

## Reply to Anderson, Ellen Hanazaki, Hunn, Rival, Si, Slater, Weiskopf

This article was motivated by the goals of developing a historical diagnosis and a positive perspective for debates about ethnobiological classification. First, I wanted to understand the relative isolation of ethnobiology from wider ontological and taxonomic debates in fields such as anthropology, biological systematics, and philosophy of science. Second, I wanted to outline a framework for fostering exchange between these academic fields with overlapping research interests: how can ontological and taxonomic debates become better informed by decades of careful ethnobiological research, and how can ethnobiologists become more engaged with the wider theoretical implications of their work? I am very grateful for the excellent comments, which contribute substantially to both goals of the article and provide a direct illustration of the benefits of building bridges between different intellectual traditions in ethnobiology.

### Historical Diagnosis

Three of the commentators—Anderson, Ellen, and Hunn— have played key roles in shaping the debate that is described in my historical narrative of the “rise and fall of convergence metaphysics.” While I appreciate Anderson’s and Ellen’s seals of approval, I am also grateful for Hunn’s more critical remarks and his objection that my “characterization of the ‘demise’ of this intellectual program overstates the case.” One of Hunn’s concerns relates to my description of recent developments as a “demise” rather than a “marginalization” of convergence metaphysics. And indeed, there is a clear case for marginalization in the late twentieth century as ethnobiologists encountered a much more general hostility toward cognitive approaches in anthropology that had little to do with the internal state of the program of convergence metaphysics.

Hunn is correct to point out the relevance of wider intellectual trends in anthropology that also affected related programs, such as kinship studies (see Wilson 2016), and a comprehensive historical account would have to say more about how these trends interacted with the development of ethnobiology. That being said, I think that Hunn may be overstating his case when putting the “subsequent postmodern turn” at the center of the “marginalization of cognitive ethnobiology [that] was driven by shifting intellectual fashion.” One reason is that conflicts about postmodernism were especially important in Hunn’s disciplinary (i.e., anthropology) and national (i.e., United States) context but were less formative for other areas of the increasingly global and multidisciplinary field of ethnobiology. For example, Brazilian ethnobiology is much more closely aligned with biology than with anthropology and largely employs methods, from “evolutionary ethnobiology” (Albuquerque, De Medeiros, and Casas 2015) to scientometrics (Campos et al. 2016; Hanazaki 2015), that are very distinct from Hunn’s characterization of postmodernist hermeneutics. Similar developments far from Hunn’s “subsequent postmodernist turn” could be pointed out in other disciplinary contexts, from DNA bar coding in ethnobotany and the cognitive science of folkbiology to the rise of TEK, applied ethnobiology, and the focus on preservation of biocultural diversity. The fading role of cognitive ethnobiology in these areas (and the complete neglect in areas such as biological systematics and philosophy of biology) cannot be blamed on postmodernists and poststructuralists alone. Instead, it also requires reflexivity about how cognitive ethnobiologists (with important exceptions, e.g., Zent and Maffie 2009) missed opportunities for making a case for the continued originality and relevance of their research in changing disciplinary landscapes.

In addition to “marginalization,” Hunn also brings up “exhaustion of the topic” as an additional factor and writes that one “wonders how many additional case studies highlighting the sophistication of local community-based natural histories are required to prove the point.” One may combine this remark with Ellen’s observation that adequate documentations and analyses of ethnobiological classification systems “consume enormous amounts of research time, are logistically complex, and difficult to fund.” Furthermore, Hanazaki also makes a related observation when suggesting that “researchers simply grew tired of the debates about utilitarianism and intellectualism, or about relativism and universalism.” The remarks by Hunn, Ellen, and Hanazaki complement each other in making the point that even productive research programs eventually reach an intellectual and institutional stage where it is time to move on. That being said, I am too optimistic about the continued relevance of research on ethnobiological classification to put “exhaustion” at

the center of the historical narrative. As I tried to show toward the end of the article, the topic itself is far from exhausted, and many relevant (applied and theoretical) implications remain insufficiently explored. Instead, I would argue that the appearance of exhaustion was, at least in part, a product of the insufficient recognition of the relevance of ethnobiological classification for a wide range of issues from theoretical debates in anthropological theory and biological systematics to applied issues of TEK and the preservation of biocultural diversity.

Taken together, the comments indicate that a more comprehensive historical story would require a multicausal account that includes factors from the article (e.g., the “missed opportunities”) but also further factors (e.g., “marginalization” and “exhaustion”) that have been emphasized by the commentators. A wider lesson from this conversation is that it is time for professional historians of science to turn their attention to ethnobiology. Indeed, there has been increased interest in the history of field, and my own account would not have been possible without previous historical reflections by the commentators (e.g., Anderson 2011; Ellen 2006; Hunn 2007). However, virtually all historical accounts have been written from the internal perspective of senior ethnobiologists who reflect on the development of their field. Our conversation about different factors in the development of ethnobiology illustrates the need (and potential) for historical research that makes more careful and extensive use of the methodological tools of history of science.

### Pluralism, Realism, Normativity

While the history of ethnobiology is a worthwhile topic in its own right, my historical diagnosis also set the stage for a discussion of potential directions of future debates. None of the commentators challenge the overall project of a “revamped metaphysics of ethnobiological classification,” but several comments raise concerns about core elements of the positive proposal. These concerns are important, because my proposal includes at least three elements that are far from trivial: pluralism, realism, and a normative orientation.

Slater and Weiskopf develop philosophical challenges of my proposal of “pluralist realism” that point in different directions. For Slater, the concern is mostly a moderation of pluralism to “prevent it from sliding into abject relativism.” Fortunately, Slater provides not only a challenge but also a positive proposal of how to address it. I wholeheartedly agree with his suggestion of looking for higher-level agreements regarding classificatory norms. Slater’s more extensive (2017) discussion of examples such as “avoidance of lonely categories” as classificatory norms resembles Berlin et al.’s (1973) classic “general principles” that postulate universal norms in folkbiological classification such as the use of groupings with varying degrees of inclusiveness, hierarchical arrangements of these groupings, their grounding in a small number of “unique beginners” at the most fundamental level, and so on. Given the influence and productivity of these principles, it may be fruitful to revisit the debate about their universality with Slater’s account of classificatory norms.

That being said, there are many reasons why norms of local and Western scientists will not always converge. Different classificatory interests may be the most obvious factor, but there are also other aspects, such as a preference for more frugal norms in communities without written language. Even in cases of norm divergence, however, Slater’s framework may still help to identify higher-order agreements about the appropriateness of specific norms in specific contexts. For example, I have argued elsewhere (Ludwig 2016b) that Western soil scientists can understand why Maya farmers classify soil types in ways that diverge from the World Reference Base for Soil Resources. Given the (epistemic and practical) goals of Maya farmers, it makes sense to employ different norms that lead to different distinctions. In this sense, a focus on higher-level norms can reveal important agreements: a Western soil scientist and a local farmer may use different classifications at the object level but still agree on the appropriateness of their respective norms in their respective domains of application.

In contrast to Slater, Weiskopf does not want to stabilize but rather question some of the realist commitments of my proposal. While I argued that a revamped metaphysics of ethnobiological classification can provide novel impulses for debates about the “ontological turn,” Weiskopf correctly points out that these debates often focus on deep metaphysical disagreements about animism and spirituality that “are strongly resistant to straightforward realist treatments.” My ethnobiological examples (e.g., alternative classifications of birds) seem well-suited for realist arguments: local communities may focus on different patterns and regularities, but that does not make them any less real than the ones that are singled out by Western

scientists. In contrast, proponents of the ontological turn tend to focus on cases of “radical alterity” (e.g., shamanistic transformations or the very distinction between culture and nature) in which realist interpretations are at least less straightforward. I agree with the core of Weiskopf’s observations (see also Ludwig 2016c:42–44). While there are good reasons to start “bottom up” with more restricted ethnobiological cases of ontological difference, it remains open how far the proposed strategy can be extended toward more radical differences that have captured the attention of many anthropologists. I am optimistic about extending pluralist realism toward some wider issues such as divergent attitudes toward the distinction between culture and nature, but I do not mean to suggest that every ontological commitment of every community can be interpreted realistically. However, these limitations of realist interpretations are not very surprising, because they also need to be acknowledged for Western scientific ontologies that include plenty of intentional simplifications, useful fictions, and false assumptions. No matter whether we look at indigenous or Western scientific ontologies, realist ambitions will often be best spelled out in terms of a “semirealism” (Chakravartty 2007) that is reflective about its own limitations.

While I agree with Weiskopf on the importance of exploring the limitations of realism, I disagree with his suggestion that the proposed realism amounts to a “taming” of the ontological turn. On the contrary, it is precisely the realist ambition that has made much of the literature on the ontological turn radical by claiming to be “engaging not just with a plurality of worldviews, but with a real multiplicity of worlds” (Carrithers et al. 2010:174). For example, Kohn’s *How Forests Think* would have hardly surprised anyone with the claim that the Runa of Ecuador’s Upper Amazon are ontologically committed to thinking forests. What has made Kohn’s book a much-discussed example of the ontological turn is the additional realist claim that the Runa are right: forests do actually think, and “anthropology can make general claims about the way the world is” (Kohn 2013:10). Despite some recent backtracking about the realist ambitions of ontological anthropology (see Holbraad and Pedersen 2017), my realist interpretation of ethnobiological ontologies was, at least in part, an attempt to make sense of these more radical claims rather than a case of taming. Furthermore, I consider it one of the strengths of the proposed framework that it provides resources for spelling out this realism (see Ludwig 2018, for more details) rather than relying on often cryptic remarks about “worlds not worldviews” that have generated much understandable frustration among anthropologists.

Although realism about diverging ontologies constitutes an intriguing theoretical topic, it also needs to be related to the politics of indigenous knowledge and self-determination. Many indigenous scholars and activists (e.g., Wildcat 2010) embrace realism as a strategy for challenging marginalization in the negotiation of issues such as conservation practices, food security, and responses to climate change. The article aims to be at least compatible with such an unapologetically normative stance that emphasizes the epistemic and ontological resources of indigenous and local communities to address their political positioning and to challenge epistemic injustices.

As Rival correctly observes, however, anthropologists will often be uncomfortable with this blend of epistemology, ontology, and politics, because it involves “normative pronouncements . . . that anthropology has always firmly resisted, or at least approached with great caution.” Indeed, this caution is a core lesson of anthropological reflection on its own colonial legacy, and I acknowledge that it can create tension with the normative orientation of my article. It is tempting to evade Rival’s challenge by pointing out that I am not an anthropologist and that the overtly normative aspects of my proposal should be located in different contexts, such as political ecology. The problem with this response is that it presupposes a neat disciplinary division of labor that has little to do with the reality of ethnobiology and other debates about the status of indigenous and local knowledge. Instead, ethnobiology has become a meeting ground for empirical research projects in anthropology (as well as biology, linguistics, psychology, and sociology) and normative issues of multistakeholder governance of biocultural diversity. If anthropologists want to be part of these debates, they have to engage with the normative and political dimensions of their work. This does not mean that they have to give up caution regarding normative pronouncements. However, caution should not translate into an unattainable ideal of nonnormative (and therefore value-free) research, but ethnobiologists (including ethnobiologically interested anthropologists) should be careful in making their normative commitments explicit and in reflecting about the relation between normative commitments and empirical data.

Although the comments provide valuable challenges of my historical and philosophical claims, I am especially grateful for the many original suggestions that expand engagement with ethnobiological classification beyond my discussion in the article. By developing several aspects that I had neglected or entirely missed, the commentators illustrate the potential of ethnobiological research to provide innovative impulses for multidisciplinary debates.

Si emphasizes the importance of field linguistics in ethnobiology and suggests to expand my list of missed opportunities with “three further items—context, variation, and diachronic change.” I wholeheartedly agree with Si, and his own work (Si 2016) constitutes an impressive illustration of how linguistic research can connect ethnobiological classification with wider concerns about TEK. Aside from applauding Si’s case for greater attention to linguistic research, however, his comments also nicely illustrate some of the difficulties in integrating concerns from very different disciplinary constellations in ethnobiology. For example, Si remarks that he “was amused to note the author’s use of the phrases ‘merely describ(e)/ing’ or ‘mere description’.” I was trying to make the point that engagement with TEK inevitably becomes normative because it cannot be separated from practical questions about cocreation and comanagement of biocultural diversity. Although I still think that ethnobiology is not “merely descriptive” in this sense, it is not surprising that Si finds this overtly normative formulation “amusing” from his own disciplinary perspective. One of the challenges for a multidisciplinary engagement with ethnobiology is to navigate these differences in methodology, priority, and vocabulary.

Hanazaki provides a different but equally valuable suggestion for broadening the discussion of the article. While I focussed on ethnobiological classification, she correctly emphasizes that “ethnobiology goes beyond ethnotaxonomy, just as biology goes beyond taxonomy (or systematics).” I did not mean to suggest that taxonomy is the most important (or even the only worthwhile) area of ethnobiology, and I entirely agree with Hanazaki on the importance of broadening research agendas. The growth of ethnoecology and debates about TEK illustrate the importance of engagement with practices beyond taxonomy, and it would indeed be deeply misleading to assume “that ethnobiology is almost equivalent to ethnotaxonomy.” However, my aim in the article was not to claim the priority of taxonomy over other issues in ethnobiology. On the contrary, at least part of the motivation was to address how taxonomic questions can be better integrated with wider issues of local practices that have rightly become more prominent in recent decades (see “Bridging Applied and Theoretical Ethnobiology”). Rival provides a third suggestion for expanding the focus of the article by emphasizing “Atran’s and Medin’s highly original program” at the intersection of ethnobiology and the cognitive sciences. Although Atran and Medin’s work has been very influential in the cognitive sciences, it may indeed be time for a reconsideration of the “cognitive factor” (to invert Hunn’s phrase) within ethnobiology as there is a noticeable lack of engagement with the current state of the cognitive sciences in publications such as the *Journal of Ethnobiology*, *Journal of Ethnobiology and Ethnomedicine*, or *Economic Botany*. Both the historical and the constructive part of my article could have been expanded with a more detailed discussion of this situation. First, the relative neglect of the cognitive sciences in recent ethnobiology clearly has to be placed in the wider history of mutual marginalization of cultural anthropology and cognitive sciences. While cultural anthropology grew increasingly hostile toward cognitive research methods, the cognitive sciences became increasingly consolidated around experimental psychology with rapidly vanishing interest in anthropology as one of the founding disciplines of the “cognitive revolution” (see Beller, Bender, and Medin 2012). Second, a better historical understanding may also contribute to changing dynamics in ethnobiology and a better integration of the cognitive sciences as an integral part of ethnobiological research.

Although I fully agree with Rival’s praise for Atran and Medin’s research program, I find her contrast with STS less helpful. For example, social studies of ethnobiology and related fields have made important contributions to the understanding of interactions between indigenous and Western scientists and of multistakeholder dynamics more generally. While Rival writes that she is “not sure that STS has much to offer to ethnobiology,” it seems to me that social studies of ethnobiology provide indispensable resources for engaging with issues such as comanagement of local environments, participatory action research, and knowledge rights of local communities. Of course, Rival would be correct to respond that not every fashion in STS has done ethnobiology (and anthropology more generally) favors. However, one of the main challenges in moving forward is to bring cognitive perspectives on folkbiological reasoning in fruitful exchange with social science perspectives on ethnobiological and ethnoecological practices. Given the recent history of mutual marginalization of cognitive and social science perspectives, it may be more helpful

to ask what fruitful perspectives STS can provide and to acknowledge that STS has itself grown into a highly heterogeneous field ranging from poststructuralist theory to scientometrics.

To sum up, the comments provide diverse and original opportunities for broadening the scope of the article through further disciplinary contexts from linguistics and cognitive science to ecology and STS. Taking this diversity of relevant disciplines seriously creates an integrative challenge but also creates opportunities for novel research directions in ethnobiology.