

Path for Early-Career Scholars Toward Real-World Impact: From Predictions to Possibilities

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“Management research should generate a positive social impact by tackling societal grand challenges” is not a new catchphrase among management scholars. There has been an urgent call for management scholars to work on important topics that are meaningful to our society and practically relevant to organizations while also making a theoretical contribution to the academic literature. Nevertheless, progress in producing relevant and impactful management research has been quite slow [due to the entrenched research culture and reward systems](#) that place little value on impact and on the relevance of our work.

Management scholars have offered a wide array of solutions to this imbalance between academic theory and real-world impact, including, but not limited to, [engaged scholarship](#) where stakeholders are included in the research process to keep the research grounded in the real world and [co-created research](#) where scholars collaborate with practitioners on a publishable paper. Yet, several problems remain. Scholars have not agreed on what constitutes research impact despite a commonly-used definition—offered by [the UK Research and Innovation](#)—as “the demonstrable contribution that excellent research makes to society and the economy.” Scholars have also not agreed on what is the best way to achieve that impact. Moreover, for early-career scholars, trying to simultaneously achieve the goals of publication and impact tends to become their own grand challenge because they have relatively little slack to tackle a big, important question while meeting the expectations for tenure and promotion. Thus, there is no easy way for early-career scholars to navigate how their work can produce meaningful impact and serve as a force for good.

For these reasons, an important question is how early-stage scholars can survive the peer-review process when publishing a study with real-world impact. To garner insights into potential answers, the Impact Scholar Community organized a professional development workshop titled “Publishing for Real-World Impact: Helping Engaged Scholars Navigating the Publication Process” at the 2022 Academy of Management Annual Meeting in Seattle, Washington. The workshop was formed by three teams of top editors—*Administrative Science Quarterly*, *Strategic Organization*, and *Business & Society*—paired with the early-career scholars who recently published their papers at these journals. Its aim was to offer novice scholars practical advice on how to achieve the dual outcomes of publication and real-world impact.

In this reflective essay, we first begin by debriefing key points made at the workshop. Then, from the perspective of doctoral students, we propose that early-career scholars consider the bold

move of shifting their research focus from questions that ask *what is predictable* to questions that ask *what is possible* to generate a positive social impact. We further propose that they also take a more interdisciplinary approach when directing that impact at complex problems with a very wide scope such as societal grand challenges.

Briefing workshop: Can early-career scholars kill publications and impact with one stone?

The workshop was organized around three questions: (i) what does research impact mean to the journal?, (ii) how do early-career scholars frame research impact through the publication process?, and (iii) what do editors and early-career authors do to connect publications to practice?

What was striking about the workshop was the first question asked by a member of the audience. He characterized the panelists as dancing around the issue of what impact meant and emphasized how the literature was not making an impact despite all the methodological rigor and theoretical advancement research generated. We believe these comments get at the heart of the issue for many who insist current research lacks sufficient real-world impact and sets the stage for discussing the distinction between what approaches are used to generate impact and whether those approaches are successful.

There seemed to be a consensus on how impact should be generated among the panelists. Impact should not be generated by producing publications meant to target practitioners; instead, impact should be generated by focusing on good science. When asked how impact was related to the publication process, one of the editor panelists stridently declared that her journal is not looking for impact. They are an academic journal, so they are looking for good science, and good science eventually leads to an impact.

To justify this claim, she illustrated how the physical sciences create impact. The vaccines played a leading role in combatting the pandemic because the vaccines were based on good science, not because the scientific papers effectively targeted those who took the vaccines. She suggested the same holds true for management research. Its impact will be based on how good its science is, not who its papers target. In fact, she argues that this is what separates academics apart from consultants. Both want to have an impact on practitioners. But academics achieve that impact through a commitment to producing good science.

There was visual agreement among the other panelists on this point. Another editor panelist built on it by commenting that he is triple vaccinated, but this was not because he read up on the vaccine's scientific papers. It was because the vaccine was based on good science. Thus, the purpose of a scientific paper should not be to influence a practitioner but rather to contribute to the creation of a body of knowledge along with other related scientific papers. It is this accumulated scientific understanding that goes on to create an impact on practitioners.

Although this reasoning is sound and persuasive and clearly works for the physical sciences, there are many who believe this approach is failing to work as hoped for the management sciences. The central issue the first audience question raised was that despite all the good science produced, an impact was still not being generated. Some scholars, who approach research in an alternative way and operate from a different set of assumptions about our object of study, reason that the problem with this orthodox approach is that management research is not the same as the physical sciences. We study people with agency, and they do not respond in the same way as viruses do to a vaccine. Agency enables people to defy any relationship scientific theory discovers. People are not at the mercy of some set of causal mechanisms but rather act because of subjective reasons based on their unique interpretation of the world. The physical sciences, by contrast, do not study objects with agency. Agency is what makes the physical sciences fundamentally distinct

from management science. More importantly, it often makes theory, which is the product that emerges from good science, problematic.

Each of the panelists confirmed that good research makes a contribution to theory, and this, rather than impact, is what leads to publication. In other words, the aim of good science that gets published is to contribute to theory. For all the debate that occurs within an academic community over nearly every aspect of research, there is remarkable consensus on what constitutes theory. Theory, in part, explains generalizable causal relationships. Therefore, good science discovers, explains, and empirically confirms these relationships. However, if people have the agency to act in ways that run counter to the prevailing literature, the relationship does not hold. This severely limits the explanatory power of any theory. Moreover, it will, in turn, severely limit the utility of theory in the minds of practitioners. Good science, therefore, will only have limited impact.

According to this view, there is a fundamental issue with the type of research management academics are conducting that limits its value to practitioners. Management research attempts to discover relationships, but because of agency, theoretical relationships inevitably prove to have unstable and unreliable effects. If scholars want to have an impact, it will be difficult to achieve that by reporting relationships to practitioners.

Laying a stepping stone for early-career scholars to generate impact through publications

It is clear to us that generating impact requires a fundamental change in the type of knowledge that research studies produce. As doctoral students, we believe early-career scholars will be most open to exploring alternative research approaches to the mainstream, dominant form of research since they do not have to overcome having spent an entire working lifetime conducting research in a particular kind of way. And for those who have a strong commitment to making a difference and value research impact highly, they may be willing to tolerate the additional struggles operating outside of the mainstream may present. These intrepid researchers should consider the following as a viable path toward making an impact.

If agency makes the discovery of relationships that predict outcomes futile, management researchers should consider shifting their focus away from asking questions about *predictions*. Instead, they should consider asking questions about *possibilities* as an alternative. Studies should ask, what solutions or practices are possible to achieve some desired end? And those possibilities should be developed in an academically rigorous way. For example, [rigor can be achieved](#) by showing how small-scale implementations of a possible solution can be scaled to impact the whole of society.

This is particularly pertinent to early-career scholars interested in sustainability. As scholars in this area are aware, “[There is No Planet B.](#)” The planet and humanity are up against multiple grand challenges—such as [public health deficiencies](#), [climate change](#), and [violent conflict](#)—which only become more troublesome the longer they persist. These challenges demand the implementation of solutions today, which studying predictions and theoretical implications have been unable to provide. Thus, an alternative path for early-career management scholars to consider is to produce research that asks questions about what solutions to these grand challenges are possible instead. We offer two suggestions to those who want to consider this as a path.

First, we suggest that management research can be key to addressing grand challenges with possible solutions if management scholars understand the distinction between each of the grand challenges. For example, communicable diseases are not the same as non-communicable diseases, and the same goes for the relationship between climate change and global warming. Each of these grand challenges is generated from different processes and therefore requires exploring different

possible solutions to adopt in solving them. To achieve this nuance in understanding, scholars should draw insights from neighboring disciplines, including climate science, public and environmental health, and international development. The point is that a more refined approach—based on an interdisciplinary effort—is needed to generate real-world impact when faced with the scope of problems as vast as societal grand challenges. By collaborating with others outside the discipline, management scholars can provide the necessary organizational expertise that other disciplinary scholars may not have so that their contributions become more robust.

Second, this paradigm shift toward asking questions about possible solutions accepts the presence of agency and the role it plays in generating effects. It assumes that outcomes are made to happen by exercising and directing that agency rather than relying on outcomes being causally determined by the right set of circumstances. The challenge for new scholars is publishing this kind of work in journals that require a theoretical contribution and require that you meet the typical academic expectations of rigor. What we learned from this panel discussion is that these are the current standards that must be met, and impact is only a byproduct of these standards.

Although the core management orthodoxy may be rigid in its expectations and standards, the management academic field overall maintains an open mind toward alternative approaches. This presents an opportunity for new ideas to form on the periphery that brave new explorers can take advantage of. Jaime Bonache offers some instructive guidance on how scholars who might want to adopt a more heterodox approach can penetrate that periphery. Her [recent article](#) offers several helpful tactics to get through the peer review process at leading journals when you assume your object of study acts based on subjective interpretations rather than lawlike statistical tendencies. For example, she argues it is necessary for you to set the standards the reviewers should use to judge the quality of your submission since it was generated based on alternative methodological assumptions. Otherwise, they will evaluate it through a lens that is inappropriate for the kind of research question you are asking. Careful consideration and planning can get you through the review process so that you do not have to sacrifice publishing for impact.

Given the state of the world, we can all agree a greater impact is necessary. Hence, for those of you who may be restless with the pace of progress and eager to make a greater difference in society, it may be time to rethink standard orthodoxy and consider whether a radical change in research is just what is needed to generate a radical change in the world.

Short Bios

Junghoon Park is a Ph.D. candidate in Management at the Zicklin School of Business, Baruch College, the City University of New York. His expertise is in global sustainability strategy, stimulated by an interest in how firm strategies address intractable grand challenges. For more information, please visit www.park-junghoon.com.



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