Making Public Administration Relevant:
Stop Conceding Policy Research to Economists

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I like economists. Many of my favorite colleagues are economists. I sometimes publish my research in journals targeted to economists. In my area of inquiry, it is the economists who are invited to the table alongside policymakers and bureaucrats to inform important public problems, such as reforming affordable housing programs, new minimum wage policies, or consumer financial protections. Yet, I am trained as a public affairs scholar, not an economist. Sometimes I manage to sneak in-- despite my lack of "credentials," by ditching my PA hat for a "policy analyst" hat. Perhaps I should have pursued a PhD in economics instead of public affairs.

But wait—I pursued my PhD in public affairs precisely because I was passionate about making society better through better government programs and policies. Our field was created and inspired by generations of scholars who held this same objective. Yet we still find ourselves on the border of being relevant for real world problems, often speaking to ourselves in our journals rather than to the outside world. This is not a new criticism—being "relevant" and "rigorous" has been a reoccurring theme of Minnowbrooks' past. Some will argue that it is time to stop worrying about being relevant. As a scholarly field, we should just do what we do well: increasingly rigorous, theoretically grounded, empirical research. There is some truth to this. However, in this essay, I will argue that *relevance* is imperative to the future of our field, it is not optional. To be relevant, we must demonstrate how our research is uniquely positioned to inform *substantive public problems* that face society—we fill a gap that cannot be filled by other disciplinary fields, including economics. To do this, we must gain *legitimacy* by sacrificing a bit of our generalist mindset and becoming more *substantively embedded* within particular problem areas, without selling our souls to the problem of the day.

First, the future of our field rests on our ability to be relevant. That is, for our program of study to be fundable, to attract students, to be viewed as making a difference beyond the ivory tower, we must be relevant. Pragmatically, that is where higher education is going, and why our schools are often doing better than the core disciplines like political science (and economics!). Students, particularly those in our core MPA programs, come to us to be trained with specific skills that will improve their ability to address public problems and lead change in government and nonprofit organizations. Our students benefit from theory to be sure, but they need and demand more. They demand relevance. Philosophically, if we as public administration-management scholars do not have a seat at the proverbial government table, we (as a field) are doing something wrong. This is, or perhaps should be, what sets us apart from our disciplinary social science peers.

Second, to be relevant, we must show how our research connects to end outcomes. Here, I define outcomes as changes in the behavior or conditions of society, citizens or target populations (Lynn et al. 2001; Moulton and Sandfort 2016). What are the impacts of a policy or program on the target population or societal problem it was intended to serve or address? Often, analyses of end outcomes are considered "policy research." Applied economists and policy analysts trained in economics dominate this space, offering predictive models to help isolate the causal impact of a policy or intervention on given outcomes. Economic theories of behavior also tend to dominate the identification of explanatory variables and specific mechanisms that contribute to variance in observed outcomes. This is not a new evolution; indeed, it reflects the creation of policy analysis departments within the U.S. government back in the 1960s and the simultaneous creation of policy

analysis think tanks and schools of public policy.[1] Yet I fear we too quickly conceded policy research to "those" scholars, and by doing so, we have handicapped our ability to be relevant.

While public administration, public management and public policy scholars are often combined now under the umbrella of public affairs, the foci of our research too often remain in silos. Public administration/management scholars tend to focus on the *system* of governance, often agnostic of (or at least downplaying) the substantive policy context or target population, while applied micro-economist/policy scholars focus on the impacts of particular substantive policy outcomes, often agnostic (intentionally or unintentionally) of the governance context.

Public management scholars have made substantial progress theorizing about micro-processes within public governance settings, drawing heavily from organizational theories in sociology, theories of human behavior in psychology, and institutional theories in political science and economics. Our research examines the relationships between structure and agency in public organizations, and how different organizational forms may be more or less effective for coordinating joint action. We have moved beyond formal political control of policy implementation to consider the role of less formal institutions, norms, and values on frontline interactions. Despite this progress, we have done less well empirically connecting these micro-processes to actual substantive outcomes.[2]

There are notable exceptions. For example, recent research on administrative burden connects institutionalized routines and administrative procedures to substantive outcomes on target populations, such as enrollment in Medicaid (e.g. Herd et al. 2013). For more than a decade, research on representative bureaucracy has tested the link between the social or racial identities of administrators and front-line workers and the equitable (or inequitable) outcomes of target groups (e.g. Meier and Crotty 2006; Selden 2015). These types of studies offer promising examples of areas of inquiry that connect to end outcomes. Yet in many cases, the primary purpose of these studies is to advance theory rather than to inform substantive outcomes. This is not a problem per se; part of being a scholarly area of inquiry is our careful attention to theory. However, what makes our field distinct from core disciplines like economics, political science, and sociology is our concern for affecting real world change. A focus on substantive outcomes within a system of governance helps us move from either/or (theory or practical contribution) to "both"/"and".

Third, to be at the table, we must be perceived of as legitimate. We, as social scientists, many with training in organizational theory or political science, know something about legitimacy. It is time to apply this to understanding to how we approach our research, not just as a subject of our research. One way to be legitimate is to make sure we know what the heck we are talking about. This means understanding the policy context within which we are conducting our research. Many of us (myself included) have leveraged data in a particular policy context (e.g., mortgage lending, school district administration) to inform theoretically interesting relationships in public administration. It is imperative that when we use data in this way—even if the purpose is not to directly inform policy--that we do not give short shrift to the contextual details of the dynamics within this policy area. Doing so is a quick way to lose legitimacy from the actors (including other scholars) who spend their careers embedded within the context.

[1] For a review of the history of the policy analysis movement in the U.S. and abroad, see Rivlin (2013).
[2] We have done a better job at developing frameworks that connect the governance system to end outcomes. For example, the logic of governance framework by Lynn, Heinrich and Hill (2001) connects political inputs, managerial processes and frontline interactions contribute to end outcomes.

Gaining legitimacy requires not only an understanding of the problem context, but also an understanding of the dominant empirical logic or language that is viewed as valid by other actors within that context. Many areas of policy research speak a disciplinary language. In my areas of research, this is predominately economics. There are many substantive policy areas (housing and consumer finance among them!), where fundamental economic theory is quite useful for informing the mechanisms that contribute to observed outcomes. Indeed, "econ" is the dominate language used within these contexts. However, the best economists also know that there are limits to what pure economic theory can (or cannot) explain. Other complementary perspectives that call attention to implementation and institutional dynamics are often welcomed. In these types of inquiry areas (and there are many), legitimacy is often earned by forging a bridge to economists and other disciplinary scholars rather than drawing boundaries and building a wall.

In an effort to establish the boundaries of our field, I fear that we have built a wall that places policy research on one side (typically published in journals like JPAM, AEJ-Policy or other more econ oriented outlets), and public administration-management research on the other side (typically published in journals like PAR and JPART). This is a disservice to both areas of inquiry. Our increasingly narrow focus on the internal governance and management of government organizations allows us to test (and to a lesser extent, build) theory, but it loses sight of the end goal for which our field was created—and the reasons most of us pursued our PhDs in this field in the first place. To be sure, policy research is not the only avenue by which we can contribute to substantive outcomes and ensure the relevance of our field. But, it is one path, and a path that is readily available to us (just across the hall!)—if we choose to take it.

Our perspectives on public administration as a field of study are likely influenced most heavily by where we sit. It could be that this essay is simply a cathartic exercise for me to justify my chosen area of doctoral study. At the very least, to the extent that there are others like me who wish to bridge the policy research-administration divide within ourselves and our universities, perhaps this essay will encourage continued steps forward.

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