

COMMENTS ON COMMENTS ON THE GENUS *PAILSUS* HOSER 1998

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The following comments are in response to an undated note published on an internet website (<http://www.uq.edu.au/~ddbfrj/index.html>) by David J. Williams and Brian A. Starkey (listed here as Starkey and Williams 1999) as well as a similar posting by Williams on a 'Venom and Toxin Discussion Group' on Bryan Fry's website dated 1 November 1998 and following comments by Williams dated 6 March 1999 (two postings) and postings by Bryan Fry on the same site dated 20 January 1999, 5 March 1999, 6 March 1999, and 7 March 1999.

Although the ICZN rules do not recognize material on the internet as published (unless also published in hard copy and appropriately distributed), I will for the purpose of this note treat it as having been published. Refer to Williams (1998a, 1998b, 1999a and 1999b) and Williams and Starkey (1999) as well as Fry (1999a, 1999b, 1999c, and 1999d). This material was located by this author following advice from Scott Eipper and addresses my recent description of *Pailsus pailsei* sp. nov. (see Hoser 1998a).

Williams and Starkey, are both well-known and highly regarded herpetologists. In their comment they agree that most herpetologists with experience of the genus *Pseudechis* don't accept it as it stands in its 'current phylogenetic arrangements'. While not stating specifically what that is, I can only presume it is that as outlined in recent publications by Cogger (e.g. Cogger (1992)).

Williams and Starkey do not propose any new arrangement or offer any concrete reason for maintaining the status quo, although they end up maintaining the arrangement of Cogger (1992).

I believe it is inevitable that an arrangement along the lines of that proposed by Wells and Wellington (1983, 1985) and others as cited in Hoser (1998a) will be adopted by the general herpetological community when they eventually get over the stigma of using names proposed by the 'amateurs' Wells and Wellington. From my own perspective I have no brief as to what names are used, so long as they are 'correct' in terms of ICZN rules and those of accepted taxonomy. I also have no concern as to whether the authors of those names are 'amateurs' or 'professionals' as I find use of the two terms artificial and unnecessarily divisive for all concerned.

Williams and Starkey stated 'HOSER relies almost solely on entirely single subcaudal scale arrangement in *Pailsus* to distinguish it from *Pseudechis*.' They then stated 'An essential error in HOSER (1998) is the author's failure to test the morphological characters for creation of *Pailsus* against all of the known species of the donor genus, *Pseudechis*.' They then cite

individual characteristics of *Pailsus pailsei* shared with given members of the genus *Pseudechis* as described by Cogger (1992) as a basis for rejecting it being placed in a separate genus.

Williams and Starkey then go on to state that they are of the view that the *Pailsus pailsei* as described by Hoser represents a form of *Pseudechis australis*.

All the above arguments and the final conclusion by Williams and Starkey are defective. Furthermore had Williams and Starkey properly read the original paper by myself they would realize why all the above arguments by themselves are defective.

Of necessity I will repeat some of the original data as produced in Hoser (1998a). *Pailsus pailsei* is distinguished from all known *Pseudechis* by a suite of characteristics unique to it. No other species shares these. The single subcaudal arrangement is used to distinguish *Pailsus pailsei* from the species it is likely to be confused with where it is known to occur. That is the Mount Isa area. The species it is likely to be confused with are *Pseudechis australis* and snakes of the genus *Pseudonaja*. All are readily separated from *Pailsus* on this basis (subcaudals).

It is routine in a taxonomic paper to identify one or two simple characteristics to enable differentiation of similar species by lay people and experts alike when in the field. The use of the subcaudal arrangement to key out the similar looking but different species where they are known to occur sympatrically is optimal and sensible. It takes into consideration and overrides similarities between the various species that may arise through factors like age, sex, state of health, ground colour and so on, all of which are variable and on their own, non-diagnostic characters. Not only has this been overlooked by Williams and Starkey, but in a separate letter not cited here, Brian Bush of Western Australia similarly seems to have overlooked my reason for treating subcaudals in *pailsei* as diagnostic within the Mount Isa area. No author has produced any evidence to the contrary.

Also by way of example, scalation similarities between *Pailsus* and *Pseudechis guttatus* or *Pseudechis colletti* (as cited by Williams and Starkey) are given little if any weight as it is impossible to confuse *Pailsus* with either species.

What generic arrangements are ultimately proposed by Williams and Starkey or used by them is not of major concern to myself as the genus level is an artificial category which cannot be 'proven' in the same sense as a species can. Hence I expect the widespread adoption of the name *Pailsus* gen. nov. to take some time, particularly noting the stigma some may have for

using a 'Hoser' name.

A species can be crudely 'proven' in as much as when different animals mate and reproduce in the wild they are regarded as one and when they do not, they are split into more than one. By way of example, Brown Tree Snakes (*Boiga irregularis*) and Green Tree Snakes (*Dendrelaphis punctulatus*) are sympatric and do not interbreed and so are different species. Red Sydney Death Adders and Grey Sydney Death Adders do interbreed and are therefore a single species, namely *Acanthophis antarcticus*.

In the case of *pailsei*, all evidence available strongly indicates that there is NO interbreeding with *australis*. Thus the point cannot be escaped that at this stage, they must be regarded as separate, albeit similar looking, species. There is proof of sympatry, but none of interbreeding. Furthermore the stark physical and scalation differences between *pailsei* and *australis* in the Mount Isa area, coupled with the lack of known intermediates is further compelling evidence that both are separate species. This point has apparently been overlooked by at least one of the authors (Williams) who in a post dated March 6, 1999 wrongly implies that I have classified all Mount Isa *Pseudechis australis* as *Pailsus pailsei*. In my original paper, I specifically refer to 'normal' and sympatric *Pseudechis australis* in the Mount Isa region.

Williams and Starkey have not, to this author's knowledge, inspected either of the snakes on which the description was based. However I concede that the substantial detail presented in the original description readily allows them to form opinions based on the data presented, including the high quality photos of the type and live specimens.

The comments by Williams and Starkey about the small size and gracile build being a reflection of sexual dimorphism or parasite load are not credible. The original paper specifically noted that adult female *australis* from the same area were of substantially larger size and more solid build, as is typical for the species throughout its range, thereby refuting the sexual dimorphism argument. To make my point abundantly clear, *australis* (of either sex) from the same area attain roughly double the length (av. about 2 m vs av. about 1 m) and several times the weight of *pailsei*, regardless of sex. A photo of a *P. australis* from Winton in Queensland in Hoser (1989) also indicates obvious differences between the two taxa.

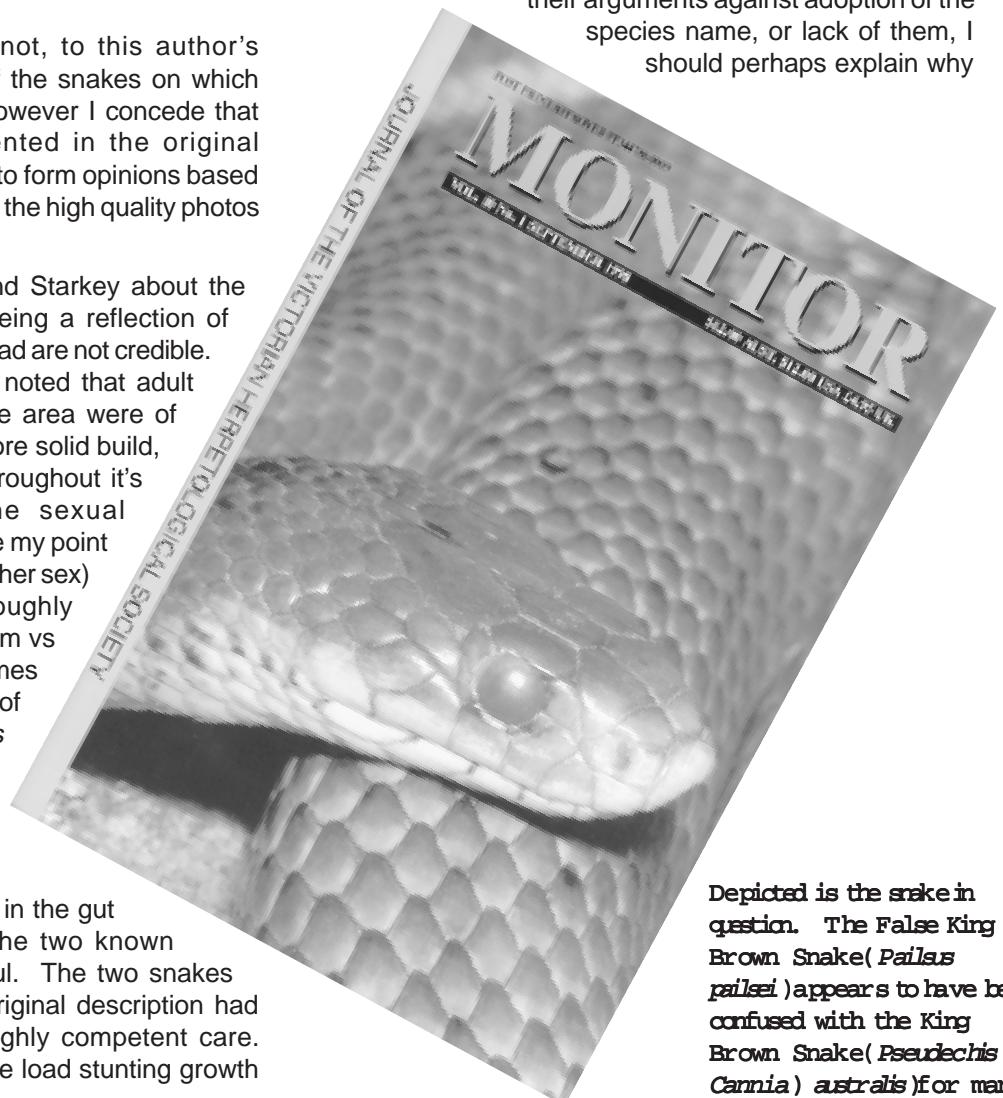
As for an alleged parasite load in the gut causing the gracile build in the two known *pailsei*; well, that is just fanciful. The two snakes that formed the basis of the original description had been long term captives in highly competent care. There is no evidence of parasite load stunting growth

or otherwise affecting morphology. Also a good look at the photos of the live specimen of *pailsei* in Hoser (1998a) will show a different head morphology to *australis* from the same parts of Queensland, or for that matter anywhere else they occur. Again refer to Hoser (1989) for illustrations of *P. australis*. And yes, again this confirms that *pailsei* is a separate species.

Some of the other reasons given for rejection of *Pailsus pailsei* as a valid species seem to defy logic. They refer to previous authors who have looked at *P. australis* but not identified any *pailsei* (or something similar) in their samples. Besides the fact that there may have been none of the latter species in their samples (highly likely), the assertion is basically meaningless. By way of example, Glenn Storr of the WA Museum in the early 1980's looked at a number *Acanthophis wellsei* in his sample of *A. pyrrhus* and called them the same species, namely *A. pyrrhus*. Nearly 20 years later, I named these different snakes as a new species (*A. wellsei*), which incidentally is also agreed as valid by Storr's own successor at the WA museum, Ken Aplin.

Williams and Starkey quoted an e-mail (Hoser 1998b) from myself stating that I was effectively disinterested in the species as I had now done the description.

Although the quote had nothing to do with their arguments against adoption of the species name, or lack of them, I should perhaps explain why



Depicted is the snake in question. The False King Brown Snake (*Pailsus pailsei*) appears to have been confused with the King Brown Snake (*Pseudechis (= Cannia) australis*) for many years

I made the statement. The legislative and financial hurdles to myself legally obtaining live specimens for further research are relatively great and current commitments elsewhere prevent me from undertaking such matters. By way of example my wife has a child due in May 1999 and I am in the process of publishing two major police corruption books due out at about the same time. However noting the important need for further research on the species, I have already written to the Queensland NPWS (Hoser 1998c) asking them to look favourably on any and all applications from people interested in procuring specimens of *pailsei* for any worthwhile purpose. The letter was sent within a month of general publication of the original description and for the department's benefit included a media release and the original description.

Notwithstanding my comments above, I must say that I agree with an earlier comment by Williams in an e-mail to me dated 26 October 1998, (Williams 1998) where he said 'Let me say that the assessment of the "Pseudechis" group is long overdue, and the more work tendered the better. I could not agree more with your remarks re government wildlife authorities.'

Finally, while I note that the comments by Williams and Starkey in their online paper about *pailsei* lacked merit when scrutinized, I am pleased they chose to publish them, as it has given me an opportunity to reaffirm the reasons why *pailsei* is separate from *australis*, as well as again show why the two have been confused for so many years and may again be confused in future. I also have little doubt that the views expressed by the two authors may also be shared by a small number of others with little or no proper knowledge of *pailsei* and/or people who failed to properly read the original description. It is for this reason I have chosen to comment on their note, to address the issues raised, even though it is likely it's readership to date has been small.

My major problem with their publication (if that's the right way to put it) is that they chose to rush into print without properly appraising the original paper they were attempting to review. All questions raised by Williams and Starkey had in fact been addressed in the original paper; a fact reflected in the relative length of the description.

As an ending comment, my own acquaintance with the species *pailsei* was only shortly before the description was published. I cannot claim to have discovered it in the strictest sense. A number of other herpetologists were aware of it and it was they who brought it to my attention. I had never seen or heard of the species prior to 1998. However in terms of it's differences to *P. australis*, I must say that close inspection of the *pailsei* revealed a number of stark contrasts, not all of which were spelt out in my original paper, but most of which can be seen from close inspection of the photos published with the paper or inspection of the specimens themselves. In short, they cannot be missed.

The above view is shared by most herpetologists I have

spoken to with familiarity with the new species and also includes Sutherland (1999).

Unless and until there is firm evidence of interbreeding between the two taxa where they occur, that is confirmed by captive studies, it would be patently reckless to attempt to regard *pailsei* and *australis* as one species.

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