The history of science and nomenclature debates: Case 3463 and the Aldabra tortoise

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[Note: this article was received too late for publication as a Comment on Case 3463 but was submitted to the Commissioners before the vote on that Case. It is published here for the record, although correspondence on the Case is now closed; given the general relevance of the observations and arguments to nomenclatural considerations, the Secretariat feels that this warrants consideration as a general article]

My involvement with the discussion about the name of the Aldabra tortoise began when I was asked to provide sources of detailed information about published works attributed to James Petiver, some of which were cited by J.E. Gray and Linnaeus. Later I also provided advice, from a historian’s point of view, about interpreting abbreviations and other details from 17th and 18th century publications that have been discussed in relation to the Aldabra tortoise (see Frazier & Matyot, 2010). While my experience with tortoises is limited, I am well aware of the critical role that scientific names play in the advance of science, and the central role that history plays in these considerations. I have, for example, analysed the development of pre-Linnaean taxonomic conventions in the late seventeenth century in my biography of the conchologist and arachnologist, Dr Martin Lister (1639–1712) (Roos 2011). With the help of colleagues, I also have made species identifications in the correspondence of Lister, particularly with the naturalist, John Ray, for a forthcoming edition (Roos, [2014–17] in prep.).

As a historian of science, my reasoning is based on close examination of the primary sources and a careful weighing of evidence. Suppositions are used only very sparingly, and then only when adequate primary evidence provides a foundation for making testable assumptions.

In that spirit, looking at several comments from March 2010 I note the following, which serve as examples of arguments that would be suspect to a historian of science:

1. Bour, Pritchard & Iverson (BZN 67: 73–77) state: ‘The Code must not be taken apart; it must be understood, accepted, and followed.’ This is a rhetorical technique called the ‘fallacy of the slippery slope’, or the assertion that some event must inevitably follow from another. It is sometimes called ‘the camel’s nose’: once a camel has managed to place its nose within a tent, the rest of the camel will inevitably follow. In this context, the statement by Bour, Pritchard & Iverson indicates that conservation of the name *Testudo gigantea* will mean the nomenclatural Code will be taken apart. So perhaps we will re-term this technique the ‘tortoise’s nose fallacy’.

2. The same authors state: ‘Finally, why should we reject the name *Testudo dussumieri*, which honours the memory of Jean-Jacques Dussumier, the first traveller who brought back an Aldabra tortoise with its precise locality and offered it to science? If one operates by the letter of the law (Code), as we have, and not by passion or emotion, it is clear that the first valid name for the Aldabra tortoise is *Testudo dussumieri*.’ This is an example of the appeal to emotion or *pathos*, which Aristotle
mentions in his work on rhetoric, as it appeals to our respect for the memory of Dussumier. In the same issue of the Bulletin, Dubois, Ohler and Brygoo made the point about these sorts of arguments being irrelevant.

Nonetheless, Bour, Pritchard, and Iverson’s statements do bring us to some more substantive questions.

1. Did Dussumier ever visit Aldabra, or collect any specimens from there? Thus far, the sources cannot verify that he did. Cheke 67: 79–81 rightly noted that the historical evidence does not support Dussumier’s visiting Aldabra, and he presented some interesting and well-considered suppositions that Dussumier could have obtained an Aldabra tortoise via merchant networks. Nonetheless, there is a complete absence of historical evidence that shows this definitely and, interesting as they are, his suppositions are suppositions, not evidence.

2. Another area that historians examine is the validity of primary source documents, and two different examples can illustrate this. The speculations about the origin of the lectotype of Testudo dussumieri seem particularly to centre upon the primary source evidence of Gray’s note and the old label on RMNH 3231. We also, in Cheke’s most recent (BZN 68: 294–297) communication, have reference to the work of Luis Ceríaco and his claim that taxidermy of the specimen regarded as the holotype of Testudo gigantea demonstrates that it was done in Portugal.

(a) Gray’s note: It seems that the most critical component of what Gray wrote about the new species description was ‘Schlegel MSS (v. Mus Leyd).’ I would interpret this as saying: ‘Schlegel manuscripts, (see Museum Leyden)’. Gray’s note could thus suggest a few things: First, it is quite probable Gray was referring to manuscripts by Schlegel. ‘v.’ indeed usually stands for ‘vide’, Latin in the imperative case ‘to see’. But, what ‘v. Mus Leyd’ means is very open to question. It could mean ‘see a particular specimen at the Museum’, it could mean ‘see a label on a specimen at the Museum’, or it could mean ‘see the Schlegel manuscripts at the Leyden museum’. We don’t know. In this respect, it worth noting that two former curators at the Leyden museum, Hoogmoed and Smeenk, gave slightly different interpretations of this same passage.

(b) The Label: Now we come to the old label associated with RMNH 3231: The pencil annotations on the label are different from the secretary hand, which was clearly the original script on this particular label. Pencil annotations were added later. What seems nearly impossible to know is when the annotations were made, who made them, when the information was entered in the register and why the specimen was identified as a different species, Testudo nigrata replacing the earlier Testudo elephantina.

Nonetheless, in his comment Hoogmoed (BZN 68: 72–77) notes (p. 74) that ‘Temminck & Schlegel (1834) made the published, printed statement about name, collector, locality and specimen on the basis of documentation (in whichever form) they had received from Paris with the specimen concerned. Hubrecht (1881) did the same, basing himself on the register and data on the label fixed to the bottle in which RMNH 3231 was (and still is) kept. In the RMNH it always has been good practice to trust the data provided with material, until the contrary is proven. In this case there was no reason for any doubt, and Gray (1831b) was of the same opinion.’
The problem is that there is reason for doubt about this label, as well as Hoogmoed’s statement that ‘it always has been good practice to trust the data provided with material’. First, supposing that something has always been done in a particular manner is not the same thing as knowing that for a fact. Hoogmoed even admits: ‘The collection of the RMNH was established in 1820. About the early history of its management we know little and it even is not quite certain when the present numbering system for reptiles and amphibians jointly was started.’ His admission thus makes his following statement a bit puzzling:

‘As to the labels and other paper concerning RMNH 3231 there have been some unfortunate statements and mistakes in transcribing handwritten texts. Grünwald (2009, p. 139, upper figure) showed an old label on the outside of the jar in which RMNH 3231 is kept and gave as a legend ‘Het oorspronkelijke label van RMNH 3231, geschreven door John Edward Gray zelf’ [The original label of RMNH 3231, written by John Edward Gray himself]. This statement led Frazier & Matyot (2010) to several wrong conclusions, even after Grünwald explained to them that his text should have included ‘possibly’. There is no reason at all for such a statement, because the RMNH never let (foreign) visitors write labels that were attached to bottles etc.’

If the RMNH does not precisely know the early history of the management of its collection, how would it be possible for researchers to know who was writing the labels and what was allowable procedure? It seems that the earliest procedures for documenting and cataloguing specimens at Leiden were not precisely known, because, at least from what Hoogmoed has written regarding the herpetological collections, no one has yet done the systematic, detailed historical research to find this out (which would be a valuable project indeed). Holthuis (1995), for example, did a fine overview of the history of the Leiden Museum from 1820 to 1958, reconstructing its institutional structure and identifying key personnel. If there were examples of the handwriting of the different officials at the museum in the relevant time period to identify who was entering what information, doing a paleographic analysis of the work of these key personnel would be the place to start to reconstruct these procedures. Paleographic analysis to reconstruct working practice is a common technique; telltale scripts by scribes or clerks can date material, as can marginalia. For example, I analysed the monograms of copperplate engravers, signatures and sketchbooks to reconstruct how Lister’s Historiae Conchyliorum was created and published (Roos, 2012).

Another point to consider is that a secretary hand usually indicates the script of a clerk, who routinely just copied what was put in front of him, without much understanding. Usually, the nicer the handwriting, the more lowly the writer. (This conclusion is based on hundreds of hours studying archival material in the Royal Society, London). It is entirely possible that the label in question was a clerk’s copy of an earlier label, which might explain the absence of diacritical marks in the French phrase: ‘Testudo elephantina Jav. Test. indica Ile Aldabra, pres de Madagascar / Dussumieri’. From Gray’s note and the label evidence, it would be quite dangerous to assume that Gray saw the original French label with Dussumier’s name on it. From a historical point of view, the primary source evidence to make such an assumption is just not there. In this regard, after having asserted that the old label was the original that accompanied the specimen from Paris, Hoogmoed later
admitted ‘Thus, there is a good chance that the old label on the bottle of RMNH 3231 is not the ‘original’ label as stated by Hoogmoed et al. (2010), and that it possibly stems from after 1835 as suggested by Frazier & Matyot (2010).’ This example emphasises the importance of paying close attention to identifying true primary sources.

The second example of the importance of paying close attention to the primary source deals with a detail of taxidermy. Cheke mentions a communication from Luis Ceríaco, who, it turns out, has written articles on oral tradition and Portuguese geckos as an independent scholar. Dr. Ceríaco thought the taxidermy of the purported type specimen of *Testudo gigantea* (MNHN 9554) was specifically Portuguese. In this context it should be noted that the French naturalist Pierre Belon wrote the earliest known instructions for taxidermic procedures in 1555. While in the eighteenth century, there were certainly specific regional trade secrets in taxidermy (for instance Jean-Baptiste Bécoeur’s use of arsenical soap to stop insect infestation of bird skins), by the nineteenth century, many of these secrets had been disseminated quite widely in manuals where they became standardized (Rookmaaker et al., 2006). Thus, from a historical point of view it would be helpful to know in some more detail what Dr Ceríaco’s basis is for detecting time-specific or distinctive regional variations in taxidermic practice.

[In the interim between my initial submission of these comments on 6 July 2012 and their publication, Dr. Ceríaco and Professor Bour published another paper with more details about the taxidermy of *Testudo gigantea*. (2012). Their abstract is as follows:]

‘The work *Prodromus Monographiae Cheloniorum*, published by Schweigger in 1812, has recently been the subject of several studies. One result of these studies—the rediscovery of the *Testudo gigantea* Schweigger, 1812 holotype—triggered an intense debate in the Bulletin of Zoological Nomenclature, where, among other issues in dispute, the identity and nature of the specimen indicated as the holotype for the species is put in question. Using historical sources, mostly unpublished, and analysis and comparison of taxidermic characteristics of the specimen with other specimens of the same nature, we can clearly trace its origin to the extinct Royal Cabinet of Natural History of Ajuda in Lisbon, from the ‘philosophical journey’ of Alexandre Rodrigues Ferreira to the specimens trans-ported to Paris by Geoffroy Saint-Hilaire in 1808, thus helping dispel any doubts regarding the identity and nature of what is being identified as the *Testudo gigantea* holotype, along with other chelonian specimens. This information is of great importance in the current taxonomic debate as well as in recognizing the historic importance of the Royal Cabinet of Natural History of Ajuda and Geoffroy Saint-Hilaire’s 1808 mission to Lisbon.’

The authors also conclude:

‘The doubts raised by Frazier (2006, 2009) and his supporters in comments on the Case 3463 (see Appendix) about the origin and nature of specimen 9554, the *Testudo gigantea* holotype as claimed by Bour (2006b), are definitely clarified with
the present historical and material data, and it is objectively proven that specimen 9554 originated from the Royal Cabinet of Natural History of Ajuda, as already inferred by Schweigger (1812).”

Let us examine these claims systematically. First, to my understanding, the status and veracity of the holotype is not central to the petition, the petition invoked Article 75.8 to set aside all previous type material. Thus, while I would agree that the authors’ archival research establishes the historic importance of the Royal Cabinet of Ajuda, particularly for the history of natural history, I would be far more cautious about the importance and relevance of their findings to Case 3463.

In their paper, Ceríaco and Bour claim that fibre analysis in the stuffing of specimens and the distinctive style of eyes in turtle specimens from the cabinet ‘prove’ that specimen 9554 originated from the Royal Cabinet of Natural History of Ajuda. The wooden eyes in the specimen are certainly distinctive empirically in Ajuda specimens. I may have been more convinced that the evidence was definitive by the application of relevant archaeological techniques to the type of paint utilised and the age of the wood; in studies of material culture in the history of science and conservation, archaeological analysis is employed as a matter of course in cases which need further clarification. Hesitation also extends to the fibre analysis. Fibre analysis extends far beyond the use of a binocular magnifier employed by Ceríaco and Bour. I would refer the authors to Appleyard & Wildman (1970), Bisbing (2002), Eyerin & Gaudette (2005) and Rowe (2010) for a discussion of the relevant techniques in forensic hair and fibre examinations.

It seems, looking at the written and material evidence regarding Case 3463 from a historical point of view, that there is inadequate evidence to do more than speculate on several critical points. Given that the lectotype of Testudo dussumieri has been proposed as the ‘name bearing type’ of the Aldabra tortoise, this seems to create a situation of unnecessary risk. Whilst I would not indulge in the ‘tortoise’s nose fallacy’ and claim that the use of Testudo dussumieri would cause the nomenclature of tortoises to fall apart, it seems its use will continue to promote a situation of unstable nomenclature and ongoing debate.

References


