 2008 paper.

There is however at the present time, no evidence to support the contention that *L. albertisi* as recognised by Hoser 2000b should be split into five or more very similar species at the species level using any liberal interpretation of any liberal species concept.

Morphological data does not support any such split and so far based on the quoted studies above, there seems to be no prospect of molecular data supporting any such split either. Unless and until any such evidence is published in a clear and unequivocal form, *Leiopython* as generally recognised should be regarded as a genus comprising two distinct species only, namely *L. albertisi* and *L. hoserae*.

The paper by the self-admitted "amateur herpetologist" Wulf Schleip (2008a) was sloppy, desceptive and amateurish and written by a novice reptile enthusiast who was way out of his depth and who was badly advised by his closest associates who clearly sought a pre-determined outcome, regardless of the evidence.

In the first instance it should have been totally rejected by the editors of the *Journal of Herpetology* as a case of "Big Name Hunting" by a man who (presumably unknown to the journal's editors) has a history of dishonesty, gross misrepresentations and at times who makes outright lies.

Schleip has been found to be culpably guilty of creating unnecessary confusion in terms of the taxonomy of an otherwise well-known and well-defined group of snakes that has been competently examined by numerous recognised experts in the past.

Schleip has been reckless by presenting a poorly written paper which by his own admissions within it, does nothing to stabilize taxonomy of the group and leaves no options other than for another person to revisit the taxonomy of the "species" *albertisi* with a view to either creating yet more "species" via yet more "taxonomic exaggeration" for the snakes he claims are of indeterminate taxonomic status, or via the molecular data he should have provided with his 2008 paper, the eventual (and at this stage likely) position being that his newly created "species" will again be relegated to the synonymy with *L. albertisi* (at the species level).

The use of dishonest and improper methods by Schleip and his closest associates, the convicted reptile smuggler David Williams and questionable academic Wolfgang Wüster, as well as potential emulation by others should seen as a threat and impediment to legitimate scientific inquiry and associated search for the truth.

The adverse effects of their actions will invariably impact far beyond the boundaries of routine discourse or debate by scientists. It will descend into areas as diverse as conservation, waste of public resources, legislation, defamation, law courts, improper censorship of legitimate and appropriate views, unwarranted confusion among disinterested third parties and elsewhere.

ACKNOWLEDGMENTS

Numerous herpetologists and others, including too many to be named here provided access to papers, specimens and other materials as requested, including material that by nature is not herpetological, but assists in related projects. My family contributed the most by putting up with many hours of my absence as I dealt with various herpetological matters which invariably cost as opposed to earn money.

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