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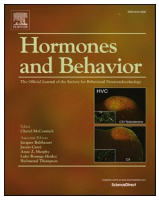

Editorial

Why biodiversity matters in the lab

Many in the field of behavioral neuroendocrinology, including those most likely to read the articles in this special issue, have appreciated the study of diverse organisms since or even before the value of doing so was articulated in Frank Beach's, 1950 article, *The Snark was a Boojum*. In that article, Beach argued that it is critical to study the neuroendocrine mechanisms regulating behavior in a diversity of animals, not just the "model organism"[1] of the time – the lab rat – lest studies of those mechanisms no longer apply to animals in the real world, much less generalize to any other species. He therefore warned that continued focus on lab rats and a small handful of other species would risk making the study of behavior, and the neuroendocrine mechanisms that influence it, obsolete.

Beach's arguments have since been echoed in several articles by authors in the field of behavioral neuroendocrinology using the same poetic metaphor, from *Is the Snark still a Boojum*?…, by Elizabeth Adkins-Regan (1990), *Beating the Boojum*…, by Steve Phelps (2010), and most recently *An updated field guide for Snark hunting…in the era of model organisms*…, by one of the guest editors of this special issue (Thompson, 2020). Like Beach's original, these articles have all argued that studies in diverse organisms are necessary to identify the fundamental neuroendocrine and genetic mechanisms regulating behavior, as well as to understand how biological diversity can be created through the evolution of those mechanisms, and all articles described exemplars of such work. Further, all warned that a narrow focus on relatively few organisms, no matter how tractable in the lab or how powerful the molecular tools available to study them, makes it impossible to assess the generalizability of findings that come from these "model organisms" or to challenge dogma established exclusively through work in them. Such a narrow focus thus limits our understanding of the world beyond the lab and makes it difficult to translate anything we learn from such studies to other animals, including humans. For while many of us would argue that the primary goal of our work is simply to understand how hormones affect behavior, we suppose that all of us have wondered at some point how our research in other organisms relates to our own species, and the government funding agencies that distribute the most taxpayer dollars certainly care about that question.

However, we do not think that we will be bursting anybody's bubble by saying that it is unlikely many administrators at those funding agencies will read many of the articles in this special issue. And even if they do, it is unlikely that they will immediately begin urging their grant panels to support work in non-traditional organisms more than they do now (which is, as we know from conversations with many of you within the field, not very much). But wouldn't it be nice if some administrators did read articles in this issue and found something exciting that prompted them to urge their panels to at least stop criticizing proposals simply because they are not working in standard "model organisms," provided appropriate methodological tools have been or are being generated in the "new" species? Because grant proposals *are* currently rejected for this reason, as we are sure many of you are aware. Indeed, one of us knows a scientist whose contact at one governmental agency worried that panelists might question whether their research in a non-traditional laboratory animal would generalize to rats. Unfortunately, while our community was reading Snark articles sounding alarms about the dangers of channelizing more and more work into relatively few "model organisms," we missed the memo circulated in some circles that the goal of neuroendocrinology, and perhaps neuroscience more generally, is to understand domesticated rats. Beach himself anticipated this myopic perspective over 70 years ago, asking "are we building a general science of behavior or merely a science of rat learning?"

When another scientist recently asked an agency contact about work in a non-traditional species, they were told that the agency was not running a zoo. Apparently, this agency contact either did not give much credence to August Krogh's principle that "for a large number of problems there will be some animal of choice, or a few such animals, on which it can be most conveniently studied," (Krogh, 1929), or thought that laboratory rats, genetically inbred mice, zebrafish, fruit flies and nematodes - arguably the big 5 "model organisms" - happened to be the most convenient and ideal animals to understand nearly every problem in neuroendocrinology and behavior.

However, most of us can quickly think of problems that cannot be adequately addressed in these "model organisms", like how neuroendocrine mechanisms work in natural contexts (what is a natural context for a highly inbred lab mouse?). One of us studies stress and anxiety, and often feels frustrated by how much stress research is done in domesticated species where artificial selection has effectively eliminated all but the most stress-tolerant individuals capable of thriving in a lab environment. It is often even unclear whether results from model organisms can be generalized to other model organisms, or even to other genetic strains of the same organism. Indeed, if we cannot answer those questions, then it is possible that much of the work done in genetic models will not translate to other animal species, including humans, and the frustration voiced by former NIMH director Tom Insel that scientific research has not alleviated as much human psychological suffering as hoped for will continue (Barry, 2022). Although Insel did not attribute this shortfall to a dearth of comparative research, we believe that NIMH's selective funding of research programs only using standard laboratory "model organisms" does contribute to our lack of progress moving from "bench to bedside." For such translation is difficult if the animal work lacks ecological "real world" validity or has not considered evolutionary relationships and pressures that help predict whether and

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when we should expect similar mechanisms to operate humans. And if we cannot predict when, why, or how neuroendocrine mechanisms are likely to produce normative patterns of human behavior, how can we possibly hope to fix dysfunctions in those mechanisms when they arise?

We, your guest editors, do not know how difficult it was "back in the day" to obtain funding for neuroendocrine work in prairie voles, as none of us have worked with this species, but we presume there were challenges like those described above for work in any non-traditional organisms. Yet the vole story – from being a "new species on the block" to what could be argued is now a traditional lab model - highlights what can happen when scientists and funding agencies recognize the utility of a species that had not been traditionally studied in the lab to reveal the neuroendocrine regulation of specific behaviors – in the case of voles, pair bonding and paternal responsiveness. That insight was grounded in field studies showing that, unlike laboratory rodents and most other mammals, prairie voles form lasting pair bonds and exhibit biparental care. One could argue the early work on the neurochemical mechanisms that facilitate those behaviors sparked what might now be considered an entire, and well-funded, subfield on social peptides, and there have been some indications that this research may ultimately help us develop strategies to alleviate some of the suffering associated with PTSD, autism, and schizophrenia. For even if effect sizes are small, as Insel lamented, one could argue that every little bit helps. From a purely scientific standpoint, prairie voles are still being used in a neuroethological and comparative framework to generate basic knowledge about how neuropeptides like vasopressin acquired some of their social functions. For example, in this special issue, Steve Phelps describes how selective pressures have influenced novel regulatory elements in the V1a gene that are associated with the evolution of pair-bonding tendencies in these animals.

So, while it may still be an uphill battle to obtain funding for work in many non-traditional organisms, this special issue celebrates the excellent science currently being done in such organisms, both to emphasize its importance and to illustrate that success stories are possible. Perhaps this may, even if to a small degree, lessen biases that some scientists and administrators have against such work. Hence this special issue is not simply another Snark article that briefly highlights exemplary research in non-traditional organisms, but rather an entire issue that serves as an in-depth showcase for the scientists working with those organisms to describe the work themselves. We have asked each of them to emphasize the unique and useful "Krogh-like" attributes of the organisms they work with, and which thereby allow us to gain new insights that might not be possible in traditional model organisms, or to highlight how their work has demonstrated fundamental principles of neuroendocrine regulation, challenged dogma, or discovered something new and perhaps unexpected. Work in over a dozen "non model" species will be described in these articles, although we regret that we were unable to procure any submissions describing work in invertebrates (perhaps this could be the focus of a future special issue, for a motivated invertebrate neuroendocrinologist guest editor?). Further, the articles themselves are diverse in type, from reviews that cover a career's worth of research in a particular species to summaries of studies within a relatively understudied taxonomic group to hybrid articles that integrate new data into reviews, and finally to "pure data" articles in non-traditional organisms. We hope that you find something new and exciting in at least one of these contributions!

Footnote:

1. One of the authors, Thompson, recently referred to "scare quotes" he put around the term model organism instead of "air quotes" to make a point, he hoped, in a slightly humorous way (An updated field guide for snark hunting…, 2020). We use the same "scare quotes" here to again highlight concerns we have with the term "model organism," concerns like those articulated by Paul Katz (2016). No other animals are simple models of humans that easily scale up; they have unique evolutionary histories and thus adaptations that may not be conserved or present in humans. On the other hand, some organisms may have evolved abilities to exhibit specific, often robust forms of behaviors we might be interested in as humans, and can thus be used, within a neuroethological framework, as model systems to understand how neuroendocrine systems regulate those behaviors.

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