CEO WORKING PAPER SERIES

JULY 2019

RIGOR, RELEVANCE, AND RESILIENCE IN MANAGEMENT RESEARCH

CEO Publication: G19-03(694)

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Rigor, Relevance, and Resilience in Management Research

In her introductory article, Kovoor-Misra observes that the debate around whether business school research has become increasingly less relevant has gained momentum in recent years. There is indeed a legitimate concern whether the attempt to become more rigorous and gain more scientific legitimacy has come at the expense of producing practically useful and actionable insights. I have no doubt that only research that meets the highest standards of scientific rigor can yield insights that endure over time and generalize across contexts. The question is not whether to conduct rigorous or relevant research. I believe there is increasingly a shared understanding among management scholars that research needs to be both rigorous *and* relevant. The challenge lies in how to conduct such research. Research that is rigorous, accessible, relevant, and enlightening may be rare, but we must conduct such research. More importantly, such research will stand the test of time and be resilient and impactful in the face of the many challenges noted in the introduction to this dialogue. Are there examples of such research topics? I believe there are and will identify some in the section that follows. Are there continued opportunities and threats to our ability to conduct such research? I will identify a few of these as well. Finally, can we do more to encourage impactful research? I will close this piece by sharing some observations on this question.

Resilient and Impactful Research

I am encouraged that many research topics that have sustained rigorous enquiry from management scholars for many decades have also yielded answers to questions that are of significant concern to managers[1]. These research topics share the following characteristics. First, they are grounded in strong theories that provide compelling answers to the "what," the "why," and the "how" of important organizational phenomena. Second, the questions

[1] It is beyond the scope of this essay to provide the specific actionable insights from the topics I briefly describe here. There are readily available review articles on any of these topics.

examined within these research topics are amenable to examination at multiple levels of analysis—the individual, the group, or the organization as a whole. Third, scholarship within these research topics has used different theoretical lenses and both qualitative and quantitative research methods. Fourth, scholarship in these topics has remained true to the fundamentals in our research training including a deep understanding of the seminal works in the field and an uncompromising adherence to the most rigorous standards of research design and implementation. Examples of such research topics[2] include, among others, organizational learning and innovation as well as human capital.

The antecedents and the outcomes of organizational change and adaptation and the role of key individuals as well as teams in organizational adaptation and survival remain versatile areas for scholarly examination. Within the broader research area of organizational adaptation and survival, there are two research topics that have been particularly successful in blending rigor and relevance—organizational learning and innovation and human capital. Research on organizational learning and innovation has provided answers to the following questions: How and when do organizations learn? How does learning that takes place at an individual or team level get institutionalized at an organizational level? What distinguishes incremental from transformative learning? What are the relationships between organizations' learning capabilities and their innovative capacities? Empirical studies on these questions span several decades of research and multiple management fields including those of organizational behavior, organization theory and strategic management. Several researchers (Levitt & March, 1988; Gavetti, Greve, Levinthal, & Ocasio, 2012) have examined the phenomena at multiple levels of analysis and invoked a variety of theoretical lenses and methodological approaches.

The role of human capital in the creation and sustenance of competitive advantage has similarly captured the imagination of scholars across multiple management fields although

[2] These are very broad research topics and include many specific research areas with varying levels of appeal to stakeholders beyond an academic audience.

each field may focus on a different facet of human capital. While organizational behavior and human resource management scholars are interested in how motivations, rewards, and performance appraisals influence employee behaviors and outcomes (e.g., Lawler & Boudreau, 2018), strategic management scholars have focused on the antecedents and consequences of variations in the human capital of top management teams (e.g., Finkelstein & Hambrick, 1996). Also, entrepreneurship scholars have studied, among others, the effects of employee mobility on entrepreneurial outcomes and firm performance (e.g., Campbell, Ganco, Franco, & Agarwal, 2012). More and more organizations today confront the constantly changing landscape of the knowledge economy and employ highly mobile and skilled workers who spend less time in a single organization. Hence, more than ever before, organizations need research that helps them understand how to identify, nurture, and retain valuable talent. Rigorous research that answers these questions will naturally also be highly relevant.

Opportunities and Threats

As we aspire to do research that meets the dual objectives of rigor and relevance, are there some broader environmental trends that pose opportunities and/or threats? One such trend relates to "big" data. On the positive side, big data presents a significant opportunity for management researchers, but it also has the less positive and even disturbing potential to make us preoccupied with the mundane rather than creative aspects of research. I agree with Kovoor-Misra (in the introductory article) that the technological advances in how we collect, analyze, and utilize data have opened up new avenues for researchers. The access to millions, instead of hundreds or thousands of observations, is indeed transforming the way research is being conducted. Increasingly robust and sophisticated empirical models can be estimated and more refined and multi-dimensional causal pathways can be identified. Populations can be divided into several homogeneous sub-groups without losing the statistical power afforded by large samples. As noted in the March 2019 report of the ***MIT Ad Hoc Faculty Task Force on Open Access to MIT's Research,*** the ability to access and share large data sets is crucial to the advancement of scholarly research. However, reminding us about the serious unanswered questions in this realm, the report also notes the following: "While new capabilities for sharing data provide the opportunity to support robust validation and replication of research—a core aim—broad sharing also raises questions about how to maintain appropriate levels of privacy for sensitive human subject data, and appropriate security for other types of sensitive, classified, or proprietary data." In addition to recognizing these largely unresolved concerns around data access and data sharing, we also need to be sensitive to other challenges. For instance, the time and effort devoted to understand and analyze gigantic data sets can become all-consuming such that the researcher may lose sight of what really matters—the core research question and a fine-grained understanding of the phenomenon itself. The ease of data availability and the constant advancement in analysis methods to manipulate huge datasets can, perhaps unwittingly, tempt researchers to substitute complex statistics for more accessible insights. We should remain vigilant that the time and care devoted to conceptualizing interesting and important research questions is not replaced by vacuous data mining. I concur here with Barry Staw's observation (from the March 2019 JMI Scholar session) that it is important that scholars develop the first draft of their hypotheses before any data collection.

As a strategy researcher, I believe in the importance of research that is focused on the core question that created the field: Why do some firms outperform others? The good news is that the availability of large data sets and sophisticated empirical methods enable the unpacking of core performance differentiators and underlying causal mechanisms more rigorously than we thought possible just a few years ago. The bad news is that fewer researchers seem to be asking the really interesting and relevant questions that plague organizations and their managers.

Another trend that can be both a threat as well as an opportunity relates to how we invoke disciplines (like economics or psychology) when it comes to defining our research identity, the questions we study, and the methods we use. While economics, sociology, and psychology can definitely lend theoretical rigor and methodological sophistication to help guide our research, we have to ensure that the research questions most likely to yield actionable insights do not become secondary or tangential concerns. We undoubtedly need to meet the highest standards of the underlying discipline that informs our research question/s, but we cannot afford to blur our identity as management scholars by relegating the questions that differentiate us and are central to our scholarly identity to the periphery of our research agenda.

In that spirit, I hope strategy research will continue to focus on the questions that are of central importance to firms and their managers: What are the relative effects of strategic choices, top managers, and environmental factors when it comes to understanding performance heterogeneity? What are the consequences of modes of organizing, governance choices, and innovative processes on firm performance? I offer similar observations for the consideration of my organizational behavior colleagues. Psychology and the experimental methods associated with it have clearly enhanced our ability to unpack causal mechanisms and measure core constructs rigorously. However, the organization (including individuals and teams within the organization) has to remain at the center of scholarly discourse. Otherwise, we risk losing relevance and becoming just another sub-field of the broader psychology discipline with little if any differentiation.

Some Strategies for Change

I conclude this essay by noting some other changes that will be required to ensure we continue to produce management knowledge that is impactful. First, management scholars need to engage more directly and for longer periods of time with organizations not just as sources of data and short-term interactions but through more immersive longer term experiences. Spending a semester or even a year in a company that has innovative business models can help generate innovative and relevant research questions. Corporate externships—where researchers test their ideas with managers grappling with real-time challenges and perhaps even collaborate with the in-house research team—have the potential to yield not only unique research contributions but also enhance the production of management knowledge that can be used in the creation of cutting-edge course materials. Research sabbaticals that are spent within a business organization can also be very fruitful in motivating questions that are particularly relevant (and timely) when it comes to addressing complex business challenges.

We also need to creatively incentivize and reward research that generates enduring practical insights. At my institution, we recently launched an "Impact on Practice Award" to recognize rigorous research that has significantly influenced practice. It was challenging to develop criteria for the award, but a group of thoughtful scholars representing multiple fields of business research identified a number of criteria to help assess the quality and reach of the research activities with specific examples of evidence. Examples include awards bestowed by external academic as well as non-academic organizations (such as the Edelman Award by INFORMS for Operations Research or the AQR Insight Award given by hedge funds), national or international book awards for books based on scholarly research (such as Axiom Business Book Awards), practitioner equivalents of Google Scholar or Altmetrics measures, and influential leadership roles at regulatory or standard-setting institutions such as the SEC or FASB.

At a more general level, we need mechanisms to encourage closer interactions between the business community and research faculty, perhaps through research centers that examine theoretically interesting research questions with the potential to meet the high standards of our peer-reviewed journals even as they generate useful practical insights. The Center for Effective Organizations at USC Marshall (under the decades-long leadership of Professor Ed Lawler, arguably one of the most influential management scholars around) is an example of a successful research center that has built long-standing corporate relationships even as it has generated highly impactful research. Partnerships with key stakeholders over several decades have helped the Center sustain a robust and relevant research agenda sensitive to the most pressing issues faced by organizations. A key to the survival and resilience of the Center has been its ability to identify (in a timely manner) the most crucial challenges faced by human resource practitioners and offer actionable insights that are grounded in sound theory and cutting-edge research methods.

Finally, we need to encourage and reward inter-disciplinary work that takes place at the intersection of multiple fields. Inter-disciplinary work will often take more time, and it will inevitably be challenging to get it published in traditional top-tier journals, but such work is crucial if we are to address the more vexing managerial and societal problems that cut across disciplinary boundaries. The good news is that inter-disciplinary collaborations are particularly natural for management research given the disciplinary underpinnings of our research in psychology, sociology, and economics. Management scholars trained in business schools can leverage the theoretical and/or methodological expertise of their discipline-trained collaborators to blend rigor and relevance in a manner highly conducive to the generation of robust and actionable insights.

I close this essay with the sobering reminder that few of us can afford the luxury of choosing between rigor and relevance. Many, if not most, business schools depend on tuition dollars to fund scholarly work, and we are obliged to create knowledge that yields teachable insights and motivates more effective management practices. Individual researchers must accept the responsibility of pursuing research that is both rigorous and relevant because only such research can remain resilient and impactful. However, their institutions also have to create and support the organizational structures and processes that appropriately incentivize and reward such efforts.

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