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Abandoning animal personality would cause obfuscation: a comment on Beekman and Jordan

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Beekman and Jordan (2017) claim that animal personality research offers few insights and distracts from more important research. Some of their objections are impossible to satisfy (because any studies deemed to have merit should not be called animal personality) and others are applicable to any area (a mix of insightful and more run of the mill work; some studies contradict one another). Their key points indicate a disparity between what they understand by the term “animal personality” and what many authors (certainly myself) using the approach think the term means. Below I outline some characteristics of animal personality studies.

In discussing dominance status changes in African cichlids, Beekman and Jordan (2017) state: “[an] understanding of the mechanisms underlying this switch seem[s] to preclude the need to refer to these behavioral states as personality.” Indeed, personality is not a behavioral state that an animal could be in. Rather, the presence of animal personality is a property of populations, where a significant proportion of variation in the data is due to differences between individuals. Here “data” must refer to longitudinal data where each individual has been observed more than once. Without longitudinal data, we can test for differences in average behavior between categories of individual but it is not possible to analyze variation between individuals. One thing we can do with longitudinal data is express variation between individuals as a proportion of the total of variation between and within individuals (i.e. repeatability can be calculated) but each variance component can also be investigated in its own right. So as well as thinking of animal personality as a biological property of populations, we can also think of it more broadly as a framework that utilizes longitudinal data for investigating behavioral variation. If early studies tended to document repeatability, the scope soon expanded to encompass the partitioning of variation across a wider range of levels. This includes the proportion of variance that could be explained by plastic responses to changes in situation (using manipulative experiments) and the interaction between individual and situation (Dingemanse et al. 2010). More recently, it has been realized that within-individual variance can be interesting in its own right (Stamps et al. 2012). Therefore, we can use the term animal personality in 2 ways: first in a biological sense to denote the presence of behavioral repeatability and second as a descriptor for studies that aim to partition variation in behavior between and within individuals. Significant repeatability is something that might be uncovered but it does not have to be present for a study to be described as animal personality research in this sense. I actually see animal personality as a framework (Sih et al. 2004) that helps me to ask questions about “why animals differ in the way they behave.” Neither sense (biological or methodological) implies that a “higher psychological process” is at work. On the other hand, studies often aim to understand the ultimate and proximate causes

of behavioral differences by comparing variance components across situations or physiological states. While the subject has developed (which would usually be seen as a positive) these aims still correspond with the ideas (Dall et al. 2004; Sih et al. 2004) that sparked so much interest in animal personality.

The authors object to other terms as well. For instance, they describe “boldness” as a secondary phrase that simply makes research more accessible to nonspecialist audiences. I agree that secondary phrases would be problematic without clear definitions of specific behaviors but I am unaware of any study where behavior has simply been described as boldness and left at that. The term is usually used to link a well-described behavior (e.g. latency to emerge from a shelter), or suite of behaviors, into a larger domain of analogous behaviors associated with risk, which may differ between species. The use of secondary terms is actually widespread and using “boldness” is not much different from saying “aggression,” “courtship,” or “social behavior.”

As the authors point out, “we need to understand why animals differ in the way they behave.” Without collecting longitudinal data, it is difficult to know how important those between-individual differences really are and I find it unsurprising that many behavioral ecologists, as well as other ethologists who also test hypotheses (Tinbergen 1963), see the value in this approach. In essence, the article adds to the corpus of opinion that deals with terminology around animal personality. Regardless of what we call it, I argue that we can define a framework that utilizes longitudinal data for the analysis of between- and within-individual variation in behavior. Why abandon a term that has become, by and large, an acceptable descriptor for such work? In my opinion, doing this would risk the type of obfuscation that the Beekman and Jordan (2017) rightly want to avoid.

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