

FEATURED ESSAYS

Problem-Solving Sociology

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Ronald Reagan said an economist is someone who asks, “it may work in practice, but will it work in theory?” In that regard sociologists do not seem much different from economists. In sociology today a scholar could solve the problem of genocide, and someone somewhere would say “yes, but what is this a case of?” The urge to “theorize”—to pull out general implications from a specific situation, to see the universe in the grain of sand—is deeply rooted in our discipline and assumed as a requirement of scholarly publication.

But coexisting with this urge to make the specific general is an equally strong urge to be involved with the social problems of the day. Many sociologists choose this discipline precisely because sociology seems to be anchored in studying issues of poverty and inequality. Repeatedly over the last few decades we have seen projects, including by the most eminent members of our profession, focused on bringing sociology closer to the study of contemporary social problems, from Michael Burawoy’s “Public Sociology” to Theda Skocpol’s Scholars Strategy Network. It’s hard to blame scholars for thinking that the science of society ought, after all, to have something to say about contemporary social issues. But this urge stands in tension with the urge to transcend the local and the particular.

Recently, several scholars have suggested an approach that can reconcile these conflicting impulses. This approach begins with the observation that the distinction between basic and applied sciences within the natural sciences is overdrawn. In fact, many innovations in basic science have emerged from struggle with applied problems. The most famous example is of Louis Pasteur, whose research into the very

practical question of how to prevent beetroot alcohol from souring led to research that provided strong evidence for the germ theory of disease (Stokes 2011). More recently, the attempt to produce better yogurt led to the revolutionary advance of the CRISPR gene-editing technology (Grens 2015).

Scholars have suggested something similar may hold for the social sciences (Watts 2017; Pearson et al. 2016). To really solve a problem like racism or sexism, for example, requires deep understanding not only of the structures that give rise to racism and sexism—including social stratification, labor market opportunities, and the psychology of group formation—but also the mechanisms that can induce change, such as organizational innovations and social movements in addition to formal changes in rules and laws. Or consider one of the most pressing social problems in America today, how to reintegrate ex-prisoners into society. The consequences of mass incarceration include managing the reentry of millions of Americans who may not have up-to-date skills or training, networks of support, or plans for their lives. But once we begin to examine the question with the intention of solving it, we quickly discover that we are speaking to Durkheimian questions of how communities enforce their boundaries, how they manage symbolic transitions of status, and how they repair breaches (Braithwaite 1989). The problem provides an empirical testing ground for the theories, and the theories become resources for the solving of the

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problem. For this reason, attempting to solve real-world problems can be a catalyst for breakthroughs in the basic understanding of society: posing new questions, suggesting new research paths, and demanding new methods.

This problem-solving approach differs from some of the other proposals mentioned above. It differs from efforts like the Scholars Strategy Network, which focuses on the very useful goal of communicating things that social scientists already know to audiences outside of social science. Problem-solving sociology is rather about discovering things that social scientists do not yet know.

It differs from Michael Burawoy's "public sociology," which is defined as sociology that is in "conversation with publics, understood as people who are themselves involved in conversation" (Burawoy 2005:7). By this definition merely *studying* racism—for example, cataloguing the discourses of racism that emerge in a given setting—would count as public sociology if the study is read by enough non-sociologists. Problem-solving sociology, on the other hand, is about trying to *solve* racism. Only if the research project then moves into trying to understand how racist discourse can be changed (and whether the discourses have anything to do with attitudes or actual behavior at all) would it count as problem-solving sociology. From that point of view, problem-solving sociology is a more challenging—and therefore potentially more creative—task. Studying racism is easy. Solving it, much less so.

And this is exactly what makes problem-solving sociology more likely to lead to theoretical innovation. Simply describing the discourse of a site is not particularly interesting in theoretical terms. But trying to solve the problem demands engagement with the most basic and the biggest questions of social science and often requires furthering social theory. It also gives us a way to shake up a conservative tradition of scholarship while still remaining true to the most important thing that tradition bequeaths us, the ideal of attempting to be objective. Polemics are about as relevant to solving poverty as they are to solving cancer. There is a role for those who want to draw attention to a problem, but trying to solve the problem is a different sort

of enterprise, one that does not benefit from giving an analyst's prior convictions free reign.

Because there has always been an urge to solve problems in sociology, we find plenty of examples of sociological works that successfully use theory to suggest new ways of approaching social problems and use the analysis of social problems to advance sociological theory (for just a few examples beyond those discussed below see, e.g., Andrews 2014; Paschel 2010; Kellogg 2011). But the methodological lessons of successful problem-solving sociology have not yet been organized in a coherent manner. For that reason, attempts to study social problems often fall into three common traps. Here I discuss the traps and pick out three pieces of scholarship that show how to escape them.

Trap 1: Describing and Complaining Rather than Solving

Much scholarship that begins with trying to solve a problem ends up only describing the problem or complaining about the problem, or giving us detailed descriptions of how people talk about the problem.

One way to avoid this is to adopt a comparative method: identify a site that has solved the problem and a site that has not, and turn to sociological theory for help in identifying how the problem has been solved and how and why the solution was reached.

A canonical example is Peter Evans's *Embedded Autonomy*. Evans begins with a very practical question: how have some very poor countries managed to become rich, while others have not? He does not spend his time describing poverty, critiquing poverty, or telling us how people talk about poverty. Instead, he tries to actually solve poverty by asking how and why some countries manage to industrialize. Specifically, he asks why three countries that should not have been able to create an informatics industry according to the tenets of neoclassical economics—South Korea, India, and Brazil, none of which had the requisite comparative advantage—showed different outcomes in being able to create such an industry, with Korea successful and the others only partially so.

His investigations into the question draw heavily on Max Weber, but Evans's practical quest forces him beyond Weber to a theory of how the meritocratic bureaucracies that Weber emphasized need to be in close communication with business for late industrialization to take place. Evans did not completely solve the problem of late industrialization, but he was one of several scholars who helped to reorient the attention of the development community onto questions of governance and set a new research agenda for several subfields.

Comparison is also useful because it helps the scholar of social problems avoid two dangers. The first is stigmatization. Focusing only on the problematic or dysfunctional aspects of a social setting inevitably stigmatizes the population studied and fails to see the research setting in its fullness. But trying to correct for this by highlighting only the functional and healthy aspects of the setting falls into the problem of romanticization. In fact, urban ethnography sees periodic disputes between scholars who accuse each other of either stigmatizing—focusing only on the negative—or romanticizing—ignoring the negative. Both problems can be avoided through a strategy of systematic comparison, in which problematic sites are contrasted with successful sites in order to understand what drives the difference. This is what Mounira Charrad does when she explains why women's rights are more extensive in some Muslim countries than in others, thereby exploring the reasons for gender discrimination under Islam without suggesting that the entire religion is opposed to women's rights (Charrad 2001).

I used to think it was a limitation of this method that you could only study things that have actually happened, so if no country or locality has actually solved racism or sexism, then you are out of luck. But recently I came across the fascinating work of the UCLA sociologist Aliza Luft (2015), who breaks through this constraint very creatively. Luft studies the Rwandan genocide, and she compares not across locations, or even across people; rather, she compares *within* people. To explain genocide she compares the same person at times when they killed someone and times when they did not, and thus highlights the situational factors that lead to

violence. Her work suggests that there really is no limit if we're willing to be creative.

Lesson 1: Compare.

Trap 2: Studying the Victims Rather than the Villains

Another common trap that scholars who want to study social problems fall into is studying only the consequence and not the cause of the problem—the victims rather than the villains. It's understandable that a scholar wanting to study social problems would think that the goal is to tell the stories of the victims. Such research does play an extremely important role. Studying the victims allows us to understand the precise nature of the problem, especially if there is limited prior research on this population, and is a crucial first step in eventually posing solutions. At our current moment descriptions seem particularly relevant in research on developing countries, where American sociology has traditionally had less of a presence. But descriptive research of the victims cannot give us a full picture of the process causing the problem.

A better approach is suggested, although not fully implemented, by Matthew Desmond's *Evicted*. In explaining why he chose to study eviction, Desmond writes of prior research on poverty: "Where were the rich people who wielded enormous influence over the lives of low-income families and their communities—who were rich precisely because they did so?" (2016:317). In other words, when we study poverty, why do we tend to study only those who are the victims of the problem, rather than those who are actually causing the problem? Instead of studying a population, Desmond chose to study a process—eviction—that brought rich and poor people into relation with each other. He does study many evictees, but he also studies those who do the evicting, shadowing a landlord as she goes about her normal business. Through this Desmond shows us empirically that when we study only the victims we don't necessarily understand the process that renders them victims.

By suggesting we study the "villain" I don't mean to favor an excessively agentic theory. A full accounting of the problem of

eviction, for example, would not lay it at the feet of the landlords, but would analyze how they too are caught in a system of rewards and punishments; and perhaps eventually research would begin to identify how that system was created and how it might be changed. The general point is to study the cause, not only the consequence, in order to identify points for intervention (keeping in mind that some interventions do not or cannot remove the cause—which often lies far in the past—so much as bypass it).

Moreover, studying only the victims can lead to misguided policy solutions. For example, if we study only the behavior of the poor and become experts in the behavior of the poor, when pressed to develop policies that could help the situation the only thing that we will truly be qualified to speak about will be the behavior of the poor. Studying only the victim gives ammunition to those who want to blame only the victim (see, e.g., Schneider 2017) and leaves us with an impoverished understanding of how to change the situation.

This dynamic may be why the “policy suggestions” section of books that study the victim sometimes seem naïve and artificially tacked on at the end. The author’s expertise on the behavior of the poor can only lead to suggestions that the author does not want to give, and the suggestions the author does give have nothing to do with his or her actual area of expertise, because that expertise has not been developed through trying to solve the problem.

Indeed, I would argue that we need to push Desmond’s method even further. In specifically choosing to study a process that brings victims and victimizers into direct contact, Desmond elides the many processes that result in poverty but occur far from the victims they create: in banks deciding on foreclosure policies, among policy-makers choosing which problems to address and which to ignore, businesses considering whether and whom to hire, activists on college campuses being channeled into focusing on certain issues rather than others, middle-class families wondering how to vote. Beginning with a focus on direct relationships between the rich and the poor smuggles in an unwarranted assumption about the

causes of poverty. And in fact, despite his stated method, for most of the book Desmond’s gaze remains on the bodies and behaviors of the poor. If we want to understand how to solve poverty, we need a tradition of research on locations far away from the urban poor (Watkins-Hayes 2009; Rodriguez-Muniz 2015).

Lesson 2: Study the cause of the problem, not just the consequence—the villain rather than the victim.

Trap 3: Critiquing Other Solutions Rather than Providing New Solutions

A third trap that research on social problems falls into is critiquing solutions proposed by others without providing alternative solutions. Solutions proposed by policy-makers or economists are often criticized for their reductive and individualistic assumptions, but non-reductive and non-individualistic alternatives are not often proposed.

Marina Zaloznaya’s study of corruption in post-Soviet states, *The Politics of Bureaucratic Corruption*, offers lessons into how to go beyond critique of existing solutions to propose new solutions. (Full disclosure: Zaloznaya is a collaborator of mine. But she’s a collaborator of mine because I admire her work, rather than the other way around.)

The problem Zaloznaya addresses is corruption. She started her project comparing corrupt and non-corrupt settings and quickly discovered that corruption varies at sub-national levels. This was her first clue into both the theoretical nature of the problem and a potential way to fight it. As she writes, sociologists have given much less attention to corruption than economists and political scientists, and therefore the existing theories of corruption that drive corruption interventions fall into a dynamic that is familiar to sociologists: “On one hand, [corruption intervention] includes countrywide outreach programs to eradicate the alleged national culture of corruption. On the other, it entails the tightening of legal and administrative accountability mechanisms and harsh treatment of apprehended offenders . . . The evidence presented in *The Politics of Bureaucratic Corruption* suggests that the basal premises of many such initiatives are as empirically

unsupported as they are mutually contradictory (the first one assumes that corruption is a norm, and the rest treat corruption as a deviation from a norm)" (p. 162). The existing approaches to corruption are, in other words, oversocialized or undersocialized, precisely the problem that Mark Granovetter (1985) identified for the study of economic phenomena in general.

Next Zaliznaya asks, how do people *actually* decide whether to participate in corruption, if not by following country-level scripts or through individual-level cost-benefit calculation? Through a concerted effort of ethnography and interviews in multiple cities, she demonstrates that people learn about whether and how to give bribes through gossip and informal relationships inside organizations. Again and again she asks her respondents some variation of the question, what would happen if you did not pay the bribe, or did not pass the student the dean is pressuring you to pass? And again and again they respond that they don't want to know and they don't think they need to test it. This leads her to Karl Weick's (1995) observation that organizational routines rest on untested assumptions, and quickly Zaliznaya has moved from the realm of a very practical question about solving corruption to a very theoretical question about what makes organizations tick—and back again to the practical question, this time drawing on what organizational theorists know about how organizations can change.

Note that Zaliznaya does not rest content with simply critiquing the existing solutions. She actually provides a sociologically informed alternative, arguing that organizations should be the target of policy interventions rather than countries or individuals, which have been the foci of traditional corruption reforms.

This suggests that one strategy for problem-solving sociology is to scrutinize from the point of view of sociological theory the existing attempts at solving a problem, and then use that theory to suggest a more sociologically informed alternative. This move between practice and theory, if done well, can bring the insights of an entire theoretical literature to a specific policy issue, which means being able to bring insights

from *other* empirical cases to bear on this particular case. Being able to find the theoretical question provides a translation key that allows both the framing of practical research in theoretical terms and being able to call on a theoretical literature that might be able to guide the policy attempt.

A group of remarkable young sociologists has been developing a new agenda around state capacity by following this method. In addition to Zaliznaya, Erin McDonnell (2017) and Michael Roll (2014) have been producing some of the most exciting work I have seen in sociology today, which is combining to produce a sub-national, organizational-level approach to the question of state capacity. It is deeply rooted in their efforts at finding practical solutions to global problems, and it both draws on and advances sociological theory.

Lesson 3: Find the theoretical question inside the practical question.

It is not always easy to make this translation of practical questions into theoretical ones, of course. One strategy is to create communities around problem-solving sociology in which this project of translation is a key focus, and some such efforts are already in the works. (And despite my grouching above, I do think the question "what is this a case of?" is an excellent way to think about what larger issue this particular problem is manifesting.)

There is, moreover, no guarantee that if and when sociologists start trying to solve problems, problems will get solved. We will only know twenty years from now if anyone paid any attention to Zaliznaya, McDonnell, Roll, and others like them.

But problem-solving sociology can be revelatory even if the research detours away from trying to solve the problem. Consider the case of Margo Mahan, who started out trying to solve the problem of domestic violence. To do so, she began by interviewing men who batter women. And then as she moved deeper into the issues, the project took an unexpected historical turn. Mahan discovered and wrote a stunning dissertation documenting the origins of domestic violence legislation in laws designed to uphold white supremacy (Mahan 2017). It's an

amazing piece of historical scholarship. It does not solve the problem of domestic violence, although it makes the important point that restorative forms of justice might be more appropriate in such cases. But this important insight about the origins of domestic violence laws *would not exist* had Mahan not begun with that practical and problem-solving goal. Serious attempts to solve problems will always lead to novel sociological insights, because a social problem is an indication that something is occurring in the world that we do not understand.

Thus, whatever it does or does not do for the world, problem-solving sociology offers prospects for something all those of us reading this article must want: a more ambitious sociology.

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