



Type specimens, samples of live individuals and the Galapagos Pink Land Iguana

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Abstract

The nomenclatural implications of Gentile & Snell (2009)'s innovations in the designation of a type specimen - that is sampled and tagged but not killed - are discussed. The paper also responds to Nemésio (2009)'s criticism of Donegan (2008), where I argued that descriptions based on samples of live individuals are valid. I also discuss whether recent descriptions of such a nature involve a different degree of scientific rigour to other descriptions.

Key words: Nomenclatural availability, vouchers, collecting, specimen, type specimen, sample, ethics, framework, Code, Commission, *Conolophus marthae*.

Introduction

In this issue of *Zootaxa*, Gentile & Snell (2009) describe a critically endangered new species of land iguana from the Galápagos islands, named *Conolophus marthae*. Aside from being an important discovery, this description includes noteworthy innovations in the designation of a type specimen based on a live individual. In a recent exchange of papers in *Zootaxa*, the validity of descriptions based on a holotype which is not a full, dead specimen (such as this) have been discussed (Dubois & Nemésio 2007, Donegan 2008, Nemésio 2009). Dubois and Nemésio have expressed the viewpoint that descriptions based on live individuals are not valid nomenclatural acts. I have expressed the contrary view.

Other recent descriptions based on holotypes which are not killed have relied solely upon photographs and, in some instances, samples taken from live individuals (see discussion in Donegan 2008). In Gentile & Snell (2009), the individual in question was branded with a number and had a Passive Integrated Transponder (PIT) with a unique serial number hypodermically inserted allowing the individual's location. Gentile & Snell (2009) stated that "*The legal authority governing biological diversity of the Galápagos Islands, the Galápagos National Park Service, has agreed that if continued monitoring of the population of pink iguanas suggests that the population is increasing, the Holotype will be captured, moved to an existing captive facility at the Galápagos National Park Center, Puerto Ayora, Isla Santa Cruz; where it will be maintained until it dies. Upon the Holotype's natural death, it will be preserved and deposited in the Governmental Galápagos collection, maintained by the Charles Darwin Foundation, Puerto Ayora, Galápagos.*"

The methods and text used by Gentile & Snell (2009) in their designation of a type specimen raise novel issues in the context of the validity of descriptions based on live individuals, which are discussed in this paper. I also take this opportunity to respond to certain points made by Nemésio (2009) in his criticism of Donegan (2008).

The Galapagos Pink Land Iguana and the Code

Article 16.4.2 of the International Code of Zoological Nomenclature (Code) states that: '*Where the holotype or syntypes are extant specimens, [the description must be accompanied by] a statement of intent that they will be (or are) deposited in a collection and a statement indicating the name and location of that collection ...*'. This provision is at the nub of controversies concerning type specimens based on live individuals. Dubois & Nemésio (2007) argued that the provision falls unsatisfied in such instances. Donegan (2008) argued that the requirement is satisfied where a sample of a specimen is deposited in a reference collection. In cases where only photography is used, there are two interpretations: either the requirement does not bite because it is irrelevant and inapplicable; or the requirement falls unsatisfied. Gentile & Snell (2009) comply with Article 16.4.2 of the Code in setting out details of the reference collections where samples of the holotype are deposited. However, they go further in stating their intent to deposit the full specimen of the holotype in a museum collection upon its death.

Nemésio (2009) disagreed with Donegan (2008)'s proposition that samples of a live individual amount to a "part of" an "example of an animal" for purposes of the Code. As noted by Donegan (2008), a part of an example of an animal qualifies as a "specimen" for purposes of the Code (with similar wording used in Article 72.5.1 of the Code) and could therefore be used as a holotype. Nemésio's rationale is that the taking of samples (as opposed to whole specimens) is not "*the modus operandi*" or a "*common practice*" (p. 6). Nemésio (2009) adopts a strongly purposive interpretation to the term "part of" an "example of an animal", stating that "*this article must be placed in its actual context and not opportunistically used*" and concludes that this interpretation is "*so obvious that it should not be a matter of discussion, and the description of the bird published by Athreya (2006) [which was based on samples] should be categorically treated as nomenclaturally invalid and consequently the new nomen as unavailable*" (p. 6). This approach is inconsistent with the literal approach adopted to the interpretation of certain Code provisions by Dubois & Nemésio (2007, p. 8) (which was itself criticised in Donegan 2008). Furthermore, as Nemésio (2009, p. 6) notes in the context of describing how bird and mollusc specimens have been prepared over time, the *modus operandi* of sampling biodiversity has changed as new techniques have been developed. The techniques used by Athreya (2006) in describing a new bird species are facilitated by digital photography, for example. The availability of Longmire and other buffers to take blood samples in the field are also relevant to the changing nature of techniques used to sample animals. The use of transmitters by Gentile & Snell (2009), in order to keep track of the holotype until its death by natural causes and only then prepare a specimen, is a further innovation. Taxonomy should not be a static science.

New standards for live type specimens?

Gentile & Snell (2009)'s stated intention for a full specimen eventually to be made available gives zoologists an assurance that a study specimen for this species is likely to be available in the future. As a result, similar statements are to be welcomed in future descriptions based on samples of live individuals, where practicable. For studies of large, slow-moving, terrestrial vertebrates with a small, human-accessible geographical range, the tagging of an individual with a view to its *post mortem* deposition in a collection is likely to have a successful outcome. However, such an approach may not be practicable for the relocation of individuals of more mobile species (e.g. birds, fish) or those found in inaccessible areas (e.g. deep sea, remote mountains). For such organisms, it is likely that previously used methods for the designation of a holotype which is not killed, such as photography and the taking of samples, would be more appropriately used. I do not discuss the feasibility of any of these techniques for invertebrates and other small organisms.

Despite Gentile & Snell (2009)'s intention eventually to deposit the holotype in a collection and the nature of the species being described, that intention still might not be realised. For example, the holotype could outlive the authors or could be buried or destroyed by a volcanic eruption. In future descriptions, one could envision a statement of intent to deposit the remains of a live individual being qualified (e.g. "if it can be

recovered") or in which such an intention might be fanciful or have little prospect of success. It would not be sensible for future descriptions to include a statement of this nature unless there is a reasonable prospect of a full specimen being recovered.

Further comments on Nemésio (2009)

The publication of Gentile & Snell (2009)'s paper is an appropriate juncture at which to consider recently published arguments that descriptions based on holotypes which are samples or live individuals are not valid (Nemésio 2009). It is worth noting, however, that several passages of Nemésio (2009) do not constitute reasoned arguments against points of view expressed in Donegan (2008) but rather include arguments against other points of view incorrectly attributed or incorrect summaries.¹ Notably, the points of view expressed in Donegan (2008) were subject to several caveats and qualifications, many of which were ignored in Nemésio (2009)'s rebuttal. When one authors a paper of the nature of Donegan (2008), expressing controversial minority points of view, one expects to be criticised and ostracised. However, Nemésio (2009) includes a number of statements that on their face appear to be personal slights,² which are inappropriate and irrelevant to a discussion about taxonomy. Below, I focus on the substantive points raised by Nemésio (2009), particularly in the context of Gentile & Snell (2009)'s description.

I thank the editors of *Zootaxa* for allowing me to comment on Gentile & Snell's (2009) and also for the opportunity to give more precision in relation to certain concepts discussed in Donegan (2008), which I consider were misinterpreted or misrepresented by Nemésio (2009). In personal correspondence about certain points where I could not accept his interpretations of Donegan (2008), André Nemésio responded that he "*never had the intention of attacking anybody*" and attributed possible mis-statements or personal comments to linguistic issues.

(a) The Code and Type Specimens

Nemésio (2009) states that if the Code allowed descriptions based on samples of live individuals, this would be "*impractical*" (p. 3) and "*result in as many codes as zoologists*" (p. 3). These propositions are questionable in the context of a discussion over one small aspect of the Code. A not dissimilar development - on which some people also have strongly opposing views - concerns the publication of descriptions in electronic-only as opposed to printed journals (International Commission on Zoological Nomenclature 2008). If electronic-only journal publications are to be acceptable for descriptions, this would not make paper-only journal descriptions invalid nor result in as many codes as zoologists. Simply, two methods are acceptable. The same goes for descriptions based on samples and photography as opposed to descriptions based on specimens of dead organisms.

The notion of an "*ethically correct*" tyranny" forcing zoologists to suffer non-dead-specimen-based descriptions was discussed by Dubois & Nemésio (2007, p. 16). This language was again adopted by Nemésio (2009, p. 13): "*Trying to stop zoologists of doing the most basic procedure, collecting, to catalog the diversity of life, is a tyranny*" (p. 13). A "tyranny" is usually defined as an abuse of power involving imposition of rules in an arbitrary fashion. Describing persons with other viewpoints as tyrants is not helpful or productive in reasoned debate. For that reason, it is perhaps regrettable that I turned Dubois & Nemésio (2007)'s question on its head in asking whether the supposed victims of any "tyranny" were not in fact

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1. Examples include the following parts of Nemésio (2009): (i) p.3, last paragraph, first sentence; (ii) p.4, last paragraph, wording before (a) to (d); (iii) p.7, first new paragraph, first and 9th to 11th sentences; (iv) p.7, last paragraph, first sentence; (v) p.9, "Ethical considerations" section, first paragraph, penultimate sentence and entire second paragraph.
 2. An example is at p.5, third new paragraph, fourth sentence, opening phrase of Nemésio (2009).

proposing their own version of the same in preventing some taxonomists from using certain methods (Donegan 2008, p. 45).

Nemésio (2009) states that Dubois & Nemésio (2007) "*were trying to make clear that the general rules of zoology should be applied to the whole field of zoology ... If there is any dogmatic action here, it is the one proposed by Donegan (2008), who want [sic] to impose to [sic] the whole community of zoologists a point of view shared by a few ornithologists and primatologists*" (p. 3). However, at least it should be clear that there is no suggestion in any proposals to allow descriptions based on live specimens that any zoologist should be stopped from using any method or from cataloguing the diversity of life (e.g. Donegan 2008, p. 45). If a specimen based on a dead organism exists, then it is better to use it for a type specimen, whatever the circumstances in which it was procured. It is simply argued that the Code should allow descriptions based on samples of a type specimen which is released *and also* descriptions based on traditional specimens. In contrast, Dubois & Nemésio (2007) (as clarified by Nemésio 2009) would allow only one method - that of a full, dead specimen - to be acceptable as a general matter (subject to limited and time-consuming exceptions). I agree with Nemésio (2009) that the Code needs to work for the whole field of zoology - which includes some vertebrate genera where the morphology of type specimens can be documented well and samples can be taken without killing the study piece.

Dubois & Nemésio (2007) propose Code amendments that would be backdated to 1999, which amendments would state explicitly that certain descriptions based on live individuals or samples of live individuals are invalid. In Donegan (2008), I noted that this would retroactively affect existing descriptions, based on the point of view that *per* publications by members of the Commission secretariat (Wakeham-Dawson *et al.* 2002, Polaszek *et al.* 2005), such descriptions are currently allowed. Nemésio (2009) makes the good point that if the Code in its current form *does* require the deposition of dead type specimens, then Dubois & Nemésio (2007)'s proposed amendments would not be retroactive. He also notes, and I agree, that the taxa discussed in this exchange of papers based on live individuals are all relatively recently-described. As a result, if the Code currently requires a type specimen to be based on a dead organism, then nomenclatural instability resulting from backdating proposed amendments to 1999 would not be as destabilising as some other taxonomic changes. However, each change of a species or subspecies name brings with it a resulting potential loss of information and confusion, so I stand by the point that changing these names would cause instability in nomenclature which is undue. Nemésio (2009, p. 5)'s position that "*stability will not be threatened*" by changing names for descriptions based on samples may be better stated as being that any resultant nomenclatural instability would *not be undue*, given his view that type specimens based on dead organisms are important.

(b) Ethical issues

Nemésio (2009, p. 9) refers to a quote from Donegan (2008, p. 43) that "*some vertebrates apparently show parental care, suffer distress at the death of closely related individuals or partners, have complex communications systems and have nervous systems similar to our own, resulting in such organisms arguably having a stronger right to life than less complex organisms*" (emphasis added). Nemésio (2009) concludes that Donegan (2008) had "*defined who is right in the ethical debate*", and "*decided which live beings or organisms ... have a stronger right to life*" (p. 9). These statements are exaggerations and misinterpretations in light of the qualifications in the quoted text and surrounding paragraphs.

Nemésio (2009) goes on to state that, *per* Donegan (2008): "*trees would have no right to a life at all, since they are organisms with no parental care and nervous systems; as a consequence, deforestation would be morally justified*" and that "*all other animals have a weaker right to life*" (p. 9) - a further incorrect summary of Donegan (2008). As should be clear from the paper and some of the articles cited favourably therein (e.g. Loftin 1992, Agar 1995), experts in animal ethics consider that cutting down a tree or swatting a mosquito are in principle moral transgressions that should be justified by a greater good. Such authors consider that some organisms arguably have subjectively stronger rights to a life than others, for example as a result of factors at

the level of the individual (e.g. nervous systems) or population (e.g. threatened species). There are threatened tree species. There is aesthetic damage caused by felling a tree. The felling of trees ultimately will result in more carbon dioxide being released into the atmosphere and contributes to global warming. Some trees are located in places which raise health and safety issues for humans. There may be threatened mosquito species. Mosquitoes are human disease vectors. Mosquitoes cause itchiness when they bite humans and keep one awake at night. The foregoing are examples of considerations, none of them of themselves determinative, that one might take into account in deciding whether or not to kill a particular organism for a particular reason. It does not follow that "*it should be as wrong to kill a mosquito as to kill a parrot*" (Nemésio 2009, p. 9). Quite the contrary: it is more widely thought that all organisms have a right to life, but some warrant greater protection than others and therefore could be regarded as having a stronger right to a life. If neither this paper nor Donegan (2008) failed to mention some factor relevant to some species, that does not mean that non-mentioned species "*all ... have a weaker right to life*" or "*have no right to life at all*" as per Nemésio (2009)'s *reductio ad absurdum*. Where there are subjectively greater negative consequences of killing a particular organism, including in the context of collecting type specimens, the justification for doing so should be stronger, that is all. The taxonomist making the study should be the person that takes the decision. However, decisions should not be taken in a moral and ethical vacuum. I agree that the ethical considerations discussed in this paragraph and in Donegan (2008) are "*not science at all*" (Nemésio 2009, p. 10) but that does not mean that such considerations are irrelevant or cannot be discussed. Ethics courses are often part of undergraduate science courses and many institutions require researchers to act in accordance with ethical guidelines.

Nemésio (2009)'s foreword (p. 1) quotes Patterson's (2002) *reductio ad absurdum* on ethical issues, namely "*as metazoans, humans are heterotrophs that must consume other organisms as food before making any other declarations or prescriptions about the world around them*". In light of some of Nemésio (2009) misinterpretations of Donegan (2008), it is necessary explicitly not to disagree with the proposition that humans are metazoans or heterotrophs nor that we consume other organisms as food. However, Patterson's (2002) statement is a weak argument on which to base a stance that ethical issues are irrelevant to the issue of scientific collecting. Decisions relating to human nutrition are (or should) also be taken ethically. Many humans, if they thought about the issue, would consider that killing other organisms for food is justifiable because food keeps them alive. They would value their own life above, for example, that of a carrot. However, many humans would not agree with, at an extreme, eating critically endangered primates, when wheat or a cow would suffice. Some humans eat only plants and fungi (vegetarians) due to concerns over welfare of certain organisms. Some vegetarians eat animal products and others do not. Some persons do not eat mammals but would eat fish or chickens. All such viewpoints are criticised as inconsistent by fully carnivorous humans; fully carnivorous humans are criticised by vegetarian humans. The proposition that human nutrition raises no ethical issues is not the subject to this exchange of papers so I do not digress, lest I again be accused of "*[spending] most of [my] paper taking about 'ethics'*" (Nemésio 2009, p. 9).

Donegan (2008) noted that "*Taxonomists are often more comfortable discussing issues relating to dead specimens in a moral vacuum (or following reductio ad absurdum of moral concerns, which amounts to the same thing). The question of the utility of taking a specimen over use of other materials then has an obvious answer.*" Nemésio (2009) clearly took this advice to heart. However, as stated in Donegan (2008) "*A robust rationale considering ethical issues in favour of responsible and necessary scientific collecting of type specimens would be of value in convincing a sceptical public of its importance.*" (p. 42). Loftin (1992) and Collar (2000)'s guidelines would form the basis for an ethical approach to scientific collecting activities.

(c) *Minority or majority views*

Nemésio (2009) asserts that the points of view expressed in Donegan (2008) are "*shared by a few ornithologists and primatologists*" (p. 3). Following the publication of Gentile & Snell (2009), perhaps a few herpetologists may now be added to that list. Nemésio (2009) implies that alternative views, which may include some of Nemésio's and Dubois' views, are shared by all other zoologists. I recognise that my own viewpoints may not be mainstream among taxonomists. However, I am not aware of any opinion poll having

addressed this assertion nor is it self-evident that a method should be prohibited merely if it is practised by a minority.

The quality of descriptions vs. the availability of a name

Gentile & Snell (2009) (together with Tzika *et al.* 2008 and Gentile *et al.* 2009) carried out detailed morphological and molecular studies before concluding that the Galapagos Pink Land Iguana represents a new species. However, other recent descriptions based on live individuals have a chequered quality history, referred to briefly by Nemésio (2009, p. 9). The quality of these descriptions bears further discussion as it is relevant to how, as a policy matter, one ought to go about documenting and designating type specimens. In my view, there is a stronger policy argument that descriptions involving dead specimens may involve a greater degree of scientific rigour than some other descriptions, compared to a weaker policy argument that such descriptions are or should be invalid. The controversies affecting descriptions based on samples or photographs are noteworthy and numerous.

- *Cebus queirozi* is the name of a primate described by Mendes Pontes *et al.* (2006) based on photographs depicting the holotype. Nemésio (2009, p. 9) referred to Oliveira & Langguth (2006) who collected a specimen from close to the *C. queirozi* type locality and designated it as a neotype of *Simia flavia* Schreber, 1774 (now *Cebus flavia*), a primate whose original description was based on a drawing. Although characters differentiating *C. queirozi* from *S. flavia* were not discussed in the Mendes Pontes *et al.* (2006)'s description, Mendes Pontes (2009) subsequently claimed to have considered the issue at the time of the description and stated that the type specimen illustrated by Mendes Pontes *et al.* (2006) differed from Schreber's drawing in various respects. In an addendum, Oliveira & Langguth (2006) asserted that Mendes Pontes *et al.* (2006)'s description was invalid (not a junior synonym of *S. flavia*, although this would be another interpretation). They cited only references supporting the proposition that descriptions based on photographs of the holotype are not valid; and did not cite or discuss, for example, Wakeham-Dawson *et al.* (2002), Polaszek *et al.* (2005) or any viewpoints expressed therein. It is arguable that Mendes Pontes and colleagues should have considered other possible nomenclatural solutions for this population and advisably should also have designated a sample as the holotype. It would also appear arguable based on Mendes Pontes (2009) that Oliveira & Langguth (2006)'s neotype designation may not have complied fully with the requirements of Article 75.3.5 of the Code, which requires "evidence that the neotype is consistent with what is known of the former name-bearing type from the original description and from other sources". Not being an expert in mammal taxonomy, I would not wish to comment on this episode in detail but note that both the description of *C. queirozi* and neotypification of *S. flavia* are controversial.
- *Lophocebus kipunji* is a name for another primate described based on photographs depicting the holotype by Jones *et al.* (2005). This species was transferred to (and made type species of) a new monotypic genus *Rungwecebus* a year after its description (Davenport *et al.* 2006). An appropriate generic treatment for this species has been subject to much discussion (e.g. Olson *et al.* 2008), with a recent suggestion that *kipunji* and the baboon genus *Papio* are monophyletic (Zinner *et al.* 2009).
- *Laniarius liberatus* is a name for a bird (a shrike) described by Smith *et al.* (1991) based on blood and feather samples, molecular analysis and photography. Nguembock *et al.* (2008) recently considered *liberatus* to be a junior synonym of a polymorphic *L. erlangeri*. The proposition that *liberatus* may represent a valid subspecies, under certain subspecies concepts, of *L. erlangeri* was not considered in detail by Nguembock *et al.* (2008), who based synonymy on molecular data. Some questions as to the taxonomy of this group therefore perhaps remain open.

As discussed in Athreya (2006), *Liocichla bugunorum*, a bird species with a holotype of feather samples, could be considered a subspecies of *L. omeiensis*, depending on the species concept deployed. Further molecular and other studies are required to address this question definitively (Mishra & Datta 2007).

In light of the papers cited in this section, I retract the reference in Donegan (2008, p. 41) to Smith *et al.* (1991) and Mendes Pontes *et al.* (2006), among others, being "*generally regarded as having documented instances of previously undescribed taxa*". There may be a reasonable argument that certain of the descriptions mentioned above are subjectively not as strong as some other descriptions, including some based on dead specimens. There are often practical constraints on collating all the data that one might wish to have in connection with a description. Such constraints are not exclusive to sample-based descriptions: for example, *Atlapetes blancae* was based on a non-ideal data set where a series of dead specimens was available (Donegan 2007). On the other hand, it is not necessary to engage in a full study of generic relationships of a particular group in order to describe a taxon at species rank, so long as the most similar or closely-related species are plausibly identified and the diagnosis of the new species is sound (and a type specimen is designated, etc.). Finally, the risks of a discoverer of a new taxon being usurped by an unscrupulous opportunist are widely-known with several historical examples and concerns in this regard may bear on the timing of certain publications.

Perhaps as a result of some of the above factors, controversies relating to generic placements, exhumed old descriptions and taxonomic rank are not exclusive to sample-based or photograph-based descriptions, including among recent descriptions. Names described based on specimens in recent years have been considered synonyms of other names: for example, *Ficedula beijingnica*, a bird described by Zheng *et al.* (2000), is now considered to be based on a subadult specimen of *Ficedula (narcissina) elisae* Weigold, 1922 (Töpfer 2006); and Nemésio (2009, p. 9) cited the example of a parrot described in 2005 as apparently being of an already-described species. Some newly-described taxa have also been suggested as candidates for changes in taxonomic rank. For example, *Xenoperdix udzungwensis obscurata*, an African bird described by Fjeldså & Kiure (2003), was promoted to species rank just two years later (Bowiea & Fjeldså 2005). Generic changes also occur frequently, increasingly as a result of phylogenetic studies revealing novel relationships

When species are moved to a new genus or names are considered subjective synonyms of other names, it is important to have a good understanding of the characters of relevant type specimens. As a policy matter and from a purely nomenclatural perspective, the use of samples in descriptions has not been shown to raise issues due to possible disconnection between the type specimen and name for any of these descriptions. Such possible disconnections are among the main objections to photograph-based or sample-based descriptions raised by Dubois & Nemésio (2007) and Nemésio (2009) (e.g. the latter at p. 7 "*If there are no name-bearing specimens deposited in museums we will never be sure to which species each nomen belongs to and vice-versa*"). As far as I am aware, such assertions remain unsubstantiated by examples, despite the number of taxonomic events affecting recent sample-based and photograph-based descriptions as well as the considerable literature criticising descriptions of this nature.

On the other hand, it would not be unreasonable to conclude that certain persons who took what could be perceived as "short-cuts" in connection with the designation of a type specimen also took short-cuts in relation to other aspects of descriptions, such as literature reviews, studies of plumage variation of described taxa and studies of species or generic limits. It would not be unreasonable further to conclude that specimens based on dead organisms facilitate more detailed taxonomic research and greater scientific rigour. An alternative view would be that the use of a samples on the one hand and other issues relevant to a description on the other hand are distinct; and that it does not necessarily follow that a description based on a sample would have other shortcomings where the description is otherwise fully researched and documented. This would appear in principle to be a more interesting subject to debate than one based in nomenclature or over proposals to prohibit or restrict certain techniques.

Collecting endangered species and the Galapagos Pink Land Iguana

Nemésio (2009) stated that "Mr. Donegan not only determined that Dubois & Nemésio are wrong but also presented no "framework for the taking of decisions" (p. 9). This is a misinterpretation. Donegan (2008) stated that "Loftin (1992) elucidated 10 criteria for ethical and responsible scientific collecting from a utilitarian or consequentialist perspective" (p. 42) and noted that this was "a helpful starting point for discussions" (p. 42) and also referred to Collar (2000).

It is noteworthy that *Conolophus marthae* is proposed for IUCN critically endangered status, with an estimated maximum population of 200 (Gentile & Snell 2009). This is relevant to Collar (2000)'s recommendation that "All Critical species (except those qualifying only under Criterion A), and species that qualify as Endangered under Criterion D1, should be prohibited from collection at all times" and Loftin (1992) recommendation that "Endangered species should not be collected under any but the most extraordinary circumstances". Given that the morphology of the holotype and natural history of the new species were documented comprehensively without taking a dead specimen, it is also questionable whether such a procedure would have met any of Loftin (1992)'s first three requirements of necessity, importance or novelty.

Nemésio (2009) commented on the possibility of Gentile & Snell (2009)'s description eventually proceeding without a dead type specimen, stating that: "I still hold the opinion presented in Dubois & Nemésio (2007) that if the remaining population is larger than, let's say, twenty or thirty specimens, an old, probably non-reproductive male could be taken to be preserved as holotype." This is a constructive suggestion that should be considered in similar fact situations. Either way, it is important that threatened species like *C. marthae* are described because descriptions are often a gateway to conservation attention. In the event, Gentile & Snell (2009) opted to publish a description based on samples and photography. Either approach would set a good example to local communities and ensure that if some other event were to affect the population, such as a natural disaster, their research team could not be accused of having contributed to any reduction in its population.

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