ETHNOBIOLOGY LETTERS

Reviews

A Reply to Whitney's Review of *Why the Porcupine is Not a Bird*

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REPLYING TO Whitney *Ethnobiology Letters* 9, https://doi.org/10.14237/ebl.9.2.2018.1204 (2018)

I am grateful to Cory Whitney for reviewing my book *Why the Porcupine is not a Bird* (Forth 2016) and am naturally pleased that the review is on the whole positive. The reviewer, however, makes several statements that are erroneous or questionable and therefore require a response.

I begin with specific points registered early in the review. Whitney states that the Nage people live on the islands of Flores and Timor. In fact, they inhabit only the island of Flores. He also says I became interested in the Nage because of their "unique funerary practices and water buffalo sacrifices." While I have certainly written on both topics, neither was a subject that first attracted me to the Nage. Also, two of my papers the reviewer mentions in this connection concern neither topic, and one (Forth 1988) is actually about ritual speech on the neighboring island of Sumba. Several other publications Whitney cites in reference to my several research interests similarly do not match these interests, and whereas the reviewer states I have been conducting fieldwork in Indonesia for over 30 years, the figure at the time my 2016 book was published was over 40 years.

As regards matters of ethnographic substance, Whitney (2018:103) describes Nage "taxonomic systems for animals" as including "three sub-taxa (excluding invertebrates)," identified as "1) flying animals; 2) snakes; and 3) fish and other animals including humans and nonhuman animals." Contrary to what this implies, Nage do not classify "humans and nonhuman animals" in a single category with fish. In fact, as I make clear, Nage, like most folk zoologists, distinguish humans from all animals (the category *ana wa*). As I also demonstrate, "mammal" (excluding humans) must be understood as a largely covert folk taxon contrasting with other life-form taxa, which although not consistently named, Nage sometimes distinguish with the label *lako wawi*, "dogs [and] pigs" (Forth 2016:61–65, 141, 142, 146, 153–54).

In his final paragraph, Whitney (2018:104) identifies two respects in which the book might be seen as "com[ing] up short." One is that it is "regionally and intellectually very specific," which he immediately explains to mean a "comprehensive look at the folk classification of the Nage from a taxonomic perspective, a close and meticulous study about organizing information" (Whitney 2018:104). I would not necessarily argue with either of these characterizations, though I am not sure what is meant by "intellectually very specific"—other than the fact that the work takes a particular theoretical approach. The specification of a "taxonomic perspective" is not quite accurate, as the book concerns rather more than folk taxonomy, as the reviewer himself makes plain. But by the same token, it is unclear how this can be taken as a criticism. Certainly, earlier book-length ethnozoological studies have focused on single ethnolinguistic groups (see e.g., Hunn 1977 on the Tzeltal, Ellen 1993 on the Nuaulu, and Rea 1998, 2007 on the Northern Pimans), and to place Nage folk zoology in a broader perspective I cite comparative ethnographic evidence throughout. If regional specificity needs further defending, I would add that, apart from the aims of exploring original

material and treating human-animal relations comprehensively and systematically, the theoretical argument I wished to make—largely a critique of ontological relativism—could only be made by focusing on the Nage, especially as their folk zoology appears different in several crucial respects from the way knowledge of animals has been represented by other ethnographers dealing with similar small-scale societies. In anthropology generally, theoretical and methodological positions can often best be advanced or criticized through comprehensive and detailed studies of particular ethnographic cases—as for example, Durkheim (1915) famously did with Australian Aboriginal totemism in developing his theory of religion.

The second perceived shortcoming, identified as one that might concern "activist readers," is more diverse. Whitney begins by stating that the questions I pursue "did not originate with the Nage" but are my "own concerns." Again, this is partly correct, but also partly incorrect, insofar as I make clear that my interest in human-animal relations among Nage—and not just ones pertaining to taxonomy or classification more broadly conceived—originated in uses Nage themselves make of animals, conceptually, practically, linguistically, and in various ritual and symbolic respects. At the same time, my position—which I realize reflects my own values—is that any sort of scientific work or scholarship must be substantially shaped by the concerns of the disciplines involved (in this case anthropology and ethnobiology) and not solely by the interests of the community concerned. As if to qualify his criticism, Whitney then asserts that "critical readers should keep in mind that much of [my] work in the region took place during a time when any *political activity* would have risked being banned from the country [i.e., Indonesia], jailed, or worse" (emphasis supplied). If this claim refers to the Suharto era, which came to an end in 1998, I should point out that much ethnography pertaining to the topic of the book was conducted after that time. In the same connection, the reviewer states that since I began my fieldwork "the local ecology of Flores and Timor has dramatically changed," and that "Nage culture has nearly been lost," having been "assimilated by other more dominant regional ethnic groups." Certainly, there has been ecological change on Flores, as I discuss in several parts of the book (e.g., pp. 129–130 regarding monkeys; pp.181–183 on birds; and pp. 213–214, 222, 308–309 on fish), but the changes the reviewer likely refers to began earlier in the twentieth century. It is moreover questionable how far such changes concern the topics I chose to pursue in my book—as opposed to a very different book which "activists" might conceivably wish I had written. There is even a question of whether local ecological change is a topic Nage themselves would particularly wish I had written about. More likely foci of their interest would be the history of the Dutch-appointed native rulers of the Nage region, sacrificial ritual, or disputes over land (some going back well over 100 years)—all of which I have written about in previous publications.

As for the claim that Nage culture is now virtually lost and has been assimilated by "more dominant regional ethnic groups" (Whitney 2018:104), I am at a loss to know what this means or on what information it is based. If these "more dominant" groups are other ethno-linguistic populations of Flores, then the statement is simply wrong, the Nage being no more subordinate to culturally distinct neighbors than the latter are to them. Although before 2007 the Nage were conjoined with the Ngadha, their western neighbors, in an administrative district (or "regency") named "Ngada," since the 1980s not only have three leaders (or "regents") of Ngada been ethnic Nage, but together with the culturally similar Keo region to the south, Nage has now separated to form a separate administrative district named "Nagekeo." Having worked with several ethnic groups on Flores, I would add that the people of central Nage especially, are in several respects culturally more conservative than others I know, not least in regard to maintaining indigenous beliefs and practices disapproved by the Catholic Church.

One must always be grateful for the effort taken by colleagues who review our books, and again, I appreciate the service provided by the present reviewer. However, in the interests of research and scholarship, where challengeable statements are made they must be challenged.

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