Point/Counterpoint introduction: The future of theory in organizational behavior research

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Introduction: the future of theory in organizational behavior research

Since Hambrick (2007) and (Locke, 2007) published papers critical of how a deductive theory-driven approach to research has become dominant in the organizational sciences, a growing discussion has emerged about the appropriate role of theory. One notable example is a 2015 debate on the issue at the Society for Industrial and Organizational Psychology conference that is the basis for this point/counterpoint exchange. Jessica Nicklin served as the moderator for that debate, and here, she takes on the role as co-editor for this installment of point/counterpoint in Journal of Organizational Behavior. The other debate participants are authors of the four papers that comprise this exchange.

The organizational sciences were traditionally viewed as largely atheoretical and in fact were once criticized for being too empirical in nature. This is a far cry from where we are as a discipline today—where theory is heavily emphasized, and so much so that it is difficult for one to get published without making a "solid" theoretical contribution. This has created some controversy among scholars, with some purporting that theory is essential for organizational research, and others suggesting that the emphasis on theory is hindering the advancement of the field. We currently face a sort of ironic dead-end cycle, whereby researchers are expected to generate elaborate, novel, and interesting theory to publish in premier journals (such as Academy of Management Review, Academy of Management Journal, and Journal of Applied Psychology); yet, these theories rarely, if ever, get tested because testing and replication of a previously published theory would not be viewed as a significant new theoretical contribution. This then begs the question: What is the future of theory in Industrial/Organizational Psychology and Organizational Behavior Research?

The current point/counterpoint exchange begins with a paper by Jeffrey M. Cucina and Michael A. McDaniel, who argue that much of what is labeled as theoretical contributions in our field is actually pseudotheoretical writing, often composed of poorly constructed hypotheses that lack empirical support and replication. They further suggest that by placing too much emphasis on pseudotheory, the field is not following the scientific method, leading to a poor understanding of organizational issues. They go so far as to suggest that the current publication process in organizational behavior research is "dysfunctional and often a detriment to the trustworthiness of our cumulative knowledge." They urge scholars to refocus research on the scientific method (which defines theory as a well-supported and well-replicated hypothesis), as opposed to emphasizing and rewarding the development of pseudotheory.

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At the other end of the spectrum, Neal A. Ashkanasy argues that credible research in the organizational sciences cannot advance without theory and that theory-free science is simply unscientific. He provides two alternative models of the research process, whereby Cucina, Hayes, Walmsley, and Martin (2014) suggest that Step 1 of the scientific method is make an observation, while he argues that that Step 1 of the scientific method should actually be based in theory, make an observation. Furthermore, he provides several illustrations of empirical findings that would have been unlikely had it not been for a solid theoretical foundation. Contrary to the concerns raised by the other authors, he supports the mission of the top tier journals in management such as Academy of Management Review; and he suggests that these journals serve to challenge and engage us. While he does not deny that there are some fundamental problems with what authors in our field consider “theory” and a “theoretical contribution,” he nonetheless concludes that theory is “not only necessary for scientific advancement, it is indispensable.”

The next two papers each offer a potential compromise between the sentiments offered by Cucina and McDaniel, and Ashkanasy. John E. Mathieu argues that the issue is not necessarily whether or not theories are used, but rather the accuracy and value of the theories being published. While he agrees with Neal Ashkanasy that theories are essential, he discusses, at great length, how many management journals have “lost their way” by overemphasizing the entertainment value of theories, rather than focusing on solving real-world issues and understanding behavior in organizations. He cautions scholars of the dangers associated with “the publish novel theory phenomenon” and urges future researchers to consider how theoretical advancements can lead to better practice, not just new theoretical advancements. He suggests that by starting with real-world challenges and drawing from existing theory, and then developing new theory to understand and change it, we are also making a theoretical contribution.

José M. Cortina agrees that our field is enamored with interesting and new findings, yet his argument is centered on the trend for authors to feel compelled to match Introduction sections of articles with Results. Borrowing from the work of O’Boyle, Banks, and Gonzalez-Mulé (2014), he cites the “Chrysalis Effect”—the tendency to modify a priori hypotheses little by little until they are consistent with the findings, in order to be published. What results, according to Cortina, is theory that is actually post hoc, being proposed as a priori. He also discusses the widespread problem of HARKing (Hypothesizing after Results are Known; Kerr, 1998) and the lack of replication studies because of the culture of our publication process. He provides several unique recommendations to address some of these concerns, such as embracing the layering of overarching theories and judging Introduction and Method sections on their own merits. However, he does caution that such recommendations require change among the practices of our top leaders (and editors) in the field.

Taken together, these four papers provide an in-depth discussion of the pitfalls and potential for organizational behavior theory of the future. What is especially interesting is that despite their varying opinions, they all caution future researchers to be wary of some of our scientific missteps, such as HARKing, focusing solely on novel and interesting theory, and inadvertently disregarding replication studies. While their opinions regarding theory and the publication process may differ to some degree, they all urge us to rethink how we define theory and what makes a theoretical contribution in the field of organizational behavior. As the field continues to evolve, this point/counterpoint is an essential read for anyone conducting research in the organizational sciences.

**Author biographies**

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