

***Pailsus* - a story of herpetology, science, politics, pseudoscience, more politics and scientific fraud.**

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Preamble

What follows is a summary of an exciting journey. It begins with the science of the initial discovery of a new species of snake. The story then winds through a series of twists and turns into three hitherto unknown species and a reassessment of a group of snakes (The King Browns). They were a well known group that we all thought we were familiar with, and then in the last few years realized just how little is yet really known about even the basic taxonomy of these snakes. However all this is then overshadowed by a sinister tale of vendetta, politics, pseudoscience and a serious case of outright scientific fraud.

It's a story that all herpetologists and others with an interest in science in general should be made aware of, and that's why it's being told here.

Now it's hard to put a start date on this story.

Perhaps it's a few hundred thousand or even a few million years ago when the basal stock of snakes that are now known as "*Pailsus*" diverged from the main "*Cannia*" or King Brown/Mulga Snake lineage.

Despite new whiz-bang genetics and other technology, we can still only guess when that was.

Or perhaps it was way back on 17 June 1934 when a snake specimen from Riversleigh in far north-west Queensland was lodged with the Australian Museum in Sydney, and given the specimen number "R11359"

Identified as a "Mulga Snake" ("*Pseudechis australis*"), the snake was shoved in a jar of preservative and it went effectively unnoticed until early 2001, when the specimen was re-identified by myself as that newly described species *Pailsus pailsei* Hoser 1998.

Ditto for Queensland Museum specimen number J59015 from Mary Kathleen Dam in Queensland, that was also misidentified as a "*Pseudechis australis*" until early 2001, when this author dug it out of the jar and declared it to be one of this newly described species, namely "*Pailsus pailsei*".

Or perhaps the start date of this story could be 12 January 1987 when John Weigel and others collected what they thought was an immature "*Pseudechis australis*" from the Mitchell Plateau in North-west Western Australia. About five months later, the now deceased snake was sent to Richard Wells who in partnership with Ross Wellington wrote a description of this animal, describing it as a new species, "*Cannia weigeli*".

Or perhaps the start date of the story should be some other past event that I have failed to recall here.

However as none of these events had any discernable impact on Australian herpetology in terms of these snakes being generally recognised as different to the "normal" King Brown/Mulga snakes, I shall start the story at the point that I became involved in these snakes, as this also coincides with most people's initial knowledge of these hitherto unknown snakes.

However before I get to this point, I should make a few passing comments to make sure all readers get a proper understanding of what I am going to talk about.

For many years the "Black Snake" genus "*Pseudechis*" was thought to include the likes of the King Brown (*australis*), Butler's (*butleri*), Blue-bellied Black (*guttatus*), Collett's (*colletti*) and Papuan Black (*papuanus*) Snakes. After all, they superficially look alike. All are large and thick-set snakes. And all are similar in many other respects, such as being generalized terrestrial predators, mainly diurnal in habits, except in hot weather and so on.

But more recently (since the mid 1980's), many herpetologists have since had these species, split off into their own genera as a result of DNA and morphological studies showing the group to be polyphyletic.

These are namely *Cannia* (comprising *australis* and *butleri*) and *Paracadechis* (comprising *colletti*, *guttatus* and *papuanus*). This division was formally made by Wells and Wellington (1983, 1985a), but has since been corroborated by numerous others including Shea, Shine and Covacevich (1993), and Greer (1997).

And yes, it's *Cannia* which includes the King Brown/Mulga snakes which at this

stage appear to be the most similar snakes to *Pailsus*.

In 1998 I described a smallish species from North-west Queensland as *Pailsus pailsei*. In 2000 I described a similar species from New Guinea as *Pailsus rossignolli*, while as already mentioned, way back in 1987, Wells and Wellington wrote a description of what is thought to be yet another similar species, namely *Pailsus weigeli*.

All *Pailsus* are usually differentiated from the King Brown/Mulga Snakes (*Cannia*) by their small adult size (1 metre versus 2 metres), more slender build, more aggressive and flighty nature, higher ventral scale count, all or most subcaudals single (versus a greater number usually divided) and a host of other differences as outlined in the original descriptions and elsewhere.

In fact many traits shared by all other "*Pseudechis*" group snakes are not found in the three *Pailsus* species most recently described, which is of course why these species were assigned a new genus.

In Queensland and western Australia, *Cannia* and *Pailsus* are sympatric and there is no known cross-breeding so there cannot be a dispute as to the fact they are different species. In New Guinea it appears that there are no *Cannia* (contrary to most of the recent literature), a fact only discovered by this author as he made his investigations into the New Guinea *Pailsus*.

That more or less sums up what we are talking about. Or at least in terms of the "*Pailsus*" snakes.

I'll also take it as given that most readers here are already familiar with the King Brown or Mulga Snake (*Cannia australis*) and the various regional races, such as the large animals from the top-end of the Northern Territory, the different looking animals from the Eyre Peninsula in South Australia and so on.

For those who want a better picture of all this, refer to my original descriptions or a more recent paper (Hoser 2001), which goes further and describes three Island forms of *Cannia australis*.

For other information on *Cannia*, such as husbandry and the like, refer to publications such as Eipper (2000) and the sources cited therein.

A brief history of the *Pailsus* descriptions

This is where the present story begins, ... or does it?

In 1998, I was promoting and selling copies of my book *The Hoser Files - The Fight Against Entrenched Official Corruption* (Hoser 1995) in Ballarat (Victoria). I visited a prominent local herpetologist, and VAAH member, Mr. Roy Pails. As per most visitors to his residence, Pails showed me what he alleged was a new species of "King Brown Snake".

Roy had been doing this for years, so in reality these snakes, or the new species he said he had, was perhaps the worst kept secret in the world.

In fact he'd even come under scrutiny from the local wildlife authority (DNRE) in relation to his unusual snakes.

Previously, officials with the local authority had taken extra time to inspect these snakes and concluded (for want of alternatives) that they were in all probability just a variant of *Cannia australis*, as was recorded on Pails' record books and so no action was taken against him.

Legal action against Pails had been considered, but the idea was dropped after the department was unsure as to what, if anything they could charge him with in connection to these snakes of doubtful taxa.

You see they keyed out as *Cannia australis* as per Cogger's books and the like, but even Blind Freddy could see that they were somehow different.

During the 1998 visit, Roy asked me to describe the snake as a new species.

Why me?

Well Roy said he was impressed with my Adder (*Acanthophis*) descriptions in Monitor 9 (2) 1998 (then just published) and so he thought it made sense that I also described this new species.

And so I immediately commenced inquiries.

For the record, Pails has also identified other as yet undescribed reptile taxa.

Now in terms of the new snake, I asked Roy to "Please explain" why his snake was different.

He gave a host of explanations. However none of them satisfied me as being diagnostic in that they could definitively separate the snakes (*Cannia australis* versus this new species).

A subsequent scale count of this "new" species and a series of "normal" Mulga Snakes (*Cannia australis*) by the pair of us revealed consistent differences in subcaudal counts and the size and shape of the rostral (top-snout) scale between specimens of each species from the same area. Coupled with the suite of other characteristics already outlined by Pails (smaller adult size, slimmer more gracile build, etc., all of which on their own could be said to fit within the normal range of *C. australis*) I was satisfied that we had a new and sympatric species.

As the species were sympatric and clearly different, there was no need to engage in molecular biology methods to separate the two and so none was done.

The fact is that most species of reptile known to science were described without having their DNA inspected and claims to the contrary by some of my detractors are just plain lies.

However before I could go ahead and describe the "new" species we had to do a check of the literature to see if there was any descriptions that already fitted the "new" species and had somehow been overlooked and/or later erroneously placed in synonymy with *C. australis*.

This part was a straight forward process of elimination.

A check of Cogger et. al (1983) and Wells and Wellington (1983 and 1985a) revealed several other descriptions since regarded by most authors as being

redescriptions of *C. australis* and/or regional variants (see Hoser 2001 for more details).

Through a process of further elimination we were able to ascertain that none of these were of the same "new" species Roy Pails had.

Again the process was simple. In most descriptions the scale counts alone excluded the snakes, while for those which lacked this information, we were able to go to the institution and look at the type and/or contact the relevant curator to do this for us.

Further telephone checks with people at these various institutions and what are best described as well-informed amateurs revealed no other "descriptions" of relevance to our "new" species.

Thus it was effectively certain that we had a "new" species.

For a herpetologist in the late 20th century the opportunity to describe and name a new species of reptile - a snake no less, is something most of us would only ever dream about.

Quite excited by all this, I then published the description of this snake calling it "*Pailsus pailsei*" (Hoser 1998b).

And yes, because of his long-term contribution to herpetology, the species and genus was named in honour of Roy Pails.

But in terms of scientific descriptions, the whole process was very boring and routine.

This probably is the main part of the "science" in this story.

New Guinea

The story in regards to *Pailsus rosignollii* is also similar.

In a casual phone conversation between Italian herpetologist Joe Mara and myself, Mara mentioned a "King Brown Snake" that he had, that hadn't grown more than three feet in length and he thought may have some kind of unknown ailment.

Mara was somewhat perplexed as he'd done a swag of tests on the animal and came up with nothing.

Why did his "King Brown" only get to three feet instead of the usual six foot plus?

Further questioning led me to ascertain that the snake was in fact "*Pailsus*", which of course explained the relatively diminutive size of the specimen.

Mara also advised it was from the island of New Guinea.

Further inquiries to all relevant institutions and other relevant sources, such as keepers, dealers and the like revealed that all so-called King Brown Snakes known from New Guinea were in fact of this species and that it was sufficiently different from the two known Australian specimens to be placed in a separate species. Subcaudal counts, head colouration and other attributes separated the snakes from their Australian cousins.

That description was then submitted to, and after some unexpected delays, published in *Litteratura Serpentina* in late 2000.

During all these periods of inquiry there was never any secret in the fact that I was describing these snakes and all letters, e-mails and the like sent all over the place to dozens of individuals consistently made this point clear.

Furthermore there was never any indication in relation to any of these descriptions that other people intended describing any of them and based on the information just given, there was no reason to suspect anyone was intending to do so (more on this later).

Later inquiries

In the period from 1998 to present, I've continued this study and since inspected hundreds of live and dead specimens, photos, etc, identified by others as *Cannia* and *Pailsus*, including relevant type specimens and the like.

Central elements of this (still ongoing) study included to ascertain the relative status of snakes of the genera *Pailsus* and *Cannia* as defined by Hoser (1998b), distribution of and comparisons between various formally named taxa including "*Cannia weigeli*" and *Pailsus pailsei*, and to identify any hitherto unnamed taxa.

A list of most material examined to mid 2001 can be found at the internet link below:

<http://www.smuggled.com/boycan1.htm>

By mid 2001, I'd managed to inspect the entire available holdings of "Mulga Snakes" (*Cannia australis*) at the Queensland Museum and a number from the Australian Museum in Sydney. Included were samples from all mainland states, excluding Victoria.

The exact distribution of both *Pailsus pailsei*, *Pailsus rosignollii* and *Pailsus weigeli*.

These have been burning questions and are not yet properly answered.

However there has been considerable misinformation and speculation on this topic published on the internet by various persons and this is just the beginning of the part of the story where politics and frauds apparently overtook the science.

By way of example, in a post dated: Sat, 3 Feb 2001 11:20:02 +1100 on australianherps@yahoo.com John Weigel claimed to have inspected large numbers of "King Brown Snakes" in Museums and found lots of *Pailsus pailsei/Cannia weigeli* from the top end of the Northern Territory, going on to state that it was a common species. Weigel even asserted that the type specimen of "*Pseudechis australis*" was in fact a *Pailsus pailsei/Cannia weigeli*.

He then claimed to have lost his data in the big reptile park fire of mid 2000, and thus his claims went effectively unsubstantiated.

I knew Weigel was wrong about the latter argument at least.

You see we had already ascertained that the type specimen of *P. australis* was NOT *Pailsus pailsei/Cannia weigeli*. The subcaudal count alone excluded that possibility!

(For the record, the type specimen of *C. australis* has 41 single subcaudals followed by 23 divided, a 94 cm snout-vent and 17.2 cm tail (also see McCarthy 2001)).

And as of August 2001, the only positive locations for any of the *Pailsus* species are as follows:

Pailsus pailsei - North Queensland in the vicinity of Mount Isa (4 specimens only).

Pailsus weigeli - Mitchell Plateau, Western Australia (1 specimen only)

Pailsus rosignollii - Irian Jaya, south coast region (Numerous specimens)

For *Cannia australis*, the species is found throughout most parts of Australia save for most of the far south and far south-east. Detailed distribution information for the species is provided by Cogger (1992), Hoser (1989), Ingram and Raven (1991), Longmore (1986), Wilson and Knowles (1988), Worrell (1972) and others.

Now that's not to say that *Pailsus* don't occur elsewhere, but so far, none are known.

The snake described by Wells and Wellington in 1987 and since known in the herpetological community as "*Cannia weigeli* Wells and Wellington 1987"

Following publication of the original description of *Pailsus pailsei* Hoser (1998b), I got a phone call from Richard Wells.

Wells stated that he had described a similar snake in 1987 and named it "*Cannia weigeli*".

I called on Richard Wells to provide a copy of this "description" but he said he didn't have a copy.

Subsequently, John Weigel e-mailed me with the same story, but also said he didn't have a copy of the description.

Checks of the Australian and Victorian Museums also failed to locate a copy of this alleged description. The persons spoken to claimed ignorance of the "description", which wasn't altogether surprising, as this concurred with my own inquiries preceding the publication of the first *Pailsus* description.

Sometime after this, (31 October 1998 to be exact), Peter Mirtschin faxed me a copy of this "description", having himself received the "paper" by fax from co-author C. Ross Wellington the same day.

This "description" if that's the correct word, was written by Wells and Wellington in 1987. It consisted of a typed "paper" and line drawings that was then photocopied and disseminated.

As far as I could ascertain, only a handful of copies were ever distributed. It was never published in a journal as such and based on the fact that it was merely presented in the form of a few photocopied sheets, it possibly failed to fit the ICZN's code, namely section 8.1.3. which says to be published properly and in accordance with the code "it must have been produced in an edition containing simultaneously obtainable copies by a method that assures numerous identical and durable copies." (see below).

In my view, the relative lack of copies and the relatively undurable nature of the publication (mere sheets of paper) may mean that the publication is not valid within the bounds of the ICZN's code.

However the content of the description itself, several pages in length, certainly complied with the rules of the ICZN as applied at the time, even if I disagreed with some of the taxonomic conclusions drawn in the same paper following the formal description of the type specimen.

Further discussion with Richard Wells led to him telling me that he thought that *Pailsus pailsei* and *Cannia weigeli* were probably separate species or at least subspecies, with no *Pailsus* being known from the intervening parts of the Northern Territory.

He said that himself and others had collected extensively in this region and found no snakes that fitted the *Pailsus* description.

I took what I accepted as Richard's better judgement on this and tentatively went along with this proposition.

Wells stated that he agreed with the erection of the new genus *Pailsus* and that "his" species "*weigeli*" should be transferred to the genus *Pailsus*.

Why did he agree?

Well in his original description Wells (and Wellington) placed their snake in the genus *Cannia*, but then asserted that the species was in many ways intermediate in characteristics between *Cannia* and *Pseudonaja*. As it was not tenable to merge those two genera, it seemed reasonable to assign these new species to a third genus ... the one I called "*Pailsus*".

At that time the whereabouts of the alleged type specimen of "*Cannia weigeli*" was not known so I was effectively unable to proceed further with inquiries into the matter.

Wells directed me to the Australian Museum (in Sydney) who subsequently directed me to the West Australian Museum (Perth). The former institution (Ross Sadler) said they had sent the type specimen to the latter institution.

However notwithstanding all the above, Wells' discovery of and effectively unknown description of yet another *Pailsus* was newsworthy and so I alerted the herpetological community of this other description in the next issue of *Monitor* (10(2/3)), published in 1999.

It was through this publication (Hoser 1999b) that the herpetological community at large first became aware of this hitherto unknown form,

(although this salient fact hasn't stopped my detractors from later falsely claiming that I was somehow involved in a grand conspiracy to suppress the name).

In 2001, following the e-mail campaign against me by Williams and Wüster on the internet, Glen Shea contacted me on 5 February via e-mail (Shea 2001) to tell what he knew of "*Cannia weigeli*".

He stated that he had published a mention of the Wells and Wellington description in *Herpetofauna* the following year (by way of synonymy with *P. australis*) (Shea et. al. 1988) and that he also regarded the Wells and Wellington "paper" as being validly published and/or the *Herpetofauna* reference somehow validating the original description.

A check of the Shea et. al. paper in *Herpetofauna* confirmed what Shea said was correct. It also indelibly gave the Wells and Wellington description priority in terms of date, over the Hoser descriptions.

However it was my first republication of information on the Kimberly *Pailsus* in 1999 - 11 years after Shea et. al. had effectively buried it, that *Pailsus weigeli* once again became known as a valid and/or distinct Australian species.

In March 2001, Jeanette Covacevich told me that by her interpretation of the ICZN rules, she disagreed with the assertion that the Wells and Wellington "paper" (Wells and Wellington 1987) was "published" and/or validated by Shea et. al. 1987.

There have been mixed views on this point expressed to me by numerous herpetologists, some of whom appear to have axes to grind in one direction or other (not those named above) and as seen by the results published in Hoser (2001), the final determination of this matter by herpetologists (other than myself) will probably have no impact on the ultimate acceptance and usage of the species names *pailsei* and *rossignollii* as assigned by me. The reason being that the three names *weigeli*, *pailsei* and *rossignollii* all evidently identify three distinct taxa!

For the benefit of readers of this journal who may be confused, the earlier Wells and Wellington papers published in 1983 and 1985 (the ones that caused a big storm and an attempt to have them suppressed by the ICZN) were published in a proper magazine style journal called the "*Australian Journal of Herpetology*".

The "*Cannia weigeli*" description was not.

Thus I was in a situation whereby the *Cannia weigeli* description had been effectively overlooked due to the fact that next to no one, including those who should, knew of it.

If nothing else, it appears that Wells and Wellington failed one of the ICZN Code's recommendations to widely disseminate their "paper".

However in defence of the pair, they were at the time busy fighting a rearguard action to "save" the names they'd proposed in a total of 357 taxonomic and nomenclatural acts/changes in relation to Australasian herpetofauna (in three papers) in the face of actions by other Australian herpetologists to have them suppressed by the ICZN, and so the pair were probably otherwise preoccupied. Refer to Anonymous (1987) and later publications in the *Bulletin of Zoological Nomenclature* relating to ICZN case number 2531 for details.

What was 100% certain to me by end 1998 was that "*Cannia weigeli*" and *Pailsus pailsei* were very similar, if not the same.

And yes, as already stated, the picture of the relationships between the two have now become somewhat clearer.

Throughout the period 1998-2001, Wells and Wellington and myself have been united on one important aspect. This has not been so much, what the snakes are ultimately called and/or who is listed as the describer, but rather that they are identified as distinct species and named properly as per ICZN rules.

Thus in this respect at least, we have been united as one.

Finally and notwithstanding the comments above, I suggest usage of the name "*weigeli*" be preserved unless and until the ICZN is called upon to rule otherwise and after such ruling is made. Quite frankly this makes common sense.

For the first time ever, photos of the holotypes of "*Cannia weigeli*" and "*Cannia centralis*" (the latter animal not yet recognised by myself at the species level), both formally named by Wells and Wellington in the 1980's were published by Hoser (2001) as an aid to assist other herpetologists in forming their own views about the taxonomy of these snakes.

***Pailsus* specimens known to date**

As already mentioned, there are not too many so far.

Firstly there was the original pair that Roy Pails had in 1998. One live and the dead one that was lodged with the National Museum of Victoria, that was later labelled the "Type specimen".

In 2001, an inspection of the entire available holdings labeled "*Cannia australis*" at the Queensland Museum revealed just one further specimen of *Pailsus pailsei*. This was specimen number J59015 from Mary Kathleen Dam, Qld. Lat 20.47 S Long 139.59 E.

The snake was a mature male and relatively light in colour (as compared to typical *Cannia australis*). It had 64 single subcaudals (none divided), a wide v-shaped rostral typical of the species and a divided anal plate.

In an oversight, I failed to count the ventral scales.

All other specimens inspected were confirmed as *Cannia australis*.

A later inspection of a sizeable sample of "*C. australis*" from "likely locations" (translated to be the northern third of Australia, excluding most of Queensland) at the Australian Museum, revealed just one specimen of *Pailsus pailsei*. This was specimen number R11359 from Lilydale Spring, Riversleigh Station, Queensland (Lat. 138.45 E, Long 19.50 S). The snake was like other *Pailsus*

pailsei in being a relatively light animal, but this may in part be due to the fact that it had been collected in 1934. It had 65 single subcaudals (none paired), 219 ventrals, 17 mid body rows and a divided anal plate.

The snake's sex was not determined, but it had no everted hemipenes. The specimen measured 110 cm total length, with a 92 cm snout-vent.

Thus in reality, the known distribution of *Pailsus pailsei* remains effectively confined to the hilly country around and north of Mount Isa, in Queensland, covering a known (straight line) distance of about 150 km of relatively uninhabited and unexploited country.

No snakes conforming to "*Cannia weigeli*" were inspected by me. However "standard" *Cannia australis* from the Kimberly district, WA, were inspected by me, indicating that it is sympatric with "*Cannia weigeli*".

Typical of these snakes was specimen number R111021 from the Australian Museum. This was an adult male from the Mitchell River area of Western Australia (Lat. 125.45 E, Long 15.45 S), the type location for "*Cannia weigeli*", which was otherwise typical for *Cannia australis*. It measured 195 cm in total length, 170 cm snout-vent, with 201 ventrals, 42 single subcaudals (excluding numbers 35-36 which were paired), and 14 divided subcaudals, excluding number 3 of these which was single.

(Note: the counts run from the anterior end).

The snake had 17 mid body rows and a U-shaped rostral which is fairly typical of *Cannia australis*.

Wells and Wellington (1987) gave a detailed description of their "*Cannia weigeli*" snake, including it's having 17 mid body rows, 222 ventrals and 75 subcaudals (allegedly all single).

No snakes of intermediate characteristics were found from this immediate region, indicating sympatry between the forms - in line with Queensland *Pailsus*.

The type specimen of "*Cannia weigeli*" is from here on identified as *Pailsus weigeli*.

It bears the Australian Museum tag R123995 and WA Museum tag R98871. It is separated from *Pailsus pailsei* by a suite of characteristics including the following:

- A sizeable amount of dark pigmentation on the head and neck scales. This amount is unknown in *Pailsus pailsei* or for that matter *Pailsus rossignollii*.
- The front nasal scale has a distinct protrusion into the rostral in *Pailsus weigeli*, while in *Pailsus pailsei* it runs more or less flush to a single line created by the merging of the borders of the supralabial, nasal and internasal. Because of damage to the head of the type specimen of *Pailsus pailsei* this is best illustrated by comparing the photos of the *Pailsus weigeli* type and the *Pailsus pailsei* depicted on the cover of *Monitor* 10 (1) 1998 (see Hoser 1998b), and/or looking at the head or photos of Australian Museum R11359 and/or Queensland Museum J59015.
- In *Pailsus weigeli* the third supralabial is relatively large and elongate (rectangular shaped) (the long sides running towards the top of the head), as compared to the same scale in *Pailsus pailsei*.
- Correspondingly, the preocular is much smaller in *Pailsus weigeli* than in *Pailsus pailsei*.

In most other respects *Pailsus weigeli* is essentially similar to *Pailsus pailsei*.

The known distributions of the two snakes are also separated by a vast distance (well over 1000 km in a straight line).

The colouration of the type specimen of *Cannia weigeli* conforms with Smith's 1982 account of juvenile *C. australis* from the Kimberley region.

Based on my own inspections of *C. australis* from most of Australia, dark markings on the head and neck are not uncommon in younger specimens of *C. australis* from some locations and as Smith asserted, they tend to fade with age.

However it remains uncertain if any of the specimens inspected by Smith were in fact *C. weigeli*.

Since the description of *Pailsus rossignollii* several private keepers in the northern hemisphere have come forward to advise this author that they possess the species and/or done likewise via the "Kingsnake.com" forums. Ditto for a number of Indonesian reptile dealers.

All specimens identified as such have conformed to the species as identified by Hoser (2000) and based on subcaudal scale counts received to date, the diagnostic differences between *Pailsus rossignollii* and *Pailsus pailsei* remain 100% applicable in terms of separating the species in the absence of any other available data, (as in the usually lower subcaudal counts for the Irian Jaya species).

In spite of this, Messers David Williams and Wolfgang Wüster have made a point of deliberately creating confusion by making numerous posts on the internet and elsewhere claiming authority on these snakes and then trying to lead these people to believe that they merely have unusual looking *Cannia australis* (see later).

However by virtue of the fact that nothing further of substance has been published on these snakes (other than Hoser 2001) and that all further known specimens continue to derive from the Merauke area of Irian Jaya, little further knowledge of the species is yet available.

Pailsus rossignollii is separated from *Pailsus weigeli* by several characteristics including it's different head colour, relative lack of dark blotches and markings on the forebody and other characteristics.

In the original published description of "*Cannia weigeli*", Wells and Wellington stated that the type specimen has all single subcaudals. But from the photos

seen by myself, it appears that the last two may in fact be divided. However such a feature (if present) does not appear to alter the present status of this species or others that are similar (*Pailsus pailsei*, *Cannia australis*), as such a trait (paired subcaudals in limited number) has already been seen in *Pailsus pailsei* and it has been clearly demonstrated by Pails that it is specifically different to *Cannia australis* (see Hoser 1998b for further details).

***Cannia australis* and the status of other previously described forms**

Still sticking with the science part of this story, I'll talk a little bit about *Cannia australis*.

For a taxa such as *Cannia australis* (recognized here as all snakes previously known as "Mulga/King Brown" snakes, excluding *Pailsus*), with a near Australia-wide distribution and known regional variants, it is obvious that there must be a number of different species and/or subspecies within the group. Authors have traditionally lumped all together under the one label, or in the other extreme tended to split off a number of regional variants as new species (like Wells and Wellington 1985a), a view apparently shared by a number of other respected Australian herpetologists.

Further complicating things has been a higher than expected degree of variation within a single locality, particularly in the Centralian and West Australian parts of the range, as well as clinal variation between regions (as noted by Smith (1982)).

Previously assigned names include the following; derived from Cogger et al. (1983), Wells and Wellington (1985a) and Hoser (2001):

Naja australis Gray, J.E. (1842). Description of some hitherto unrecorded species of Australian reptiles and batrachians. in Gray, J.E. (ed.) *Zoological Miscellany*. London : Treuttel, Wdrtz & Co. pp. 51-57 [55]. Type data: holotype, BMNH 1946.1.20.39, from Port Essington, N.T.

Pseudechis darwinensis Macleay, W. (1878). Notes on a collection of snakes from Port Darwin. *Proc. Linn. Soc. NSW* 2: 219-222 [220]. Type data: holotype, AM R31927, from Port Darwin, N.T.

Pseudechis cupreus (part.) Boulenger, G.A. (1896). *Catalogue of Snakes in the British Museum (Natural History)*. 3. London : British Museum xiv 727 pp. pls 25 [329]. Type data: none; description based on literature, from "Murray River", NSW./Vic.

Pseudechis denisoniodes Werner, F. (1909). Reptilia exkl. Geckonidae und Scincidae. in Michaelsen, W. & Hartmeyer, R. (eds.) *Die Fauna Sudwest-Australiens*, Jena : Gustav Fischer 2: 251-278 [258]. Type data: holotype not found, from Eradu, (near Geraldton), WA.

Pseudechis platycephalus Thomson, D.F. (1933). Notes on the Australian snakes of the genera *Pseudechis* and *Oxyuranus* *Proc. Zool. Soc. Lond.* 1933: 855-860 [859 pi 3 figs 1-2]. Type data: holotype, NMV D12355, from East Alligator River, Arnhem Land, NT.

Denisonia brunnea Mitchell, F.J. (1951). The South Australian reptile fauna. Part 1. Ophidia. *Rec. S. Aust. Mus.* 9: 545-557 [551 fig 21]. Type data: holotype, SAMA R3151, from Mount Wedge, near Elliston on the W coast of Eyre Peninsula, SA.

Pseudechis butleri Smith (1982) 35-45 Type data holotype WAM R22345 from 19 KM SE of Yalgoo, WA

Cannia centralis Wells and Wellington (1985) 1-61. Type data holotype AM R60317 from 8 km north of Tenant Creek, NT.

Cannia australis aplini Hoser (2001) 26-60. Type data holotype WAM R82994 from Koolan Island, WA. Lat. 123.47 E Long. 16.08 S.

Cannia australis burgessi Hoser (2001) 26-60. Type data holotype AM R135292 from Gemco Mining Lease Area, Groote Eylandt, NT. Lat 14.00 S 136.40 E.

Cannia australis newmani Hoser (2001) 26-60. Type data holotype AM R13474 from Bathurst Island, NT. Lat 11.77 S Long 130.23 E

Of these formally described variants, only "*australis*" as the King Brown or Mulga Snake and "*butleri*" as the Butler's Snake have been generally accepted as distinct species by most Australian herpetologists.

Due to the high biodiversity of the Australian herpetofauna and the relatively small number of herpetologists in this country, taxonomy of species at the subspecies level has been largely ignored.

However I expect higher recognition of reptiles at this level in Australia in future years.

Perhaps the trend is being led by the likes of myself who in 1998 described a subspecies of *Acanthophis* (Hoser 1998a) and more recently by Dr. Glen M. Shea, who also by way of example described two new subspecies in 2000, namely, *Tiliqua gigas evanescens* Shea 2000 and *Tiliqua scincoides chimaerea* Shea 2000 to identify regional variants of well-known and familiar species.

The trend has also been continued by myself in 2001 by the naming of the three regional variants of *Cannia australis* as listed above.

Based on examination of living and dead specimens in Australian collections and museums, I also formed the view that *butleri* is a distinct species from *australis*, while the same may be true for some of the above described variants.

The type specimen of "*australis*" has been examined on my behalf and is typical for the species as known. It has numerous paired and divided subcaudals. "*darwinensis*" is regarded as a junior synonym to "*australis*".

"*cupreus*" is regarded as also being of the same species as "*australis*". While it has been impossible to examine the type specimen, "*australis*" from NSW have been examined and appear not to be substantially different from specimens in Queensland and in turn the Northern Territory, or at least not enough to warrant being treated as a different species. The differences between NSW and NT *Cannia australis* appear to be clinal, rather than being two well-defined and disjunct gene pools, a position corroborated by the

various published distribution maps based on Museum collections (e.g. Longmore 1986).

The snakes known generally as "*denisoniodes*" from south-west Western Australia are different from "*australis*" found elsewhere in Australia and should be accorded at least subspecific status.

They attain a far smaller adult size than northern "*australis*" and differ in other characteristics (see Wells and Wellington (1987) and/or Smith (1982) for details).

"*brunnea*" are the distinctive Eyre Peninsula form of the Mulga Snake and while similar in build to northern "*australis*" are noted for the fact that the scales have a distinct dark/light contrast between the anterior and posterior of each scale, giving the snake a two-toned colour appearance.

While this is seen in *Cannia australis* from elsewhere, it is not as pronounced as in this form.

Furthermore it appears that the variation from north to south and east to west in these snakes may be clinal and that is why many authors refuse to recognise them as separate species (e.g. refer to the photo at the top of page 184 in Greer 1997 for a similar patterning in a Western Australian *Cannia australis*, albeit to a lesser degree).

Wells and Wellington not only recognised "*brunnea*" as a species in its own right but went further and described a centralian "*australis*" as a new "species", namely "*centralis*".

Further investigations are needed to determine whether or not "*centralis*" is a clinal variant of either "*australis*" and/or "*brunnea*", (which is as generally indicated by the findings of Smith (1982) and my own ongoing study).

Whatever is finally determined in this regard, it is reasonable to note here, that all conform to the typical stout "*australis*" form and not the more gracile *Pailsus*. Refer to Hoser (2001) for details.

Shea and others (personal communications) have stated that they only recognise subspecies or species when there is no evidence of a cline between regions.

I concur with that view, save for circumstances whereby the zone of clinal variation between populations is relatively small.

And yes, as already inferred, the ultimate determination of whether or not the above named forms are distinct species or subspecies will require a substantial amount of field work in the relevant regions to ascertain whether or not clines are involved and/or any forms are sympatric in regions of potential overlap; if that occurs.

Smith's description of "*butleri*" as a distinct species has been accepted without question by most herpetologists and is agreed by me. It too is of the stout "*Pseudechis*" form and not the more gracile *Pailsus* appearance.

The three Island forms described by myself in 2001 all appear to be insular races of the nominate species. They appear to be confined to their given island habitats and like the various *Notechis* subspecies described from southern Australia, these designated subspecies names will probably be accepted by the herpetological community without much question.

Subcaudal and other variation in *Cannia australis* examined

Sticking to the science part of the story, I'll report a few more facts that may be of interest to readers.

Examination of *Cannia australis* in the Queensland Museum from Queensland revealed a general pattern of *Cannia australis* having ten or more divided subcaudals (usually over 15).

Exceptional specimens from the Longreach area and to the south of here (Nareena, 48 km South of Longreach) had between 5 and 10 divided subcaudals and one specimen, J45761 from Nareena, had just the last 3 out of 61 subcaudals divided.

However it was later suggested that these specimens may have been faded and misidentified Collett's snakes (*Panacodechis colletti*).

Regardless of the proper identities of the snakes from the Longreach area, it remains true that for Queensland at least, it appears that the trait of all single subcaudals, can alone be used by field workers to separate *Pailsus pailsei* from *Cannia australis* with a reasonable degree of confidence.

However during my examination of "*australis*" specimens in other museums and private collections it became clear that there were specimens of this species with all or most subcaudals single. In all other respects these snakes were the typical heavy bodied *Cannia australis* and not *Pailsus* spp., and so there was no difficulty in assigning them to *Cannia australis*, even before resorting to counting either subcaudals and/or ventrals (*australis* having the lower counts), although in all cases we checked subcaudals at least in order to positively identify the snakes to species level.

These specimens usually, but not always, also had the lower average single subcaudal counts that separate *Cannia australis* from *Pailsus* spp.

These specimens tended to come from the Northern Territory and spanned a host of different regions within the state and adjoining areas.

Examples included:

Qld Museum specimen number J47008 from the Simpson Desert in the NT, which had all single subcaudals, except for the last three and was 1.5 metres (approx.)

Qld Museum specimen numbers J54594 and J54583, both from Gomarren Stn 100km SE of Cunnamulla Qld, which both had just five single subcaudals

Australian Museum R51945 from Alice Springs NT, which had just 3 of its 53 subcaudals single.

Australian Museum specimen number R139849 from Barkly

Highway, NT which had 56 subcaudals (all single), missing the end of the tail.

Australian Museum specimen number R10232 from Groote Eylandt, NT which had 66 subcaudals, just the last of which was paired (described by myself as a new subspecies).

A number of live specimens from various parts of the NT were examined that also fitted this general profile - details of one of which is reported later in this article.

However in these same general regions were *Cannia australis* that were identical in appearance, but had high numbers of divided subcaudals.

For example:

Australian Museum specimen number R26248 from Mount Olga, NT which had 13 divided subcaudals.

Australian Museum specimen number R60317 (the type specimen of "*Cannia centralis*"), from 8 km north of Tennant Creek, NT, which had 49 single subcaudals (no. 46 divided), then 18 divided subcaudals.

Australian Museum specimen number R60318 from Barrow Creek, NT, which had many single and many divided subcaudals.

Australian Museum specimen number R32639 from Port Essington, NT which had 31 single and 31 divided subcaudals.

Typical examples from outside the NT, included:

Australian Museum specimen number R60315 from Bourke, NSW which had numerous divided subcaudals.

Australian Museum specimen number R82560 from Weipa, Far North Queensland which had 30 single and 27 divided subcaudals.

Excluding the Groote Eylandt and Bathurst Island specimens, there was little if anything to separate the snakes with mainly single subcaudals from those with mainly divided ones, and hence the treatment here of all NT and north-west Western Australian "*australis*" inspected by myself as being of the same species.

Neil Sonneman observed a similar pattern in WA "*australis*" in the Western Australian Museum. However his inspection was rushed and he may have inadvertently looked at some *Pailsus weigeli* at the same time; a point he readily concedes. Also refer to Hoser (2000a) re an alleged *Pailsus* from Wyndham, WA.

Sonneman also drew this author's attention to unusual specimens at the West Australian Museum from Koolan, Cockatoo and other Islands in north-west Western Australia, and adjacent mainland areas, stating that further investigation was warranted and provided evidence of dwarfism in some of these island populations.

Koolan Island *Cannia australis* were described as a new subspecies in Hoser (2001).

Groote Eylandt *Cannia australis* were first brought to my attention when in 1998 or early 1999, and immediately following the description of *Pailsus pailsei*, Victorian herpetologist Rob Valentic gave a talk at a VAAH meeting.

He reported that he had seen a road-killed specimen of this species (*Pailsus pailsei*) on Groote Eylandt in late 1998 and identified it as such by the all single subcaudals. He said he'd caught another.

Save for a small piece published to this effect in *Monitor* 10 (2/3) (Hoser 1999b), nothing further came to light until early 2001, when I was able to examine relevant specimens from this locality at the Australian Museum.

Later Rob Valentic gave another talk and said "Hoser's description was wrong. All the snakes on Groote Eylandt are not *Pailsus pailsei*, but instead the animal described by Richard Wells and should be called "*Cannia weigeli*".

Getting into the politics side of things, Rob was still upset that I hadn't named a Death Adder after him in 1998, so was seizing on an opportunity to get stuck into me so to speak.

And yes, it's a pity that so-called herpetologists have to stoop to such levels.

You see if Rob had really been interested in the science and not the politics, he'd have properly read either my original *Pailsus* description and/or the Wells and Wellington paper, which by that stage I had photocopied, converted to pdf format and sent all over the place. Had he done so, then he'd have been able to work out that the Groote Eylandt snakes were not the same as what either of us had described.

And yes, it's an even greater pity that Rob Valentic had stooped down to the level of herp politics instead of science, as his general competence as a herpetologist is not in dispute.

But all this was insignificant compared to the herp politics being engaged by David Williams and Wolfgang Wüster as detailed shortly.

The Groote Eylandt specimens, while evidently of the species *Cannia australis*, exhibited a number of characteristics different from other *Cannia australis* and therefore warranted being identified as a different taxa.

So I described them as *Cannia australis burgessi* in 2001.

Characteristics as seen include a tendency in scalation (subcaudals and rostral) towards that of *Pailsus pailsei*. The reasons for this can only be guessed. But I raised a load of possibilities in Hoser 2001.

A similar situation was also seen in *C. australis* from Bathurst Island, also described as another separate taxa (*C. australis newmani*).

And as already inferred, it is also likely that specimens from other Islands in northern Australia may warrant recognition at the subspecies level.

Separating the three new subspecies

Distribution is probably the best and most certain means.

Cannia australis aplini comes from Koolan Island, WA, *Cannia australis burgessi* from Groote Eylandt in the NT, while *Cannia australis newmani* occurs on Bathurst Island (and probably Melville Island as well) in the NT.

Cannia australis burgessi is similar in most respects to *Cannia australis* from which it can be identified by the following suite of characters. An average lower count of divided subcaudals and a wider rather than horseshoe shaped rostral scale, although this latter trait is not universal as seen from the series of specimens examined by myself. These snakes are separated from *Pailsus* (all forms) by their larger adult size and more stout build, as typified in the type specimen. There are also differences in the head scalation, including the fact that the preocular in *Cannia australis burgessi* tends to of a more triangular shape (in an up/down direction) than seen in *Pailsus*. Also refer to head photos and/or a comparative inspection of the type specimens. Based on the theory of "character displacement" in similar species as referred to by Kluge (1974), it's unlikely that there are any *Pailsus* on Groote Eylandt.

Cannia australis newmani is separated from *Cannia australis burgessi*, other *Cannia australis* (excluding *Cannia australis aplini*) and *Pailsus* species by its much broader parietal scales. The line separating each supraocular and frontal is more strongly curved in this subspecies than in *Cannia australis*, *Cannia australis burgessi* or *Cannia australis aplini*, (as described by Hoser 2001).

Cannia australis aplini is similar in most respects to the type subspecies. It is separated from the type subspecies (as indicated by the holotype, BMNH 1946.1.20.39, (deemed here as a typical example) from Port Essington, N.T.), through it's usually lower divided subcaudal count and generally smaller adult size. *Cannia australis aplini* (including the type specimens) does in common with most typical *Cannia australis australis* have a horseshoe shaped rostral, which serves to separate the species from *Pailsus* (v-shaped rostral). In contrast to the type specimen of *Pailsus weigeli*, (a species likely to be confused with this subspecies) *Cannia australis aplini* tends to have distinctively broader parietals, and a generally more thick-set build, particularly around the head and neck. The latter trait on it's own is usually more than enough to separate the two species (for any person with some experience with these taxa).

An updated assessment of the relationships between species in the genera *Pailsus* and *Cannia*

Still sticking with the science, Laurie Smith in 1982, p. 44 suggested that *Cannia australis* may have expanded their range in relatively recent geological times, at the expense of other "*Pseudechis*", including *colletti* and *guttatus*, which as a result tend to have relatively restricted distributions.

Wells and Wellington (1987) in effect said much the same thing, when they asserted:

"We believe that the discovery of *Cannia weigeli* indicates an archetypic remnant of the original basal stock that may have given rise to the genera *Cannia* and *Pseudonaja*. *Cannia weigeli* may well be pivotal in the enunciation of one of the major proteroglyph speciation events in the post Gondwana Australasian region."

While it is true that in many respects *Pailsus* appears intermediate between *Pseudonaja* and *Cannia*, this author's view is that *Pailsus* split from *Cannia* group well after "*Pseudechis*"/*Cannia* and *Pseudonaja* diverged.

However as *Pailsus* is clearly of the more "standard" elapid form, it would be reasonable to assume that the archaic stock was more in line with *Pailsus* than *Cannia*.

And in line with Smith's conclusion in relation to *Cannia australis* usurping other "*Pseudechis*" in recent geological times, it appears that the same reasoning could be applied to *Pailsus*, which appears to have declined at the expense of *Cannia*, the result being a relatively patchy and disjunct distribution in Australia.

On the other hand, in southern New Guinea, where it appears that *Cannia australis* has failed to appear and/or *Panacedechis papuanus* is sufficiently differentiated from *Pailsus*, these snakes remain relatively common and prominent where they occur.

I understand that *Panacedechis papuanus* has been found on Sabai Island in Torres Strait, while no *Pailsus* are known from this area. On that basis (and further based on the crude assumption that *Pailsus* do not occur in the Torres Strait area) it may be reasonable to infer that even *Panacedechis papuanus* (thought to be the more widely distributed) is a more recent entrant to the New Guinea fauna and that it's relatively greater distribution there has been to an extent at the expense of *Pailsus*.

This is even though both remain locally abundant in the Merauke area of Irian Jaya.

A credible and contrary view may be that both *Panacedechis papuanus* and *Pailsus* entered New Guinea at about the same time and that *Panacedechis papuanus* has simply been more successful there, with the populations on the islands to the immediate south merely being part of a more recent migration towards Australia.

Furthermore while it is hard to speculate as to the evolutionary advantages/disadvantages or roles of character manifestations, including single versus paired subcaudals in elapids such as *Pailsus* and *Cannia*, it is reasonable to assume that the manifestation of these traits (one way or other) coincides with other adaptations that enable the snakes to survive better in their environments.

Kluge (1974) noted "character displacement" in *Lialis jicari* from Southern New Guinea as compared to northern specimens (sometimes known as *Lialis cuneirostris*).

It appears that the same phenomena may have occurred in *Cannia australis* in northern Australia. Thus in regions known to have *Pailsus* spp. it appears that

Cannia australis are more likely to have lower ventral counts and higher counts of divided subcaudals (e.g. Queensland). In areas where only one form is known to occur (e.g. Groote Eylandt), the *Cannia australis* tend towards more intermediate characteristics, in that subcaudal counts are up and there is a far stronger tendency towards all single subcaudals and/or even the rostral may tend towards being V-shaped rather than horseshoe shaped.

It's assumed that these observed traits are indicators of other as yet unobserved character manifestations.

Assuming the "character displacement" phenomena to be real in *Cannia australis* in relation to *Pailsus* spp. it becomes reasonable to infer that where this is detected (*Cannia australis* tending towards *Pailsus* traits) the likelihood of *Pailsus* occurring in the same areas is in fact reduced. On that basis it would appear that if *Pailsus* do in fact occur in the Northern Territory (presumed highly likely), their distribution would at best be very patchy.

However the areas most worth looking at would be places such as Port Essington, where *Cannia australis* with high divided subcaudal counts are known.

Furthermore, of note is that while *Pailsus* are so far known in Australia only from very rocky locations, the reverse seems to be the case in Irian Jaya. The Merauke area is essentially without rocks.

This may further indicate that *Pailsus* have in fact been displaced from these non-rocky habitats in Australia by the apparently more effective and competing *Cannia australis*, with *Pailsus* only holding out in a few relictual habitats.

Mengden, et. al. 1986 presented a series of morphological, electrophoretic, karyotypic, ecological and behavioral data in order to make an assessment of the genus "*Pseudechis*".

They in turn concluded that this was a monophyletic group.

Wells and Wellington (1987) revisited this assessment and using the same data, they concluded to the contrary, that their own earlier division of the genus "*Pseudechis*" was correct. As already mentioned, they had split the group into *Pseudechis* (being *porphyriacus* only), *Cannia* (including *australis* and *butleri*) and *Panacedechis* (including *colletti*, *guttatus* and *papuanus*).

And yes, as already mentioned at the start of this article, I concur with this division.

Furthermore, the fact that *Pailsus* has apparently held out far better against *Panacedechis* in Southern New Guinea than it appears to have against *Cannia* in northern Australia could be taken as further evidence to support the division of "*Pseudechis*" into these other genera.

Based on initial and comparative observations between *Cannia* and *Pailsus* in Australia, versus *Panacedechis* and *Pailsus* in New Guinea, it would be fair to assume that had *Cannia* made it into the Merauke area of New Guinea, then *Pailsus* would likely have been exterminated from there.

Notwithstanding this assessment, it may be that *Pailsus* is in relative decline in New Guinea as demonstrated by their relatively restricted range when compared to *Panacedechis papuanus*.

It's understood that "*Pseudechis*" (comprising *Pseudechis*, *Cannia* and *Panacedechis*) are being genetically tested at the moment (2001) by Ulrich Kuch and others with a view to reassessing the relationships between the known species. Following the original *Pailsus* description in 1998 I provided Kuch with a slough of the skin to aid his tests.

While his data is welcome, it is likely that herpetologists will dispute the taxonomy of these genera for some time regardless of what data he produces.

This is in part due to differing interpretations of the same facts.

Furthermore this ongoing dispute will no doubt be fuelled in part from an inertia by some people to use correctly assigned "Wells and Wellington names" in the likely event that their stated position is supported by the data.

And that could also be because of the politics and pseudoscience that sometimes gets in the way of things herpetological.

We've already seen this in the last two decades as numerous prominent herpetologists have refused to use validly assigned Wells and Wellington names, not because of their science, but rather because of the politics in being seen as the first to do so.

The Generic name "*Pailsus*"

My definition of the genus "*Pailsus*" was based on a raft of reasons and characteristics known in the snakes. However they initially hinged in the main on the physical morphological characteristics of the snakes as compared to their closest relatives (thought to be *Cannia*).

This being the smaller size and much more gracile build. Behaviorally the snakes are also considerably more aggressive (as a rule) and do not settle down in captivity in the same manner as most *Cannia*.

Because medium to large elapid snakes in Australia are usually physically conservative in terms of scalation, size, shape and colour it is not at all surprising that *Pailsus* are similar in many respects, not just to *Cannia*, but also *Oxyuranus* and *Pseudonaja* (the dominant genera of large elapids here in Australia).

In fact in terms of build and temperament, *Pailsus* is more like the latter two genera than the former, and all other "*Pseudechis*" as defined by Mengden, et. al. 1986.

And yes, even Wells and Wellington (1987) suggested that *Pailsus* (then called "*Cannia weigeli*") was somehow an intermediate form between the genera *Pseudonaja* and *Cannia*. In which case this was itself an argument for placing either *Pailsus* in a genus on its own or merging the three.

Like I already said, as the latter alternative was untenable, the only other

logical one was the former. On this basis, I believe that the three species currently identified under the generic name *Pailsus*, should remain placed within this genus unless and until compelling evidence to the contrary appears.

Furthermore, if one accepts the conclusions of Smith (1982) and Wells and Wellington (1987) to the effect that *Cannia australis* is of relatively recent stock and that it has displaced earlier forms, and then combine this assertion (based on distributional evidence) with the relationships within "*Pseudechis*" as published by Shea, Shine and Covacevich (1993) p. 308, in turn derived from Mengden et. al. (1986), one sees that *Cannia butleri* is presumed to be potentially more archaic than the more widespread *Cannia australis*. Even if the reverse is true, (*australis* the more archaic form) it all points to the common ancestor being a large and stocky snake as per the genus *Cannia* as now known. *Pailsus* (all three forms) do not fit this prototype, indicating at best, divergence a long way back in geological time and therefore should for the time being remain placed within a separate genus.

Furthermore, the wide and apparently disjunct distribution of *Pailsus* (Qld, WA, Irian Jaya), effectively precludes any suggestion that the *Pailsus* form is recently derived.

One should also note that the apparent absence of *Cannia australis* from southern New Guinea and/or wide areas of the region, also suggest that the evolution of this species (*Cannia australis*) has been recent, bearing in mind recent (in geological terms) rises in sea level preventing migration across the Torres Strait, and further noting that other northern Australian forms such as *Lialis burtonis* and *Chlamydosaurus kingi* had already been able to get across to southern New Guinea / Irian Jaya.

The present distribution of *Pailsus rossignolii* (confined to southern Irian Jaya / southern New Guinea) also indicates that it's basal stock came from the Australian side, particularly when reconciled with the known distribution of cogeners *Pailsus pailsei* and *Pailsus weigeli*.

Suggested pointers for other people to investigate

Australians and others with an interest in *Pailsus/Cannia* are advised in the first instance to inspect at least some known specimens of *Pailsus pailsei* and/or *Cannia weigeli*.

With specimens now held at Australia's major museums, including at Sydney, Brisbane, Perth and Melbourne, this is not any longer a difficult task for most seriously interested persons. Particularly so, as the relevant curators have had these specimens pointed out to them and can presumably access them without too much difficulty.

As a second option, persons are directed to read and re-read the relevant descriptions by myself as reposted on the web and study the various published photos, including those of the live *Pailsus pailsei*, depicted on the cover of *Monitor* 10 (1) (Hoser, 1998b).

In many respects, it is actually easier to examine the photos than the specimens themselves. You see they sit still!

Ted Johansen (of Browns Plains, Brisbane, Queensland), stated that he had found a road-killed snake some years back near Yeppoon in Queensland that he thought fitted the profile of *Pailsus pailsei*.

Ted Johansen said that he recalled it having all single subcaudal scales. In spite of this claim, (and further noting that Johansen is generally credible) I think it's unlikely that any *Pailsus* will emerge from that region and at this stage can offer no decent explanation in relation to Johansen's account.

The Queensland Museum has a relatively good coverage of specimens from within Queensland and based on my inspections of them, believe that no more *Pailsus* are likely to be found in that state, save for the far north-west, which is from where they are known already.

New South Wales and South Australia are not believed to be likely places for *Pailsus* and this has been confirmed via my own looking at numbers of live and preserved snakes from both states.

Although I've inspected a sample from the Northern Territory and to a lesser extent Western Australia, these are the two states deemed most likely to have more *Pailsus* (as in where these species are likely to have wider ranges). Also refer to comments in Hoser (2000a, 2001).

The State Museums in each state are both likely to have further specimens of *Pailsus*, and although it's my aim to scrutinize both collections, this may be some years away, due to other competing tasks.

And based on results so far (as outlined above), it's also unlikely that either institution will actually have a great number of *Pailsus* specimens - in spite of their sizeable holdings of "*Cannia australis*".

Notwithstanding this, herpetologists in the field are also most likely to make discoveries of specimens in the tropical regions of these two states, particularly in hilly areas and perhaps on some offshore islands.

Should a person find a "King Brown Snake" of more gracile build than usual, a high subcaudal count (215 or over), all or mainly single subcaudals and a distinctive v-shaped rostral scale, then they may well have a *Pailsus*.

If possible, these specimens should be legally collected and/or lodged in the nearest institution of note.

And yes, I always welcome any verifiable information in this regard.

Besides a single and aged, live specimen of *Pailsus pailsei* in Victoria, there are still no known captives in Australia. That is as of August 2001, some three years after the original description was published and notwithstanding a number of unsubstantiated rumors heard by me since 1998.

It is extremely important that specimens be brought into captivity, bred and studied. This is true for both *Pailsus pailsei* and *Pailsus weigeli* and any other related taxa that may yet await discovery.

Furthermore, based on a number of obvious misidentifications of *Cannia australis* with all or most subcaudals single, thought in the first instance to be

Pailsus, that have come to my attention, including the Grootte Eylandt specimens as cited above, it can be expected that similar misidentifications will happen again.

On that basis I issue a very strong warning to researchers looking at other properties in these snakes, including venom properties, DNA and the like, which do not by necessity involve a sighting of the source specimens by the individuals doing the study.

The risk of a misidentified snake being used and thereby confusing the final results is potentially large and therefore preventative steps should be taken.

In my considered view, this should include (preferably) sighting the specimen and counting the scales (ventrals, subcaudals (single and paired), size and shape of the rostral and parietals), in order to positively identify the specific status of the animal in question, and/or have this information available so that if and when doubts are raised later on, they can be effectively resolved.

By way of example, in late 1998 I facilitated venom and DNA tests on *Pailsus*, based on the sole known living specimen, held by herpetologist Roy Pails, a specimen for which all relevant data is known.

In the case of New Guinea *Pailsus* the above pre-empted problems are less likely, because it seems that there are no *Cannia australis* on the island. Thus identification of the source snake is less likely to be in error.

Fortunately there appears to be a reasonable interest in *Pailsus rossignollii* in the USA and Europe, largely following from the publication of my original description and then in part further fuelled by the endless and contradictory posts by David Williams and Wolfgang Wüster on this species that they have repeatedly described as a "non-taxa" and more should become known about these snakes as hobbyists breed them and publish their data.

Fortunately these snakes are still being legally exported from Indonesia by the local dealers there and yes, they report demand from the USA and Europe for the species is at the highest level ever!

Locally in Irian Jaya, feral animals such as Cane Toads (*Bufo marinus*) and other species may cause a long-term threat to the species, however the exact magnitude of the potential threat isn't known.

Island forms

Three Island forms of *Cannia australis* have been described by myself this year.

Expect further distinct variants to be found in various islands to the north of Australia, if and when further investigations are carried out.

It is common knowledge that islands are an excellent catalyst for a strong genetic push towards speciation. This is because of the small and isolated gene pools captured in these areas, with little if any inflow from outside, combined with the often very different habitat and predator/prey circumstances of such habitats when compared to adjoining "mainland" habitats.

In southern Australia a number of island forms of Tiger Snake (*Notechis*) have been formally described at either subspecies or full species level. Trends towards gigantism or dwarfism, dictated by available food sources has been documented in the literature (including Hoser (1989)).

Based on inspection of limited samples of *Cannia* and *Acanthophis* from islands along Australia's northern coastline and elsewhere, it appears a similar speciation push is happening and has probably been going on for the period since sea levels rose to current levels (since the last Ice Age), when many areas now islands were joined to the mainland.

In the case of *Cannia*, populations I deem in need of further study include those on the many Islands of the Bonaparte Archipelago in north-west Western Australia and Mornington Island in the Gulf of Carpentaria (Lat. 139.30 E, Long 16.30 S), any of which may have either *Cannia* or *Pailsus*.

Based on Longmore 1986, there are no records for *Cannia australis* from any Torres Strait Islands and checks should be made of any possible populations on these islands, as well as other islands in the general area between New Guinea and Australia.

It is noted that "normal" *Cannia australis* are known from the mainland areas in adjoining Cape York Peninsula.

For *Pailsus rossignollii*, and/or potential *Cannia australis*, it is important to ascertain the exact distribution of these species in New Guinea.

Obviously with the high level of political instability in the Irian Jaya / PNG border region, this is not an easy task.

However the area most in need of survey work (meaning most likely to yield positive results) is probably the coastal region between Moibut, PNG (Lat 9.10 S, Long 141.55 E) and Merauke, Irian Jaya, (Lat. 8.28 S, Long.140.2 E).

Political instability in the area makes this somewhat difficult at present.

However there is even more political instability in the herpetological community being generated by the likes of David Williams and Wolfgang Wüster who in 1998 made an entry into the *Pailsus* story in terms of the politics and the scientific fraud aspects.

The politics of the science

It'd be nice to say that there is science in the following part of the *Pailsus* story, but unfortunately there isn't.

There is however a liberal dose of politics, lies and outright scientific fraud and the saga itself would perhaps make good subject matter for a "real life" novel.

It'd be nice to say that this story started in 1998 when I described *Pailsus pailsei*, but that isn't entirely accurate.

To put a start date as 1987, the date Wells and Wellington described their animal as "*Cannia weigeli*" isn't really accurate either.

For while Wells and Wellington have been the victims of perhaps more smear,

innuendo and "politics" than any other herpetologists in Australian history, their own trials and tribulations don't generally fit within the ambit of this story, save for what was written above.

Furthermore, the general facts of the *weigeli*, *pailsei*, *rossignollii* descriptions as outlined above are not in dispute and effectively immutable, so Wells, Wellington and myself all agree that we shall move forward, not back and try to work on the herps, not the politics.

So when did all this *Pailsus* politics actually start?

Try 1993!

Now there wasn't any *Pailsus* caught or described in that year, or not that I know anyway, but 1993 still seems to be the most appropriate date to start this story.

In 1993 I published *Smuggled: The Underground Trade in Australia's Wildlife* which detailed warts and all the Australian wildlife trade, both legal and illegal.

The book was initially unlawfully banned by the NSW National Parks and Wildlife Service (NPWS), but after the ban was lifted actually hit some "best-seller" lists and was rapidly reprinted.

In that book I detailed some of the smuggling activity by Queensland snake David Williams, who in February 1992 pled guilty and was convicted of the relevant smuggling charges in a Queensland court.

In the sequel *Smuggled-2*, published in late 1996, I detailed more of Williams' activities; this time his failed Austoxin venture in New Guinea which turned out to be nothing more than a front for smuggling snakes from New Guinea to the northern hemisphere.

I chose not to detail other criminal activities by Williams on the basis of relevance.

Williams didn't sue, presumably because all that was published about him had long been on the public record (including having been reported in the tabloid daily papers) and was true and correct.

However because he was adversely named in two "Hoser books", Williams has since then made innumerable cowardly attacks on myself and my credibility at every opportunity.

This has included on various internet forums, where more recently he has been joined by his small band of followers including the likes of Wolfgang Wüster et. al..

And for the record, in this country (Australia), the credibility of Williams as a herpetologist (or much else) is very little.

Wüster, an academic based in a British university, similarly has little, if any documented expertise in Australian reptiles. And based on his writings, Wüster's alleged qualifications can also only be called into serious question.

In early 2001, when the above-mentioned "debate" turned against Williams and Wüster, principally when the Williams scientific fraud was exposed (see below) he cut off access to his list server for those who were posting against him. When the same thing happened on John Fowler's much larger Australian Herps list and Kingsnake.com forums Williams threatened to sue the list managers if they allowed the posts to remain, so in an act of caution, they too were wiped.

When Victorian herpetologist and VAAH founder Neil Davie pointed out fraudulent and/or unethical practices by Williams and Wüster on the Kingsnake forums, Williams petitioned list owner Jeff Barringer to have Davie's access cut off and it was.

Other's who similarly pointed out fatal flaws in arguments by Williams and Wüster were similarly vilified and attacked by the pair, including Bernard Frome, Pete Brammell, Paul Hackett, Benjamin Dowse, and Scott Eipper (a small number of their posts are cited at the end of this article) and others.

Wüster, an erstwhile ally of Williams also posted lies and misinformation about myself on various lists, with a spamming ability that most mass-marketers would only envy, and yes, now some of the same lies have since even been published by Wüster in "hard copy" in the generally respected journal *Litteratura Serpentina* in June 2001.

This mass posting activity didn't help Williams and his case completely as other correspondents, including Bernard Frome and Pete Brammell had also got onto the case and then independently exposed Williams and Wüster for all they had. And once again, Williams then made threats and had all posts pointing out his misdeeds wiped from the various servers.

We sought legal advice in relation to suing Williams for defamation way back in 1999 in relation to another unrelated character assassination by him of myself on the internet. We were advised against suing him on the basis that he had a bad credit history (debts totaling tens of thousands of dollars) and we would be unlikely to recover any financial damages from him in the event that we won.

Oddly enough, Richard Wells' wife, Katrina was also owed a small fortune by Williams.

Small world isn't it?

Now bearing in mind the fact that myself and my publishing company, Kotabi successfully got court issued damages awards against two other persons, namely Neil Mayger (for \$7,000) and Adam Anthony Zoccolli (for \$24,000), the latter in a defamation action, and we were then unable to recover our money because they both declared bankruptcy, we were not keen to go down this path again.

And hence Williams and his cohorts continued to peddle lies and other defamatory statements against me, safe in the knowledge that we have effectively no legal redress.

Effectively the same Wüster piece published in *Litteratura Serpentina* in June 2001 was first published on Kingsnake.com in January the same year (on January 22, 2001 at 11:29:07 to be exact), and then widely circulated

elsewhere. It was rapidly discredited by numerous correspondents (e.g. Frome (2001a, 2001b), Brammell (2001a, 2001b, 2001c, 2001d)).

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

This editor (Gijs van Aken), fell for the ruse, and so Wüster's already discredited lies got a second running.

That's the politics and the lies coming into this account.

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

But the editor, who had in fact invited my response, went back on his word and e-mailed me and said he wasn't going to print the response.

That response has since been posted at:

<http://www.smuggled.com/LitSer1.htm>

A second systematic rebuttal of Wüster's false allegations was published in "hard copy" in the latter part of Hoser (2001).

The first attacks on Hoser's name

Besides a constant string of e-mails to the various internet lists claiming that Hoser's various books are fiction - at least in terms of himself, Williams began his attacks on my credit in other ways in late 1998.

And yes for the record, all the criminal activity by Williams as detailed in the *Smuggled* books is 100% true and correct and Williams knows it.

Should anyone have any doubts, a check of the magistrates courts will reveal all.

In order to attack this my credibility, Williams decided to run a campaign to stop any herpetologists from using Hoser assigned scientific names.

And yes, the first point of attack was the description of *Pailsus pailsei*.

And so you see, that's how the politics part entered the story of the science of *Pailsus*.

Williams made a number of posts stating that the species was not valid and that it was nothing more than an underfed King Brown Snake.

Subsequently he posted an "online paper" at:

<http://www.uq.edu.au/~ddbfrj/index.html>

in November 1988 and numerous other places so as to ensure that it was picked up by all the major internet search engines.

Why did it never appear anywhere in "hard copy"? is a question that many people have since asked.

The likely reason is that Williams knew that the material within his paper was so patently false, misleading and inaccurate, that no self-respecting editor would have dared to publish it.

But in the free-for-all known as the internet, Williams and his good friend Wüster, didn't have to worry about such niceties.

Using a series of rubbery figures and a comparison of the subcaudal counts between *Pailsus* and *Panacedechis*, (which has about as much relevance as comparing apples with sausages), Williams ended his so-called paper by declaring *Pailsus pailsei* as being synonymous to *Cannia australis*. Thus in his mind at least, the Hoser assigned name *Pailsus pailsei* had been formally and forever sunk. (See Hoser (1999a) for a detailed appraisal of that paper).

Williams promised to publish more in a "peer reviewed journal" by end of 1998, but three years later, no such publication has appeared.

The online paper, co-authored by Brian Starkey is now known as Williams and Starkey version 1.

Williams was evidently unaware that the code of the ICZN does not allow for the "devaliding" of scientific names quite so easily and that once a name has been assigned to a valid taxa it is effectively there for ever, provided that the taxa has not already been named.

Notwithstanding this, Williams then posted on numerous internet lists the same hair-brained idea, for the following two years.

His erstwhile friend and colleague Wolfgang Wüster got onto the same caper and posted the same sort of rubbish as far and wide as he could.

Between them, they sent thousands of e-mails and posts and using text-book bullying tactics they aggressively attacked all those who defended either my taxonomy or good name.

Wüster's posts were even more curious as up until at least early 2001, he had (by his later admission) never seen a *Pailsus* and yet had been prostituting himself as a taxonomic genius with regards to these animals and spamming everyone accordingly.

By way of example, as late as July 2001, you could find Wüster's comments denying the existence of *Pailsus* on the internet at Peter Uetz's site at:

[http://zeta.embl-heidelberg.de:8000/srs5bin/cgi-bin/wgetz?-e+\[REPTILIA-species:'Pailsus_SP_pailsei'\]](http://zeta.embl-heidelberg.de:8000/srs5bin/cgi-bin/wgetz?-e+[REPTILIA-species:'Pailsus_SP_pailsei'])

It is quoted below:

"Species:

Pailsus pailsei

Synonyms:

Pailsus pailsei HOSER 1998

Subspecies:

Family:

Serpentes: Elapidae snakes

Distribution:

Australia (N-Queensland)

Comment:

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

Now reading this first in 1999 and again in 2001, you'd be led to believe that Wüster in fact had expertise on these snakes and so was somehow qualified to voice an expert opinion.

But in early 2001 after the Williams/Wüster fraud (see below) became all too obvious, he attempted to distance himself from David Williams when he told the australianherps@egroups.com list:

" I am not personally familiar with these animals (New Guinea *Pailsus*), and I have quite openly stated this myself".

Now this comment is even more curious as Wüster has since written in *Litteratura Serpentina*, that I'd had rushed to print with his *Pailsus* descriptions.

Wasn't he being hypocritical here by himself rushing to print?

As recently as 23 January 2001, (after the publication of both *Pailsus* descriptions) David Williams posted on Kingsnake.com the following condescending comment to Scott Eipper:

"I hate to burst your bubble son, but there is absolutely no conclusive evidence whatsoever to 'prove' that these snakes (*Pailsus*) are anything other than local variants of *Pseudechis australis*."

That the Williams and Wüster lines about *Pailsus pailsei* and *Pailsus rossignollii* being nothing more than unusual *Cannia australis* was having "success" and/or gained some "legitimacy" was easily demonstrated by the currency their arguments gained among ill-informed herpetologists and novices.

Typical of this outcome was seen on two websites managed by Patrick Hughett, (Hughett 2001a, 2001b) both of which in due course made it clear that he had read the Hoser papers, and then that Wüster, Williams and Mark O'Shea all regarded *Pailsus rossignollii* as being merely "*Pseudechis australis*".

A series of 27 posts by Hughett on Kingsnake.com (Hughett 2001c) , interspersed by a series of misinformation posts by Williams and Wüster in June and July 2001 also put a date on the creation of the website and showed that the Williams/Wüster claims about *Pailsus rossignollii* being nothing more than "*Pseudechis australis*" actually continued well beyond the time that the Wüster et. al piece had been submitted for publication in the journal *Litteratura Serpentina*, and even after it had been published.

The reversal

Now the problem with Williams and Wüster wasn't the fact that they were arguing tripe. I could handle that. And quite frankly, it'd be hard to believe that someone in the snake game as long as David Williams could honestly believe that *Pailsus pailsei* was nothing more than an underfed *Cannia australis*.

The problem was that their position was about as fixed as Melbourne's weather.

In other words it liked to "chop and change".

Why?

That's the herp politics at work again.

Williams and Wüster never had a genuine interest in the snakes, their taxonomy and the like. The sole aim of their exercise was to attack myself.

Now anyone can see from the dates above that well after BOTH *Pailsus* descriptions were published by me, both Williams and Wüster were still maintaining the lie that they were the same thing as *Cannia australis*.

Now bearing in mind neither men had seen *Pailsus*, they were brave to be venturing such a strong and "expert" opinion, but in this democratic world, I was effectively unable to stop them.

Now no one can "force" anyone else to use a given scientific name and I for one am not going to attempt to make anyone do so. But the question always begs, "Why not use a valid name for a previously unnamed taxa?"

And perhaps here it is also worth mentioning that as far back as 1999, I realised that arguing with the likes of Williams and Wüster was a waste of time and so for the main part, I had taken leave from online forums and not responded to the tirade of attacks from the two men.

However, here in Victoria, where most prominent hobbyists have been to Roy Pails' residence and actually seen *Pailsus* in the flesh, nobody disputed the fact that they were different from *Cannia australis*, and so it was from this state (Victoria) that the strongest defence of my species description came from.

And it also appears that many individuals who defended my good name against the attacks by Williams and Wüster did so, not necessarily out of faith in my own work or friendship me or Roy Pails - the man who initially drew the attention of myself and others to this "new" species, but rather because they had their own axes to grind against Williams over failed snake-trading deals and the like.

The matters are unrelated to me, and a summary of some of these nefarious and failed deals, criminal activity and the like by Williams and associates was posted by Eipper on Kingsnake.com in three separate posts in July 2001 (Eipper 2001a-c).

The details are far too extensive to post or even summarize here in this already lengthy article.

Notwithstanding all the above, by mid 1999 the Williams/Wüster claims

against the validity of *Pailsus* (at the species level at least) looked more and more dodgy as more and more people actually looked into the two men's pseudoscience.

This became even more so after the publications of Hoser (1999a, 1999b), Sutherland (1999) and the emergence of the Wells and Wellington description of "*Cannia weigeli*", all of which tended to indelibly confirm the original *Pailsus* description as describing a hitherto unrecognized taxa.

Following publication of the *Pailsus rossignolii* description in late 2000, a number of correspondents from outside Australia weighed into the debate and also took on Williams and Wüster and their dodgy claims that *Pailsus* was nothing more than an underfered *Cannia australis*.

It was thus by this stage obvious that the Williams/Wüster claims lacked merit and their arguments accordingly went down the drain.

It was then that the pair then engaged in what was perhaps their biggest stunt yet.

Both started to claim that I'd somehow stolen their information and "naming rights" for the species *Pailsus rossignolii*.

The claim was curious based on the fact that they'd been denying it was different for the previous two years.

The pseudoscience as practiced by the pair then broadened into a more comprehensive attack on my credibility.

It ran (and still runs) essentially along the following lines,

A/ When the pair think Hoser has properly named a valid taxa, to falsely accuse Hoser of stealing naming rights or the science from someone else, and

B/ When they disagree with Hoser's taxonomy or nomenclature, to thereby ridicule and/or rubbish Hoser's "science" or methods, preferably by simultaneously attacking Hoser's credibility, and

C/ To improperly create as much "confusion" as possible (in violation of the ICZN's code), thereby improperly discouraging others from using "Hoser" names, even when they are clearly the correct names.

D/ To use bully-boy tactics against any person who uses or publishes Hoser assigned names, especially if they do so on the internet, which is where the pair are most adept at "spamming" and "flaming" their adversaries.

In other words I'm to be damned no matter what I do.

Why?

You see the pair have formed the view that use of "Hoser" names gives Hoser "credibility" and this must be stopped at all costs.

In a way it's all a bit strange. You see Roy Pails was also adversely named in the *Smuggled* books over an illegal deal involving Scrub Pythons.

To his credit he got over it and continued with his life.

Williams (whose crimes and misdemeanours are of a far greater magnitude) still has a bee in his bonnet over being adversely mentioned in the *Smuggled* books and still seeks vengeance at every opportunity.

The big Scientific Fraud by Williams that was supported by Wüster

The fraud involving Williams and Wüster was first identified by Geelong-based herpetologist and VAAH founder, Neil Davie, who posted the details on Kingsnake.com.

Fortunately this was one of the few occasions that David Williams was not aggressively posting messages from his home computer and a sizeable number of herpetologists archived the relevant documents before Williams could wipe them from the internet, a few hours later.

Other herpetologists, including Richard Wells also agree that what occurred was a serious case of scientific fraud.

Here's what happened!

For the benefit of readers, the original version of the online paper by Williams and Starkey, published in late 1998 is now generally known as "Version 1".

It was posted at: <http://www.uq.edu.au/~ddbfr/index.html>

The same view as published in that paper was widely endorsed by Wüster and O'Shea who both claimed to be working with Williams.

(In fact as recently as 30 June 2001 at: 17:09:41.; O'Shea is on the record in a post on Kingsnake.com as claiming *Pailsus rossignolii* "are still classified as "King Browns"), see any of Hughett (2001a-c)).

Now bearing in mind that this Williams and Starkey online paper and similar comments had been posted far and wide by these people, there was absolutely no secret in the "fact" that these people thought that *Pailsus* was nothing more than an underfered and undersized "*Pseudechis australis*".

Now on that basis one would have to be a psychic to realise that this was a "front" so that the same group could go ahead and prepare a description of a New Guinea *Pailsus* without Hoser's or anyone else's knowledge, further bearing in mind that it was common knowledge that I was looking at New Guinea *Pailsus*.

The scientific fraud really started in early 2001 when Williams and Wüster got onto the internet and started to claim that I had somehow stolen their naming rights to this species (*P. rossignolii*).

This followed by Williams reposting his paper on the internet at another site, namely:

<http://www.Kingsnake.com/toxinology/snakes/taxonomy.html>

However this time there were a few noticeable alterations and hence this

paper has since become known as "Version 2".

One alteration was the address for Brian Starkey. That part was benign.

The date at the bottom had also been removed.

That act on it's own, one could suppose was also benign.

But what wasn't quite so benign was a citation tacked into the "paper" and cited in full at the end.

It read as follows:

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

The key element here was to add credibility to the new assertion Hoser had deliberately sought to steal naming rights to the species.

However this new addition didn't quite push Williams, Wüster, et. al. over the line.

You see the ICZN's code of ethics (which Wüster et. al. are also falsely claiming I'd violated) gives a 12 month limitation on the so-called "hands off" period.

This is so as to stop people "claiming" species and then effectively monopolizing all work on them for years on end while they think about publishing a description.

Even with this fraudulent addition, Williams, Wüster, et. al. failed to place me outside of the ICZN's code of ethics.

Once Williams' attention was drawn to this shortcoming in his "paper" out came what's now become known as "Version 3".

This appeared on Kingsnake.com following requests by various correspondents and was posted the very next day by Williams at: <http://www.Kingsnake.com/forum/venom/messages/31762.html>

This one had an added in text citation 'O'Shea et. al. (1999)', thus effectively placing me inside the 12 months "hands off" period.

The problem this time was that Williams forgot to do the same to the citation at the end of the paper, which still had the 1998 date.

Williams in his haste had also neglected to remove all the earlier versions of his paper that he'd smattered all over the internet on different servers (including the two url's named above).

And as noted by other correspondents, that had been to take advantage of the benefits of so-called "search engines" to ensure maximum exposure for his "paper".

When Davie posted details of the frauds and the various websites through the various herpetological list servers, people everywhere downloaded their own copies in anticipation of Williams wiping them from the world wide web from the very servers as named above..

The wiping of the fraudulently altered papers was done by Williams (as anticipated) a few hours later.

As all this happened, my phone was suddenly running hot with the likes of John Fowler and Tim Mensforth in South Australia, both telling me of the fraudulently altered papers that they had by now archived onto their own computer hard drives before Williams realized the mistake he'd made and wiped them from the internet servers.

But the horse had bolted so to speak. You see shortly thereafter, the three versions of the perennially morphing paper were then re-posted on Kingsnake.com by Pete Brammell and yes, once again were archived all over the place.

Williams got onto Kingsnake to have them wiped again, but it was all too late for him in terms of the fraud being exposed.

Williams had been using the internet as a weapon to peddle his lies and then he had fraudulently enmeshed himself in his own lies using this very same technology - technology that was now coming back to haunt him!

And furthermore, Wüster came in and actively supported the Williams fraud with a whole raft of lame excuses.

In a post to Kingsnake.com dated February 03, 2001 at 04:19:28: Wüster stated that the scientific fraud perpetrated by Williams had occurred but then went on to say that it wasn't "relevant" to their arguments.

He then went on to threaten to sue anyone who dared repost the earlier versions of the fraudulently altered paper claiming "copyright" over the material.

But Wüster was right about one very important thing.

It was himself and David Williams that had the copyright on committing scientific fraud!

And yes, in spite of repeated requests from numerous other concerned correspondents, Wüster refused to divorce himself from the Williams/Starkey fraud.

Now in fairness to Mr. Starkey, David Williams' business partner in "Black Knight Reptiles", who incidentally and recently pled guilty to illegally smuggling a Black-headed Python (*Aspidites melanocephalus*) through the post and was fined by a magistrate's court, I have no evidence to show that he was a part of the Williams fraud, even though his name appeared as the junior author to the paper in it's ever morphing versions.

And while I have differences of opinions on some matters with Richard Wells (the man who co-described "*Cannia weigeli*"), we are at one in being of the view that the alteration of the Williams/Starkey paper is one of the most blatant cases of scientific fraud ever perpetrated (Wells pers. comm. dated

4 Feb 2001).

The Williams fraud coupled with the attacks against me got even worse when it became clear that Williams was posting on the various internet forums under a series of different names. The IP address (as found by viewing each e-mail's "document source") gave Williams and his antics away. It was during this period, Wüster even let it be known in another post that he hadn't even seen any New Guinea *Pailsus* and knew nothing of them. Although more recently, in July 2001 on Kingsnake dot com venomous forum, he's been again claiming expertise on these snakes.

Like I said - how can you argue with a pair whose alleged views change more often than Melbourne's weather?

But the salient fact was that by mid 2001, the totally false assertion that I was somehow poaching a species name from Williams, Wüster and others was well and truly buried.

Most of the relevant posts, including the three versions of the Williams/Starkey "paper" can be found on one my websites.

Namely:

<http://www.lexicon.net/adder/SlandI.htm>,

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

and mirrors.

The Wüster and Williams' lie exposed for once and for all!

And perhaps we should actually enlighten people as to what Wüster had to say after the whole Williams/Wüster fraud was exposed via the Kingsnake.com forums.

It was Neil Davie of Geelong (whom both Wüster and David Williams falsely claimed was me posting under a bogus name) who finally got Wüster to retract the lie that I'd somehow stolen "naming rights" to *Pailsus rossignollii*.

Wüster's post on January 28, 2001 at 03:59:11: read thus:

"Hi Neil,

Thought you'd gone?

: Did Hoser really steal naming rights for *Pailsus rossignollii* from

: Williams as Williams has recently claimed?

No, and Williams did not claim so..."

Oops!

David Williams had earlier claimed I'd stolen his research.

But that in itself would have been some mean feat.

You see I sit here based in Melbourne (Australia), while Williams, somewhat itinerant, tends to hover somewhere in the general vicinity of Cairns, Queensland, a distance of over 3000 kms.

Now bearing in mind he's been at loggerheads with me and that I've never seen anything written by him of herpetological note, save for a couple of minor (and in this instance irrelevant) papers, I'd have no idea as to how I was supposed to have got into his filing system and stolen key data on anything!

Further lies and misinformation by Wüster and Williams

In his piece of fiction published in *Litteratura Serpentina*, Wüster accused me of publishing my *Pailsus* and other descriptions in non-peer-reviewed publications so as to avoid scrutiny of his taxonomy.

The argument is a furphy because, put simply, if my taxonomy is wrong, it simply won't be used - period!

And that is the case wherever the paper is published - peer reviewed or otherwise.

And by way of example, that's why the name *Varanus teriae* Sprackland 1991, which was published in a peer reviewed journal, is not being used anymore.

(Refer to ICZN case 3043 (Sprackland et. al. 1997) and later relevant comments and findings as published in the *Bulletin of Zoological Nomenclature* for further details).

And the above is in no way an attack on Sprackland as a herpetologist.

It's not terribly rare in the overall scheme of things for a taxonomist to describe a species and inadvertently overlook an earlier description of the same species as occurred in the above case. And while it's unfortunate for the later person, it is by no means a hanging offence.

But more importantly, the above comments by Wüster are also a case of serious double standards by Wüster himself.

Wüster's own piece was deliberately sent to *Litteratura Serpentina* a journal that is NOT peer reviewed, where he was able to exercise significant pull with the editor and avoid any external scrutiny of his own litany of lies.

In light of the events following the publication of Wüster's piece in *Litteratura Serpentina* it is also clear that Wüster is keen to forcibly and unethically censor me from having any proper right of reply, and/or even the liberty of correcting some very obvious lies.

Furthermore none of the thousands of posts by himself or his erstwhile colleague David John Williams attacking me has ever been submitted for peer review.

Why?

They'd probably be rejected by any half decent editor and both of them know this.

Again it seems that the politics and the fraud have usurped the science in terms of David Williams, Wolfgang Wüster and *Pailsus*.

Cited at the rear of this paper are just a few dozen of the pairs posts that evaded any form of peer or other review prior to publication.

Space reasons prevent me from listing them all.

(But if the editor makes *Crocodilian* up to 1,000 pages, I'd give it a go!).

Oh and yes, three years later we still wait for the peer reviewed paper by Wüster's buddy, Williams as promised online by Williams (he said then it was to be published by end 1988!).

Now if all this sounds bad, it actually gets worse!

In 2001 and at the same time, the two men were both arguing opposite alleged "facts" on different internet forums at the very same time!

The facts simply didn't seem to matter - just so long as they could twist them to attack myself.

What were these opposite facts they were arguing at the same time?

You guessed it - one argument remained that *Pailsus* were nothing more than underfed King Brown Snakes, the other argument being that I had somehow stolen naming rights to "their" new species.

In fact, even now in August 2001, you can go check this one out.

Go to one of the major internet search engines like Yahoo dot com or "Google".

Type in the name "Pailsus" and see what you get.

Check out all the Wüster and Williams postings and you'll soon get to see just a sample of these contradictory posts as posted by them.

Sinister motives fabricated by Wüster

Wüster wrote that I'd had requested the editor of *Litteratura Serpentina* (Gijs van Aken) speed up publication of the *Pailsus rossignollii* description. Wüster then went on to imply some kind of sinister motive.

He knew he was lying.

But the full story can be confirmed by van Aken's own e-mails.

Another editor (Marcel van der Voort) received the paper in early 2000 and said that it would be published in the August 2000 issue. This was promised!

Typically I receive the journal here in Australia in the first week of the month after publication date and so by week two of September I put a notification of the description on the smuggled.com website as I have done for dozens of other publications.

After that, I received the journal and the description wasn't in it.

I was also advised by a mutual friend of myself and Brian Starkey that David Williams had seen the notification and intended "publishing" his own "description" of the species and then back-dating it to claim priority.

Based on Williams' past form, I accepted that the story was credible and then contacted the other editor of *Litteratura Serpentina* (van Aken) and asked what was going on with the paper in terms of publication.

I was told that the paper was not due to be published for a while. Van Aken was then told of the concerns re Williams and asked that the paper be published as soon as possible.

Aken contacted the other editor, confirmed the original undertaking to publish in August and that it had been inadvertently broken and then published the paper in the December issue.

It's understood that the Williams plot failed after he was advised that "publication" on the internet does not constitute a valid description as per the ICZN's code and because of his inability to find or lodge a "type" specimen in a Museum.

That also put paid to what appeared to be yet another lie by Williams; namely that he had access to loads of *Pailsus* specimens.

Ditto for the rest!

Wüster also ran his internet campaign of misinformation in relation to the other Hoser descriptions, such as for the *Acanthophis* descriptions, by posting via a number of Uetz's internet sites (including: [http://srs.embl-heidelberg.de:8000/srs5bin/cgi-bin/wgetz?-e+\[REPTILIA-Species:'Acanthophis_SP_crotalusei'\]](http://srs.embl-heidelberg.de:8000/srs5bin/cgi-bin/wgetz?-e+[REPTILIA-Species:'Acanthophis_SP_crotalusei']), [http://srs.embl-heidelberg.de:8000/srs5bin/cgi-bin/wgetz?-e+\[REPTILIA-Species:'Acanthophis_SP_barnetti'\]](http://srs.embl-heidelberg.de:8000/srs5bin/cgi-bin/wgetz?-e+[REPTILIA-Species:'Acanthophis_SP_barnetti'])), etc, that the species named by Hoser in Hoser (1998a) were not valid.

By early 2001, and after a number of the names had come into general usage, in particular *Acanthophis wellsei* Hoser 1998, Wüster also reversed his tune and falsely alleged that Hoser had somehow deliberately "stolen" naming rights from elsewhere.

Now I want to make it entirely clear that it is always a possibility that myself and/or anyone else may inadvertently "jump the gun" on someone else by naming one or more species that another person was also intending to describe.

This almost happened in the case of three pythons from Halmahera, Ambon and the Tanimbar group that I'd named (refer to Hoser (2000b)), and then withdrew from publication at the last minute after I became aware that David Barker, et. al. were also intending to assign names to the taxa (as stated in Hoser (2000b)).

That I was able to withdraw the three names assigned by himself was a fortuitous event and had there not been a "chance encounter" between himself and David Barker at a herpetological conference in Sydney in October 1999, three species of snakes now widely known as *Australiasis clastolepis*, *Australiasis nauta* and *Australiasis tracysae* by Harvey et. al (Barker being a co-author) would probably have been instead known as *Australiasis valentici*, *Australiasis haydnmophiei* and *Australiasis greeri*.

(Now for those who may question what I think of Rob Valentici's skills as a herpetologist, the fact is, that on the whole, I must think quite a bit of him to

have wanted to name a species after him!)

And yes, the history of herpetology is littered with cases of one herpetologist inadvertently or even intentionally naming taxa that another person had been in the process of formally describing.

However based on the proven conduct by Wüster, Williams, et. al. such a claim by these men as recently asserted in *Litteratura Serpentina* in June 2001 in relation to my *Pailsus* descriptions clearly cannot be credible.

And based on the misinformation put out by these men, the idea that I'd somehow jumped the gun on any research project by themselves, either deliberately or even unintentionally is a pure fiction.

By way of example, besides the long-awaited and unfulfilled promise by Williams made on 1 November 1998 (Williams 1998), that he'd publish a piece repudiating *Pailsus pailsei* as a species in a "peer reviewed" journal by year's end; nearly a year after publication of the *Pailsus rossignolii* description in December 2000, Williams et. al. have failed to produce a shred of evidence to suggest that they were working on either this or any other similar species and that's in spite of repeated requests by numerous independent herpetologists for the men to do so.

It seems even more odd, bearing in mind that the pair have rushed to print literally thousands of times to peddle their false assertions about me and the *Pailsus* species described ("against" and later "for" their existence as valid taxa, and yet later again "against"), but they have continually refused to produce any data or evidence to suggest that they were working on any relevant taxa.

Noting that the said species had been already been formally described and the ICZN's rule of priority, there would have been nothing gained by the pair to with-hold any data or evidence that they had, and so by this late stage, one could only conclude that the pair lacked such data and had been lying about this since mid January 2001.

The January 2001 date is when they reversed their assertions about *Pailsus pailsei* and *Pailsus rossignolii* not being valid species.

By virtue of the time now effluxed (well over six months), it could be fair to assume that should any relevant data be published at some far-flung date by Wüster, Williams or immediate associates, then it had been gathered well after publication of both *Pailsus* descriptions and purely as a result of my own initial bringing these hitherto undescribed taxa to the attention of science.

This is particularly so, noting that more recently in June and July of this year (2001), and after grudgingly conceding that *Pailsus* are in fact different from *Cannia australis*, Wüster and Williams have been actively touting for data and material in relation to *Pailsus* spp. via Kingsnake.com, private e-mails and elsewhere (refer to Huggett, 2001c, Williams 2001k and Wüster 2001s, 2001t, 2001y)

Yet More of Wüster's lies, misinformation and inconsistent statements

Wüster et. al. claimed in *Litteratura Serpentina* that I had a "deep-seated antagonistic feelings towards the scientific establishment, perhaps as part of the general anti-institutional attitudes Hoser displays in his writings".

It seems hard to reconcile this assertion with the fact that in every taxonomic paper I had to rely on the assistance of the "scientific establishment" and has acknowledged them in every case.

Or perhaps using Wüster's own warped sense of reality, anyone who dares voice a different view to anyone else's views could be described as "anti" and if the person they disagree with are with an institution they could then be labelled "anti-institutional".

On that basis we could include almost every herpetologist in Australia.

To back his assertion that I'm "anti-institution" he cited a paper published in *Crocodilian*, namely (Hoser 2000c).

But seriously, if Wüster wanted to claim that my paper, "What's in a species name" (Hoser 2000c) is an attack on the staff at the Australian Museum (as he asserted), then perhaps he should think again.

For those who haven't read the article or got a copy of that issue of *Crocodilian* handy, it's main thrust is complaining about a new practice at the Australian Museum whereby some curators are selling "naming rights" on species at \$5,000 a pop with the money to be put towards further research.

Now I didn't complain about the Museum's intentions or ethics, and made that much perfectly clear. But rather that if the system became widespread and entrenched it could lead to a whole host of unforeseen problems and abuses as listed in the article.

And guess what?

Privately at least the herpetology curators at that very same museum, namely Sadlier and Greer said they agreed with me!

Based on Wüster et. al's perverse logic you could argue that these two are "anti" themselves!

Wüster's widening the attack on Hoser

In *Litteratura Serpentina*, Wüster et. al. made numerous false and defamatory allegations in relation to my other taxonomic papers, including Hoser (1998a, 1998b, 1999a, 2000a, 2000b).

For them it seems, nothing is sacred.

Most of the claims were ridiculous in the extreme.

And yes they ran along the lines already outlined above.

Wüster made the bogus claim that I had an uncritical acceptance of Wells and Wellington taxonomy, including in Hoser (2000c): the python taxonomy paper.

The statement is a barefaced lie.

By way of example, in Hoser (2000c) I made it point blank clear that he didn't recognise *Aspidites collaris* as cited by Wells and Wellington.

And to the pair's credit, we simply have agreed to disagree.

And this author has previously published other obvious differences of opinion in terms of the potential status of the names "*Cannia weigeli*" and "*Acanthophis armstrongi*" (see Hoser (1998a) for the latter and Hoser (2001) re the former).

And again it appears that Wells and Wellington are sufficiently mature enough to accept that taxonomists may agree to disagree or have reasoned scientific discussion rather than engage in a major process of vilification.

Or based on Wüster et. al's perverse logic maybe I'm "anti" Wells and Wellington as well?

Or based on their various conflicting internet posts, I should be damned for using Wells and Wellington names and then I should also be damned for not using them!

Why haven't Wüster et. al also condemned the likes of Greer, Cogger, Shea and others who have used some but not other Wells and Wellington names?

Like I said before, a large part of this Williams and Wüster et. al story is not about science, but rather lies, politics and outright fraud, sometimes thinly masked as science.

Wüster also alleged that my python descriptions published in late 2000 described what he called "non-taxa" as part of his bid to undermine usage of the names assigned.

The claim seems odd based on:

- 1/ Wüster's own lack of experience with the said taxa and
- 2/ The fact that these hitherto unnamed taxa had been already recognized by numerous herpetologists including Hal Cogger who is regarded as being a fence-sitter and conservative when it comes to taxonomic and nomenclatural matters.

By way of example, the South-west Woma was recognized as distinct as far back as 1983 by Cogger et. al. who singled it out for conservation measures in their action plan.

This author's naming of this form as *Aspidites ramsayi panoptes* merely formalized the process.

That's the harsh reality.

Wüster then published a comparison between my own python descriptions and the new species diagnoses with those of Harvey et. al., claiming that because mine had a lower word count (per taxa described), they were somehow inferior.

But in rebuttal of Wüster's flawed argument, and by way of example, Laurie Smith's "diagnosis" for *Morelia carinata* (which he called *Python carinatus*) has not been called into question even though it consisted just one short sentence.

If Wüster et. al. sought to claim brevity as a criticism of my species descriptions, then there are numerous well-known names in herpetology who stand out in front of me!

The public claims by Wüster in *Litteratura Serpentina* seem even more odd, bearing in mind that I have received (by forwarding from others) a number of private e-mails from Wüster acknowledging that in every scrutinized case, the Hoser descriptions fit within the rules of the ICZN and describe valid taxa.

Ethics and the ICZN code

It has been seriously violated by Wüster et. al.

The code states:

"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."

Based on the innumerable posts on the internet and other materials peddled by Williams and Wüster et. al., the code has been seriously breached in a manner perhaps never seen before.

One of many examples was Wüster's post on Kingsnake.com at: January 23, 2001 at 15:25:33.

Or try the posts by Williams on Kingsnake.com at: January 23, 2001 at 07:09:37, January 24, 2001 at 02:53:06 reposted widely by Williams, or Wed, 24 Jan 2001 20:03:19 +1000 on his own ozherps@egroups.com list server which perhaps represents one of the greatest violations of the ICZN's code of ethics ever to appear in print.

The latter of these commenced thus:

"Y'know on second thoughts this list is all too quiet, so I think I'll just comment on a couple of things in Raymond's post..."

before he got into yet another of his baseless character assassinations of myself.

When the likes of Neil Davie and others came to my defence, Williams true to his form cut off their access to the lists.

But is hasn't just been myself who has been the victim of the lies and deception as carried out by Wüster and Williams. Hiding behind the relative anonymity of their personal computers, Williams and Wüster have driven countless keen herpetologists away from the hobby, and/or at least away from the various discussion forums on the internet.

Persons lied about and vilified by the pair have included such prominent and competent herpetologists as Neil Davie, Scott Eipper, Benjamin Dowse, Tim Mensforth, Roly Burrell, John Fowler, Mick Pugh and others.

All this has been against the rules of the ICZN and ordinary ethics in general, but it seems that the only part of the ICZN code Williams and Wüster have been concerned with adhering to, is the recommendation for "wide dissemination"!

The Loch Ness Monster

I've been to Scotland and even to Loch Ness.

And put simply I regard the Loch Ness Monster as a hoax.

However, maybe David Williams does believe in it.

After Wüster's post in January 2001 stating that Hoser hadn't stolen naming rights to *Pailsus rossignolii*, Williams put out a series of messages stating that he was working on another species of "*Pseudechis*" from New Guinea.

Now if I was genuinely into stealing naming rights on species and/or Williams actually thought I was, the Williams post was probably quite stupid.

But the reality is that Williams is probably just lying again.

You see detailed inquiries by myself, including in Papua New Guinea, had already failed to reveal any "new" "*Pseudechis*". A point already noted in my paper (Hoser 2000a).

And/or based on the fraudulently altered Williams and Starkey (1998/9/01) versions 2 and 3, they are now outside the ICZN's one year hands off period anyway!

In other words this so-called new species allegedly being described is perhaps just a variant of the Loch Ness Monster!

Put it this way, if you hold your breath waiting for this alleged Williams description to appear in print - you will probably suffocate!

The end game for Wüster and Williams

As already mentioned, the real issue here is quite simple. Wüster and Williams have commenced a long-term campaign to bully and bludgeon people not to use "Hoser names" for validly named taxa, for fear that their "enemy" may gain some added and perceived credibility.

They have lost the three-year battle over *Pailsus*, which at the species level at least are now generally recognised as distinct from *Cannia australis* - and that's in spite of the best efforts by the pair to stop this from happening.

So instead they will peddle their new lie that I've somehow stolen their naming rights.

The same lie has now been peddled by the pair in relation to *Acanthophis wellsei* Hoser 1998, since Aplin, Maryan and others have also accepted the name as valid (also see Aplin and Donnellan (1999), Maryan (2001)).

And yes, expect to see more of the same for any other taxa I describe and formally name that over time become generally known under those names.

And for the record, if either of the pair expect their lies and misinformation to somehow dissuade me from naming new taxa when appropriate, they should have a serious rethink.

And on a related matter, the pair should realise that no matter how often they repeat a lie, it will always be just that ... a lie.

Another lie I fought for quite a few years was that "there is no corruption in the Victoria Police."

After my books *Victoria Police Corruption 1* and *2*, were published in 1999, the chief commissioner (repeatedly adversely named in the books) unexpectedly quit two years ahead of the end of his contract and several hundred other police also retired before their time.

And yes, the *Victoria Police Corruption* books showed that things were so bad in this state that the government finally ditched the old lie and got a new commissioner from New South Wales.

And everybody knows how clean the police were in that state!

An electronic posting

Perhaps encapsulating the seriousness of the fraudulent activities by Williams and Wüster, in particular the fraudulent alteration of Williams and Starkey (1999) versions 1, 2 and 3 was an electronic message from Jim Paull in Gippsland, Victoria posted on the Kingsnake.com academic forum.

The post came well after the pair had been exposed by Neil Davie, Pete Brammell and others. The following message is reproduced below in full so as to avoid charges here of misquoting:

'<http://www.Kingsnake.com/forum/acad/messages/274.html>

Papers sought online

[Follow Ups] [Post Followup] [The Academic Forum] [FAQ]

Posted by Jim Paull on February 01, 2001 at 21:24:36:

I agree with Pete Brammell and the others in relation to Hoser and his posting of his papers online.

A group of us here in Gippsland, Vic. were after copies of Williams and Starkey papers Marks 1, 2, and 3, preferably online, as well as the various posts to the Kingsnake forums over the last week or two.

We found them an instructive study of how a respected scientist such as Hoser could be attacked and smeared by use of various quotes, misquotes and in the case of the 3 papers, plain fabrications, by his adversary/s.

The reason we found the matter of interest was because it was a rare and very public and open example of how sometimes so-called scientists can stoop as low as anyone else to foreword their own agendas, that sometimes are very unscientific.

(particularly when the protagonists are still alive - as we usually only hear about these things, long after their deaths)

Could someone here at Kingsnake, Hoser, or someone else who may have archived the various posts, including those no longer on the Venomous forum at Kingsnake put them all on a website somewhere permanently for us to refer to in future?

A look into the future of what Wüster claims are "non-taxa"

The pattern of behaviour by Wüster is now well established.

Based on precedent it can be said that any descriptions by me will usually and in the first instance be condemned and described by Wüster as describing "non-taxa", which is a term Wüster seems to like to use.

Ditto for Williams.

As and when the names move into general usage, and in spite of the best efforts by the pair to stop this happening, we can expect further false and baseless claims by the pair to the effect that I have either stolen naming rights and/or research from elsewhere ... probably themselves.

In terms of what I can do to stop this - the answer is probably very little.

I have little if any control over the internet and the ability of the pair to "spam" messages to thousands of recipients.

However, I do issue a serious caution to editors of printed journals and other similar publications against publishing the various diatribes of lies, half-truths and misinformation as written by Wüster and Williams as happened in the case of the piece seen in *Litteratura Serpentina* (Wüster et. al. 2001).

In that example, the editor of the journal failed to make even so much as a cursory check of the allegations, further noting that such a check would have readily revealed Wüster et. al.'s statements as grossly inaccurate.

As a final irony, it's worth noting that had either of Wüster or Williams diverted just a fraction of their immense time and effort in their campaign against me to looking at and describing some of the many presently undescribed reptile taxa in the Austro-papuan region, they could have easily done competent investigations and descriptions of several, perhaps including some of the presently undescribed "high-profile" species.

This includes such taxa as varanids and skinks known to inhabit the region to Australia's immediate north.

If the pair had taken this path instead, they could perhaps have effectively pre-empted yet more baseless claims by themselves against me and/or anyone else they have taken a mainiac dislike to for allegedly "stealing" naming rights to newly described taxa.

Conclusion

And so there you have it.

What started off as an exciting journey into the discovery of a new species of snake from north-west Queensland soon became a wider journey of discovery of similar species and subspecies from across Australia's north and even into Irian Jaya.

However all this was eventually overtaken by the pseudoscience, politics and outright frauds as perpetrated by David Williams and Wolfgang Wüster.

And that's the *Pailsus* story so far ... a tale of herpetology, science, politics, more politics and scientific fraud.

End note

Unconnected to herpetology, I was invited to give a keynote address to a corruption conference on Saturday 24 March 2001 at the northern NSW town of Inverell. On the Friday preceding this conference, I phoned the reptile curator at the Queensland Museum, Jeanette Covacevich and asked for access to the museum's archives of "*Cannia australis*" specimens.

For her, the call was effectively out of the blue.

She asked when? And I replied "next Monday would be nice".

Upon arrival at the Museum, Covacevich, then nursing a broken wrist and fellow curator Patrick Couper asked "How long do you expect to be here?", the reply being "As long as it takes".

A one-day stay translated to be three.

I hadn't spoken to either for the best part of a decade, but that made no difference, the welcome mat was always there.

Now this wasn't just a one off situation. While visiting the museum, the same welcome mat was laid open for HSQL member Will McGrath and others.

We weren't so much as colleagues, but almost like family (and that's in spite of a number of disagreements over matters of taxonomy, wildlife laws and the like).

(No I couldn't get them to re-label their Water Pythons "*fuscus*" instead of "*mackloti*" - I was told "We stick to Kluge!").

The following week it was a similar story at the Australian Museum, where Ross Sadler and Allen Greer were only too glad to offer any assistance's they could.

Being an ex-Sydney-sider, I've seen more of these men over the years, but again that probably didn't matter.

You see the curators at the Australian Museum have always bent over backwards to help other researchers - and that ranges from the high-profile academic in an institution to the ten-year old boy who has just got his first pet Bluetongue (*Tiliqua*).

You see we were all herpetologists/people with an interest in reptiles and we all sought to work together as best we could.

Curators in most other Australian (and for that matter overseas) state Museums have a similar track record to those in NSW and Queensland.

The following week, my delapidated old house was the "Melbourne Hilton" for some Sydney-based reptile enthusiasts that I had never met, who'd merely invited themselves into the house on the basis of their common interest in reptiles.

In the week after, a keeper in Geelong gave me unfettered access to his collection to allow me to inspect and photograph an aberrant *Cannia australis*, and several pythons in his collection.

For the record, that snake, initially identified "as possibly a 'Pailsus' from Hayes Creek in the NT", was identified by me as a standard (but immature) *Cannia australis*, based on it's having 53 subcaudals, all single, excl. no.s 47, 52 and 53, 187 ventrals (approximately) and a horseshoe shaped rostral typical of *Cannia australis*.

And yes, since the original publication of the *Pailsus* descriptions, I've been inundated with information from people seeking to further the common goal of herpetology, all of whom never sought anything in return.

Going back in time, I recall how a couple of decades back I went out and caught some Centralian Bluetongues (*Tiliqua multifasciata*) and gave them to an unkempt, acne ridden youth for free. The youth who at the time had no experience in the reptiles, merely said he wanted a few as pets and to study them. He was then an unknown face in the crowd, attending a few meetings of the Australian Herpetological Society (AHS) in Sydney.

These days that formerly unkempt, acne ridden youth is nowadays better known as Dr. Glenn Shea.

Who knows? Maybe if he never got a start in herpetology when he did, he may have become a lawyer instead?

Thankfully most of the time in herpetology the spirit of cooperation overrides any artificial barriers erected by "spoilers" like Williams and Wüster that separate "amateur", "professional" or whatever other pigeonhole is erected.

This is just as well, as there are far too few of us working on far too many different reptiles.

And yes, in spite of constant debate on a whole host of matters, (which in itself must involve a level of dialogue, dispute and civilized argument, particularly as available information changes), most herpetologists can conduct themselves with decency, decorum and in an overriding spirit of cooperation.

It is my contention that the practice of lies, frauds, deception and vilification as practiced by the likes of Wüster and Williams, particularly through their excessive spamming of internet "lists" such as "Kingsnake.com" and "australianherps" are not welcome elements in Australian herpetology, and the quicker that either these two men and/or their perverse and warped attitudes are banished from herpetology, the better.

Acknowledgements

Numerous herpetologists have assisted me in relation to the *Pailsus/Cannia* project and related matters over the period 1998-2001. Many are named at the end of the earlier *Pailsus* papers cited below.

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Refer to Hoser (2001), or the website:

<http://www.smuggled.com/boycan1.htm>

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Notes:

1/ Relevant papers by Hoser can also be downloaded from the internet at: <http://www.smuggled.com/pap1.htm>

2/ Colour photos of the type specimens for the three subspecies of *Cannia australis* described by Hoser (2001) as well as the first ever published photos of *Pailsus weigeli* can be found at: <http://www.smuggled.com/boycan1.htm> or relevant links.

3/ Relevant references cited above in relation to Williams and Wüster and their campaign against this author and the lies peddled, including those that were directly referred to in the paper above have been cut back to the barest minimum and include only those directly referred to - not others that said much the same thing.

4/ All posts cited above (and others) were archived by this author in full for citation and reference purposes and for the purpose of being to indelibly identify the lies and inconsistent statements by Wüster and Williams. However over time, the number of posts was far too great for even this author to be able to save all of them.

5/ The original "paper" now known as "Williams and Starkey version 1", was actually posted by David Williams on the internet in mid November 1998, but has erroneously been cited by this author in this paper and Hoser (1999a) and others as Williams and Starkey 1999 (which in turn is the original date of download from the internet by this author).

The two fraudulently altered versions first appeared on 29 and 30 January 2001 (which are the dates Williams first physically posted them).

This paper since placed online at:

<http://www.smuggled.com/cancro1.htm>