

## **A Response to Reviewer Comments Regarding the Manuscript, “Rule-Governed Behavior and Self-Control in Children with ADHD: A Theoretical Interpretation”**

*Leasha M. Barry and Melissa A. Kelly*

We appreciate the opportunity to respond to the issues raised in a review of the manuscript, “Rule-Governed Behavior and Self-Control in Children with ADHD: A Theoretical Interpretation” submitted by Leasha M. Barry and Melissa A. Kelly (this issue). Many of the points brought to light in the review are indeed useful for discussion and provide additional research directions for further model development and testing. Solely Leasha M. Barry authored portions of this response and this will be indicated as such by the use of the pronoun “I”. Alternately, the final paragraph in this response was a joint effort and reflects the thoughts of both myself and Melissa A. Kelly and is indicated as such by the use of the pronoun “we.” I will address the reviewer’s comments in roughly the same order as they were raised for simplicity.

The fruitfulness of attempts to interpret applied research findings in light of theoretical perspectives is identified as a major issue in the review. I will say that this exercise was challenging in light of the theoretical perspectives to date regarding ADHD and the lack of identification of Rule-Governed Behavior (RGB) within them. I must assume that this lack of connection between applied research and theoretical (basic) research was the reason that the editors of JEIBI invited me to write the manuscript on this topic specifically. I see no need to defend the parameters of such an invited manuscript further. As to the fruitfulness of the endeavor, or lack there-of, it is my opinion that the development of theoretical models, professional discussion of theoretical models, and the pursuit of both basic and applied research to test these models can be highly fruitful indeed. This is the tradition of experimental research and the progression of knowledge in any scientific field. In the area of ADHD, there is a clear need for such discussion, model development, and testing as we continue to have difficulty treating the disorder and/or symptoms.

I agree that a clear operational definition of RGB is necessary prior to embarking on a discussion of the relationship between RGB and theoretical perspectives of ADHD. My attempt at a definition of RGB was to lean on the work of Mather and Goldstein (2001), “Rule-governed behavior is behavior that is controlled by verbally mediated rules that describe the contingency between a behavior and consequence without the actual behavior or consequence necessarily being present” (paragraph 3 of the manuscript). The reviewer also mentions this work as the closest to date at creating a clear definition. The reviewer’s agreement in terms of the most useful current definition available leaves me unclear as to why we are debating it here.

This definition of rules and RGB, included in the manuscript, leads me to question the reviewer’s repeated argument that rules and RGB must be distinguished from contingencies and contingency shaped behavior. By definition (Mather & Goldstein, 2001), rules are verbally mediated statements that describe a contingency relationship between a behavior and consequence, with or without the actual stimuli being present. In fact, the reviewer later defines rules as contingency statements. If we can agree that rules are in fact synonymous with contingency statements (albeit a specific category of contingency statements in that rules are verbally mediated and specific stimuli need not be present), then many of the repeated issues raised by the reviewer proclaiming a need to distinguish between these ideas can be put to rest. I believe this agreement is possible as the reviewer later acknowledges that some researchers question the distinction between RGB and contingency-shaped behavior. Further the reviewer alternately refers to rules as contingencies when not arguing that the two must be distinguished.

I agree that RGB cannot be implicated in diagnosis of ADHD just because children with ADHD have language, are unresponsive to long term rewards, and fail to conform to expectations. Clearly, making the case for diagnosis based on these three criteria would be irresponsible. Asserting that RGB be included as diagnostic criteria for ADHD was not the intention of the authors of the manuscript. A lack of RGB is documented in the literature as a common symptom of ADHD, however.

The reviewer states in more than one instance that children with ADHD “have language.” Because children with ADHD do in fact “have language” more so than children diagnosed with other neurobehavioral disorders, their language development has gone largely uninvestigated in the field. Current models propose that altered language development might be an important developmental aspect to ADHD symptom progression over the life span and within family systems (Camarata & Gibson, 1999; Sagvolden, Johansen, Aase, & Russell, 2005). Altered language development is an area for further investigation in regard to verbally mediate contingencies or rules and ADHD.

As pointed out by the reviewer, it is very interesting to consider ADHD and RGB from a behavioral analytic perspective (or bottom-up approach in general). The reviewer contributes additional resources for reviewing this line of thought specifically, and I applaud the efforts in illuminating additional behavior analytic work in the area of ADHD. As I pointed out in the manuscript, some of the most useful aspects (in my opinion) of the reviewed theories were those discussions of antecedent–behavior–consequence interaction taking into account environmental and social variables in context, hence my heavy leaning on the work of Sagvolden et al (2005). I do take issue with the assumption of the reviewer that specific researchers were excluded from the manuscript because of their “bottom-up” perspectives. This assumption is totally unfounded. The theoretical perspectives reviewed included top-down, bottom-up, and combinations of both perspectives. Once again, I am unclear how the reviewer could possibly ascertain the idea that bottom-up thinking was specifically excluded from the manuscript. Most of the last third of the manuscript deals entirely with bottom-up ideas specifically.

Further, I do not agree with the reviewer that to make a case for the investigation of Verbal Behavior (VB), one needs to discount other approaches. In fact, the “other” approaches the reviewer mentions: contingency shaping, modeling, stimulus/control, and discrimination training are methods that are addressed in the manuscript as potential means of intervening, while contingencies are addressed specifically in the definition of RGB offered in the manuscript. I do not see these interventions/concepts at odds with RGB or VB.

The reviewer goes on to explore an interesting and, in my opinion, affirming review of rules as contingency statements or possibly setting events that potentially alter later behavior. Again, I applaud this direction of thought in the behavior analytic field and find it most interesting. The reviewer makes many worthwhile points concerning more specific aspects of the environmental variables affecting behavior including setting events and function-altering stimuli, simple verbal behavior and rules, and true self-monitoring/management scenarios in comparison to scenarios with potential demand characteristics. Each of these areas offers future testable research questions that can help contribute to the body of knowledge currently available in the field.

Finally, I take issue with the statements made regarding explanatory fiction. I view these statements as related to the primary issue raised by the reviewer in that the reviewer sees little value in the pursuit of theoretical interpretation or relation of such theory to models and applied practice. I find it difficult to believe that anyone reading the manuscript thoroughly would walk away with the idea that discussions of possible connections between observed behaviors and theoretical perspectives included in the manuscript are to be taken as ultimate truths. Throughout the manuscript these ideas are presented for the purpose of discussion and then proposed as future research questions to be tested.

We understand a reluctance to jump on the bandwagon for anything touted as a revolutionary, research-based panacea. However, condemning or minimizing the value of theory for improving practice suggests that practice continually improves itself in spite of theory development and hypothesis testing. When evidence is found that a theory needs to be revised or discarded altogether, the development and application of the theory does not imply failure. If learning and refinement occurs, there is value for practice, because it puts us a little closer to understanding the phenomenon we are trying to describe. If we focus only on outcomes and not on process, we may have a view of theory that is somewhat shortsighted. By grounding practice in current thinking (i.e., theory), we can achieve a greater understanding of factors that influence and improve practice, and we position ourselves to refine thinking and follow a path of continuous improvement. We do not disagree that the path from theory to utility is often disjointed or even nonexistent, but it is certainly worthwhile to attempt it. Again, we are grateful for this opportunity to respond and hope that the discussion fosters continued investigation.

### References

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