

from across years, decades, and even centuries, not from yesterday's newspaper, policy brief, or blog post. In fact, this is precisely the appeal of historical research. It is not that I am indifferent to contemporary concerns; it is rather that the significance of contemporary

I sense an inherent tension in Prasad's insistence that comparative historical sociology be "up-to-the-minute" (as engagement with policy requires) and that it also address "big" questions. In my view, the biggest questions tend to be those we discern from across years, decades, and even centuries, not from yesterday's newspaper, policy brief, or blog post. In fact, this is precisely the appeal of historical research.

events appears differently in historical perspective. It is over these longer expanses of time that it becomes possible to trace recurrent patterns, observe social institutions as malleable rather than natural, and acquire the necessary vantage point to reflect in a thoughtful way on our human condition.

My thanks to Monica Prasad for curating such a stimulating discussion!

Steinmetz's Reply:

I would like to start by thanking Monica Prasad for giving me the chance to respond to her response to my brief comment. I want to start by responding directly to what I think are the most obvious distortions or misunderstandings of my argument and then try to clarify my arguments on four specific issues: ethics; the philosophy of historical social science; the different models of sociologists' interventions

in policy and politics; and the slightly diminished appeal of historical sociology in very recent years.

Part of the problem here is located in the confusion between (1) policy, (2) the political field proper, that is, the realm of electoral and party politics, and (3) political practices everywhere other than the political field. The word politics cannot be restricted to action in the political field. Politics means practices directed toward maintaining or subverting dominant, hegemonic, doxic, or orthodox beliefs and practices in any realm of practice. This is why the word politics in the phrase the "politics of method in the social sciences" (Steinmetz 2005) is not a metaphor. Prasad misunderstands my argument when she writes that "we should not confuse ourselves by calling a quest for knowledge for knowledge's sake "politics." Indeed, I did not, since I wrote that "good social science, including historical sociology, is already contributing to 'saving the world' simply by existing." You will note that I do not use the word politics here, since this a different argument about ethics and human flourishing (*eudaimonia*) and about knowledge as part of this flourishing. Social science of this sort, as knowledge seeking, becomes political when the conditions it is trying to explain are oppressive and maintained through distortive systems of knowledge. It can denaturalize social institutions by revealing their historicity and arbitrariness, and reveal the hidden interests served by normative orders. Social science also becomes political when it is forced into defending its right to exist, as when governments and other institutions try to turn it into policy science.

Prasad writes that "scholarship that is not explicitly studying the major issues of the day is, rather, part of a machinery of pacification of the public that works to reinforce the status quo." This is fundamentally wrong. If you limit your attention to the present you will

never understand the present. If anything is likely to “reinforce the status quo” it is this sort of ahistorical presentism.

At a more basic level I am worried about the proliferation of radically different arguments in favor of a policy orientation in Prasad’s discourse both above and in her final word below. Policy orientations are presented as having an inherent claim to ethical goodness, disregarding centuries of cautionary tales about Cameralism (the “lessons of history”) and recent demonstrations in American foreign policy that action is often much worse than inaction. Her point about preserving autonomy has a hollow ring once we read (in her second response) that space can only be preserved for “historical sociologists who do *not* want to save the world” by kowtowing to the “concerns” of “outside audiences.” We should also pay close attention to her language of “collectively organizing the sub-discipline around trying to find solutions to big social problems.” Here again, the distinction between policy and politics is germane. This is a political goal and a hegemonic project.

The Relations between Ethics and Policy

Prasad misunderstands my argument when she writes that I think that “we need to have a thorough and complete ethical philosophy before taking action” or indeed, most absurdly, before even doing any historical research. I never argued that we need to have a complete ethical program before doing any research, but only before *intervening* politically in the more complex situations discussed. The idea that if we think about ethics we will remain inactive in the face of catastrophes is absurd, and this certainly was not my argument. First, ethics is inherently about seeking a guide to practical action rather than being separate from practice. There is no need, as Prasad argues, to have “historical research rather than ethical debate”—they are not mutually exclusive.

Second, much ethical judgment emerges from everyday practice, as many of the authors I cited argue, from Dewey to Keane. Third, some ethical problems (e.g. going to war, abolishing private property, creating a guaranteed minimum income, or extending human rights to animals) are too complex for spontaneous decision making and need to be confronted consciously and deliberately. Yet these are exactly the sorts of less obvious, more complex moral issues with which we are confronted, as humans, as social scientists, and as historical sociologists working on foreign countries and the past (which is also “a foreign country”) and on the confrontations or incommensurable value systems. Those who would reject ethical deliberation in these kinds of cases have to ask how they can justify their actions. The decision whether to invade a foreign country, for example, presents conflicting ethical imperatives: sovereignty and self-government stand opposed to whatever values are being violated by that foreign government.

Indeed, historians and historical sociologists constantly confront these complex ethical questions in their work. In much recent research on imperial and colonial history, for example, there is an implicit or explicit argument that colonial conquest and foreign rule are fully illegitimate. Yet from the standpoint of other human values, colonial takeover by a more advanced power may be a fairly unambiguous good in certain historical contexts. Another example, familiar from discussions of colonial legislative history, concerns interventions aimed at changing deep cultural commitments that foreign rulers find objectionable (polygamy, veiling, human sacrifice), as in the “Repugnancy” standards in British colonial law. These are the kinds of ethical questions for which there is no obvious, common-sense answer, and certainly no answer that can be extracted pragmatically by observing everyday actions. But surely we would not want to ban sociologists from dealing with these complex moral questions.

The fact that “ethical debates have no end” does not distinguish ethics from science, which also has no end.

Philosophy of historical social science

From the argument about simpler and more complex ethical dilemmas I turn to the argument that there is little that historical sociology specifically can contribute to solving these complex problems, due to certain ontological and epistemological peculiarities of social practice and historical social science. Solving problems is a different question from *explaining* historical events, which is something that historical sociology is very good at. The time span of causal regularities in the social world is usually short and the spatial area of validity for such generalizations is limited. Only a small subset of social practices takes the form of “demi-regularities,” that is, repeated conjunctions of causes and outcomes. I have long defended a philosophy of social science (critical realism) that argues explicitly that there *are* demi-regularities in the social world. But arguments for general laws of social practice are undercut by the time and space dependency of social causal structures and by the causal openness of the social as an emergent level of reality – i.e., the rainforestlike profusion of causal powers, interacting in ever new and unexpected ways. I have also suggested that the ethically most important social events tend to have singular, not repeatable, explanations. I have also argued that “unique” events are determined by causal mechanisms that are *not* unique—this is at the core of my methodological program of comparison across causal mechanisms or powers (Steinmetz 2004; 2014). Social science cannot *predict* these events, even if it can *explain* them retroactively. But this does not provide a formula for intervening in the present or future. Structure-changing events – those events worthy of the name, in Sewell’s (2005) view – are conjuncturally overdetermined,

contingent, and therefore unpredictable. The more historical the historical sociology, and the more focused on structure-changing events, the less suited it is for contributing to policy interventions. For example, I offered an explanation of the German genocide in Southwest Africa in 1904-1908 (Steinmetz 2007). Some of the key causal determinants in 1904 included dehumanizing racism, an underdeveloped international system of human rights law, and terrible social scientific policy ideas. These are certainly things that we can and should try to eliminate. But the idea that the same set of causal determinants might re-emerge in the same causal nexus at some future

Solving problems is a different question from explaining historical events, which is something that historical sociology is very good at. The time span of causal regularities in the social world is usually short and the spatial area of validity for such generalizations is limited. Only a small subset of social practices takes the form of “demi-regularities,” that is, repeated conjunctions of causes and outcomes.

date, leading to the same fateful results, stretches credulity. It would be presumptuous on my part to claim that my book might prevent future genocides. My own book’s political effects, if any, should be sought in discussions of historical responsibility, reparations, and collective memory. We should certainly try to eliminate racism and bad social science and to strengthen international human rights law, but understanding the ethical importance of these goals does not really require a historical study of genocide. I am not suggesting that historical research is useless, but that its practical deployment will be

predicated on ethical values that are generated outside of the historical research.

Prasad does not actually address the epistemological and ontological issues I broached in my comment. Since I have no way of directly assessing her position on these methodological questions I will try to distill them from her example of a study of human trafficking. Prasad juxtaposes her ideal approach to this project with one that “does nothing more than describe a situation” or that limits itself to studying “the comparative discourses of human trafficking.” By setting up her ideal project in this way she avoids confronting the alternative sort of social science that is actually on the table. No one is talking about projects of “mere description” or “mere discourse.” These fictional alternatives represent two of the perennial targets of positivist disdain in American sociology, but they are completely irrelevant in the current context. Historical sociology since Max Weber has been descriptive, hermeneutic, and explanatory all at the same time, and historical sociology nowadays is all of this as well as being engaged with normative questions. Prasad rejects “description,” yet theory development and causal explanation both involve description. She also rejects the study of “discourses,” but discourse is *causally* efficacious in social life. Surely these swipes against culture and description have nothing to do with saving the world! My point is that policy research within sociology carries with it a lot of additional epistemological baggage.

Three main ways in which sociologists intervene in policy and politics

Sociologists intervene in policy in three main ways. To understand these different approaches we need to stipulate a basic distinction between the field of science and the field of politics, discussed above.¹

The first model involves the sociologist acting as a human being, citizen, or political actor. Much of what gets called “public sociology” is simply political action by sociologists. There is nothing wrong with that; we are not Prussian civil servants. Still, this political action should not be called sociology, unless it draws explicitly on sociological research. I think that Prasad agrees with this, since she argues against “an *activist* focus in our scholarship,” by which she means “the kind of scholarship that already knows the answers before any research is actually done.”

The second model involves sociologists intervening as behavioralist policy experts. This is the dominant approach to “problem solving” or policy-oriented work in US social science. It is based on a faulty understanding of the ontology of the world. But that does not prevent behavioralist ideas from being built into public policies (Appelbaum 2015). These policies themselves provide a fertile field for critical historical analysis (e.g. Solovey 2013).

The third model involves the sociologist as *specific intellectual* (Foucault 1976). This idea is predicated on strengthening the autonomy of social science (Lebaron and Mauger 1999). As Bourdieu (1989: 105) argued, the “struggle for autonomy is ... first and foremost, a struggle against those institutions and their agents which, from within their disciplines, introduce dependence with regard to external economic, political and religious powers.” Only on the basis of such autonomy is a coherent political or policy intervention possible—one that does not degenerate into political prophecy or technocratic managerialism. As long as Prasad’s “problem solving” policy research takes this form, I have no problem with it at all - as long as it does not attempt to displace other forms of historical research.

Explaining the diminished appeal of historical sociology

Prasad is worried about the effects our discussion will have on graduate student morale (see her second response below). It would indeed be a cruel irony if the dramatically increased quality of historical research in sociology during the past two decades² were partly responsible for its apparently diminished appeal to graduate students. But in fact, these diminished numbers have nothing to do with anyone discouraging policy-oriented work: just policy work of a

The call to turn historical sociology toward policy research represents a clear and present danger to the autonomy of that subfield. With limited resources, the less “useful” forms of historical sociology will be the first to disappear. The effects of a concern with saving the world, however well intentioned, may reinforce managerialist pressures to make knowledge serve efficiency and utility.

certain behaviorist type. The two international intellectual communities to which I am most closely tied – Bourdieusian sociologists and critical realists – have very distinctive approaches to the question of policy research and very clear critiques of the versions of policy work that predominate in the sociology discipline. Above all, they both have a different understanding of the political aspects of critical social science. Political and policy-oriented do not necessarily mean the same thing. That is the discussion we should be having.

Rather than turning toward policy, historical sociologists should explore different forms of politicization. They will find that work on policy can be profoundly apolitical.

The diminished numbers of historical sociologists, as everyone active in this subfield knows, has to do with completely different factors. One has to do with changes in universities, including the increased pressure on students to get through PhD programs quickly, the orientation of undergraduates toward other fields, and explicit managerial pressure to produce useful, policy-oriented research. By encouraging graduate students to move in these directions we are hastening the demise of historical sociology.

A second reason is more internal to the sociology discipline. Historical sociology never broke decisively with the neo-positivist approach that made it seem impossible to study unique events, crises, and discontinuous or non-progressive histories while remaining a sociologist. These ideas block historical sociology’s access to the most politically pressing historical topics and focus attention on the ideas of repeated events, causal regularities, and causal laws. One would think that these ideas of causal regularities would be more aligned with the interests of managers than with the concerns of “idealistic young students.” The policy orientation that Prasad recommends is likely to harden this managerialist epistemology and in so doing to diminish the attractiveness of the subfield.

The third reason for the putative crisis of historical sociology has to do with the presentist regime of historicity that prevails globally (Hartog 2013; Spiegel 2014). The “loss of a vision of the future” in our time is directly related to the “neglect of the past” (Spiegel 2014: 165, note 53; Hartog 2013). Encouraging students to ignore the past is not just inimical to the very existence of historical sociology. It actually undermines students’ ability to imagine “changing the world.”

This was very different in the past, even as recently as a decade ago. Ancient Rome

seemed urgently important in Weber's Wilhelmine Germany, or for that matter to people concerned with American imperialism after 2001. Revolutions in 19th century France seemed to speak to concerns about the shape of the world among budding social scientists in the United States during the 1970s and 1980s. Precolonial Africa seemed critically important to sociologists in the decade after independence. The history of the fields of literature and art seemed relevant to Pierre Bourdieu in the 1980s and 1990s because of the erosion of artistic and scientific autonomy by the neoliberal policies analyzed by Prasad (Bourdieu 1989, 1996, 2013; Prasad 2006) and also because of the possibility of reanalyzing them in terms of his mature theory of social fields - a "contemporary concern" within social theory. This contemporary concern led Bourdieu inexorably back into the past, to an historical focus on the genesis and transformations of the artistic field.

Conclusion

Scientific autonomy should not be located only at the scale of the discipline or the individual but is also urgently important for the health of sociological subfields. The call to turn historical sociology toward policy research represents a clear and present danger to the autonomy of that subfield. With limited resources, the less "useful" forms of historical sociology will be the first to disappear. The effects of a concern with saving the world, however well intentioned, may reinforce managerialist pressures to make knowledge serve efficiency and utility. Historical sociologists should not conform to the taste criteria of the discipline as whole but should elaborate and defend their own criteria of judgment.

Notes

1. On science as a field see Bourdieu (1975) and Lenoir (1997); on the field of politics see Bourdieu (2000).

2. The increased quality of historical sociology in recent decades is due in large part to greater, not diminished levels of "ambition" among participants in the subfield. Ambition is something that Prasad claims that non-applied sociologists lack, especially those concerned with culture and discourse. This denigrates the entire subfield of cultural sociology.

References

- Appelbaum, Binyamin. 2015. "Behaviorists Show the U.S. how to Nudge." *New York Times*, Sept. 30, B1.
- Bourdieu, Pierre. 1975. "The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason." *Social Science Information* 14(6): 19-47.
- , 1989. "The Corporatism of the Universal. The Role of Intellectuals in the Modern World." *Telos* 81: 99-110.
- , 1996 [1992]. *The Rules of Art*. Cambridge, UK: Polity Press.
- , 2000. *Propos sur le champ politique*. Lyon: Presses universitaires de Lyon.
- , 2013. *Manet: une révolution symbolique*. Paris: Seuil.
- Foucault, Michel. 1976. "La fonction politique de l'intellectuel." *Dits et écrits* vol. 3, n°184, p. 109
- Klingemann, Carsten. 2009. *Soziologie und Politik: Sozialwissenschaftliches Expertenwissen im Dritten Reich und in der frühen westdeutschen Nachkriegszeit*. Wiesbaden: VS Verlag für Sozialwissenschaften.
- Kuklick, Henrika. 1991. *The Savage Within: The Social History of British Anthropology, 1885-1945*. Cambridge: Cambridge University Press.
- Lenoir, Timothy. 1997. *Instituting Science: The Cultural Production of Scientific Disciplines*. Stanford: Calif.: Stanford University Press.
- Prasad, Monica. 2006. *The Politics of Free Markets: The Rise of Neoliberal Economic Policies in Britain, France, Germany, and the United States*. Chicago: University of Chicago Press.
- Sewell, William H., Jr. 2005. *Logics of History*. Chicago: University of Chicago Press.
- Solovey, Mark, 2001. "Project Camelot and the 1960s Epistemological Revolution: Rethinking the Politics-Patronage-Social Science Nexus." *Social Studies of Science* 31(2): 171-206.
- Steinmetz, George. 2005. "Positivism and its Others in the Social Sciences." In George Steinmetz, ed., *The Politics of Method in the Human Sciences: Positivism and its Epistemological Others*, 1-56. Durham, NC: Duke University Press.
- , 2007. *The Devil's Handwriting: Precoloniality and the German Colonial State in Qingdao, Samoa, and*

Southwest Africa. Chicago: University of Chicago Press.

----. 2004. "Odious Comparisons: Incommensurability, the Case Study, and "Small N's in Sociology." *Sociological Theory*, Vol. 22, number 3, pp. 371-400.

----. 2014. "Comparative History and its Critics: A Genealogy and a Possible Solution." In Prasenjit Duara, Viren Murthy and Andrew Sartori, *A Companion to Global Historical Thought*, 412-436. London: Blackwell.

Prasad's Reply:

It's late November as I write this, and here are "yesterday's newspaper" headlines: Turkey shoots down a Russian fighter jet, Syrian refugees continue to flee Daesh, and the pharmaceutical industry consolidates. Nothing comparative or historical about any of that! Indeed, the "big questions" are the ones the whole world is struggling with, and therefore exactly the ones always in the newspaper headlines. As for the slow pace of comparative historical research vs. the fast pace of policymaking, I am not as worried as Krippner, because most of the real issues (how do you solve poverty? how do you bring peace to the Middle East?) are not going away any time soon, and allow plenty of time for relaxed reflection.

There is some slippage in how Krippner talks about "norms." Her project as she describes it was to explain what financialization was, and how it happened. But these are not questions of "ultimate ends" at all, of the kind that she says CHS is best suited for. Rather, norms only come into it at the stage of explaining why and how financialization happened. I suspect scholars would be better off doing what Krippner does (examine the causal origins of a broad, policy-relevant concern, with norms as one possible causal factor) rather than what Krippner says (focus on the "ultimate ends").

Steinmetz says "action is often much worse than inaction," referencing recent American foreign policy. Of course we may come to the

conclusion that inaction is the best course - that is a policy conclusion as much as any other. What is not warranted is inaction because we're perennially trying to figure out our complete ethical philosophy (because we'll never get there), or because we are afraid of unintended consequences (because inaction can also have unintended consequences).

Steinmetz says "surely we would not want to ban sociologists from dealing with these complex moral questions." I outlined a procedure for addressing complex moral questions in point 3 above, one which applies equally well to the new round of "complex issues" Steinmetz invokes. If you think that colonialism did both good and harm, identify why the local powers were not able to bring about the good themselves, or what we can learn now about how to realize the good and minimize the harm. If you are a feminist but worry about imposing your views on other contexts, identify the indigenous feminists in your fieldsites (there are always indigenous feminists) and investigate the history of why and how their voices are mobilized out of politics, while anti-feminist voices are promoted. It is not necessary to resolve these issues theoretically to move the debate forward empirically - indeed, the more complex an issue, the less likely it is to admit of theoretical resolution no matter how long one studies it, and the greater the need for an empirical path forward.

I suspect on most of these issues my differences with Steinmetz are not actually so great, but there are three areas where there are clear differences. First, Steinmetz suggests that lessons from history can only be drawn if and when *everything is the same* as in the original episode, giving his own work as an example. But it is an empirical question whether genocides emerge only under the exact set of factors Steinmetz identifies. In fact, comparative analysis of genocide might find