



PROJECT MUSE®

---

Experiential Avoidance and Superstition: Considering  
Concepts in Context

Roger Vilardaga, Steven C. Hayes

Philosophy, Psychiatry, & Psychology, Volume 15, Number 3, September  
2008, pp. 269-271 (Article)

Published by Johns Hopkins University Press

DOI: <https://doi.org/10.1353/ppp.0.0188>



➔ *For additional information about this article*

<https://muse.jhu.edu/article/262335>

EXPERIENTIAL  
AVOIDANCE AND  
SUPERSTITION:  
*Considering Concepts  
in Context*

ROGER VILARDAGA AND  
STEVEN C. HAYES



KEYWORDS: acceptance, contextualism, influence, therapy

THE TARGET ARTICLE (García-Montes et al. 2008) explores the application of the concept of superstition, examined from a Sartrian perspective, to psychopathology such as obsessive-compulsive disorder and psychosis. They compare their analysis to two different technical terms taken from current research programs in psychology, which are the notions of Thought-Action Fusion and Experiential Avoidance (EA). We have been asked to respond because our work in Acceptance and Commitment Therapy (ACT; Hayes, Strosahl, and Wilson 1999) and Relational Frame Theory (RFT; Hayes, Barnes-Holmes, and Roche 2001) are part of the foundational work in EA, and thus in this short commentary we comment on the article from the point of view of our research program.

The article notes several areas of overlap between the ACT/RFT work on EA and their Sartrian perspective on superstition, such as the holistic and contextualistic nature of the analysis, but also several differences which the authors feel advantage their own view.

It is difficult to compare the two lines of work without appreciating their context and purpose. Sartre was not a scientific psychologist; ACT/RFT are part of a specific effort at scientific system building, which we term here “contextual behavioral science” (AKA, post-Skinnerian behavior analysis).

Contextual behavioral science is a social enterprise that has as its purpose the development of increasingly organized statements of relations among events that allow actions embedded in their historical and situational context to be predicted and influenced with precision, scope, and depth. “Precision” means that a limited set of analytic constructs apply to any one event; “scope” means that a limited set of constructs are needed to analyze a broad range of events; and “depth” means that constructs should cohere across different levels of analysis.

Although a real world (or at least the one world) is assumed, the partitioning of the world is thought to be the result of our continuous interactions in and with it. These partitions are not ontological constructions from the perspective of contextual behavioral science because they, too, are actions that are purposive and embedded in their historical and situational contexts. The

deliberate attempt to analyze the world scientifically does not contradict the holistic standpoint of contextualism. When analytic goals are accomplished, constructs are “true,” but multiple truths are possible given multiple goals and historical and situational contexts. Conversely, all forms of contextualism become dogmatic and self-contradictory if their goals are not stated. It is common for contextualists of all varieties to state and defend the ultimate apprehension of reality as a whole, for example, without realizing that *any* statement about the world including such a defense is a form of breaking down this whole into parts. As we will try to show, the target article seems to fall into this usual contextualistic trap by arguing for what is better, without stating clearly “better for what, assessed how” and without viewing concepts with relevance to their purpose.

Prediction and influence with precision, scope, and depth is a purpose, stated naked and in the wind. It is not defended, justified, or given ontological status. To generate powerful ways of speaking *as measured against that purpose*, contextual behavioral science has embraced a wide variety of methodologies, including laboratory research designed to refine behavioral principles with high levels of precision and scope, and applied research such as randomized controlled trials, analog research, and measure development that test the scope of the principles and theories that result (e.g., Hayes et al. 2006). Whether concepts like EA are helpful is a matter to be considered relevant to the goals of the analysis.

As example of the kind of problems the authors create for themselves, consider the following quote from the target article:

Although the post-Skinnerians who proposed the EA concept call themselves “contextualists,” they pay no more than lip service to the role of the “social/verbal community” and the cultural context. Their research focuses almost exclusively on laboratory experiments, and does not consider the important role of particular social practices and contexts, such as modernity. (García-Montes et al. 2008)

There are many examples to show otherwise, such as the analysis of the expansion of EA due to cultural and commercial factors (Biglan, Hayes, and, and Pistorello, in press), or the attempt to

deal directly with cultural issues such as prejudice (e.g., Lillis and Hayes 2007), but more to the point this comment fails to understand the purpose of contextual behavioral science, and implicitly appeals to ontology instead of being explicit about the authors’ own goals. In essence, the article is arguing we need to consider culture, because culture is *real* and it *really* has an impact. For example, the authors speak of “dealing, not with simple individual behavior, but rather with standardized behavior, a kind of ritual whose elements cannot be separated out and studied in isolation from their sociocultural context.” For a contextualist, all behavioral events are contextually embedded, but partitioning behavior into individual and standardized forms on the basis of real involvement with a real sociocultural context is an unwelcome intrusion of ontology into a contextualistic perspective.

A contextual behavioral science takes a different approach. Skinner said his approach was to create “a promising conception of human behavior” by a research focus “which began with simple organisms in simple situations and moved on, but only as its growing power permitted, to the complexities of the world at large” (Skinner 1966, xiv; preface to the seventh printing of *The Behavior of Organisms* 1938). Within this tradition, the social/verbal community is not an abstract concept that can be characterized effectively with concepts like “modernity” or other similar terms that are not part of experimental science. Laboratory experiments are designed to deal with specific and well-defined verbal/social contexts so that high-scope, high-precision concepts may be inductively derived. In other words, laboratory research is a very powerful way to investigate the *what*, the *when*, the *how* of that verbal/social context, in contrast with the *perhaps*, the *maybe*, and the *may*; that is, the result of tying this term to even broader abstractions (such as modernity) that achieve scope at the cost of precision and that are not linked to prediction *and influence* as analytic goals.

The same problem occurs in the area of ecological validity. Validity is a matter of scope. Contextual behavioral science has proven its ability to produce concepts with high scope. Speaking in

terms of operants has broad applicability in daily life situations. For example, operant conditioning is applicable in the laboratory, in school settings, in special education, in I/O settings, and so on. The same applies to other behavioral principles, which is why they are called “principles.” RFT is carrying that strategy forward into the analysis of human language in terms of relational conditioning, which is having broad impact in areas such as language training (Rehfeldt and Barnes-Holmes, in press) and therapy (Hayes et al. 1999; Hayes et al. 2006). In a contextual behavioral science approach, multiple fronts of research simultaneously inform each other. Analog research constitutes a bridge between those basic behavioral processes and how they affect more psychologically relevant phenomena (such as anxiety or task persistence). Model development (e.g., ACT) bridges from laboratory findings to protocols and techniques that make basic research available to practitioners. Randomized controlled trials serve to test the utility of such findings in broader contexts and populations. There is no specific directionality in how those different fronts of research inform each other, but their goals are aligned.

In contrast, the authors’ criticism that the ACT/RFT model does not pay enough attention to everyday life situations and thus lacks “ecological validity” is another implicit appeal to ontology. The authors seem to feel that we can know that analyses are ecologically valid by their form: They are valid when they *really* refer to what is *really* there in human complexity. That approach is a common one, and it has a predictable outcome: High-scope concepts that are imprecise and fail to lead to both prediction *and* influence because they fail to orient the listener to the key manipulable features of the historical and situational context. Any fair critic looking at the list of empirically supported procedures emerging out of behavior analysis as compared with, say, social constructivism will see the pragmatic differences between approaches that seek ecological validity inductively and functionally as compared with those that seek it formally.

The term “superstition,” as the authors are using it, has high scope, but at the cost of precision. If the purpose is a personal appreciation of the par-

ticipants in a whole event (what has been termed descriptive contextualism [Hayes 1993]) there can be no complaint, but if that is the purpose, it should be stated. We have stated our purposes, and measured against them we see nothing empirical to recommend “superstition” as a technical term. Conversely, EA has already proven to be a powerful concept useful as a moderator, mediator, and target of behavioral change (Hayes et al. 2006), that comports with the neurobiological evidence (Cochrane et al. 2007). Said in another way, EA is already proven to assist in the prediction and influence of events with precision, scope, and depth. Superstition has not.

## REFERENCES

- Biglan, A., S. C. Hayes, and J. Pistorello. In press. Acceptance and commitment: Implications for prevention science. *Prevention Science*.
- Cochrane, A., D. Barnes-Holmes, Y. Barnes-Holmes, I. Stewart, and C. Luciano. 2007. Experiential avoidance and aversive visual images: Response delays and event-related potentials on a simple matching task. *Behaviour Research and Therapy* 45:1379–88.
- García-Montes, J. M., M. Pérez Álvarez, L. A. Sass, and A. J. Cangas. 2008. The role of superstition in psychopathology. *Philosophy, Psychiatry, & Psychology* 15, no. 3:227–237.
- Hayes, S. C. 1993. Analytic goals and the varieties of scientific contextualism. In *Varieties of scientific contextualism*, ed. S. C. Hayes, L. J. Hayes, H. W. Reese, and T. R. Sarbin, 11–27. Reno, NV: Context Press.
- Hayes, S. C., D. Barnes-Holmes, and B. Roche, ed. 2001. *Relational Frame Theory: A post-Skinnerian account of human language and cognition*. New York: Plenum Press.
- Hayes, S. C., J. Luoma, F. Bond, A. Masuda, and J. Lillis. 2006. Acceptance and Commitment Therapy: Model, processes, and outcomes. *Behaviour Research and Therapy* 44:1–25.
- Hayes, S. C., K. Strosahl, and K. G. Wilson. 1999. *Acceptance and Commitment Therapy: An experiential approach to behavior change*. New York: Guilford Press.
- Lillis, J., and S. C. Hayes. 2007. Applying acceptance, mindfulness, and values to the reduction of prejudice: A pilot study. *Behavior Modification* 31:389–411.
- Rehfeldt, R. A., and Y. Barnes-Holmes, eds. In press. *Applications for learners with autism and other developmental disabilities: A progressive guide to change*.
- Skinner, B. F. 1938/1966. *The Behavior of organisms*. New York: Appleton-Century-Crofts.