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How to do policy-oriented social science

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Sociologists and other social scientists can improve human well-being by asking useful questions and figuring out the correct answers. Some of these questions are "policy-oriented"; they are about the impact of policies and institutions. For example: Do social policies reduce poverty? What kind of healthcare system yields better health? Will reforming schools improve education? Do gun control regulations reduce crime? Do high taxes impede economic growth? How can government boost happiness?

Not too long ago, it was fairly common among sociologists to see policy-oriented research as best left to other disciplines such as economics, public policy, and political science. For sociology, in this view, policies and institutions ought to be dependent variables, the things we seek to explain.

My dissertation, written in the early 1990s, was a cross-country comparative analysis of the effects of economic cooperation, economic constraints, income inequality, government size, and union strength on outcomes such as economic growth, unemployment, and inflation. One prominent faculty member in my department, when I approached him about being on my PhD committee, responded that while the dissertation sounded interesting, because of the topic he would be able to write a job market letter for me only to policy schools, not to sociology departments.

This reflected a needlessly narrow notion of what sociologists ought to do. Thankfully, this conception has, for the most part, gone by the wayside. Policy-oriented analysis is now commonplace in our discipline, and as best I can tell, hardly anyone objects.

How should we do policy-oriented research? I favor embracing a multitude of analytical strategies. We can generate theories (hypotheses) or test them. We can analyze assorted units —individuals, groups, texts, rules, beliefs, countries, the world system, to name just a few. We can identify correlations or trace causal paths. We can gather and analyze data that are quantitative or qualitative. We can interview, observe as participants, mine historical archives, run experiments, crunch numbers, and more. Anything that enhances our understanding is, to my mind, a step forward.

Not everyone shares this view, however. Many comparative-historical sociologists work with "macro" data. Our units of analysis are countries, or other large geopolitical units such as regions or states. (For ease of exposition, I'll refer to countries from here on.) We engage in a type of analysis I call "macrocomparative" — we compare across countries and/or over time

within countries. Until recently, it was widely agreed that this type of research has considerable value. But as the use of randomized controlled trials (RCTs) and other experiments to evaluate policies has proliferated — especially, but not solely, in economics — the role and importance of macrocomparative analysis has faced new skepticism.

The worry is that macrocomparative studies fall short in identifying causality. Sometimes we work with a small and/or nonrepresentative sample of countries. Sometimes we have limited over-time data. Often we lack good measures of potentially important confounding factors. It's difficult to rule out selection bias. It's hard to nail down causal mechanisms that link correlated variables. A randomized controlled trial avoids most of these potential pitfalls. Units are randomly assigned to a treatment group or a control group, so we have less reason to fear biased effects estimates due to selection processes or omitted confounders. This analytical strategy, in most instances, is much more effective at achieving internal validity.

Yet the randomized controlled trial is far from a panacea. It has three important limitations. First, while it does well on internal validity, it can lack external validity. That is, it's difficult to know how generalizable the conclusion from a randomized controlled trial is beyond the particular circumstances in which the trial was conducted. Suppose we have a well-designed RCT studying the impact of high-quality affordable early education in Chicago in the 1970s. Is it safe to assume that the estimated effect will be similar if California were to implement the policy in 2016?

Second, we shouldn't draw inferences about country-level patterns based solely on individual-level data. We can't tell from studies of individuals how strong the effect will be at the level of society as a whole. Nor can we tell whether the cause will have other effects on the outcome that offset the effect identified at the micro level. Suppose studies of individuals find that people tend to respond to higher taxes by reducing their work effort. We shouldn't infer that increasing federal income tax rates in the United States will reduce economic growth. At the aggregate level the impact may be small and thus overshadowed by other determinants of growth. And higher tax rates may have other effects on economic growth (enabling more investment in infrastructure or research, for example) that offset a negative impact on individuals' work effort.

Third, many policies, policy packages, and policy approaches can't be effectively tested using a randomized controlled trial. Suppose we want to know whether a particular type of tax mix — a combination of income, payroll, and consumption taxes — enables governments to increase tax revenues. That almost certainly can't be tested at the local or even at the state level. Or suppose we want to know whether a single-payer healthcare system is more effective than a mostly-privatized system or a hybrid system at controlling healthcare cost increases. An RCT could conceivably be done using states, but only if a large number of state governments voluntarily agree, which is extremely unlikely.

So macro analysis, despite its limitations, will continue to play a central role in our investigation of policy effects.

At the same time, it would be good if an experimental orientation featured more prominently in macrocomparative analyses themselves. One useful avenue is to take advantage of natural experiments. Suppose we're interested in the effect of unionization on wage growth. And suppose we identify a pair of countries that are broadly similar in their institutional and policy configurations — say,

Denmark and Sweden, or the United States and Canada — and unionization declines significantly in one of the countries but not in the other. The subsequent wage patterns in the two countries should give us helpful information about the existence and magnitude of the hypothesized effect.

A second is to make greater use of a difference-in-differences approach to large-N quantitative macro analysis. We now have relatively lengthy time series for many policies, institutions, and outcomes, which enables us, in some instances, to examine the degree to which cross-country differences in change over time in a policy or institution are correlated with cross-country differences in change in an outcome. This gets us closer to an experimental design, reducing the likelihood of mistaken inferences.

Policy-oriented research won't automatically improve well-being. Sometimes we'll get the answers wrong, and even when we get them right, good science may not win the day with policy makers and other actors. Still, it's good to try, and we're getting better at doing so.

I also think policy-oriented research would benefit from more frequent use of multiple sources of evidence. Seldom do we have the evidence we'd ideally want — the "smoking gun" that gives us a clear and true signal about policy effects. So we should examine various types of data. For each, we should ask: "What would we expect to observe if a particular hypothesis were true? Is that what we in fact observe? If so (or if not), what does that tell us about the answer to our research question?" Then we piece together a conclusion from our multiple imperfect and incomplete bits of evidence. The social scientist, in this account, is more like Sherlock Holmes than like a chemist in a lab.

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Comparative historical sociology and liberating social changes in the last two centuries

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I suspect I'm not the only one in this symposium who brings up Marx's famous saying that "philosophers only interpreted the world, in various ways; the point is, to change it." To be sure, what he means by philosophy is more about sociology, a discipline he helped found inadvertently. Marx analyzed the historical development of capitalism and class struggles, and he illustrated how comparative-historical knowledge can aid the analysis of the interplay of class forces in his numerous commentaries on current events around the world, with the *Eighteenth Brumaire* and *Civil War in France* as the most well-known ones. These analyses did not necessarily directly lead to actions all the time, but were important stepping stones toward dispositions taken by the international working class movement that he and Engels led at the time.

Guiding Strategies and Policies

Marx's linking of historical sociological knowledge to political practice was not restricted to proletariat revolutionary actions