TAXONOMIC CONTRIBUTIONS IN THE 'AMATEUR' LITERATURE:

COMMENTS ON RECENT DESCRIPTIONS OF NEW GENERA AND SPECIES BY RAYMOND HOSER

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INTRODUCTION

Reptiles and amphibians hold considerable fascination for a growing number of people. In recent years, this has led to an almost exponential increase in the number of reptile aficionados, spanning the entire spectrum from children keeping a couple of corn snakes as pets to persons professionally employed by research institutions to carry out research work on reptiles. In between are found a wide variety of individuals who do not make their living studying reptiles, but invest an often considerable amount of energy and resources into the study or husbandry and reproduction of their reptiles. It is clear that the majority of reptile enthusiasts fall into this latter group, rather than among the institutional professionals. While institutional and non-institutional herpetologists are sometimes artificially segregated into 'amateurs' and 'professionals', this is an artificial dichotomy which misrepresents what is in reality a continuum, as is illustrated by the list of authors of this article.

The increasing size and importance of the non-institutional sector in herpetology has led to a parallel increase in the number of journals and magazines catering to this group. The contents of these 'amateur' publications reflect primarily the interests of this sector in captive husbandry and breeding, but also include field reports, natural history information, and occasionally papers on systematics, including new species descriptions. Some of these publications produce primarily well-illustrated accounts for readers with little technical knowledge, whereas others publish sophisticated technical reports. They have in common that, unlike in scientific journals, the contents are not normally subjected to the process of peer-review, in which manuscripts submitted for publication are sent to other experts for comment prior to publication.

From the outset, we emphasise that these publications have made a very valuable contribution to our understanding of the biology of reptiles and amphibians. None of what is written here is in any way intended to discourage participation in herpetology or the publication of observations by non-institutional herpetologists. Moreover, none of it is intended as a criticism of the editors of either *Litteratura Serpentium* in particular,

or of amateur herpetological publications in general. However, the beneficial nature of amateur contributions in the area of systematics is more controversial. It is here that clashes have been most frequent and acrimonious. The most notorious example was the controversy surrounding the publications of Wells & Wellington (1984, 1985), who described or revalidated hundreds of species and genera of Australian reptiles and amphibians with minimal evidence. This led to attempts to have their work suppressed by the International Commission on Zoological Nomenclature, and resulted in years of highly publicised acrimony among herpetologists in Australia. Although the level of controversy surrounding this case was exceptional, the underlying problem is by no means uncommon (see McCranie & Wilson, 1979; Nussbaum & Raxworthy, 1996; Lötters & Vences, 2000). One non-institutional herpetologist responsible for several recent controversial descriptions has been Raymond Hoser, from Melbourne, Australia. Prior to his forays into systematics, Hoser was best known in international herpetological circles for his well-illustrated book *Australian Reptiles and Frogs* (Hoser, 1989), as well as for various books and other publications exposing alleged corruption in the Australian government and its authorities. None of our criticisms of Hoser's taxonomy are intended to detract from his other achievements and contributions, nor do we wish to belittle his considerable knowledge of the Australian herpetofauna.

Hoser's recent taxonomic works include the description or revalidation of a number of species and subspecies of *Acanthophis* (Hoser, 1998a), the description of *Pailsus pailsei*, a new genus and species of elapid (Hoser, 1998b), the description of two new genera, two new species and seven new subspecies of Australasian python (Hoser 2000a), and the description of *Pailsus rossignolii* from Irian Jaya (Hoser, 2000b). All appeared in non-peer-reviewed herpetological publications. Hoser's revision of *Acanthophis* was critiqued by Aplin (1999), and the description of *Pailsus pailsei* by Williams & Starkey (1999). Many of the points in this article parallel those of Aplin (1999) and Williams & Starkey (1999). We do, however, feel that this is appropriate, given subsequent developments and the largely separate readership of *Litteratura Serpentium* and *The Monitor*.

In a similar vein to Aplin (1999), we aim to establish what might be regarded as sensible standards for the description of new taxa in herpetology. The recent works of Hoser will be compared to these, and their deficiencies analysed. Finally, we offer some recommendations on systematic works in the amateur literature.

DESCRIBING SPECIES AND GENERA

Nomenclatural versus biological validity of new species

Describing new species is both easy and difficult. Conforming to the formal rules is easy, writing a description that is convincing to others may be much more difficult. A frequent source of confusion concerns what constitutes a 'valid' description. In order to discuss this, we need to distinguish between the validity of a description under the rules of the International Code of Zoological Nomenclature, and the biological reality of the species involved.

Although many non-systematists imagine the description of species and taxonomy in general to be highly

regulated, this is not the case. Certain taxonomic acts, including the description of new species, subspecies or genera are subject to a set of rules, the International Code of Zoological Nomenclature, published by the International Commission on Zoological Nomenclature in London. In addition to the publication of the Code, the Commission has powers to rule on aspects of the interpretation of the Code in case of doubt, and may override some of its provisions for the purpose of furthering the stability of the nomenclature. However, the vast majority of taxonomic activity does not involve any interaction with the Commission.

For a new species name to be valid under the Code, the description needs to fulfil only a few basic criteria:

1 It needs to be published in a manner that ensures multiple identical copies that can be obtained for free or purchased. Web pages do not qualify, neither do manuscripts.
2 It needs to contain the name of the new species, in Latin letters, and a clear indication that the author does indeed intend to describe the species as new.
3 A holotype must be designated.
4 A diagnosis must accompany the species name. This simply means that features supposedly distinguishing the new species from others must be indicated. Note, however, that no standards for these diagnoses apply: a single sentence is enough. If the supposedly diagnostic features are actually inadequate for the purpose, this does not affect the validity of the description under the Code.

If a description of a new species fulfils these requirements, then the name is validly published and available under the provisions of the Code.

The question of the biological validity of a species is a different set of problems. Although biologists argue frequently about what exactly a species is and how it should be defined, most would agree that a species is fundamentally an independent evolutionary lineage (de Queiroz, 1998). Some prefer diagnosing such lineages on the basis of reproductive incompatibility, others on the basis of diagnostic characters, others based on molecular evidence, but the fundamental question is the same. The point is that the description of a new species must not only satisfy the criteria of the Code to make the new scientific name available, but it must also convince readers that a 'real' biological lineage is being described. The important point is that a validly published name and a biologically valid species are not the same thing. It would, for instance, be entirely possible to describe every single population of tree frog from western Europe as a distinct species, diagnosing it on spurious grounds such as 'slightly darker than species A, slightly larger than species B'. Such descriptions would be valid under the provisions of the Code, but, of course, they would be biologically absurd.

For a new species description to be useful as well as nomenclaturally valid, it needs to convince the reader that what is being described is indeed a biologically valid species. This is best accomplished by providing adequate information on the new species, its variability, and comparing this with the variability of the most similar and closely related species. In other words, sufficient evidence must be presented to justify the description of the new species. Thus, in addition to the formal requirements of the Code, an adequate description normally contains much additional information. Much of this is covered in detail in Winston (1999).

The diagnosis should include precise measurements or clear descriptions of characters, and how they can help differentiate the new species from all potentially confusing species. The aim of the diagnosis should be to allow even persons relatively unfamiliar with the taxa concerned to distinguish between the new species and its closest relatives (although this may sometimes be difficult in practice). In the case of very clearly and obviously distinct forms, the diagnosis can be brief, whereas in more difficult groups, a very extensive diagnosis may be required.

The description of a new species should include all the characters used as standard in the taxonomy of the group concerned. This means all the standard scale counts, arrangements of head scales, etc., as well as any characters particularly useful within the genus. Variation in the new species should be described: what is the range of the different scale counts, measurements, etc.? Do juveniles or males and females differ? The variation in the new species should be compared with that found in related and potentially confusing species. Normally, this would involve the examination of a substantial proportion of the specimens of the new species (if available) and potentially confusing species preserved in the world's museums. All of this is essential to ensure that the new name can be reliably attributed to the species concerned, and that the new species can be reliably identified.

The holotype of the new species must be described in detail. The holotype is the individual specimen to which the new name will remain tied in case of future taxonomic changes. If a species is split into two separate species on the basis of new evidence, then the species that keeps the original name is that to which the type belongs, whereas the other species receives a new name. Consequently, it is essential that the type of the new species should be well characterised, especially in case it later becomes lost, destroyed, or otherwise unavailable for further study. A full description is thus required, which should include all features of importance in the systematics of the genus concerned. The description of the type needs to be sufficiently precise to allow the unambiguous identification of the type in case of later taxonomic developments.

The author of a new species needs to be intimately familiar with the taxonomic literature on the group to which his new species belongs. This is essential, because once a name has been proposed for a species, it remains available, even if the original publication was obscure and the name has not been used since. Many well-known species of reptile were described as new several times, under different names. If such a species is to be split on the basis of new evidence, then the older names remain available and take precedence over any more recent names. Although everyone can make mistakes, describing a species as new, only to find that it had been described previously in an overlooked publication, can cause considerable nomenclatural confusion, as well as embarrassment to the perpetrator. Avoiding this requires the assessment of all other names currently regarded as synonyms in the group to which the new species belongs.

Additionally, one would normally expect new systematic papers to be presented against the background of previous work on the same group. If previous workers have proposed a systematic arrangement based

on evidence, then it would be customary to follow this arrangement, unless one has evidence to the contrary. For instance, Kluge (1993) proposed a new generic arrangement for pythons, based on his phylogenetic analysis of 121 behavioural and morphological characters. It would therefore be normal practice for further studies of python systematics to follow that arrangement, unless they provide strong evidence contradicting Kluge's findings.

Finally, as Hoser (1999a) himself states, a fundamental requirement for any scientific work is that it should contain sufficient detail on methodology for others to repeat the observations. In the context of a species description, this would include precise details of characteristics recorded, and, very importantly, a list of the specimens examined as part of the study. This would allow others to examine the same specimens to verify the conclusions of the original author, or to gather additional data or to select further material not examined by the original author.

It should thus be clear that the validity of a description under the provisions of the Code is not an indication that it provides sufficient information to be useful to others. Claiming that comparing published descriptions against the rules of the Code is 'the best way to judge [their] adequacy' (Hoser, 1999a) is thus nonsensical. Any glance at a peer-reviewed scientific journal will show that a majority of new species descriptions contains much of the extra information described above. Such descriptions are accepted by the scientific community without much question. Very few new species descriptions elicit instant distrust if they provide adequate evidence. Claiming that lack of acceptance of some names is due some 'stigma' attached to using names proposed by unpopular authors, rather than an issue of quality of evidence, as suggested by Hoser (1999b), is simply a form of self-delusion.

What is a genus?

In addition to species, Hoser also described two genera of python (*Lenhoserus* and *Katrinus*) and one of elapid (*Pailsus*). The definition of a genus is much less clear-cut than that of a species. Species are generally regarded as 'real' entities, the units of evolution. On the other hand, genera are groups of species grouped together for classification; in that sense, they are arbitrary entities, not 'real' biological units.

Under the provisions of the Code, the description of a new genus requires primarily the selection of a name, the designation of a type species, and a diagnosis of the new genus. Again, in practice, the description of a new genus is subject to a number of further conventions designed to ensure that new genera or revised generic arrangements are supported by adequate evidence.

In modern systematics, all groups above the level of the species, including genera, are recognised on the basis of common evolutionary descent, not similarity. A natural group (a monophyletic group, in the jargon of modern systematics) is a group that includes all the descendants of a common ancestor, and only the descendants of that ancestor (Fig. 1-i). This can be visualised by thinking of the tree of life as a physical structure: a natural group is a group that can be pruned from this tree of life with one single cut at its base.

On the other hand, a group which only includes some

Morelia Viridis (juvenile)
Foto: A. Bening

(i)

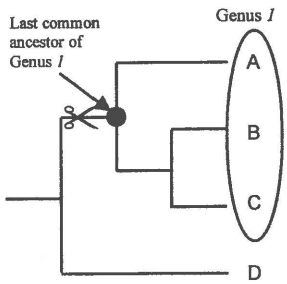
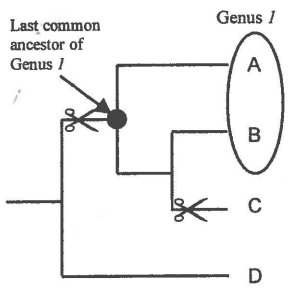


Figure 1. (i) Genus *1*, consisting of species A, B and C, forms a natural (monophyletic) group: it includes all the descendants of the last common ancestor of the genus. If the tree is seen as a physical tree, this group can be cut from the tree of life with a single cut near the common ancestor of the three species (indicated by scissor symbol).

(ii)



(ii) in this diagram, genus *1*, consisting of species A and B, does not form a natural (monophyletic) group: species C is also a descendant of the common ancestor of the genus, and yet is excluded. Physically removing genus *1* from the tree of life would require a second cut, to prune species C from the genus.

but not all of the descendants of the common ancestor is termed a paraphyletic group - this is artificial, in the sense that one or several species have been removed from a natural group. If we again think of the tree of life as a physical structure, removing such a group from the tree would require more than one cut: one at the base of the group, and one or more to remove the excluded descendants of the common ancestor (Fig. 1-ii).

Most systematists today would agree that all genera should be natural, monophyletic groups. This also includes cases where one species in such a natural group is highly divergent from all the others (Fig. 2), and

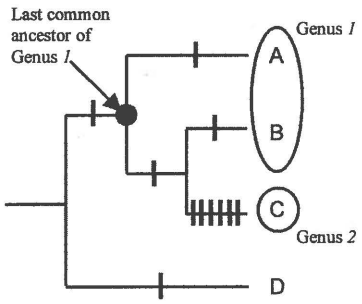


Figure 2. A common scenario: Species A and B are similar, and grouped into genus *1*, whereas species C is very distinct (vertical bars on tree branches indicate differences accrued during the course of evolution), and has therefore been grouped as a separate genus, *2*. However, genus *1* is not a natural group: it does not include species C, which is also a descendant of the last common ancestor of genus *1*. If evolutionary history is regarded as the main basis for classification, then species C should not be classified in a separate genus, irrespective of how strongly differentiated it is from A and B.

would be classified in a separate genus if similarity were taken into account: the evolutionary relationships are what matters, not similarity or differences. Although this may seem counterintuitive at first, it actually makes good sense. If similarity were the crucial factor, who would decide, and on what basis, how different a species has to be in order to be excluded from a genus? What characteristics would have to differ? What happens if, for example, scalation tells one story, and DNA tells another? In each case, the decision would be entirely arbitrary. On the other hand, since there is only one evolutionary history of life, classifying according to this is objective rather than arbitrary.

The description of new genera in any serious scientific publication would normally require the demonstration that both the new genus, and the genus from which it is split off, are natural groups. Similarly, any revisionary work which proposes changes to the generic classification of species would normally be expected to demonstrate how the new classification results in genera which represent natural groups.

WHAT IS WRONG WITH HOSER'S DESCRIPTIONS?

After clarifying what makes a 'good' description, we will now examine Hoser's recent descriptions in this light. From the outset, it is clear that all the taxa described by Hoser are validly described under the provisions of the Code. The names are thus available, and, where they are the oldest available names for biologically valid species or genera, they must be used.

However, Hoser's descriptions are much less convincing when it comes to establishing the biological reality of his taxa. Hoser almost invariably fails to provide adequate information on his species, on their types, or on the material he has examined. This leads to difficulties in identifying his taxa, in assessing the holotypes, and in repeating and testing his observations. We are in effect asked to accept his species without being able to check his data.

Hoser's names

The problems start with the names themselves. Under the provisions of the Code, the scientific names of species should be Latin names, or Latin words formed in accordance with Latin grammar. For instance, to name a species after a male individual, the Latin singular masculine genitive ending *–i* is appended to the name of the person so honoured (e.g., *Hydrophis coggeri*, named after Harold Cogger). Where the person concerned is female, the Latin singular feminine genitive ending *–ae* is added (e.g., *Ephalophis greyae*, named after Beatrice Grey).

The Code states that where there is evidence within the publication itself that a name was formed erroneously, then the name has to be emended (Article 32.5.1) - see Shea (1996) for an example in the case of *Ephalophis greyae*. It turns out that many of Hoser's names do indeed require emendation, as they were formed in a manner inconsistent with the Code. Thus, *Acanthophis wellsei* was emended to *A. wellsi* by Aplin & Donnellan (1999), as it was named after Richard Wells, not Wellse. Of Hoser's *Acanthophis* species, the name *crotalusei* needs to be emended to *crotali* or *crotalusi*, *cummingi* to *cummingae*, and the name *pailsei* to *pailsi* in Hoser (1998b). In Hoser's (2000a) python paper, the name *barkeri* requires emendation to *barkerorum*, and *bennetti* to *bennettorum*, as the subspecies were each named after two people. Although these are relatively trivial matters, sloppiness in the formulation of scientific names does not inspire confidence in the remainder of an author's work. Moreover, in the age of electronic databases, where spelling does matter, the existence of multiple versions of the same name can be a substantial impediment.

Hoser's diagnoses and descriptions

Whereas the formulation of scientific names may be regarded as relatively trivial, the provision of adequate information to substantiate a species description cannot

fall into this category. Here, Hoser largely fails to conform to the standards normally expected. His diagnoses are generally imprecise, in that many characters are ill defined, and variation within both the new species and the species they are being distinguished from is inadequately quantified. Thus, for instance, *Morelia harrisoni* 'tend to have a lower average ventral and subcaudal scale count' than other *Morelia*, but Hoser himself admits that 'the sample seen is too small to conclude if this trend is general' (Hoser, 2000a). Hardly the sort of diagnosis that would help a customs official identify a smuggled specimen, and yet, Hoser's descriptions are replete with similar statements.

In many cases, we are told that one taxon 'usually' has one character that is 'usually' absent in another. How usual is 'usually'? Does this character distinguish 98% of specimens, or 40%? In some cases, distribution appears to be the only distinguishing feature (for instance, for distinguishing between *Aspidites ramsayi panoptes* and *A.r. richardjonesii*). Other characters used frequently by Hoser include general statements about body stature and shape. These characters are affected by the condition of the animals, but this concern is never addressed. Moreover, without comparative material, it would be difficult for others to identify specimens of the new species by these criteria. In many cases, Hoser omits even the most basic standard information on his new taxa, such as dorsal and ventral scale counts for *Pailsus rossignolii*.

Where Hoser refers to illustrations in other publications, these sometimes contradict his diagnoses. For instance, Hoser (1998a) states that his *Acanthophis crotalusei* differs from *A. barnetti* in not having 'distinct black lines running up the infralabials to the mouth'. He refers a specimen depicted in O'Shea (1996) to *A. crotalusei*; however, the infralabial coloration of that snake corresponds to that described for *A. barnetti*: the black infralabial markings clearly extend to the mouth.

Hoser (2000a,b) states that several of his python taxa and *P. rossignolii* can be separated from their nearest relatives by their DNA. This is rather disingenuous when no such studies have been carried out. Diagnoses should state how species can be identified, not refer to the possibility of doing it by means the author could not be bothered with.

More fundamentally, repeatability of observations is one of the cornerstones of any scientific publication, as Hoser himself states (Hoser, 1999a). However, in his own revisions and descriptions, he usually fails to provide lists of the materials he has examined. This leaves many questions open. For instance, what were his sample sizes for establishing the range of variation in his new species or those he seeks to diagnose them from? Where can another researcher find these specimens and examine them for himself? The clue comes from frequent statements that Hoser was unable to examine any specimens in museum collections as it ' was beyond the means of this author with regards to time constraints and other commitments' (Hoser, 2000b). Such a statement is a slap in the face of those systematists who have spent years of their lives examining museum specimens in order to acquire evidence that supports their conclusions.

Type descriptions

As noted above, an adequate description of the types

of new species is essential to allow future reinterpretation of the papers concerned. On this count, Hoser fails consistently. For his *Acanthophis* descriptions, Hoser (1998a) provided at least very basic descriptions of the types of most of his species (basic scale counts and a few notes on other characteristics). It appears that Hoser had at least examined the specimens concerned, except for *A. wellsi* (Aplin, 1999). Similarly, Hoser provided a description of the type of *P. pailsei*, which he had examined. On the other hand, in the case of *Pailsus rossignolii*, Hoser (2000b) apparently based his description of the type on information from a correspondent in Bogor, and there is no evidence that he has ever set eyes on the species. In the case of his new python taxa, Hoser (2000a) states openly that 'Type material for all species listed below has not necessarily been inspected by this author, however this author has inspected a substantial number of specimens including from the type localities given.' For most species, Hoser provides no description whatsoever of the holotype. Describing a new species without examining the type is clearly less than professional, and can cause a number of problems, discussed by Aplin (1999). They include particularly the selection of unsuitable specimens as types (e.g., poorly preserved specimens, specimens with inadequate locality data, potential hybrids, etc.), as well as the possibility of specimens having been misidentified.

Table 1 compares Hoser's (2000a) recent revision of Australasian pythons with a professional revision of one particular group of pythons, the amethystine

	Harvey et al. (2000)	Hoser (2000a)
Materials and Methods	9 pages	1 sentence, stating that types were not examined
Definition of morphological characters	6 pages	No information
Results of analyses	5 pages	No analyses
Species/subspecies accounts	15 pages	18 pages
Number of (sub)species	5	41
Bladzijden per (sub)species	3	0.44
Holotype descriptions	1-1,5 pages per species, with extensive details of pattern scalation and dentition.	No data: 5 out of 9. Sex and ventral + subcaudal scalation and/or size: 4 out of 9
Onderzocht materiaal	Volledige lijst met 156 specimens	No information

Table 1: Comparison of the information content of the recent revision of Australasian pythons by Hoser (2000a), and the revision of the amethystine pythons by Harvey et al. (2000).

pythons, published in the peer-reviewed, scientific journal *Herpetological Monographs* by Harvey et al. (2000). The differences in the level of information provided are clear.

Hoser's generic classification

The generic arrangements advocated by Hoser contain a number of shortcomings that peer review might have avoided. In the case of *Pailsus*, Hoser (1998b) only differentiated his species *P. pailsei* from *Pseudechis australis*, but makes no attempt to differentiate it from *Pseudechis* as a whole, as noted by Williams & Starkey (1999). Hoser's response to criticism from Williams & Starkey (Hoser, 1999b) provides no new evidence to substantiate the status of *Pailsus*. Given the widely accepted criterion of monophyly, Hoser should have demonstrated that *Pseudechis* is monophyletic if *Pailsus* is excluded. Even if *Pseudechis* is split and the genus *Cannia* Wells & Wellington, 1984 recognised, Hoser would still have had to demonstrate the monophyly of *Cannia* if *pailsei* is excluded. He did not do so, and this seriously compromises his case for the recognition of *Pailsus*.

Hoser's (2000a) generic arrangement of Australasian pythons almost entirely ignores everything we know of the phylogeny of these snakes, especially from Kluge (1993). For instance, Kluge (1993) showed that the green tree python is rooted within the genus *Morelia*. To retain *Morelia* as a natural group, *Chondropython* was therefore synonymised with *Morelia*, the green tree python now being known as *Morelia viridis* (Fig. 3). Hoser ignores this, and retains *Chondropython* on the basis of superficial dissimilarity. Although Hoser cites Kluge's study, his statement that 'the two [genera]

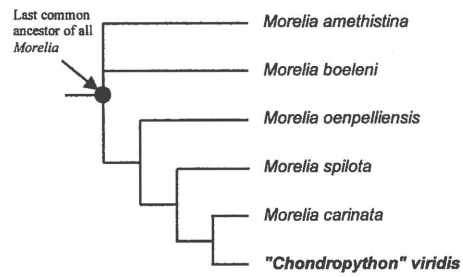


Figure 3. Phylogeny of the genus *Morelia*, modified from Kluge (1993). If *Chondropython* is recognised as a separate genus, then *Morelia* is not a natural group, as the species *viridis* is also a descendant of the last common ancestor of all *Morelia*. For this reason, Kluge (1993) assigned the green tree python to *Morelia*, as *M. viridis*.

have been separated long enough to warrant being placed in separate genera' suggests that he has not read it, or at least not understood it. Other summary rejections of Kluge's evidence include the placement of the species *Apodora papuana* in the genus *Liasis* (Hoser simply states that he does not agree with *Apodora*), and the assignment of the species *timoriensis* to the genus '*Austroliasis*' (correct spelling: *Australiasis*) together with *amethistina*, despite strong evidence grouping *timoriensis* with Asian *Python* (Kluge, 1993). No reason is given, other than that it 'makes sense'. On the other hand, Hoser recognises Wells & Wellington's genera *Australiasis* and *Nyctophilopython*, shown by Kluge to be superfluous for the purposes of retaining natural groups. He also describes two further genera, *Katrinus* and *Lenhoserus*, which are equally unnecessary in the light of Kluge's studies.

Previous literature

Gaps in bibliographic research can have serious consequences in taxonomy, due to the availability of older but forgotten names. In his description of *Pailsus pail-*

sei, Hoser (1998b) makes no mention of a paper by Wells & Wellington (1987), describing a new species of Elapid as *Cannia weigeli*. Although this description was long ignored by herpetological systematists, it is nevertheless valid under the provisions of the Code, as it was produced by the required means, and sent out to a number of Australian herpetologists and libraries at the time of publication (J. Weigel, pers. comm.), thus fulfilling the criterion of availability. Wells and Wellington's species is distinguished from *Pseudechis australis* principally on the basis of the possession of largely undivided subcaudal scales and a more slender build, just like *Pailsus pailsei*. Hoser (1998b) does not mention *Cannia weigeli*, and no attempt is made to distinguish between the two forms. The supposedly diagnostic features of *P. pailsei* and *C. weigeli* appear to be identical. It is thus entirely possible that the two names represent the same species, in which case *pailsei* Hoser, 1998 would become a synonym of *weigeli* Wells & Wellington, 1987. The net result is uncertainty not only on whether there is a separate, poorly defined species of elapid resembling *P. australis* in northern Australia, but, moreover, on whether there is only one (*weigeli*) or two (*weigeli* and *pailsei*).

Does it matter?

Hoser's taxonomic practices appear to be based at least in part on the belief that bestowing names on potentially valid taxa constitutes a service to herpetology, be it for conservation (e.g., Hoser, 2000a) or other purposes. However, the fact is a name for a 'form' is only useful if, at the same time as being named, the 'form' is also shown to be a real biological entity. Named forms of uncertain status do not help anyone.

Aplin (1999) reviewed the question of whether the taxonomic practices of Hoser or Wells & Wellington do much harm, but largely confined their comments to their effects on herpetological systematics. For professional herpetological systematists, little serious harm beyond irritation is likely to occur, since they have the background knowledge required for an overview of the situation.

However, for others, the situation may not be so benign. Available names 'cast into the nomenclatural pool, from whence they may be fished out as required by subsequent revisers' (Aplin, 1999) can cause considerable confusion among non-experts. For instance, the frequent misidentification and mislabelling of *Trimeresurus* and *Naja* species by Thai exporters in the 1980s has led to confusion among herpetoculturists to this day. Thus, black and white spitting cobras from Central Thailand are still frequently misidentified as *Naja sputatrix*, rather than *N. siamensis*, and misidentified green pitvipers (*Trimeresurus*) are the rule rather than the exception in herpetological collections. Moreover, inaccurate nomenclature in the herpetocultural literature can percolate into the non-systematic scientific literature with surprising ease. This applies particularly to the toxinological literature, as suppliers of snakes and suppliers of venom often rely on a variety of 'non-professional' references to identify their snakes. As a result, many toxinological studies are virtually uninterpretable: the venoms involved cannot be reliably identified to species level. In some groups, this affects as many as 75% of experimental venoms (Wüster & McCarthy, 1996), a tremendous waste of resources and effort.

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Conservation also depends on correct taxonomy, and Hoser (2000a) uses this as a reason for the naming of his new python taxa. However, inadequately described species of uncertain validity make poor candidates for active conservation support. Attempting to conserve taxa which are later shown to be 'non-taxa' is likely to be politically disastrous, as those opposed to conservation efforts can then argue that threatened taxa may just be figments of some taxonomist's imagination. Even more crucially, taxonomic works must be seen to be objective and based on sound evidence. The perception that species are being described for the sole purpose of furthering a conservation agenda, rather than on the basis of scientific evidence, would be disas-trous for conservation and taxonomy alike.

THE ETHICS OF SPECIES DESCRIPTIONS

Although there are millions of species of animal on Earth, the vast majority of studies concentrate on relatively few groups, particularly some high-profile vertebrates, and species of commercial interest. As a result, the interests of separate researchers often focus on the same organisms, and similar aspects of the biology of that organism. Competition between researchers can be the result. In most cases, competing researchers can find a way of avoiding head-on collisions, by focussing on different aspects of their organism, or using different methods and approaches: compromise results in acceptable levels of input and output for all concerned. Moreover, a poorly designed and executed study does not generally prevent others from repeating and expanding it in a more appropriate manner.

Biological nomenclature is unique in that, for the purpose of enhancing the stability of the nomenclature, the oldest available name for a species 'sticks' permanently, irrespective of the quality of the description. A worker who publishes a nomenclaturally valid name for a supposed new species, but in a manner inadequate to demonstrate its biological validity, will nevertheless remain the author of that name. If the species is eventually recognised as biologically valid, due to more convincing work by others, then the name conferred upon that species in the original, inadequate description will nevertheless remain the valid name of that species, despite the fact that it is the later author who deserves the credit for defining it properly.

The description of new species is thus one of the few areas of science where a poor piece of work can largely pre-empt a thorough study. In an attempt to prevent an unseemly race to describe new species with potentially rushed and inadequate descriptions (a scenario vehemently condemned by Hoser, 2000c), the Code contains a Code of Ethics. This lays down rules of conduct to be followed by authors when describing species. In particular, Section 2 of the Code states that 'A zoologist should not publish a new name if he or she has reason to believe that another person has already recognised the same taxon and intends to establish a name for it [...]. A zoologist in such a position *should communicate with the other person* [...] and only feel free to establish a new name if that person has failed to do so in a reasonable period (not less than a year).'

The Code of Ethics is, however, only a set of recommendations. Descriptions published in breach of the

Code of Ethics are nevertheless valid. Following the Code of Ethics is a matter of professional courtesy towards others, and the equivalent of displaying good manners in a normal social environment.

Do any of Hoser's descriptions represent a breach of the Code of Ethics? The available evidence would suggest that some do. Both in describing *Acanthophis wellsi* and *Pailsus rossignolii*, Hoser acknowledged specifically that others had been working on the subject. In the case of *Acanthophis wellsi*, Hoser (1998a) stated that he had been 'in regular contact with the Western Australian Museum staff for many years, and received correspondence from them implying that they may undertake and publish a second review of *Acanthophis*'. Aplin (1999) noted that the claimed contact with Hoser had been very limited, and, crucially, noted that Hoser had not informed them of his intention to publish a revision of *Acanthophis* or of describing the Pilbara death adder as *A. wellsei*. This is in clear breach of the Code of Ethics. Aplin & Donnellan's description of the same species had been submitted for publication at the time Hoser's paper appeared.

In the case of the description of *Pailsus rossignolii*, Hoser clearly acknowledges that O'Shea, Starkey and Williams were working on the description of a new species of New Guinea *Pseudechis*. He therefore had, in the words of the Code, 'reason to believe that another person had already recognised the same taxon and intended to establish a name for it', and should have notified these workers of his intention to describe the Irian Jaya *Pseudechis*. In fact, O'Shea et al. are working on another *Pseudechis* species from Papua New Guinea. Instead, Ulrich Kuch (University of Frankfurt, Germany) had a manuscript describing the Irian Jaya *Pseudechis* in the final stages of preparation. Hoser was well aware of Kuch's interest in *Pseudechis/Pailsus*, as he had even sent him tissue samples of *P. pailsei* for DNA analysis. Consequently, an enquiry or notification of his intentions would have been in order. None was made. Moreover, when submitting his manuscript describing *P. rossignolii* to *Litteratura Serpentium*, Hoser requested that the manuscript be published rapidly, 'as others were working on the same subject' (van Aken, pers. comm). None of this helps to allay the suspicion that Hoser was deliberately trying to scoop other researchers in naming the Irian Jaya *Pseudechis*.

As a reaction to criticism of the ethics of his description of *Acanthophis wellsei*, Hoser (1999a) stated that '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'. This is clearly inapplicable, as the people most affected by Hoser's descriptions were researchers whose interest in the relevant species was already known to and acknowledged by Hoser. Similarly, although Hoser had published several previous papers on *Acanthophis* prior to his 1998 revision, none had indicated his intention to carry out a taxonomic review of the complex.

The temporal coincidence of Hoser's and Aplin's description of what is now *Acanthophis wellsi* could, in isolation, be regarded as accidental or careless. However, the repetition of the same behavioural pattern, now better documented, in the case of *P. rossignolii* suggests more deep-seated antagonistic feelings towards the scientific establishment, perhaps as part of

the general anti-institutional attitudes Hoser displays throughout his writings. This is also supported by Hoser's (1999a) statement that the negative feedback he received for previous papers 'has only served to encourage [him] to do more of the same in the future', as well as by his out-of-hand dismissal of the findings of Kluge's (1993) professional study, as contrasted with his uncritical acceptance of the arrangements of Wells & Wellington (1984, 1985) (Hoser, 2000a).

Hoser (1999a) also mentions the delay between the statements of scientists working on the descriptions of species, and the actual publication of these papers. The fact is, however, that carrying out thorough studies of complex taxonomic groups is a labour and time-intensive process. Data has to be gathered, different workers working on different aspects of the same project have to co-ordinate their efforts, and at the same time, all involved are undoubtedly working on several other projects, as well as undertaking extensive administrative and teaching duties. The point is that serious systematists do not, as result of these constraints, neglect to examine important available material, unlike Hoser, by his own admission. A delay of several years in the publication of previously announced species descriptions is therefore to be expected, and does not constitute grounds to scoop a scientist known to be working on a description, especially without prior warning, as laid down by the Code of Ethics.

As noted above, a breach of the Code of Ethics does not invalidate a species description, just as belching and breaking wind loudly in an upmarket restaurant is not against any law. However, both display a profound disrespect for professional or social norms of behaviour, and reflect very poorly on those who perform them. More seriously, whereas we regard the distinction between 'amateur' and 'professional' herpetologists as largely spurious, Hoser's actions threaten to make the gap a reality. Non-institutional herpetologists frequently complain about not being taken seriously by 'professionals' (e.g., Gumprecht, 1997). However, in view of Hoser's activities, it seems likely that many professional systematists will now be more reluctant than ever to share their insights, for fear of being scooped. Therein lies the real tragedy of such cowboy taxonomy: whereas we could all benefit from the combination of the countless and invaluable observations and frequently correct 'gut instincts' of dedicated non-institutional herpetologists and the often greater technical and conceptual expertise and capabilities of trained scientists, Hoser's activities are likely to drive a further wedge between the two camps.

CONCLUSIONS - CONTRIBUTIONS TO HERPETOLOGICAL SYSTEMATICS IN 'AMATEUR' PUBLICATIONS

The problems posed by the publication of species descriptions in amateur journals leads to the final question of what can or should be done about such contributions. Obviously and fortunately, no person can be banned from publishing systematic research. However, the quality of individual publications should be controlled, and the appropriate mechanism for this is the peer review process. It is certainly true, as Hoser notes, that some species descriptions by professionals in peer-reviewed publications also leave a lot to be desired. However, the present authors feel strongly that two wrongs do not make a right, and that all who work in

science should strive to improve the level of their own work, rather than using poor work by others as an excuse for their own. Although this process certainly cannot eliminate all inadequacies in taxonomy, even fairly rudimentary peer-review could have prevented or improved many of the faults highlighted earlier.

The Code notes that the best vehicle for the descriptions of new species are peer-reviewed journals with wide circulation. Scientific and 'amateur' journals exist for different purposes. Increased contact between institutional and non-institutional herpetologists is certainly to be welcomed, but this does not apply to the mixing of the purposes of the different journals.

The editors of non-scientific journals need to be aware of the responsibility that the publication of species descriptions entails. Ideally, such descriptions should not be published in amateur journals at all. However, if a species description is submitted to an amateur journal, and the editors feel that publication would be appropriate, then the journal should break with its normal routine and send the paper out for review to 2-3 qualified taxonomists. Even if they are not experts on these particular species or genera, they will be able to point out gross mistakes or lack of essential information.

Editors should also be aware of the possible ethical implications of allowing authors to circumvent peer-review. Requests to publish a description quickly 'as others are working on the same subject' should set alarm bells ringing. Paragraph 6 of the Code of Ethics of the Code states that 'Editors and others responsible for the publication of zoological papers should avoid publis-hing any material which appears to them to contain a breach of the above principles [the Code of Ethics]'.

None of what has been written in this article is intended to discourage non-institutional herpetologists from seeking to contribute to our expanding knowledge of herpetological systematics. On the contrary, a lot remains to be done, and the many dedicated individuals who have spent years studying their favourite groups at their own expense and in their own time are likely to have accumulated many valuable insights. However, we caution against the 'go-it-alone' approach of publishing new species descriptions without the aid of peer-review. The publication of inadequate and unconvincing species descriptions in unreviewed journals, or the publication of descriptions of dubious ethical standing, will do nothing to enhance the reputation of the journal, or of non-institutional herpetologists or herpetoculturists as a whole.

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