

E X C H A N G E

CREATING A POLICY-ORIENTED ORGANIZATION SCIENCE: EXPANDING ON AGUINIS, JENSEN, AND KRAUS

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The relative scarcity of policy-relevant research is the inevitable result of editorial policies pursued by most if not all of the highest status management and organization science journals. Ironically, there are a plethora of policy-oriented journals available as publication outlets, so it is possible to both publish and read policy-relevant scholarship, just not in the usual places. These facts pose a challenge to traditional journals, including those published by the Academy of Management.

Aguinis, Jensen, and Kraus (2022) comprehensively documented the lack of attention to policy in the fields of organizational behavior (OB) and human resource management (HRM). They noted that only about 1.5% of published articles in the 10 important journals they surveyed contained policy implications, with 30% of the policy-relevant articles falling into just three topic areas. Aguinis et al. (2022) also showed that the rated importance of a theory seems to be unrelated to its ability to be relevant for policy questions. They argued that the absence of policy does the fields of OB and HRM a disservice because the absence of policy relevance potentially diminishes the fields' broader impact and prestige. They then provided suggestions for how to build a stronger policy emphasis in OB and HRM.

Aguinis et al.'s (2022) empirical conclusions, although depressing, are unsurprising. As they noted, "there is little motivation for OBHRM researchers to focus on policy implications given that this is not a requirement for publication in ... top journals" (Aguinis et al., 2022: 869). Their call for more attention to policy feels completely correct. I use this Exchange to enrich and extend their arguments, but also to maintain that their plea for more policy-oriented and relevant research, while totally on-point and useful, presents challenges given current editorial practices, particularly in the most prestigious journals. Building on the insight of Kuhn (1970) that scientific paradigms are essentially conservative, a phenomenon that is particularly true for the most central and highest prestige parts of scientific fields,

I argue that the most likely trajectory for developing policy-relevant research will come either through less "prestigious" publication outlets or through the increased attention to explicitly policy-oriented journals. There are actually already well over 150 policy-oriented journals in various social science disciplines, including public administration, though since these are not the most prestigious journals in any field they were not covered by Aguinis et al.'s (2022) review of high-prestige publications. This large population of policy-oriented journals suggests that there are outlets for policy-relevant research engaging with policy-building theory, summaries, and empirical studies (e.g., Cambridge University Press, n.d.; Fox & Sitkin, 2021; SCImago Journal and Country Rank, 2021). What Aguinis et al.'s (2022) paper seems to demonstrate most clearly is the importance of the reorientation in organizational research that the Responsible Research in Business and Management (RRBM) initiative has called for (e.g., Tsui, 2022), and how the field's leading journals are, in some fundamentally important ways, failing the field.

As Aguinis et al. (2022) noted, policy recommendations should be based on ideas and evidence that are robust—that have withstood enough replication that people can have confidence in the results. However, notwithstanding the manifest crisis of reproducibility in the social sciences (e.g., Pashler & Wagenmakers, 2012) and the welcome calls for building evidence-based management and practice (e.g., Pfeffer & Sutton, 2006; Rousseau, 2006), replication studies remain exceedingly rare. That is

because many, if not most, of the leading (and, for that matter, less-prestigious) journals evaluate papers by their contribution to theory *development*, as contrasted with *theory testing* or even extending existing theory. With the exception of *Academy of Management Discoveries*, papers—even those whose virtues derive mostly from the interesting results they present—begin with long theoretical development sections. As I noted in a prior paper (Pfeffer, 2009), the contrast with medicine, where there is a much greater emphasis on both policy in journals such as *Health Affairs* and the presentation of data without the requirement for theoretical acrobatics, could not be greater. Of course, with just two cases, it is impossible to demonstrate the validity of the assertion that one reason that medicine has made more progress than management is because of its approach to doing science, including an explicit interest in policy implications and the encouragement of replication and presentations of clinical data. However, that difference between the two disciplines and its implications is a possibility worth considering.

Organizational journals continue to suffer from a fixation not just on theory (Hambrick, 2007) but also on a demand that papers contribute to or develop *new* theory, at that. Instead of assessing the breadth of the phenomena a theory can explain, or whether a study may contribute to the further testing of a theory, journal editors often insist on new theoretical ideas. It is possibly deliciously ironic that the Academy of Management sponsors the publication of *Academy of Management Review*, which no longer does reviews but instead promotes the development of new theoretical frameworks. The unfortunate consequence is that organization science confronts a proliferation of theories, many, if not most, of which have not been tested even once, let alone comprehensively. This critique is related to, albeit quite distinct from, the argument about the sorry state of paradigm development (Pfeffer, 1993) in organization science and its various subfields. Rather, the point is that proliferating ideas and frameworks and privileging new theory over testing existing ideas will invariably provide a totally insufficient empirical foundation on which to build sound policy prescriptions.

In reading Aguinis et al. (2022), I was reminded of Kerr's (1975) classic paper on the folly of rewarding *A* while hoping for *B*. The absence of policy implications and prescriptions that Aguinis et al. (2022) so effectively documented seems to be a straightforward consequence of a set of rewards and editorial practices that, while long lamented—for instance, in

calls for greater relevance delivered by numerous AOM presidential addresses (e.g., Cummings, 2007; Hambrick, 1994) and an overview with some empirical data on the relationship between relevance and rigor (Palmer, Dick, & Freiburger, 2009)—remain largely unchanged. I acknowledge and applaud the efforts of organizations such as RRBM to alter what type of research gets recognized, and note that some progress has occurred. But it is not nearly enough. Editors will sometimes privately bemoan the state of the field, but ironically are often reluctant to make decisions that will change a system that provides few rewards for taking on policy-relevant questions.

The loss from ignoring policy is more than one of prestige and status, although Aguinis et al. (2022) were undoubtedly correct in calling attention to the potential prestige costs of not being policy-relevant. OB, HRM, and indeed organization science more generally has much to contribute to policy concerns that extend well beyond the relationship between working arrangements, stress, and health (Goh, Pfeffer, & Zenios, 2016)—although these are important topics. Income inequality is in part a consequence of how labor markets are organized (Davis & Cobb, 2010) and salary decisions made (e.g., Bidwell, Briscoe, Fernandez-Mateo, & Sterling, 2013). The representation, or lack thereof, of women and people of color in higher levels of management and boards of directors reflect decisions made in and by organizations (e.g., Wade, 2003). The fact that economic outcomes are seemingly privileged even over human health and well-being, and even in scholarly research (Walsh, Weber & Margolis, 2003), and the short-shrift often afforded in organizational decisions (as contrasted with organizational rhetoric) to social responsibility, reflect leadership choices and, for that matter, the choice of leaders. Leadership selection that results in the elevation of self-serving narcissists to positions of power in both the public and private sector has seemingly clear policy implications (e.g., Grijalva, Harms, Newman, Gaddis, & Fraley, 2015). There is, in fact, an almost limitless list of important, policy-relevant topics to which OB could productively contribute. Abandoning policy research and recommendations solely, or mostly, to economists and political scientists not only risks the prestige of organization science; it leaves policy discussions and recommendations unnecessarily impoverished.

The dearth of policy-relevant research that Aguinis et al. (2022) documented is, in the end, nothing more or less than the result of a set of institutional arrangements and decisions that seemingly privilege

new theoretical contributions over studies that might evaluate or contribute to policy. The current state of the field is not preordained, inevitable, or, as Aguinis et al. (2022) made quite clear, necessarily desirable. The interesting question is whether and when this ignoring of policy will change.

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