Strategy or serendipity?

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In 2014, violence and political disarray in the Middle East prompted experts in international relations to debate a new question: "Is 'Don't do Stupid Stuff' the Best Foreign Policy?"[1] This policy slogan, allegedly first formulated in less family-friendly language, captured the stance of the Obama Administration as it grappled with the partial collapse of the Syrian regime and the rise of new insurgencies. But, taken utterly out of context, this phrase also offers a plausible standpoint for comparative historical sociologists who think that their scholarship might play some role in making the world a better place. Restated in Max Weber's more restrained language (2009: 151), how might our scholarship help publics and policy makers "gain clarity" and wisdom, however incrementally and contingently?

This question requires thinking expansively about how scholarly work influences policy (Steensland 2008). In broad brushstrokes, policy-relevant research may address variations across interventions; the relation of outcomes to more encompassing policy regimes; or they may theorize alternative models of social organization and process. Further upstream, scholars contribute arguments and alternatives, sustained by normative justifications and empirical analysis. Influence may take the form of either "policy activism" (arguments in favor of a specific position adopted for normative reasons) or contributions to some body of knowledge about a phenomenon that informs proponents despite their policy preferences. Questions about the potential contributions of comparative historical sociology may be posed at any of these levels: the assessment of specific treatments, the analysis of policy regimes, and the development of alternative models.

Which of these paths seems most promising for comparative historical sociologists? In the first installment of this symposium, many bright lines were drawn, many perils identified (*Trajectories*, Winter 2016). As George Steinmetz warns, an orientation to policy may reinforce administrative demands to document the impact of our scholarship in a metric tightly coupled to present political concerns. Monica Prasad and Mathieu Deflem address the dangers of allowing policy advocacy to drive inquiry. For Greta Krippner, the Great Recession that demanded immediate policy intervention served a different scholarly purpose. She delayed publication in order to ask whether her historical project, rooted in a debate over the 1970s crisis of the welfare state, could illuminate the present. Yet, despite her cautions with respect to policy relevance, Krippner's *Capitalizing on Crisis* (2011) exemplifies one of Prasad's opening claims: "comparative historical sociology has always been most intellectually vibrant when it has been most explicitly engaged with questions of public purpose" (Prasad 2015: 1). How does the intent to be policy-relevant come to be decoupled from the production of scholarship that has the capacity to make publics and politicians wiser?

On the way to addressing that question, I advance three claims. First, comparative and historical sociologists should not avoid policy-relevance on principle, even as we recognize the dangers in research driven too directly by our own policy preferences or those of state authorities and corporate funders. History and comparison play important roles in policy debate; we all have a stake in the quality of that scholarship. Second, scholarship and policy operate on different clocks. Therefore, comparative historical sociology is likely to be at its best in "problem-illuminating" rather than "problem-solving." But because the most powerful scholarship identifies *how* things come to be, such research also contributes, at least potentially, to the wisdom of those engaged in present-day problem-solving. Finally, in order to produce scholarship that illuminates key problems of the extended past, present and future – what some San Francisco thinkers term "the long now"[2] – decisions about research need to go beyond existing theory debates and methodological concerns to address problem choice. Comparative historical sociology is often defined by its commitment to "big questions," but we need to think hard about the nature of those big questions for our own time.

When policy science turns to history and comparison

As with so many questions, a change of perspective clarifies the stakes. Rather than diving into methodological and meta-theoretical debates, it is useful to notice when and how social scientists engaged in policy-relevant debates turn to comparative and, specifically, historical inquiry.

In some cases, archival data may constitute "found experiments" that speak to the design of policy interventions. For example, Robert Sampson and John Laub (1996) linked a 1930s study of adolescence and delinquency to data on military service as well as later outcomes with respect to work and status attainment. Their findings demonstrate that "military service in the World War II era provided American men from economically disadvantaged backgrounds with an unprecedented opportunity to better their lives through on-the-job training and further education." In addition to the expected effects of access to the benefits of the G.I. Bill, the results point to the importance of overseas duty "as a crucial life experience because it facilitated the knifing off of past social disadvantages (e.g., poverty, deviant peers) and stigmatization by the criminal justice system" (1996: 364). Recognizing that what follows a turning point varies historically, Sampson and Laub temper their claims for the potential of large-scale interventions in the life-course, highlighting the "interaction of turning points with the varying structural locations and macro-historical contexts in which individuals make the transition to young adulthood" (1996: 365). The impact of overseas duty during World War II would have been substantially different absent the post-war economic boom as it operated through labor markets structured by gender and race. By relocating discussion about treatments from empty experimental time, such research explores how macro-historical variations in context shape the effects of specific interventions, a lesson that underscores the dangers of a too-facile linking of historical research to contemporary problem-solving.

A second path to historically-informed policy-relevant research hinges on the demonstration that key social facts vary over time – and are therefore potentially subject to policy intervention. Thomas Piketty's *Capital in the Twenty-First Century* (2014) provides an example of this kind of scholarly impact. Against ahistorical, functionalist arguments that attribute inequality to differences in merit, effort, or skill, Piketty and his collaborators documented long-term shifts in distributions of wealth and income, specifically the intensification of inequality in recent decades. Through complex interactions across the transnational Occupy protests, institutional politics, and social science scholarship, Piketty's work – along with that of many other scholars – has contributed to both a highly focused scholarly debate and a cognitive remapping of past and present distributions of income, wealth, and taxation. Felt grievances about inequality are now articulated by way of the abstracted *shape* of those skewed distributions which now operate as an iconic sign, invested with political meaning and scholarly authority.

In policy-related debates, historical research de-naturalizes the present and demonstrates that current conditions are the result of historical and political processes. Historians contributed to the legal briefs submitted in *Lawrence v. Texas* on the criminalization of homosexuality (Hurewitz 2004). Historians, sociologists, political scientists, and legal scholars have made similar interventions in the political understanding of mass incarceration as the product of a complex interaction of migration and policing, competition and conflict within and between racially-segregated communities, and increasingly polarized partisan politics (e.g. Behrens, Uggen & Manza 2003; Muller 2012). This research contests analyses that attribute rates of incarceration directly to race or income or education in the present, redirecting attention to past politics and institutions. The goal is not purely critical. By understanding the interaction of contexts, events, and processes in the past, such scholarship has the potential to inform future policy interventions. Although the choice of research topic may well be rooted in normative concerns – for greater social equality, for a fair system of criminal justice – the practical professional task is to understand complex causal sequences through rigorous empirical analysis.

Since we do not know what the future will bring, almost any piece of comparative and/or historical sociology may become policy-relevant (in the expansive sense of "policy-illuminating"). Whether our scholarship actually proves to be useful in sustaining wiser policy reflection in the future depends on qualities inherent to the work, what Art Stinchcombe characterized as its development of "historically specific general ideas" (1978: 4). This linking of specificity to generality, in turn, rests on the quality of analogies that are built up through careful historical analysis and then sometimes further developed through comparison to other cases as well as to already-existing sociological theory. Each analogy highlights a particular causal sequence or mechanism – elements that are undoubtedly salient to effective "problem-solving" policy (Prasad 2016) but are also central to theoretical understanding. Stinchcombe placed his bets on the first part of the process: "people do much better theory when interpreting the historical sequence than they do when they set out to do 'theory'" (1978: 17). But if one adds "policy relevance" to "better theory" as a desideratum for comparative-historical scholarship, then topic choice should also loom large in deciding on a research project or extended program.

Given the different tempos of politics, policy, and scholarship, it would be short-sighted to tie our work too closely to present concerns even if one took the position that all scholarship should have some policy relevance (a position that I am absolutely not advocating).

The future, of course, is unknowable. It may well be that the most policy-averse historical sociologist is currently immersed in a topic that is about to be made salient by events. In the 1980s heyday of comparative research on revolutions and state-formation and welfare policies, who would have suspected that the solo scholar digging deeply into the shifting and schismatic politics of the Crusades would have so much to say to policy two decades later, in the aftermath of 9/11? Problems of empire have attracted new generations of scholars as they have become unavoidable in rethinking global politics. As we look at the unexpected character of the 2016 presidential primary season, we can all benefit from comparative-historical research on populism even as many in the United States imagine that "it can't happen here." Given the different tempos of politics, policy, and scholarship, it would be short-sighted to tie our work too closely to present concerns *even if* one took the position that all scholarship should have some policy relevance (a position that I am absolutely not advocating). The key point is that historical sociology, whatever the initial motives of specific researchers, provides a store of topically-relevant hypotheses, provisional models, and theoretical generalizations that are ready to hand as we try to make sense of the present and future.

Deep analogies and where we might find them

Much of the best historical sociology is framed by grand theories keyed to the social transformations of the late 18th through early 20th century. Many of the traumas of the twentieth century are secondary topics in that canon – war, genocide – and the challenges of the twenty-first barely imagined (or found in less well-known essays that can be resurrected to give a new question the imprimatur of high classical theory). What sorts of historical sociology might be relevant to the "big questions" – climate change and poverty – that Monica Prasad (2015) singled out in her call to consider whether "Comparative Historical Sociology Can Save the World?"

On the first of these topics, we have the advantage of widely-recognized works – within as well as beyond sociology – that use comparative history to advance arguments for basic causal relationships. In his best-selling *Collapse*, Jared Diamond (2005) surveys the end stage of many societies, underscoring the dire consequences of the exhaustion of key natural resources. In a quite different comparative-historical study, Jack Goldstone (2002) began with questions about the different modes of economic growth that have been used to make distinctions between "premodern" and "modern" economies. Using cases ranging over a millennium, encompassing both Europe and Asia, he articulated a new concept of "efflorescence" or sudden surges in social productivity, innovation, and wealth. In the process of making sense of the pattern of these findings, Goldstone also made claims about the relationship of resources to trajectories of social change, specifically focused on the development of "engine culture" as a scientific and technological orientation within both intellectual and craft circles in 17th-century Britain. By reclassifying types of economic growth and remapping them through historical comparison, he argues that a new pattern of self-sustaining growth took hold in 19th-century Britain.

Presented in general terms, both arguments are straightforward: the exhaustion of resources is bad for many social outcomes, the discovery and exploitation of new resources supports new kinds of activity and discovery. But these basic claims can generate compelling questions and innovative strategies of case selection. Are there cases where the decline in a key resource leads to social innovation? This suggestion of an inverted-U of a relationship between strained resources and innovation might then be engaged with further cases. What do we learn from the times and places where an intensive practice of agriculture depleted the fertility of the soil or once productive mines played out? Or when vibrant industries – carriage-making, type-writer manufacturing, or perhaps even fossil fuel – are left behind by rapid technological change? What kinds of adaptation and politics follow disruptive moments of economic change?

Such a research-based conversation would make all of us wiser when it comes to thinking seriously about the implications of new energy technologies. Historical geographers, climate scientists, and economic historians are already pursuing such topics. Arguably (and, really, one does not need to argue terribly hard on this point), it would have been possible to have a more informed public debate about contemporary climate change if a body of engaging comparative historical sociology had been available from the start. Given the magnitude of the changes underway, it is not too late for dissertation projects to become books that will inform policy-makers in the 2020s and 2030s and beyond. The same is true of a host of other daunting challenges of the present that might be re-imagined as "big questions." In an era characterized by political dysfunction and gridlock, we need historical research on the diverse ways that democracies fail and, even more desperately, on how some have healed and regenerated. My own bet is that historical scholarship can move us beyond the stylizations of rational voters and pocketbook politics, illuminating how organizational complexity reshapes the terrain of political conflict and the possibilities for governance. Of course, the policy impact of such scholarship depends on the openness of policy-makers to analysis – yet another contingency that is unavoidable in answering the question posed for this symposium.

Because scholarship, events, and political will operate on different time scales, sociologists may wind up being "policy relevant" even if this was never their intent. At a collective level, there are good reasons to avoid a strong demand for policy relevance that ties our topic choices too closely to the present. Yet there are also moments where we can exercise "opportunism with good taste," recognizing that the salience of questions to broader publics or powerful elites constitutes an opening to mobilize research projects that have clear scholarly rationales. A public sense of the importance of a problem may signal that it is time to wrestle with topics and puzzles that push us beyond the historical referents of classical sociological theory and the canonical topics of comparative historical sociology. But whether one chooses – for reasons biographical, normative, theoretical, or inexplicable – to wrestle with a familiar question or to go where no comparative historical sociologist has gone before, the work necessary to produce compelling "historically specific general ideas" contributes to the store of understanding that may one day allow some public or politician to deliberate more wisely, to avoid doing "stupid stuff."

Endnotes

1.http://www.nytimes.com/roomfordebate/2014/09/02/is-dont-do-stupid-stuff-the-best-foreign-policy-30

2. http://longnow.org/about/

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The comparative historical sociology of W. E. B. DuBois

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At a time when comparative historical sociologists are discussing the relationship of our scholarship to projects of social reform ("can comparative historical sociology save the world?"), we can learn a great deal from one of our discipline's founders, who was both a comparative historical sociologist of breathtaking erudition and a deeply engaged scholar whose work was always oriented towards social transformation. I mean W. E. B. DuBois. Thanks to Aldon Morris's *The Scholar Denied* (2015) and maybe also thanks to Black Lives Matter, many American sociologists are discovering, or rediscovering, the sociological contributions of DuBois. Most of us probably already knew he was a pioneering theorist of race and an early urban sociologist. Morris makes a stronger case: not just that DuBois got there first, but that he influenced his contemporaries Max Weber and Robert Park, and thereby all of the rest of us who read Weber and Park. In a provocative review of this book, Julian Go (2016) has argued for restoring DuBois to a place of honor on our graduate syllabi.

I would like to make a case for putting some of his works on the comparative historical sociology syllabus in particular.

Putting some works by DuBois on the syllabus means reading them, of course, and if your graduate education was typical of graduate education in our discipline, that may mean reading them for the first time. The prospect is daunting: DuBois lived a long time and wrote a lot. His first book was published just three years after Durkheim's *Division of Labor in Society* and his last book was published just three years before Tilly's *Vendée*. For some years I have been slowly and with pleasure reading my way through this *oeuvre*, and I am only part way through. But I will briefly describe two of his works that, I have found, repay re-reading.[1] More than a decade ago, Zine Magubane (2005) argued for canonizing DuBois as a comparative historical sociologist; my purpose in this essay is to second that nomination, belatedly but enthusiastically, and to pile on even more reasons why his work ought to interest comparative historical sociologists today.

My exhibit A is *The Suppression of the African Slave-Trade to the United States of America, 1638-1870* (1986 [1896]). Is there such a thing as progress in moral standards? What does that progress have to do with economic development? Émile Durkheim posed these questions in his 1893 doctoral dissertation on *The Division of Labor in Society*, and three years later, W. E. B. DuBois answered them in his own dissertation on *The Suppression of the African Slave Trade*. DuBois didn't address Durkheim's work directly and probably hadn't read it. But he described his dissertation as "a chapter of history which is of particular interest to the sociologist" (1986 [1896]: 193) and he used historical comparative methods to answer questions about which Durkheim merely speculated. DuBois used systematic comparison across regions and time periods to test the competing hypotheses that moral suasion, political pressure (including legislation and the threat of rebellion), or