Trajectories

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Can Saving the World Save Comparative Historical Sociology?

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After a year of discussions on the issue, the main conclusion I've drawn is that many scholars do want to conduct comparative historical research that is more relevant to contemporary concerns, but they see a distinction between policy-relevant work and "the kind of stuff that gets in the journals." They therefore develop strategies of separating their policy work and their academic work, or plan to devote themselves to policy only after tenure.

Luckily, there are ways to align the two goals of conducting policy-relevant research and publishing serious scholarship. If seen properly, they are the same goal. What academic journals want is new knowledge about the world. And what it takes to solve a policy problem is, in fact, new knowledge about the world.

I suggest here six strategies that might help younger scholars who want to figure out how to do

CONTENTS

Book Symposium

Page 6 Geva's *Conscription, Family and the Modern State*

Essays and Announcements

Page 24 Essay: Despina Lalaki on the crisis in Greece
Page 31 Debate: On the spread of the nation state
Page 33 Section awards, election results and other news

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comparative historical sociology that saves the world:

1) Think problem-solving, not activism

Much of the fear about a turn to policy-relevant scholarship is that it will devolve into activism, and activism requires repeating things that are already known. It's easy to understand this fear, as there is certainly lots of sociology that does exactly this—repeatedly showing that there is gender or race or class bias in discourse, for example. Perhaps such work is necessary to keep these issues in the eyes of outside publics, but this kind of work does not advance the discipline's understanding of society, which is why the journals and many scholars are skeptical of it. On the other hand, a lot of apolitical scholarship also repeats things that are already known, such as the yards and yards of economic sociology showing, yet again, that the economy is "embedded." The distinction isn't between policy-relevant and non-policy-relevant scholarship, the distinction is between scholarship that says something new, and scholarship that does not.

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And here is where a framework of "problem-solving sociology" can actually help: if you try sincerely to solve a problem, you have to say something new, because you have to figure out why the problem still exists and what social mechanisms might end it. An actually existing social problem is an indication of an area where new knowledge is desperately needed, because it's an indication that something is going on somewhere that we don't understand. No one wants children to die of hunger. Therefore, if children are dying of hunger—if something is happening despite people's preferences—some interesting sociological phenomenon is at play that needs to be understood. In order to move the world forward on the problem, you have to generate new knowledge about society, which requires doing more than simply saying that it's bad that children die of hunger.

2) Focus on the actual problem that you see

One response to the problem-solving effort has been "but we already know how to solve the problems, it's just that elites are blocking change." If that is the case, then *the problem that you need to solve is elites blocking change*. Study that. If you think we know how to solve poverty, but business interests won't let us do it, then don't study how to solve poverty, study how to overcome the resistance of business interests. Examine how they assert their power, when and where they weaken, or how change can happen in the face of business opposition. Follow the issue to the point where there is a problem that we don't know how to solve, and focus your efforts on that point. If we do understand when elite opposition weakens, but we don't know how to bring those conditions about, then the problem to solve is how to bring those conditions about, or how to mimic those conditions—for example, if we know that elite opposition weakens under wartime, the problem to solve is understanding exactly why it weakens in wartime, and how to bring those conditions about in times of peace. If a problem exists, there is something, somewhere, that we do not understand about society. Following the chain of causation can lead you to a creative and under-studied issue. Focusing on that issue helps to solve the problem while contributing new knowledge about society.

In the working groups we have set up for this effort, there is a debate within the climate change group on how to fight climate change, with some arguing that we should fight it directly and others arguing that it simply can't be fought under capitalism. If you think it can't be fought under capitalism, then *the problem that you should study is not climate change, it's capitalism*. If the problem as you understand it is that climate change can't be fought under capitalism, you should study how to overcome capitalism, for example by examining the conditions under which social democratic change happens, or the possibilities of worker co-operatives and other experiments with alternatives to capitalism. Focus on the actual problem—and note that doing so forces you to clarify your thoughts about what you think is the actual problem.

Note also that you don't have to be right about what the actual problem is to contribute something worthwhile: your study of when worker co-operatives are successful can be valuable even if, in fact, climate change can be fought without overthrowing capitalism, and your study of when business interests weaken will teach us something about society even if business interests aren't the main reason for poverty.

3) Reframe the problem theoretically

This is the most creative part of problem-solving sociology: you have to rethink what your problem actually is in sociological terms. For example, the problem of "corruption," when seen in broad theoretical terms, is simply the question of when social orders develop meritocratic Weberian bureaucracies, and how and why they might not. Solving the problem of corruption therefore requires answering the question of how meritocratic bureaucracies emerge and are sustained. Once you realize that, you have a translation key that allows you to read very practical and pragmatically oriented literature in theoretical terms, and theoretical literature in practical terms. You can use the theory to comment on the empirics, and vice versa.

Similarly, think creatively about what the problems of climate change or police brutality actually *are*. Is climate change ultimately a question of the social mechanisms that reproduce economic growth, and those that can put the brakes on it? Is police brutality the kind of conflict that Roger Gould theorized, which results when status hierarchies are not clear? I find the common advice to ask what your research site is a "case of" to be as good advice for problem-solving as for anything else. It forces you to think of your research site in broader, more general, and more creative ways.

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It isn't always easy to reframe your problem in theoretical terms. This is an instance when many heads are better than one. Drag your grad school friends into the question, and/or join one of the working groups that have been set up for problem-solving sociology at:

https://docs.google.com/document/d/1myGlNGAFSaxu4XtMfcZPCh1xUQrN7FN0n98l68cPLfI/edit

4) Remember the CHS secret weapon

The CHS secret weapon is comparison. Most of the members of our section are convinced about the benefits of historical narrative, but we often seem to forget comparison. This is unfortunate, because genuine causal analysis is hard, and comparison is one of the most helpful tools for it, helping us to establish plausible counterfactuals for our hypothesized mechanisms. It is simply not possible to understand most of the real problems of the world without it.

One strategy of problem-solving comparative historical sociology is to find a place that has actually solved the problem, and use historical narrative to compare it with cases that have not solved it. This is what Mounira Charrad does in *States and Women's Rights*, and what Peter Evans does in *Embedded Autonomy*. It's a tried and true strategy, but implementing it takes every ounce of creativity a scholar can muster. In some cases the comparison will not be at the level of nations. You could do a lot to further our understanding of police brutality by trying to answer why cities with similar rates of police homicides against whites vary so much in rates of police homicides against blacks: http://mappingpoliceviolence.org/cities/. What's gone wrong in Albuquerque and what is Kansas City doing differently?

If there are no cases of success, then compare across people—for example, people who benefit from things the way they are and people who don't. Or compare *within* people, as Aliza Luft does, comparing the same person at different points in time to show the situational factors that led to or prevented genocidal killing in Rwanda. Sometimes the comparison will be between the present and the past.

Adopting a comparative focus has a more subtle but no less important result. When scholars focus on "problems," there are two dangers to steer between. One is the danger of stigmatizing a social field by focusing only on what's wrong with it. Anyone who studies a developing country is familiar with this critique (India isn't only child poverty and communal violence, and you must be a neo-colonialist if that's all you want to talk about!). The other is the danger of romanticizing by neglecting to study the problems (India has serious problems of child poverty and communal violence, and you are an apologist for the oppressors if you ignore this!).

Urban ethnography periodically tears itself apart over this very question of stigmatization/romanticization. But you don't need to either stigmatize or romanticize. You just need to compare. Compare the regions that have managed to reduce child poverty with those that haven't. Compare the cities that have drawn on their inter-communal traditions to avoid communal violence with those that have given in to it. Comparison allows you to study the social field in all its fullness.

5) Use the problem to critique the literature

It can be easy to lose your way in a large scholarly literature. If you keep your problem-solving focus in mind, it can serve as a lamp to guide you through. When you ask the question of whether the problem has been solved or what we know about how to solve it, you often discover that generations of scholars have forgotten the question they set out to ask and have been pursuing tangential issues, or that an entire field has fallen into the lassitude of incremental scholarship. A problem-solving focus can have a clarifying and energizing effect on an entire field.

6) Stay in close touch with partisan news media, with activist blogs, and with law reviews

These are where problems are actually debated and discussed in our world, although it takes persistence to figure out that an article titled

"The Inequities of AEDPA Equitable Tolling: A Misapplication of Agency Law" is actually about prisoners who can't appeal the death penalty because their lawyers screwed up. (And once you become the expert, remember to give back by submitting to the section's own blog, policytrajectories.org, or writing a policy brief.)

If I were to write a sociology of sociology, the main theme would be that in our discipline, knowledge equals powerlessness. I would document the myriad incentives, structures, and trends that prevent sociologists from deploying sociological knowledge in ways that might actually reduce the oppression that we spend our lives documenting and deploring. This year's conversation has been fascinating for bringing some of those barriers into clearer view.

But there is reason to think that the situation can change, and change quickly. If I were to write a *problem-solving* sociology of sociology,

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comparing our current situation with times in the past when the discipline has been more problem-focused, or with other disciplines that are more problem-focused, I would suggest that the answer is very simple, and that it's all about being able to translate social problems into the discipline's own intellectual terms. If we can show that engaging with contemporary concerns actually leads to greater knowledge of society, we can assuage the critics' concerns.

Meanwhile, to get the conversation going, this year's effort has produced:

- the working groups mentioned above, https://docs.google.com/document/d/1myGlNGAFSaxu4XtMfcZPCh1xUQrN7FN0n98l68cPLfI/edit
- the section blog founded by Basak Kus (policytrajectories.org)
- a policy briefs series (contact Jensen Sass, jensen.sass@gmail.com, or Natalia Forrat, NataliaForrat2015@u.northwestern.edu, if you would like to contribute)
- a problem-solving mentorship event at ASA (to partiipate, please contact Nick Wilson nicholas.wilson@stonybrook.edu or Damon Mayrl damonmayrl@gmail.com)
- and a mini-conference co-organized by Johnnie Lotesta, Fiona Rose-Greenland, and others for the day before ASA (https://chsminicon2016.squarespace.com/)

Other initiatives are also afoot. Email me if you're interested in joining in, or if you'd just like to stay in touch with the efforts: m-prasad@northwestern.edu.

I'm not sure if anything we do will save the world—in this difficult summer of 2016, the world seems very far from being saved—but I am certain that they will lead to one thing all of us want, which is better sociology.