

HERPETOLOGY IN AUSTRALIA – SOME COMMENTS

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PREAMBLE

Much, but not all of what follows is a response to the article published in this journal by Ken Aplin from the WA Museum (pp. 104-109). That article includes comments and criticism about a paper I published in this journal describing new species and subspecies of Death Adders (Genus: *Acanthophis*) and use of previously proposed names (Hoser, 1998) as well as passing comments on an earlier paper by John Cann (Cann, 1997). In this article I offer some explanations and answers to Aplin's criticisms where I deem necessary, and also offer my views on some wider issues raised. Having said this, I suggest those who wish to read more of this article read Aplin's comments first, in order to appraise themselves of the issues raised, then the published response by Wells and Wellington so that readers can see where our opinions converge and where they differ.

WELLS AND WELLINGTON - THEIR VIEWS

Much of Aplin's piece is critical of earlier publications by Richard Wells and C. Ross Wellington (Wells and Wellington, 1984, 1985a, 1985b) and in particular their *Acanthophis* descriptions. In October 1998, with Aplin's permission, both were sent by myself, a copy of Aplin's comment inviting them to submit their views on the matter. Their response has been printed in this magazine. I agree with some, but not all of the views expressed by those two authors, noting that some matters addressed by the pair are outside my area of knowledge.

VALID TAXONOMIC NAMES

There is little if any disagreement between myself and Aplin as to what constitutes an available scientific name. This is covered by the 'Rules' as published by the ICZN. Aplin's summary will be useful to many readers of this magazine who may have been unfamiliar with how such names were derived. The only readily identifiable point of disagreement between the two of us is with the use of the Wells and Wellington names *Acanthophis armstrongi*, *A. lancasteri* and *A. schistos*. Aplin (along with many other Australian herpetologists) says they are not valid on the basis that the original authors failed to provide adequate diagnostic information in their original descriptions. Wells and Wellington and others claim that they adequately crossed this 'hurdle' in their descriptions, citing similar precedents to back their case.

As already seen, there is strong division among Australian herpetologists over the validity of these and other Wells and Wellington names. I have no particular brief one way or the other, or strong opinions as to the validity of the Wells and Wellington names. With myself

relegating their name *armstrongi* to subspecies level (a level often ignored by taxonomists), this left just *schistos* and *lancasteri* in contention. If I used them I was damned. If I didn't use them I was similarly damned.

Following publication of my paper the rate of approval of my use of the Wells and Wellington names ran at about 30%, those against at 70%. However that does not itself mean I have necessarily got things wrong. I merely cite that (current) statistic as it may interest readers following any ongoing debate.

Most agreed that the said taxa (all from Western Australia) should carry names other than *antarcticus* and *praelongus*, at least at the subspecies level.

What all this does mean is that whichever way I went I would be criticized over the matter. My main concern was to have the taxa named. What names they carried; Wells and Wellington ones or others is of no major concern to me.

If in the worst case scenario, the Wells and Wellington names *schistos*, *lancasteri* and *armstrongi* turn out to be nomen nudem, my 'crime' in my previous paper was merely the use of invalid names. This is not a hanging offence. Such 'misuse' of names has been done countless times in the past by most, if not all herpetologists, including Hal Cogger, Rick Shine and others (including myself) when writing taxonomic and non-taxonomic papers. Having said that, because of the fact that in my paper I accurately identified where the names I used came from, there can be no dispute or confusion as to which animals I was talking about, which ultimately is of greatest importance.

My own instinct suggests that the simplest way to resolve the dispute as to the validity of the names *armstrongi*, *lancasteri* and *schistos* is to petition the ICZN for a ruling. This is a fairly simple process and as I've already said, I have no concern which way the pendulum swings. The ICZN regularly rules on similar disputes and I believe that this is the best way to resolve future potential confusion for the genus *Acanthophis*. By way of example, I note that Wells and Wellington do not accept Aplin's assertions that their three proposed names are not valid, however an ICZN ruling is in theory 'binding' on all taxonomists, enabling the deadlock to be broken.

My use of the Wells and Wellington names appears to be Aplin's major criticism of my paper and an issue I would like to see ruled on by the ICZN for the benefit of others in future. It could also be useful in circumventing the 'confusion' feared by some.

INADEQUATE TAXONOMY

Certainly some of the Wells and Wellington descriptions appear to fit that category (see references). The description (for want of a better word) of *Acanthophis schistos* is in my view manifestly inadequate and I, like Aplin agree that the authors failed to provide enough accurate diagnostic information. An inference that acceptance of such a name would only result from the work of others (later) rather than the original describers (in this case Wells and Wellington) is agreed by me. As already stated, whether or not Wells and Wellington provided enough information in that description to satisfy ICZN rules is the key point of dispute, and one I would like to see adjudicated.

The example by Aplin of the Wells and Wellington work on the gecko genus *Phyllodactylus* and his assertion that it too consists of inadequate taxonomy is something I cannot comment on. I have no knowledge of the details of the subject.

THE HOSER ACANTHOPHIS DESCRIPTIONS

Alleged flaws or shortcomings in my descriptions as pointed out by Aplin are not in dispute. He is correct in noting that I did not inspect all the holdings in all Australian museums and that I did not consult with him prior to publication. He is also correct in noting that I did no genetic testing of specimens. He is also correct that I did not personally inspect the holotype of *A. wellsei*.

Having noted these points, I should also make it clear that all these facts were made clear in my original paper. It would have been remiss of me not to do so. Notwithstanding this, the fact remains that none of the above is essential for the publication of valid species descriptions. For example the overwhelming majority of species known today were described without any DNA testing. Furthermore many, including others in the genus *Acanthophis* were described on the basis of single specimens. By and large those descriptions are accepted as valid. A reprint of the original description of *Acanthophis laevis* is reproduced in this journal.

As with all scientific studies, there are advantages to be gained by looking at large numbers of specimens. Having said that, for the purposes of species descriptions, (including the genus *Acanthophis*) there comes a point where it becomes unnecessary to look at further specimens from a given area in order to establish to what species a given group of snakes belong. Noting the number of specimens examined from the areas relevant to my paper, it is clear that I more than adequately inspected specimens (numbers) before going to print.

In terms of whether my descriptions were adequate or inadequate, such is obviously a matter of opinion. Aplin obviously thinks the latter and while I disagree, I still respect his opinion. Perhaps the best way to judge the adequacy of my *Acanthophis* descriptions is to line them up against the published 'Rules', by the ICZN and/or compare them with other species descriptions that have been published.

The 'Rules' specifies what needs to be done to describe a given species. All these are covered correctly in my descriptions and as Aplin notes, the names I have assigned are valid (on the basis that species do in fact exist). I have set out features which differentiate these snakes from one another and the only significant argument from this point on will ultimately be if the differences (and any others) are sufficient to warrant them being labelled species in their own right. As with all who describe new taxa, it is not me who ultimately decides their validity, but rather my peers. It may also take many years for a general consensus to form.

Aplin appears to assert my descriptions are inadequate on the basis of a general lack of information provided about each new form. This view is rejected by myself. A simple comparison between my descriptions and those of others who have described Australian reptiles will show that I have provided more detailed information than most descriptions by other workers. This includes well-known names who described a large number of taxa over many years.

If any of my descriptions ultimately fail, then that will also place me in the same league as other great taxonomists (including the late Glen Storr of the WA Museum) who have described species, only to have such descriptions rejected by peers. There is no disrespect meant against Storr in the preceding statement.

As Aplin also infers, the ultimate fate of the names I have proposed may depend more on how people choose to define species and subspecies rather than on the finer detail of my taxonomic work. This fact is also noted in the original paper.

JUDGING THE TAXONOMIC WORK

Aplin's assertion that in some cases a taxonomist's work should be judged on their personal credibility is rejected by myself and I am sure most other scientists.

Taxonomic work should **only** be judged on its merits; **not** on who wrote it. By way of example, Glen Storr had a reputation second to none. In spite of this, his taxonomic work on *Varanus gouldii* (which he erroneously redescribed as *Varanus panoptes*) was shown to be flawed and was therefore ultimately rejected by the herpetological community. Aplin made note of poor quality work being counter-productive, which is something I agree with. He then inferred that it was a problem restricted to so-called amateurs. The Storr *panoptes* description is just one example of similar poor quality work (or errors) being done by a so-called professional which also caused substantial damage and confusion for some years after. In this case the damage spilled out of the arena of taxonomists and into the general herpetological community, where as recently as 1998, over-zealous wildlife officials were seizing Gould's Monitors (*Varanus gouldii*) from people on the (erroneous) basis that they were unscheduled species, to wit *Varanus panoptes*!

One of the principal rules of science is that any experiment or research finding published by a given

author can be duplicated by anyone else should they choose. If not, then the original findings are rejected. This applies to physics, chemistry and zoology. The ultimate success or failure of my descriptions will depend on whether others are able to 'corroborate' my results. Thus I must agree with Aplin when he says 'if Hoser's *Acanthophis* names ever do come into general scientific usage, it will not be on account of his 1998 publication, but through efforts of subsequent revisers who examine this problem'. Ditto for most, if not all other scientific descriptions ever published.

Thus I must agree with Aplin's other comment 'should future studies demonstrate that one or more of Hoser's additional species or subspecies warrant recognition, then more of his species-level names may well come into general scientific use'. I would be a fool to assume that all my proposed names would be accepted by the scientific community without question. In fact I'd be disappointed in their methodology if that actually happened.

American molecular biologist Gunter S. Stent summed up the way science operates when in the journal *Frankfurter Allgemeine Zeitung* he said 'Almost every new discovery in science is rejected at first before it is generally accepted – often only decades later'. He further said,

'The "good scientist" is considered to be an unprejudiced man with an open mind who is ready to accept any new idea that is supported by the facts. As the history of science shows, scientists do not always act in accordance with this popular view.'

While talking taxonomy, I do not profess to be an expert taxonomist. However I must say that within the general realms of herpetology, taxonomy is not (necessarily) one of the more complex or involved parts. Often the differentiation of one species from another is something any novice or even disinterested party can do. Likewise, the formal process of describing a species as per ICZN rules is very simple and can be done by anyone with even a high school education (assuming they have a valid (undescribed?) species in the first instance).

CONSULTATIONS

Describing *Acanthophis* was always going to be a can of worms in that almost every herpetologist and so-called herpetologist in Australia seems to be an expert or self-appointed expert on these snakes. They are a high-profile group. Although I consulted widely (and over a 20 year period) before publishing the descriptions, the fact remained that I would always be criticized by those I did not consult. In hindsight, I agree that advantages would have been gained by consulting Aplin, Donnellan, Smith and others (not consulted) prior to publication of the paper, particularly noting that those just named were themselves preparing a paper on these same snakes.

Having said that, I cannot say the same for many others I failed to consult, who have since asserted a God-given right to have viewed the paper prior to publication.

Furthermore, one shouldn't lose sight of what I actually published. I published scientific descriptions of new species. I didn't publish detailed ecological studies, a paper on captive breeding and so on. If I had consulted all those who felt they should have been, it is likely I'd still be talking to people in the year 2,500 without having gone to print.

OTHER CRITICISMS

Ken Aplin wasn't the only person to have expressed concern over my published descriptions. And as already inferred, I believe most of his comments were valid, even if I didn't agree with all his views. He played the ball, not the man and I respect him for this. Having said that, there were also sustained criticisms from other quarters (no others sought to publish material in *Monitor* and hence none appears here).

Most of this 'other' criticism was beneath contempt. For example a NSW wildlife official said 'we'll outlaw the keeping of anything Hoser describes ... then no one will adopt usage of his names'. The same official also said 'We set the rules and we'll never use Hoser's names'.

Coincidentally, Professor Wolfgang Bohme and T. Ziegler from Zoologisches Forschungsinstitut und Museum Alexander Koenig, Bonn, Germany, said in the *Bulletin of Zoological Nomenclature*, 'We think that conservation and legislative authorities are users rather than creators of taxonomic progress and possible nomenclatural consequences involved. They should therefore rely upon scientific reasoning and not vice versa.' (Bohme and Ziegler, 1998).

Much of the criticism came from the group I would call 'the usual suspects'. These included the bent officials, crooks and allies detailed in my books *Smuggled* and *Smuggled-2*. Being a corruption whistleblower has never made me popular amongst the corrupt, and these same people never cease to miss an opportunity to stick knives in my back. For the record, every defamation action against me relating to all my corruption books has come out in my favour, with several courts finding that everything written in the books (and my internet websites) to be 100% true and correct. Of course 'the usual suspects' play down these embarrassing facts.

Another criticism (also mentioned briefly by Aplin) was the fact that I was editor of *Monitor* at the time the descriptions were published. In this case, that's a red-herring. The paper was originally going to be published elsewhere and I published it in *Monitor* at the last moment after I became editor. The two reasons for the decision were (a) to alleviate what was a relative shortage of contributions and (b) because I thought as editor I could do more justice to the descriptions, particularly in relation to the illustrations and captions. Journal and magazine editors worldwide regularly solicit my contributions (as mirrored by what I've published to date) and I have little doubt that I will probably publish further taxonomic papers in some of them at later dates.

FORCED SUPPRESSION

In terms of the Wells and Wellington papers, it is my view that supposedly professional herpetologists did themselves a disservice in the way they sought to suppress (effectively ban) the papers. The original submission to the ICZN hid behind anonymity, which displayed a lack of accountability by the authors. Most of those opposed to the Wells and Wellington works sought to raise red-herrings rather than address the fundamental issues within the papers. I have little doubt that these unprofessional attacks played a part in the ICZN's eventual refusal to suppress the publications (see ICZN 1991 and refs. therein).

During the highly charged debate seven NSW biologists, C. J. Birrel, L. Dodds, P. Evans, E.J. Nield, R. Peters, D. Sell and D. Shannon cynically asked 'Will the proponents of suppression become the recognized authorities when they immediately redescribe Wells and Wellington's work the moment suppression has occurred?' (*Bull. ZN* 45(2) June 1988:149-50).

Correspondents P. Bouchet, R. Bour, A. Dubois, D. Goujet, J. P. Hugot, J. Pierre and S. Tiller from the Museum National D' Histoire Naturelle in Paris, France said that they were 'outraged' at the attitude displayed by the Australian herpetological establishment and their allies, which they said 'is best compared with the Stalinist falsification of history'. Going on to say that 'such statements demonstrate that the 'anti-Wells and Wellington' group of persons will not be satisfied with a rejection by the commission of their works: their names should also disappear from the history of herpetology...' (See *Bull ZN* 47(2) June 1990:139-40).

Noting the above comments, there is no doubt that 'the usual suspects' are particularly perturbed that I have done something as permanent as formally name new species, meaning that the names (if accepted) must be quoted all over the place. Forcible ignoring and suppression have always been the order of the day for corrupt officials and their allies and my formally naming of species makes it so much harder for them.

(Perhaps I should say that the unjustified and sustained attacks I have received from my adversaries over my describing new species has only served to encourage me to do more of the same in future!)

PEER REVIEWED? CENSORSHIP? OR SIMPLY MISREPRESENTATION?

The peer review process of reviewing papers sent to journals is great in theory. We are told that the process prevents major errors or 'rubbish' being published, thereby enhancing the quality of the 'peer reviewed' publication. I like to think that this is true in most cases. Although *Monitor* is not 'peer reviewed' and 'censorship' is kept to a minimum, the quality of *Monitor* remains high. Furthermore, as always, it is the author who carries the can if he gets things wrong, thus making a major disincentive to potential authors to make major errors or print tripe.

Within Australia, the 'peer review' process has

developed a particularly bad reputation. Authors in control of such publications have at times hidden behind this process to print papers and articles that constitute unmitigated crap and would under any process with integrity, not ever see the light of day. In fact some of the muck seen in some Australian peer reviewed publications wouldn't even be printed in *Monitor* because it is so obviously and manifestly untrue.

Without giving a tedious list of examples I shall merely make mention of two. A Dr. Shelley Burgin published a so-called review of my book *Smuggled* in a supposedly peer reviewed publication called *Herpetology in Australia*. What she said was obviously false and defamatory and was known to be by the editor of the publication, NPWS/NSW employee Dan Lunney. In a preface to the publication, much mileage was made out of the fact that everything in the publication was of (allegedly) high quality due to the so-called peer-review process.

Following a threat by myself to sue Lunney and Burgin for their published lies, a correction/retraction was printed in another of Lunney's publications, *Australian Zoologist*. Notably both had been employed as so-called scientists by NPWS/NSW, the same body that had been proven corrupt in several courts of law (refer to *Smuggled-2* (Hoser, 1996) and *Smuggled* (2nd Edition) (Hoser, 1997) for details).

Gerry Swan did a similar act, by publishing a false and defamatory review in his own journal *Herpetofauna*, which is also supposedly peer reviewed. He too was forced to print a retraction of his lies that he had deliberately and recklessly published. He probably published his piece of rubbish to get brownie points with the despots in NPWS/NSW that he appeared to have been trying to appease. (In Swan's case, his indiscretion was all the more remiss due to the high quality of much of what usually appears in the journal).

Both so-called reviews were totally counterproductive to herpetology, conservation and so on. I have little doubt that some of the criticisms against myself over my *Acanthophis* descriptions stem from corrupt officials and their allies who seek to vilify me at every opportunity. There are other examples of similar fraudulent actions done by so-called scientists documented in *Smuggled-2* (the accounts of which were verified as accurate in the failed defamation cases against me).

Perhaps it is also worthwhile noting that the eventual implementation of laws that allowed private (=amateur?) keepers to hold reptiles in New South Wales came as a direct result of the publication of *Smuggled* and *Smuggled-2* (and later failed defamation cases against them). Herpetologists should recall those who sought to slander and suppress those publications (and retain the corrupt status quo) and those who fought for truth and conservation.

What I find particularly galling is how many of those who fought tooth and nail to retain the corrupt status quo in NSW for over 20 years are now falsely claiming

credit for its change now that they appear to have lost the battle. This is after a licensing system was introduced, with senior NPWS/NSW bureaucrats and their allies kicking and screaming all the way.

MORE EDITORIAL CENSORSHIP

While talking about so-called professionals and editorial censorship as mentioned by Wells and Wellington in their piece, it is hard not to mention the so-called professionals at the ICZN and their own so-called journal *Bulletin of Zoological Nomenclature (BZN)*. Editors Phil Tubbs and Anthea Gentry have recently conducted some overt censorship and editorial manipulation of articles and comments in order to suit what appear to be their own pre-determined ends.

I refer of course to that never-ending attempt by very unprofessional professionals to violate all their own published rules and suppress *Varanus gouldii* and *Varanus flavirufus* as we know them in favour of the invalid name *Varanus panoptes*.

In the March 1999 issue of *BZN*, they published a piece by myself which had little in common with the piece they had themselves solicited from me on October 22 1998, (received by them the following day).

Following the publication of the improperly printed 'Hoser comment', Gentry then published her own comments that was not only factually incorrect, but also contained a number of potentially defamatory statements against myself.

Gentry falsely accused me of making factual errors which had in fact been made by Robert Sprackland, one of those leading the charge to resurrect *panoptes* (and that other lizard he also happened to name after his wife - *Varanus teriae*, the junior synonym of *V. keithornei*).

Gentry's comments were all clearly designed to support the factually untenable push to resurrect the name *panoptes*.

Perhaps that process could best be described as corrupt. Thankfully, I know of no similar example of such behaviour by any so-called amateurs.

However by reference to the way the ICZN seem to run things, I find it hard to reconcile any attack on so-called 'amateur taxonomy' when some (but not all) so-called professionals are anything but professional.

It's a bit like pot, kettle and black.

'AMATEUR' AND 'PROFESSIONAL'

The differentiation between 'amateur' and 'professional' herpetologist is largely artificial and from a practical point of view the erection of barriers between the two is a major hindrance to progress.

Within Australia such a barrier has been erected by some state wildlife authorities, including in Aplin's state of Western Australia to suppress the former, which has greatly hamstrung research and conservation of herpetofauna.

As Aplin himself notes, there are not enough

'professionals' in Australia to handle the sheer amount of work that needs to be done, making input from 'amateurs' desirable.

I should point out that here that I am not anti-institutional per se, however it is essential for me to point out faults (on any and all sides) in order to try and remedy things in the future. In terms of the museum community (of which Aplin is part) I only have praise in terms of my own dealings with them. Staff from most Australian state museums gratefully assisted me in the production of my book *Endangered Animals of Australia* including those acknowledged in the book. I have also called on assistance from various staff members at these institutions for over 20 years and can't recall anything but positive experiences. They have also given me unfettered access to their collections within their extremely tight time constraints, which is something I am always grateful of.

The only issue I have with the Museum community as a whole is the relative lack of support they have given to private (=amateur?) herpetologists (including those people who effectively do nothing more than keep reptiles as pets) in terms of their campaigns to have workable collecting and keeping laws applied to themselves by State Governments, as well as the associated punitive law enforcement activities against private keepers. This repressive regime has by and large left museums and their staff untouched. I believe that museum curators who are potentially a powerful lobby group in favour of the private herpetologists, have done little more than give lip service and token support to alter what is in most states an unworkable status quo. This unworkable status quo means that much meaningful research done by so-called amateurs is often illegal.

By way of example, we here in the eastern states are constantly being asked to support a long-running campaign by WASAH (Western Australia) to allow private herps the right to hold reptiles in captivity. I have little doubt that a well-orchestrated media campaign by Australia's museum heads on behalf of the private keepers in states where they are currently punitively restricted would change the status quo, almost overnight.

As I write this, CALM (WA) officials have just raided the houses of three private keepers, seized their snakes and put out the usual media releases falsely claiming the keepers to be international crime bosses trafficking in snakes. Why haven't the 'professionals' in that State, including Aplin, used their positions of power to stop this sort of lunacy from going on?

In that matter four people now face court, including one charged with the lunatic offence of feeding two 'feed skinks' (*Cryptoblepharus* sp.) to a snake. A similar case was raised in my book *Smuggled* and should have been further highlighted to prevent a re-run of such wasteful prosecution.

Also recall the recent (1998) case where West Australian 'amateurs' were charged and found guilty

after studying Green Pythons in Queensland. The money spent on the legal battles would have been better spent on the snakes themselves.

In terms of State wildlife authorities, I am not 'anti' them either. I work with many on a regular basis and without problem. Most of the corruption information I receive about these departments comes from staff within the departments who share similar conservation and honesty ethics as myself. It is hard to be 'anti' those who work with and assist you. As for the 'amateurs', there are plenty in this group who also get things wrong and I'd be a fool to pretend that they are all saints...(even I make mistakes sometimes!).

A RECENT CASE

Even more disturbing was the case in Queensland, where internationally recognised Aviculturist and Herpetologist Bob Buckley was charged by Queensland officials over his breeding of Green Pythons. Instead of getting support from the supposedly 'professional' community, this 'amateur' had some of them joining the corrupt officials who sought to destroy the country's most successful Green Python breeder.

One of those on the Qld NPWS witness list was an SA Museum curator. One would have hoped the curator would have given evidence for the keeper rather than against him. It is hard to think of any single act that could have so effectively driven a wedge between the so-called professionals and many of the 'amateurs'. Fortunately Buckley beat the charge and cleared his name (for the time being anyway), although there seems to have been a lack of remorse by those on the side against him. Is it rude for me to ask why hadn't the South Australian curator instead used his position to stop the matter going to court in the first place?

MORE IN COMMON THAN APART

Notwithstanding the comments of Ken Aplin and myself as written here, I am sure that the two of us (and hopefully most others in the herpetological community) have far more in common than that which we disagree with. Disagreements, including those broached in this publication are not in themselves unhealthy, especially if there can be reasoned discussion (argument?) over the matters at issue. When this occurs, most of the differences can be resolved and for those that can't, we can usually agree to disagree without the world coming to an end.

It is in my view also preferable for differences in opinion to be expressed openly in a forum such as this journal (even if this may appear unsavoury) rather than behind closed doors where differences in opinion may otherwise fester and become unnecessary conflicts that may go on indefinitely.

ALL IS NOT WELL IN AUSTRALIA

Finally Ken Aplin's assertion that my comments 'can only be read by the international community as a signal that all is still not well in Australian herpetology' are true and correct. I note by way of example that at the present time (early 1999) people in the USA (e.g. Casey Lazik and Frank Retes) and Germany (e.g. Holzel

Jurgen) can and do legally breed Ant-hill Pythons (*Anteresia perthensis*). However in Aplin's native state of Western Australia (which is where the snakes come from) such activity is outlawed.

Even more notable is that at the time I write this piece (early 1999), three well known 'amateurs' in Western Australia, including the internationally recognised Brad Maryan (coincidentally now working at the WA Museum with Ken Aplin), have just had their homes raided by heavily armed wildlife officers and had dozens of snakes taken from them.

I personally find it regrettable that the professionals have not tried harder in the past to prevent the constant recurrence of such scenes over the last 20 years and believe that if they had done so, the current legislative/enforcement nightmare may have since been lifted.

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- RAYMOND HOSER kept and bred Death Adders (genus *Acanthophis*) for several years. He has studied the snakes for over two decades.