

**In Response to the Defense of the USGS Report on Giant Constrictors posted by
Dr. Susan Haseltine
David and Tracy Barker**

This letter is written in response to [comments posted by Dr. Susan Haseltine](#), Assistant Director of Biology, U.S. Geological Survey. Dr. Haseltine wrote to address [charges of poor and improper peer review of the USGS report on giant constrictors](#). This report was written by USGS biologists, Robert Reed and Gordon Rodda, and published as an Open File Report by the USGS in December 2009. We make the following observations.

Dr. Haseltine wrote that she was responding to “a press release issued by a reptile-trade organization and an accompanying letter by a group of veterinarians and other scientists”. That by itself sounds a little dismissive; in particular, we’d like to point out that the “group of veterinarians” are DVMs and also PhDs, as well as tenured professors. The letter makes a valid point and it is signed by an accomplished and respected group of scientists.

Dr. Haseltine wrote: “The USGS provides unbiased, objective scientific information upon which other entities may base judgments.” She then describes the minimum requirements of the USGS review process, including a minimum of two reviews, and an assessment of the authors’ responses to the reviews to be performed by both research managers and independent scientists within the USGS.

As described, this is what Dr. Haseltine refers to as “independent scientific review.” It is also referred to as “internal review” or “in-house review”; within government agencies and some universities this is usually the first step before sending a manuscript on to a journal for further review; papers are not considered to be “reviewed” until publication. In fact, the USGS report on the nine snake species is an "Open File Report", not a refereed paper published in a scientific journal.

It’s difficult to imagine the review process described by Dr. Haseltine as being independent and it falls short of what is generally considered to be peer-review for several reasons. It is typical for authors to seek input from reviewers with expertise or experience. However, before publication, a formal review procedure would have the editor or editorial board send the entire manuscript out to independent and anonymous reviewers capable of evaluating the entire work with a critical eye.

Having the authors choose their reviewers, and then having “independent scientists within the USGS” (an oxymoron) evaluate if the authors sufficiently complied with the friendly reviews does not equal the editorial and peer review required by a refereed professional journal. It is tantamount to sending a manuscript on Intelligent Design to the Association of Anti-Evolution Fundamentalists for review—the expected critique is supportive, it is not an anonymous review, it might add or subtract minutia from the manuscript, it might correct grammatical errors, but it in no way subjects the manuscript, its details and conclusions, to rigorous and critical review.

The review process is further weakened when most reviewers see only portions of the manuscript. Dr. Haseltine claims that the authors far exceeded the minimum required two reviews, and solicited input from 20 reviewers. We point out that in the Acknowledgments of the report, the authors express their gratitude to the 20 reviewers for providing “thoughtful review of individual chapters, and *in some cases* the entire document...” [italics ours].

Having reviewers see only portions of the manuscript is a selective way to control the feedback of the reviewers. If the authors send a chapter that is primarily a taxon account based on a literature search, such as comprises the bulk of this report, the reviewer can be expected to provide a friendly review provided the topic of the chapter is competently researched.

For example, a chapter from an Intelligent Design manuscript that faithfully recounts the career of Charles Darwin can be sent to an evolutionary biologist for review who would return a favorable critique of the chapter, oblivious to the premise of the manuscript.

If the chapter on yellow anacondas was sent to an appropriate authority such as Dr. Tomas Waller of Argentina (included in the group of acknowledged reviewers), he likely provided comments that improved the scholastic aspects of the chapter, and perhaps added a personal observation or two. However, we question the reviewers’ qualifications to comment on the climate matches provided in section 10.2 of each taxon account, each illustrated with a map.

In the case of yellow anacondas, for example, the map illustrating the regions of suitable climate for the southern yellow anaconda (*Eunectes notaeus*) included all of peninsular Florida, and much of East and South Texas. In fact, surface water in at least 70% of these areas was frozen during the first two weeks of January 2010; high temperatures remained below freezing for days in a row and night time low temperatures were in the teens—such conditions are fatal to yellow anacondas.

The climate-match maps are similarly exaggerated for the seven of the eight species that are provided maps. The map for the northern yellow anaconda (*Eunectes deschauenseei*) showed zero suitable climate in the continental USA. No map was provided for the Beni anaconda, but based on the climate-space graph illustrated in figure 9.3, it would have shown no suitable climate in the continental USA other than possibly extreme South Florida.

Perhaps the sections in the accounts regarding climate-matching were not included in portions of the manuscript sent for reviews. Possibly the reviewers asked to comment on the taxon accounts were not aware of the actual weather in the areas of the continental USA identified as suitable climate.

We used Google to check out the 20 reviewers identified in the Acknowledgments and to whom Dr. Haseltine referred. Recognizing the constraints of those searches, particularly considering the dated information that sometimes remains online, it appears that six are

government biologists (three with USGS); twelve are Americans and eight are internationals; six have either co-authored articles on the “dangers” or “problems” of Burmese pythons in the Everglades, or have been featured in popular media making such statements (both authors have done both); at least five have worked or are currently working in South Florida on Burmese python management and eradication; eight can be considered as experienced and knowledgeable on some of the nine species analyzed in the USGS report. At least eight are identified as invasion-science biologists, or have co-authored papers on that general topic.

In our opinion, none of the reviewers can generally be considered to be critically inclined to the premise of this manuscript. Indeed, several of them will directly benefit financially and professionally if the nine species are placed on the Injurious Wildlife List.

It is a matter of public record that the authors, Gordon Rodda in particular, and the USGS program that employs them, have garnered tens of millions of dollars in research grants and other support from the government and the military over the past 20 years in order to study the only snake species ever registered on the Injurious Wildlife List of the Lacey Act, that being the brown treesnake.

It is not hard to understand why the USGS and its biologists would be strongly interested in seeing nine more species added to the Injurious Wildlife List. They have decades of experience getting funding for injurious snake research—they are expert at it. Because of this history and the fiscal incentives involved, there exists a tangible potential for bias, impropriety and a lack of impartiality. Due to the obvious possibility of conflict of interest and bias, the USGS should have recused itself from the contract and funding to create this report.

In her comments, Dr. Haseltine states: “While allegations have been made that the USGS report is being used as the justification of regulations on the reptile trade, it is important to note that the report offers no recommendation on policy or legislation.” We point out that the “allegations” to which she referred came from the Secretary of the Department of the Interior, Ken Salazar himself, who refers to the report as the *science* necessary for USFWS to proceed with the action to place the nine species on the Injurious Wildlife List [italics ours].

Neither Reed nor Rodda are oblivious to the fact that this report provides the ONLY scientific evidence, such as it is, that would justify any regulatory action regarding these nine tropical snake species. Perhaps Dr. Haseltine is not aware that in [an interview with *Wired Science*](#), Rodda defends and recommends the action to add these nine snake species to the Injurious Wildlife List, justifying that action with a poorly contrived and incorrect argument based on his apparent naïve understanding of economics.

The review written by Dr. Daniel Simberloff and published in *Biological Invasions* (2009), to which Dr Haseltine refers, falls short of enthusiastically embracing this report. It is better described as a brief summary of the paper with a very few qualified endorsements. It might be that with his decades of experience, Dr. Simberloff was a little

suspicious of the climate-matching predictions made in this report forecasting that Burmese pythons can survive in the climate of his university there at Knoxville, TN.

[The fact is that the report is flawed.](#) The USGS in-house review process failed the authors and the agency, and a badly flawed report is the result. The climate matching is incorrectly done. All statements and conclusions regarding the potential and the consequences of the nine species to establish are grossly exaggerated. All risk assessments must therefore be discounted. It is not to the credit of the USGS that this report was, as stated by Dr. Haseltine, reviewed by research managers and scientists employed by USGS.