Trajectories

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Should Comparative Historical Sociology Save the World?

As part of this year's "Can Comparative Historical Sociology Save the World?" thematic focus, in this issue George Steinmetz, Mathieu Deflem, Greta Krippner, and Monica Prasad discuss whether adopting a policy orientation in scholarship is a good idea.

Historical Sociology, Ethics, Policy, and Politics

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Historical sociology is barely able to reproduce itself, given the intense presentism and methodological nationalism of most sociology today. So one might wonder, after reading the prompt to this debate, how could historical sociology possibly contribute to saving anything, let alone "the world"? Despite this initial skepticism, I think that the question posed here about the relevance of historical sociology for politics and policy is an extremely important one, as long as we do not read it as a command that all historical

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sociology must be "relevant" or demonstrate an "impact" on the extra-academic world. I would also like to add an additional keyword to this discussion from the start, one that is inseparable from policy and politics, namely "ethics."

I will try to approach this question by discussing the "is" and the "ought" of social scientific conditions of production and the possibility of research that is relevant to policy, politics, and ethics. With regard to the material and ideal conditions of research, we should recognize that sociology has continually been pulled into dependency on public and private clients and that it is never entirely immune from broader intellectual trends. For anyone interested in improving the scientific quality of sociological research, resisting pressures to instrumentalize sociology or to make it immediately useful for political, economic, and social clients is an urgent and ever-present problem. The history of universities and science policy provides at least one unambiguous lesson: the conditions for carrying out non-applied research are tenuous and fragile.

This is not to say that sociologists should erect an insurmountable wall between their work and politics. But what sociologists need to understand first is the relationship between their research activity and their activity as a *zoon politikon*. We may no longer accept Weber's resolution of the "conflict over values," and Weber probably would not expect us to do so, since he explicitly connected his analysis to the historically specific conditions of the Wilhelmine university, with its deeply conservative professoriate.[1] But Weber made a very useful distinction between professors' political activities in the classroom and their political activities in the political arena. Pierre Bourdieu also argues that the scientific, political, and state-administrative fields are relatively autonomous from one another. But Bourdieu rejects the "escapism of *Wertfreiheit*" and insists that autonomy and engagement can be combined. Indeed, he writes, "you have to be an autonomous scholar who works according to the rules of *scholarship* to be able to produce an engaged knowledge" (2002: 472-3, 465).

Another argument I will make is that policy research is just one of the ways in which social research is political, and it is not necessarily the most important or effective one. Merely telling the truth about the social world is already a critical and political act. Why should we assume that sociological or other scientific research is not already part of a desired outcome in and of itself? To clear up this misconception we need to redefine political progress and human "flourishing" (more on that below) to encompass knowledge creation - including social and natural science and other kinds of intellectual and artistic knowledge. In its disparagement of scientific autonomy and such diverse phenomena as authorial voice, narrative, and complex, contingent accounts of social events (Healy 2015), US sociology often echoes the raging anti-intellectualism of American society.[2] Even social science discussions of utopia have been loath to recognize scientific, intellectual, and artistic creation as an integral part of any desired society. In a very real sense, good social science, including historical sociology, is already contributing to "saving the world" simply by existing. That may not be enough, but for sociologists that has to be acknowledged in all discussions of these matters.

The Demon of Cameralism

Over the course of its prehistory and disciplinary history thus far, sociology has faced much more pressure to prove its usefulness than to be "value free" or critical.

The most direct ancestor of sociology is Cameralism, also known as *Polizeiwissenschaft*, meaning police or policy science.[3] The origin of much proto-sociology in practical policy already tells us something important about the historical connections between social science and policymaking. This also helps to explain why the period in which sociology was founded as a university discipline in Germany, France, and the US involved efforts to hold politics and policy science at arm's length.[4]

Nonetheless, the Cameralist demon has resurfaced again and again. As the meticulous research by Carsten Klingemann has shown, sociology in Nazi Germany was essentially applied policy research, both within the Nazi "Homeland" and in occupied East and West Europe. Most of the German historical and cultural sociologists were driven into exile and those who remained turned to policy or went into "inner exile" (Steinmetz 2010a). If ever there was a cautionary tale about sociology and policy research it is this one.

Sociology has never suffered from a lack of emphasis on applied or policy-oriented research. The forces bullying sociology in this direction today are as powerful as ever.

American and European sociology were sucked into the applied policy machinery during and after WWII (Hollinger 1996; Calhoun 2007). The British and French sociology fields were reconstituted after 1945 around two expanding practical sectors, both of them dwarfing the older grouping of theoretical sociology. One group worked on practical questions like labor relations in the metropole, while a second group focused on the social and political problems of "development" in the overseas colonies (Heilbron 2015; Steinmetz 2013).[5] In the US, Lazarsfeld, Sewell, and Wilensky (1967) discussed sociology's relationship to its environment as one of scientist and "client." Gouldner argued that this dominant type of sociology, which he called the "N+I science," was providing American capitalism and the welfare state with expert solutions to "noneconomic social problems" (Gouldner 1970: 91-93). If sociologists' policy proposals were only rarely implemented, this does not gainsay the fact that they felt compelled to proclaim the applied value of their work (Steinmetz 2005).

Other extreme examples arose in Scandinavia, where sociology was dominated by applied research for the welfare state.[6] In Denmark professional sociology was mobilized by the National Institute of Social Research in 1959, and in 1986 all Danish sociology departments were closed down. The University of Copenhagen's Sociology Department was reestablished in 1994 with an official strategy emphasizing alliances with "policy-oriented and applied research institutions" and management schools (Kropp 2015, Ch. 4; Johansson, 2015). Most recently the British Research Excellence Framework (REF) called on sociology and all other disciplines to demonstrate their "impact" beyond academe (Shepherd 2009). REF echoes earlier British and French government efforts to channel science into promoting economic growth (Paul 1985; Gilpin 1968; Hull 1999) and represents the most coordinated assault on the autonomy of the human and natural sciences in any major democratic country since 1945. [7]

In sum, sociology has never suffered from a lack of emphasis on applied or policy-oriented research. The forces bullying sociology in this direction today are as powerful as ever.

The Politics of Autonomous Historical Sociology

Historical sociology (along with ethnography, cultural sociology, and social theory) has provided a partial refuge from the perennial calls on sociologists to demonstrate “impact” in order to be funded or taken seriously. This is not because historical sociologists are any less political or moral than other kinds of sociologists. Instead, historical sociologists tend to relate to politics in a different way. Like historians, historical sociologists remain alert to the role of contingency, conjuncture, and overdetermined complexity in accounting for the past, and most of them reject philosophies of science rooted in the idea of general laws, Humean “constant conjunction of events,” or universal models. Behavioralist approaches to policy questions are thus anathema to historical researchers. Most historical sociologists are interested in social transformations such as revolutions and the rise and fall of states, empires, and elites, rather than in the repeated series of events or so-called “demi-regularities” (Lawson 2013). And where a demi-regularity does exist, the historian frames it as a puzzle to be explained - a paradox of social reproduction - rather than seeing it as a normal state of affairs.

The *historicist* historical sociologist (Steinmetz 2010b) understands the social world as an open, overdetermined system (Bhaskar 2008 [1975]), while the Humean understands the social world as a system of empirical regularities. These alternative philosophies have important implications for policy science. Policy interventions based on the premise of social regularities are unlikely to succeed, except in the narrowest contexts, because of the openness of the social. Indeed, historical research reveals that social scientists rarely anticipate the ways their ideas are received and applied (Bourdieu 1991; Hauchcorne 2009), not to mention the downstream “unintended consequences” (Merton 1936) of their ideas (Steinmetz 2004, 2007). The only thing that is entirely predictable in historical social science is unpredictability. But this does not mean that social scientists should resign themselves to some vague middle ground of “middle range” phenomena. Instead they should investigate the relations between demi-regularities and structure-changing events.

Historical sociologists resist having their research questions, methods, or theories dictated by the fashions of the environing disciplinary field, by university administrations, governments, and foundations, or by prevailing “regimes of historicity” (Hartog 2015). This does not mean that social scientists can come up with their research agendas in complete isolation. But it is especially dangerous for sociologists to succumb to determination by the very external forces that are trying to reshape their work.

That said, historical sociology can be political in a variety of ways. We need to start by making an initial distinction between trying to “save the world” via policy research and carrying out semi-autonomous social research. All sociology is political insofar as it embraces a politics of knowledge as unmasking (*enthüllen*), or revealing the extra-theoretical determinations and functions of ideas, and a politics of “refuting,” which targets the correspondence between knowledge and its objects (Mannheim 1929; Hacking 1999: 53-58). There is nothing more political than revealing the truth about social relations and social institutions, including their causal underpinnings, effects, and pathologies (Carlheden 1998; Honneth 2012), as well as their arbitrarily naturalized historical origins (Bourdieu 2001).[8] Historical writing can be political simply by “giving voice to the voiceless, countering male and Western-dominated historiographies, handling the historical record with integrity” (Paul 2014: 362), or by entering into dialogic relations with past authors and actors (Day 2008:420; Gadamer 1975).

The political valence of historical sociology is perhaps most evident when it focuses on epochal transformations and massive social processes such as war, genocide, revolution, colonization, slavery, the formation or collapse of states and empires, transformations of elites, changes in regimes of social governance or capitalist accumulation, or disinvestment and abandonment of American inner cities. An historical analysis of slavery, colonialism, or the Holocaust, for example, is necessarily an intervention into battles around political memory and contemporary politics. Even if historical research can never discern general historical laws (since they are non-existent in the social world), it may be able to identify causal constellations in the past that should be avoided in the future. History *can* provide lessons, even if they are not simple ones.

Ethics and Historical Sociology

If Pierre Bourdieu already called for a combination of scientific autonomy and engagement, we need to add a third element to this program: an explicit ethical theory. In this section I want to move to the question of the ethical justification of action, including political action. This taps into an even more basic problem than the fact that sociologists cannot usually predict the likely effects of their policy interventions, which is that they usually cannot even *justify* the outcomes they are trying to obtain through policy. Social analysis may in some cases be able to help eliminate social "evil," but for this move to be rationally grounded rather than arbitrary, something more is required: an explicit ethical philosophy.

This is a complex issue and I don't have time to go into it in any great detail. What I want to do is to call attention to two different approaches to the ethical question among social scientists. The first of these is closely connected to Pragmatism. Here the analyst examines social norms that are actually practiced, or that have been practiced in the past. This approach seeks to describe the "ethical impulses, judgments, and goals" that are features of "everyday life" (Keane 2015:3; also Lambek 2015; Honneth und Sutterlüty 2013). If we follow Dewey's *The Public and its Problems*, for example, we may be able to reconstruct the ways in which democratic publicness emerges from cooperative divisions of labor (Dewey 1927; Honneth 1998). Historical research could be useful in describing the forms of normative reflection and practice that have actually occurred in different times and places. Habermas' *Structural Transformation of the Public Sphere* (1989), for example, describes forms of publicness that actually emerged in a particular time and place.

The second approach diverges from these broadly pragmatic approaches and starts from the argument that we need to be able to distinguish between "norms that are *merely socially practiced*" and norms "that are *morally justified*" (Honneth 2014a: 818; my emphasis). A pragmatist approach cannot tell us why we should prioritize egalitarianism, democracy, publicness, enlightenment, or any other ethical principles that emerge from actual human practice. Pragmatic orientations are of limited use in complex decisions involving questions like whether to abolish property, allow assisted suicide, or go to war (Walzer 1977), or in situations lacking the predictable regularities that provide cues for more routine ethical decisionmaking based on habit or "knack" (Hills 2015: 22; Gorski 2013: 550). More explicit intellectual ethical deliberation is especially needed for decisions about justice and beneficence (Hills 2015), for situations in which virtues do not have "a one-to-one relation with goods" (Paul 2014: 360), or in situations requiring us to weigh one set of goods against another. Empirical social science is of limited use in answering these kinds of questions.

These issues have been addressed by a group of philosophers known as Neo-Aristotelians, who focus on concepts such as *eudaimonia* (flourishing), goods, human virtues, capacities, and skills (see Zagzebski 1996; Hursthouse 1999; MacIntyre 1999; Annas 2011; Gorski 2013; Smith 2015). Some Neo-Aristotelians prize the fully virtuous person who "explicitly grasps why her action is right and … can always explain why it is right," for whom virtue is a honed skill (*techne*) rather than mere habit (Hills 2015: 9). Anyone can develop these ethical skills, but they take time, effort, and education.[9]

In short, we need to be able to establish that the outcome we are trying to attain is a good worth achieving. Only then can we try to identify the policies or variables that may produce that outcome. Explicit, deliberate, intellectually articulated ethical judgements need to take the place of procedures and decision rules based in instrumentalism, utilitarianism, mimicry, or tacit, habitual moral principles. Once we begin trying to connect our ethical goals to specific policy goals, historical social research can re-enter the fray. Historical research may be able to help us at this point to detect noxious social institutions and practices and to identify them as deserving targets of reform or elimination. But this leads to an additional question, which is how to go about eliminating the social conditions we have identified as immoral.

Sociologists as Scientists, Intellectuals, and Citizens

I hope it is clear by now that I am not advocating an apolitical or policy-free sociology. Nor am I suggesting that sociologists should behave like Prussian bureaucrats and abstain from political activity as private persons (Süle 1988). I am simply sounding a note of caution about the ways politics and policy have often been approached in sociology. I suggest that we should approach politics and science as distinct, semi-autonomous fields and investigate the causal paths linking them, in both directions (the effects of states and politics on science and the political effects of science).

I endorse two models of sociological intervention in policy and politics that stem from this basic distinction between the scientific and the political. One involves activity by sociologists acting as citizens in the political field. Second is the model of the "specific intellectual" associated with the traditions of the Durkheimian (Dreyfusard) sociologists, Foucault, and Bourdieu, and differentiated from the models of the intellectual as prophet, "total intellectual," or *doxosopher* (Bourdieu 1972). When sociologists intervene in politics as specific, autonomous intellectuals, their actions are based on their "specific competences linked to the exercise of their métier as social science researchers." This approach differs radically from both "the positivist ban on all normative interventions" (Lebaron and Mauger 1999: 297) and the policy science model in which "independent variables" are manipulated with an aim to producing a change in "dependent variables."[10]

I suggest that we should approach politics and science as distinct, semi-autonomous fields and investigate the causal paths linking them, in both directions (the effects of states and politics on science and the political effects of science).

This distinction between the specific intellectual and the sociologist-citizen is crucial because there are always emergencies that call on us to act before it is possible to engage in social research or ethical deliberation.

Conclusion: Treading Carefully

This paper has argued that there are several distinct disadvantages to a fullscale adoption of a “policy orientation in CHS research” - disadvantages both for historical sociologists and for the supposed beneficiaries of those policy interventions. There are more than enough cautionary tales to warn us against sending our subfield down this path. Indeed, writing the history of the human sciences, including their policy adventures and misadventures, should be a core part of historical sociology, along with an adequate philosophy of science and ethics. Historical sociology would benefit enormously from deeper connections to the history and philosophy of (social) science.

This does not mean that policy and politics should be off limits. But we should reject any implication that a policy orientation is inherently ethical or that policy recommendations should take the form of a “constant conjunctions of events” model of causality. We should adopt a radically different understanding of the relations between social science, policy, politics, and ethics than the ones prevailing in corporations and governments, which tend to demand research programs packaged in a scientistic way (change variable a, b, and c in order to achieve situation d), or among the enthusiasts of prophetic public intellectuals. We should recognize that one of the most urgent policy matters pressing down on all academic researchers involves policies around science and research, and that a central goal of science policy should be to insulate scientists from pressures to produce immediately applicable results. And we should remember that policymaking is just one of the ways in which social and historical researchers can be political and relate to the outside world, and not necessarily the most important one.

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Notes

1. Weber pointed out in 1917 that “the problems causing him to argue in favor of value-free science did not exist in the same sense 40 years earlier” (Josephson 2004: 205; Weber 1917).

2. Thanks to Greta Krippner for pointing this out to me.

3. Cameralism was most strongly associated with the Russian and Central European states in the 17th and 18th centuries but was also present in Portugal, Spain, Scandinavia, and Italy. On the history of Cameralism and its connections to social science see Small (1909); Steinmetz (1993); Lindenfeld (1997); Tribe (2006); Wakefield (2009); Michalski (2010).

4. See Deegan (1988), Bannister (1987), Rammstedt (1988), Glaeser (2014).

5. Raymond Aron is said to have described postwar British Sociology as “essentially an attempt to make intellectual sense of the political problems of the Labour Party” (Halsey 2004: 70). On the metrocentric half of the postwar French sociology field see also, in addition to Heilbron (2015); Borzeix and Rot (2010), Chapoulie (1991), and Paulange-Mirovic (2013). On the colonial half of the postwar French and British sociology fields see Steinmetz (2013; forthcoming).

6. On the close connections between Swedish sociology and social policy see Larsson and Wisselgren (2006); Wisselgren (2015).

7. “Thank God I got out,” writes one leading British historian who moved from the UK to an American university (Fernández-Armesto 2009). Not surprisingly, “impact scores” for the first year (2014) of the REF were “particularly high in the life sciences,” lower in the social sciences, and lowest in the humanities and arts (Jump 2015; Holmwood 2011).

8. For an excellent study of the ways Pierre Bourdieu and Abdelmalek Sayad construed their own autonomous social investigations in late colonial Algeria as a political intervention, see Perez (2015).

9. Smith (2010, 2015) and Keane (2015) combine these two approaches, using empirical research in psychology and other fields to generate what they think of as a universal palette of human virtues.

10. The same underlying ontology of constant conjunctions underlies recent behavioral economics (“Nudge Theory”).

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Obstinate Observations on Sociological Saving

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It can perhaps not surprise many an American sociologist today that my writing of this brief missive does not take place without some trepidation. Unless the march of the sociological profession over this past short decade has blinded even more than it has silenced me, readers will recall my publically voiced objections to the politicization of our discipline under the guise of the benign label of so-called public sociology (see Deflem 2005 for the gist of my position, and Deflem 2013 for a historical account). Whatever else the outcome of my interventions may have been, I must begin by noting that it brought about a disturbing measure of ridicule, especially in the once reasonably flourishing sociological blogosphere, even from those who should be colleagues.[1]

Changing the World

The question if comparative historical sociologists should save the world assumes that sociologists in general can as well as should save the world and that there is a world that needs to be saved. Let me forego the notion that there is anything special about comparative historical sociologists which sociologists at large, i.e., those professionals with other declared specialty interests, would also not share. Besides, as Durkheim was the first to remind us, all sociology is by definition comparative (Durkheim 1908; see Deflem 2007).

At the same time, it has to be understood that the very word 'world' assumes a distinctly comparative focus, including an outlook that is also international and/or global. From such a perspective, I agree that it is constructive, if not downright necessary and inevitable, for comparative historical sociology to ask big questions (Prasad 2015). A sociology focused on narrow questions that can be answered with great technical precision is as useless as one that is highly politicized. Yet, under the conditions of a comparative-historical sociology focused on the big issues that move the world, I would suspect that the location of the researcher is even more important and that, therefore, the danger of an imperialist attitude of comprehension can be added on to the arrogance embedded in a sociology that seeks to be more than sociology can be.

Sociology cannot be legitimately involved in challenging let alone changing the world. As I argued before (Deflem 2004), sociology is a science and as such should be involved in analyzing variation in reality in the social world. Among the special characteristics of sociology as a social science is the fact that social issues are deeply normative as well. It is for that reason precisely that a detached attitude is needed to engage in analyzing the patterns and dynamics of the social. No justice without truth. Truth-seeking in sociology should also not be selectively focused on some issues rather than others because of some political expediency, but ought to be solely rooted entirely in theoretical necessity. Challenging the world in any way shape or form apart from analysis inevitably leads us beyond the province of social science. This modest attitude is also part of the project of the Enlightenment as we know it since Kant. Doing more than sociology legitimately can do is not only irrational, but offensive as well to the many participants of public debates on social and political issues. Sociology might be useful to such debates, but such communications ought to be conducted with all due care and should always be guided by the notion that no matter of truth will readily be articulated in a singular position on any matter of justice.

I earlier also argued (Deflem 2005) that almost everybody today is, or at least appears to be, a public and activist sociologist interested in challenging and saving the world. Worse yet, I suggested, “opposition is not tolerated and not accepted” among these advocates, and that the strategy is to “pathologize the enemy while simultaneously idealizing the self” (pp. 3, 4). I am hopeful that my participation in this debate might be a sign that there is hope, at least in this one minor respect.

The Challenge of Sociology

Turning to the more specific question before me now, I begin by noting that doing sociology itself is interventionist in and of itself. Such is the nature of social inquiry and of communicating thereon in the public sphere, whether through publishing or teaching. Sociology is always a praxis that exists in the world or at least a part thereof (a part that nowadays indeed, if not wholly borderless, is surely much less bound by borders than ever before). All sociology is therefore also not only comparative and historical but public as well. The very roots of the science of sociology, ever since a Belgian scholar forced the introduction of that neologism, are part of the evolving project of modernity.

But as the question if sociologists should save the world as it is posed here is surely to be understood more ambitiously than a recognition of the praxis of sociology itself, my answer is at least as old as Max Weber’s and Emile Durkheim’s, and hopefully just as sound. I need not engage here in an undergraduate level exposition on value-neutrality, but do point out that my concerns remain entirely in opposition to an activist-oriented sociology, while I would whole-heartedly embrace any gains that can be made towards the development of sociological activism. The latter, however, is by definition not within the province of sociology but must remain among the responsibilities and rights of the participants in various public squares themselves.

Whether sociologists want to change the world in various ways or not, the point is to analyze it. It is therefore also that sociologists as sociologists should not be engaged in any form of world-saving activities. Doing sociology alone requires enough effort as it is and ought

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to be the only necessary challenge we can legitimately take on. To the extent that sociologists also relate to the world, more generally, as citizens, inhabitants, political animals, and societal actors of any and all kinds (as I think they should and, besides, inevitably do), they can no longer claim the authority of their professional identity. Besides, would it help if they did? Definitely not today given the rather low public esteem of sociology as a profession. Although I would still argue that the standing of sociology is a matter secondary to the more basic debate on the nature and objectives of sociology (Deflem 2005), the reality nonetheless is that today's sociologists interested in affecting policy or contributing to change have done themselves a tremendous disservice, on purely instrumental grounds of effectiveness alone, by their drift towards politics, activism, and whatever other forms of non-scholarly engagement they waded into. In any case, it would be a poor choice, on instrumental grounds alone, to become a sociologist if saving the world is what one has in mind.

Sociological Boundaries

The question if sociologists can save the world is perhaps not answerable at the present time because throughout the course of the history of the discipline there have not been enough moments of some consequence when a sizeable part of the sociological profession tried to engage in such saving. To be sure, the 11th thesis on Feuerbach was written in 1845, but its influence in sociology did not occur until the 1970s, and even then in mostly programmatic terms and not for long either (Manza and McCarthy 2011). The revenge of the crisis sociologists that began in the late 1990s and took on public form at least from 2004 onwards (Deflem 2013) has not fundamentally altered the inconsequential nature of this fashionable position. Otherwise, it would make no sense to even still have to debate the question today as surely the direction in the profession has otherwise been almost wholly one-sided towards the adoption of the stance that sociology both can and should save the world. Of note in this respect, I must point out that I was invited to this debate and agreed to participate even though I am no longer a member of the ASA (and, more regrettably, therefore also not of this and some of the other great sections in the organization).

The identification of sociologists today in can-save terms, once reserved for a few marginals of the profession or the odd disconnected celebrity intellectual, is clear, by example, from the presentations of sociological selves in our online world, where scholarship and activism neatly co-exist. The same disposition is also revealed from the presumptuous attitude organized American sociology displayed when the ASA passed various resolutions and took related actions on distinctly political issues, not on normative grounds, but on the basis of wholly unfounded assertions of fact, such as the fabricated incontrovertibility of the consequences of same-sex marriage. The generally favorable response to this at once normative and market-driven redirection of sociology is manifested in the number of memberships in the organization, attendance at the annual meetings, and various other gains in popularity.

But even bracketing for now the question of its professional desirability and scholarly possibility, there is little indication, as far as I know, that the can-save-the-world movement has actually accomplished anything even remotely resembling that which is aspired to. It is perhaps not just to be explained away as another folly on my part to note that the can-save sociologists have not even been able to save me. The reasons for the inability of today's world-saving sociology to fulfill its own ambitions are both logical as well as sociological and therefore need further examination than can be offered in the context of this essay. Yet, perhaps it is more than an authority argument to note that the late Lewis Coser once wrote that even when it was assumed that those in positions of power could be influenced by social scientists calling attention to certain social concerns and problems, "it would be an indulgence in unwarranted Comtean optimism to assume that such enlightenment will at all times be sufficient to alert them." (Coser 1966, p. 13). Neither subjective disposition of desire nor strategic location in the occupational structure will alter this situation. But what Coser could not realize is that such optimism nowadays is widely shared. Today everyone's a Comte. And nobody but a few (Smith 2014) are laughing at this religious project and the ridiculousness of its details.

Save Sociology

Among my efforts to counter the advent of public sociology, which has inspired much of what I have written here (even though those efforts failed), I used to maintain a website called "Save Sociology."[2] It is no coincidence that my saving objectives differed radically from the one proposed in Dr. Prasad's question, as indeed my contention was and remains that it is sociology that needs to be saved. My earlier statements on sociology as a scholarly praxis will have clarified the core of this argument. In the meantime, however, I must make an additional claim to also argue that many a contemporary sociologist would have to be saved as well. At any rate, what the public should be able to expect is not necessarily a world safe from sociology but, at the very least, a world safe from sociologists who, as the born-again priests of humanity, think they can and should save the world.

Notes

1. See Jeremy Freese's blog post, May 3, 2006, "scylla and charybdiss," http://jeremyfreese.blogspot.com/2006/05/scylla-and-charybdiss_03.html. The post contains a link to another blog post from the pseudonymous "Total Drek," who introduces a 'Mathieu Deflem Drinking Game.' Since deleted, the post is still available through the Internet Archive: https://web.archive.org/web/20070509120015/http://totaldrek.blogspot.com/2006/05/oh-hell-with-it.html.

2. "Save Sociology" website: www.savesociology.org. I maintained the site actively from 2004 until 2006, and have provided only some occasional updates since.

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Weber on Wall Street: Reflections on the "Policy Relevance" of Comparative Historical Sociology

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Can comparative historical sociology save the world? I must admit I accepted Monica Prasad's invitation to reflect on this question for *Trajectories* with some trepidation. The question is so deceptively simple that I wondered how it would be possible to say something that: a) hasn't already been said (with Marx and Weber having both famously entered the fray, albeit on opposing sides of the issue); b) isn't completely obvious (*of course, we can save the world, or we should at least try!*); and c) avoids the opprobrium of my fellow sociologists to the extent that I diverge from the expected answer (*what do you mean, we can't save the world? Nihilist!*). It seemed an impossible task.

And yet, as I mulled it over, I was compelled to take on this assignment precisely because I realized I'd already been thinking about Prasad's question for most of my professional life, even if indirectly. Like so many others, my initial attraction to sociology was in part a product of the same social justice concerns that I now see burning in the faces of my undergraduates, and my gravitation towards comparative historical sociology in particular was a reflection of my naïve (I'm tipping my hand here) belief that understanding the larger historical forces that shaped our world would provide tools for reconstructing that world. As I became socialized to professional academia in graduate school, the concerns that initially motivated my study of sociology gradually fell away, but they never disappeared entirely, and it was a foregone conclusion that my dissertation would in some way engage larger, public concerns.

My dissertation (and later book) examined the financialization of the U.S. economy in the decades since the 1970s. Suffice to say, this seemed an extremely policy-relevant topic. In fact, I have a hard time thinking of any topic that comparative historical sociologists have written on in recent years that appears more relevant to policy; and the policy salience of my subject matter only increased over the period I was writing first the dissertation and then the book (the Enron fraud was still reverberating as I finished the dissertation in 2003, and the final drafting of the book manuscript coincided with the mortgage crisis of 2008-2009). It may then be surprising that my answer to Prasad's question is a qualified "no." The qualifications I have to offer are of two kinds: First, my "no" refers specifically to the question of developing research that aims to be "policy relevant" in the sense of directly influencing policy outcomes. I will suggest that comparative historical work may possibly "save the world" in other, broader interpretations of that phrase (likely broader than intended by Prasad, however). Second, I do not intend to suggest that comparative historical work never influences the design or outcomes of policy, merely that achieving this influence should not be its *explicit* aim, for reasons I will explain momentarily.

The argument of *Capitalizing on Crisis* centered on the perennial tensions between democratic politics and market economies, and the way in which attempts to contain if not resolve those contradictions in the late twentieth century launched our society on a path that led quite inadvertently to the dramatic expansion of financial markets, with far reaching consequences that we are still coming to grips with as a society today (see Krippner 2011). More specifically, my book argued that the turn to finance in the U.S. economy originated in the state's attempts to avoid distributional conflict as the long period of postwar prosperity came to an end. In this respect, the turn to finance – or financialization, the term I used in the book – can be regarded as a kind of successor regime to the inflation of the 1970s.[1] When robust growth in the American economy stalled, inflation initially served to disguise this development, allowing Americans to feel richer than they in fact were and thereby avoiding distributional conflict. But only for a time.[2] Eventually, the jig was up, and as Americans' tolerance for inflation wore thin, policymakers faced the prospect of having to assume responsibility for directly allocating resources between competing social priorities. At each such juncture, policymakers made a fateful choice: they passed this unpalatable task to the market, first by deregulating domestic financial markets, then by tapping into global capital markets, and finally by innovating new methods of implementing monetary policy that allowed policymakers to conceal their responsibility for unfavorable economic outcomes. In each such case, the political cover offered by the market also involved a loss of control over policy outcomes, unleashing a dramatic expansion of credit, as well as introducing a great deal of volatility into the economy, both of which created propitious conditions for the turn to finance. In this sense, I suggested that the financialization of the U.S. economy was not a conscious policy objective, but an inadvertent result of the state's attempts to solve other problems.

Policy relevant, right? Well, yes and no. At the time I was finishing my book, the most salient interpretations of the financial crisis then roiling markets pointed to a series of policy mistakes in the early 2000s, and in particular to Alan Greenspan's failure to raise interest rates in response to what we now know was the beginning of a massive speculative bubble that would lift the real estate market to dizzying heights before shaking the U.S. economy to its foundations. This analysis was no doubt correct, and tellingly, my own long narrative of financialization ended with Greenspan's decision to defer to the markets in 2001. But my account also placed this consequential decision on a deeper, historical foundation, one that traced Greenspan's reluctance to lean against the financial markets to wider transformations in American political economy occurring over several decades. What became evident in this longer-term perspective was that the apparent prosperity of the 2000s rested not only on fragile economic foundations, but on fragile *normative* foundations, as well. In other words, it was not only the regulatory architecture of financial markets that had eroded over the decades since the 1970s, but also the capacity for constructing what Daniel Bell (1976) referred to as a "new social compact" delineating how burdens would be shared as our society confronted declining affluence.

My point here is not that we should necessarily privilege distant over proximate causes in explaining historical events; it is rather that different things come into view at different temporal scales. From the perspective of the

2000s, the financial crisis appeared as a series of regulatory failures; from a longer historical perspective, unresolved distributional questions – “how much should we spend, and for whom?” as Bell (1976: 278) announced – seemed to drive both the ascent of finance and its shocking collapse. Of course, as sociologists we should have an investment both in devising policies to fix a broken regulatory system and in addressing difficult normative questions about how we share resources in our society. But it is important to realize that these are not at all the same task, and in fact they require rather different intellectual dispositions.

My suggestion – it will undoubtedly be controversial – is that the particular disposition of comparative historical sociology makes it rather better suited to tackling the latter issue, and rather less well suited to the former. This it should be noted is more or less an inversion of Weber’s (1946) famous thesis that science cannot speak to the ultimate ends toward which our society should aspire, but given those ends, it can help to provide the means of obtaining them. How do I end up on the wrong side of Weber? When I think back on my own research, I started with neither ends nor means, but rather a mass of data: thousands of pages of transcripts of meetings of the Federal Reserve Board of Governors, hearings concerning nearly every piece of financial legislation considered in either House of Congress over three decades, archival materials collected at the Reagan Presidential Library, and a data series hand-compiled from Internal Revenue Service records allowing me to track the financial income of corporations from the 1950s to the present. Deep immersion in these and other sources allowed me to begin to see recurrent patterns and to understand the way in which the social and political dilemmas of the 1970s had been displaced far into the future, with the unsettling implication that the tensions and conflicts that troubled that decade lurked just beneath the surface of our credit-stoked economy, ready to resurface at the first sign of trouble.

What was clear to me then, and is equally clear to me now, is that had I attempted to closely follow efforts to reform the financial sector as the crisis was breaking, I could not possibly have written the book that I wrote. This is only in part a function of limits on time and energy, although certainly the demands of intensive historical work and policy design in a technically complex domain like finance are difficult to balance. It is rather a matter of intellectual temperament. In this regard, I did follow the events surrounding the crisis – and even delayed finishing the manuscript so that I could reflect on these events in the conclusion of the book – but I did so not to guide any immediate intervention but rather with the goal of understanding how the historical narrative I had constructed could illuminate the nature of the unfolding crisis. The conclusion I drew from this exercise was that the most fundamental questions raised by the crisis – questions concerning distribution – would not yield to purely technical fixes. Critically, this statement should not be taken as diminishing the importance of remedying the flaws in the regulatory architecture of financial markets – a

As sociologists we should have an investment both in devising policies to fix a broken regulatory system and in addressing difficult normative questions about how we share resources in our society…the particular disposition of comparative historical sociology makes it rather better suited to tackling the latter issue, and rather less well suited to the former.

task that remains as pressing today as it was in 2009. But it must also be acknowledged that engagement in policy, tied to the imperatives of a fast-moving political process, does not in most instances permit careful reflection on our deepest human aspirations and the institutional arrangements most conducive to achieving them. As an interpretative social science that tends to work on a large canvas, pursuing complex, multiply determined social processes across contexts and over expansive time scales, comparative historical sociology is especially inclined toward such reflections. The worry is that as comparative historical sociologists become pulled into the exigencies of policy formulation – as invariably will occur, for the siren call of "relevance" is difficult to resist – we perhaps foreclose on the opportunity to make the contribution to "saving the world" that we are best equipped to make.

Notes

1. I explore the turn to finance in the context of the inflation crisis of the 1970s most directly in Krippner (2010). See also Wolfgang Streeck (2011) for an argument about the sequence of debt expansions that have followed the crisis of the 1970s that is convergent in important respects with my own account.

2. As Albert Hirschman (1980) observed, as long as inflation remained at relatively low levels, it served to dissipate distributional tensions. This reflected the fact that inflation created a game of "leapfrog" in which it was never totally clear who was winning and who was losing. For example, a trade union that obtained a favorable wage settlement momentarily secured an advantage, until these higher wage costs translated into higher prices, eroding the real value of the goods and services that the wage could purchase. Once these price increases became generalized across the economy, workers whose real wage had decreased would push for another wage increase, starting the process again. This cycle could repeat endlessly, with each group securing only temporary gains, and yet the sequence of moves and countermoves tended to vent distributional conflict (see also Goldthorpe 1987). Of course, once inflation increased beyond a certain threshold, the consequences of price changes for distributional outcomes became clear, and inflation exacerbated rather than eased underlying social tensions.

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Problem-Solving Sociology

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I thank Steinmetz, Deflem, and Krippner for engaging with the question so thoughtfully. I invited seven or eight people to participate in this conversation, hoping to get a debate going, but the only three brave souls to fulfill the assignment have all responded in the negative. It thus falls on me to defend the affirmative, although I hope that others will chime in at the blog discussion of this feature (address below).

Deflem and Krippner give two reasons why comparative historical sociologists might not want to adopt a policy orientation: adopting policy concerns will prevent us from taking the detached attitude that we should be taking as scientists; and CHS is best equipped to give long-term perspectives rather than policy relevant answers. Steinmetz's essay is more nuanced, but he also gives three clear warnings: we need to understand what we mean by saving the world before taking any steps to try to do it; sociology should not have its practices dictated by external concerns; and we don't have to worry about adopting a policy orientation, because we are already behaving politically simply by conducting research.

(1) Deflem's worry is that adopting policy concerns pulls us into a normative focus that draws us away from scientific detachment. This is an important worry. But cancer researchers who think cancer is bad do not have to lose their scientific detachment, nor should poverty researchers who think poverty is bad have to lose theirs.

I think the answer is to adopt a *problem-solving* rather than an *activist* focus in our scholarship. By an activist focus, I mean the kind of scholarship that already knows the answers before any research is actually done. (You know the kind of thing: "neoliberalism is increasingly colonizing marginal spaces" or "the words of such and such group both reflect and reproduce gender binaries" or "race is deeply encoded in public discourse.") Some activism in scholarship is perhaps necessary to call attention to neglected issues, but I am not sure that we need as much of it as we currently get.

On the other hand, problem-solving scholarship asks questions like: "Which states have managed to reduce carbon emissions, and through what mechanisms?" "When does racism decline?" "How can we prevent genocide?" "How have different countries tried to engage men in the project of women's liberation?" "Why have some countries been able to develop when others have not?" "How should we regulate finance so it doesn't blow up the economy?" "How do you pay for the expenses of a welfare state without losing economic competitiveness?"

Notice that you can only answer these questions if you adopt a detached stance. It benefits no one to get the answers wrong because you were emotionally invested in one answer rather than another. And notice that if done carefully, policy-relevant work has to consider and come up with provisional answers to the biggest sociological questions. You can't engage the problem of genocide without thinking through theories of social order. You can't examine development or the welfare state or carbon emissions or financial regulation without getting pulled into large questions about the evolution of global capitalism. Of course, no single piece of scholarship is likely to actually solve these issues. The test of a good piece of policy-relevant research is whether it brings us one step closer to solving it.

I think the answer is to adopt a* problem-solving *rather than an* activist *focus in our scholarship. By an activist focus, I mean the kind of scholarship that already knows the answers before any research is actually done.

(2) Krippner argues that comparative historical sociology is better at giving long-range perspectives than answers to immediate policy questions, but this seems to me a false dichotomy, as can be shown by the very example she cites, her own work on financialization. If financialization is indeed about the erosion of a normative compact (I have discussed elsewhere my doubts about this interpretation) the policy solution would be to

think through how and why norms erode and can be resurrected. It was not clear to me why Krippner thinks this is not a policy relevant conclusion.

(3) Steinmetz argues that we need to have a thorough and complete ethical philosophy before taking action. The problem, as anyone who has tried to follow an ethical debate knows, is that ethical debates have no end. There are many reasons why we might want to study or discuss ethics, but looking for a guide to practical action is not one of them.

Indeed, the problem is even worse than that, because while we spend our lives studying to become "the fully virtuous person who 'explicitly grasps why her action is right and … can always explain why it is right,'" we are not taking explicit action - and inaction is action. If children are dying from hunger on the streets of the world, if the climate is being destroyed, if people are being raped and murdered in wars and genocides, and if we decide not to do anything about all this because we have not yet thoroughly elaborated our ethical philosophy, we have, in fact, taken and acted upon a strong decision. We have decided to do nothing - a decision that is, itself, ethically ungrounded, and a decision that can also have unintended consequences.

But the situation is not hopeless, because even the most complex issues lend themselves to empirical research. Steinmetz mentions assisted suicide, abortion, and just war as issues that are so complex that they need ethical resolution before historical research can be conducted on them. I think these are exactly the issues that need historical research *rather than* ethical debate. We may not agree on whether or not abortion is murder, but we can examine the social and historical circumstances that prevent women who want children from being able to afford them, or we can study how to minimize unanticipated pregnancies. We may not be certain if a particular war is just, but even just wars create refugees, and we can try to understand how to integrate refugees into new societies, or how to get countries to honor conventions on refugees. We may see both sides of the assisted suicide debate, but we can still try to understand whether there are social problems that lead some people to despair of life.

By all means, study the issue for a few months, but if after a few months you still don't know which position is ethical, it's time to figure out how you can move the debate forward without having a complete ethical philosophy.

(4) Steinmetz's main worry is that if we adopt a policy focus, sociologists will lose autonomy, and have our concerns dictated by external criteria. But a distinction needs to be made between sociologists deciding, by themselves, to conduct policy relevant research, and sociologists being led by external agents to pursue policy relevant research. It is not clear to me why the former would lead to the latter. If anything, a strong autonomous infrastructure within the sub-discipline that allows scholars to collectively determine the policy issues they want to address would be one way to resist the external pressures that Steinmetz worries about. Lack of such an infrastructure leaves scholars who want to be involved with thinking through the main issues of the day, but who do not find this stance rewarded in the sub-discipline, nowhere to turn but into the arms of external agencies who do value this orientation.

(5) Steinmetz suggests that "good social science, including historical sociology, is already contributing to 'saving the world' simply by existing" and Deflem also seems to agree with this. There are several different ways to understand this claim: all scholarship contributes to creating a civil society, and a robust civil society is a prerequisite for politics; by keeping themselves separate from politics,

scholars create a space that is seen as above politics, and can therefore be trusted as honest brokers in the rough and tumble of political debate; and knowledge is valuable for its own sake.

As for the first, I am not sure how strong the “robust civil society” claim actually is. One could argue 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. As for the “honest brokers” argument, we have seen that even the most apolitical research, such as on evolution or climate change, is dismissed and called political when it suits the needs of the critics. As for the knowledge for knowledge’s sake argument, certainly there are many ways to live a worthwhile life, and serving knowledge is one of them; but we should not confuse ourselves by calling a quest for knowledge for knowledge’s sake “politics.”

> ***...a concerted effort to collectively organize the sub-discipline around trying to find solutions to big social problems is worthwhile, because my wager is that, whatever effects a policy-relevant comparative historical sociology might or might not have on the world, it will lead to* better sociology.**

Of course, the fate of most policy advice is to be ignored, and it is not clear that attempting to save the world will, in fact, lead to any actual improvement in any way.

But I would still argue that a concerted effort to collectively organize the sub-discipline around trying to find solutions to big social problems is worthwhile, because my wager is that, whatever effects a policy-relevant comparative historical sociology might or might not have on the world, it will lead to *better sociology.*

Consider a scholar who decides to study human trafficking in two countries. Without a policy orientation, she might be overwhelmed as soon as she enters the field, and when she gets her act together she will probably conduct some interviews with victims and some interviews with NGO workers, and produce a dissertation on “the comparative discourses of human trafficking.” Worthy, but not particularly exciting intellectually. The kind of thing that crosses our desks and inboxes every day.

Now consider if she enters with the idea of actually solving the problem. Doing so first requires her to think about who benefits from human trafficking. She goes forth and interviews these people - not easy to get access, but because she has known for years that this is what she needs to do, she has worked for months to find them - and with these interviews she has already taken the analysis beyond where most scholars leave it.

But because she wants to solve the problem, she can’t stop there: she has to think about what it would take for her interviewees to change their behavior. Perhaps her interviewees tell her they did not really choose this line of work, but fell into it. Perhaps they leave when better economic opportunities come along. Perhaps this makes her start to think about human trafficking not only as a crime, but also as an industry. This brings her to economic sociology, but she doesn’t always find what she needs there, and perhaps writes a paper about that (“Economic sociologists emphasize the actions of the state in creating and sustaining markets. But markets have always existed that the state does not condone, and which it sometimes actively seeks to suppress...”). She formulates a general sociological question:

what does it take to disrupt a market? She thinks hard about how sociological work on the creation of markets would have to be reformulated to think through the fragility of markets, and conducts a historical analysis of her two countries' policies using this framework.

Think how excited you would be if something like *this* crossed your desk. It is not going out on much of a limb to suggest that a piece of scholarship that does nothing more than describe a situation can always be made more interesting if pushed in a policy direction, because an orientation to solving the problem forces the analyst into a deeper confrontation with the issue - even if she abandons the search for solutions along the way because she discovers an intellectually exciting side path.

Finally, let me briefly address Steinmetz's observation that comparative historical sociology can barely reproduce itself. If this is true, surely part of the reason is that the idealistic young students who start sociology programs find themselves uninterested in a sub-discipline that does not seem to speak to their concerns about the shape of the world. A policy orientation will be good for the sub-discipline in intellectual as well as pragmatic terms.

Acknowledgments

I am grateful to Isaac Martin, Peter Evans, and Richard Lachmann for comments on earlier versions of this essay.

Deflem's Reply:

I thank colleagues George Steinmetz and Greta Krippner for contributing to this debate and am hopeful they are not embarrassed by my presence. Their ideas are divergent and not alone therefore useful. I leave judgments on the merit of their arguments to the reader, but offer a few reflections on our respective positions.

The position of my programmatic essay is reasonably close to, or at least compatible with, the one George Steinmetz articulates in admirably historical-comparative terms. In his demarcation of 'is' and 'ought' questions, Steinmetz rightly makes a distinction between policy and (different kinds of) politics and dutifully brings out the relevance of the Mertonian notion of unintended consequences. I was pleased to see Steinmetz bring up the case of the *Polizeiwissenschaften*, which has been the subject of some of my own research. In that connection I must make the ironic observation that many contemporary proponents of attempts to marry sociology with non-sociology are almost completely ignorant of the distinctions between policy sociology, public sociology, applied sociology, sociological activism, and so on. The notion of a '*gute Policey*' (good police), incidentally, does still today inform quite a bit of work in the balkanized field of criminology, an area that is, ironically mostly as a result of its policy-orientation, generally pooh-poohed among many mainstream sociologists. Steinmetz's comparative focus also lays bare the distinctly American character of the question that is before us. I agree that sociology is inevitably located in the political (and that the real problem is how this relationship should be developed) but also urge to think of sociology's location in broader terms to include the economic, the social, and the cultural, the latter of which I indeed understand in the most Durkheimian and Parsonsian sense possible.

Greta Krippner also tackles the question before us in a manner related more closely to the interests of the ASA section on Comparative and Historical Sociology than I do in my essay. She and others may be interested to learn that my initial attraction to sociology as an undergraduate was wholly dominated by a social justice orientation, one that expressed itself in rather radical terms as a self-defined militant activist (with a handsome police detention record to show for it). My academic existence thereafter, however, was formed precisely by the lesson of sociology to see and practice the value of analysis, irrespective of normativity and policy alike. As to Krippner's more specific position statement, I must admit I know next to nothing about finance, least of all my own, but I commend her for the modesty of her project. Nonetheless, her position would still allow for sociology as a whole to step beyond the boundaries of scholarship. Underlying her answer on the limits of sociology in terms of policy, I even detect a desire on her part to develop a policy-relevant sociology, understood perhaps as a collective enterprise. But the same questions remain. Can such an attitude rely on any sociology-oriented policymakers? And given the long life of sociology today, why are we still addressing the question that is here before us?

Krippner's Reply:

What is noteworthy to me about the preceding exchange is that the three invited essays all answer Prasad's query in the negative by offering almost completely distinct (and in some cases, mutually exclusive) arguments. Steinmetz suggests that attempts to directly intervene in policymaking threaten the tenuous autonomy of the subfield. We ought therefore approach policy warily, although Steinmetz is much more enthusiastic about comparative historical sociology's entanglements in politics, broadly defined. Deflem's negative reply to Prasad seems to be diametrically opposed to Steinmetz, suggesting that the appropriate disposition of the social scientist is one of detachment from politics and policy alike. My own negative response argues that comparative historical sociology's intellectual disposition is better suited to interrogating the normative questions that frame policy than it is to directly attempting to make policy interventions. In this respect, I think I side with Steinmetz in privileging comparative historical sociology's political entanglements over involvement in policy per se, but it appears that I do so by violating Steinmetz's stricture against advocating an insufficiently elaborated ethics. That three scholars can all disagree while agreeing suggests that Prasad's question is generative indeed.

To respond briefly to Prasad's comment, if by "policy relevant" we mean the larger normative frameworks that embed policy formulation, then I agree that comparative historical sociology can and should be policy relevant. But this is not what I think most sociologists have in mind when they discuss "policy relevance," which refers to something that deals with, in Weber's terms, the means rather than the ends of action. In this regard, I do not think I pose a "false dichotomy" since social science has been organized around the distinction between means and ends at least since Weber proposed a "value-free" (social) science. To reiterate my position, the key issue is that the longer temporal scales typical of comparative historical sociology bring us on to the terrain of normative questions regarding the ultimate ends of our society (as opposed to the techniques we employ to achieve these ends), and in this regard historical research can serve to clarify the nature of the problems that we might seek to address as politically engaged sociologists. As I suggested in my essay, this is in part simply a matter of what comes into view at longer versus shorter time horizons, but it also reflects the fact that historical research and policymaking occur at a very different pace. In this regard, 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 for me precisely the appeal of historical research. It is not that I am indifferent to contemporary concerns; it is rather that the significance of contemporary 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.

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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 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

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.

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 decades[2] 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 certain behavioralist 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.

> ***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.***

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.

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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 that "dehumanizing racism" is the dominant factor, or dehumanizing racism in the context of economic or political instability, and these are certainly causal determinants that are likely to re-emerge in the future. Whether any of these are subject to understanding or control, and precisely what is the spatial and temporal extent of "demi-regularities," are subjects for empirical research, not matters that can be determined ahead of time in order to dismiss a policy-oriented approach.

Second, Steinmetz's enemy is behavioralism, and fighting it is his main concern. For me, the debate about behavioralism is over in sociology, and the real danger today is rather a certain lack of ambition that plagues the discipline and the sub-discipline. CHS is the subfield in which a graduate student once sat down in her study and decided she would teach the world why revolutions happen. But a Theda Skocpol in graduate school today, enabled by excessively strong assumptions about the causal power of discourse, would surely set out to teach us only how people *talked about* revolution in France, Russia, and China. Ironically, it is this very lack of ambition that is crippling the battle against behavioralism beyond sociology: if the only suggestions on offer for how to reduce poverty or prevent climate change are behavioral ones, how can politicians be expected to favor non-behavioral approaches? A scholar who truly worries about behavioralism has to fight it in ways that will actually defeat it, namely by providing convincing non-behavioral alternatives to questions of concern to audiences currently in thrall to behavioralism. I suggested above that one reason for the lack of ambition in current CHS is that young scholars don't really know how to do more than describe, even if they want to do more. Therefore I suggest they ask "what would I have to know or learn if I actually wanted to solve this problem" as a heuristic to help them formulate deeper analyses of the world.

...comparative historical sociology flourished in the 1960s and 1970s because Barrington Moore and his generation of scholars showed that CHS was indispensable for an analysis of the two most important political questions of the day: the rise of fascism and the origins of communist revolution. These scholars made a space for CHS in the academy, and in that space others with less immediately relevant concerns could thrive.

Finally, Steinmetz worries that in moving in a policy direction we will lose space for comparative historical sociologists who do *not* want to save the world. But how do we preserve space for such sociologists? Precisely by convincing outside audiences (department heads, administrators, students choosing classes) that at least some of what we do is relevant to their concerns - especially because their concerns are, after all, also our concerns. Steinmetz seems at times to suggest that in order to preserve the autonomy of CHS, a scholar who wants to research why children are starving around the world must resist doing so, because someone external to the discipline might be interested in the answer and we must not "kowtow" to their concerns. My interpretation of the lessons of recent CHS history is very different. My read is that comparative historical sociology flourished in the 1960s and 1970s because Barrington Moore and his generation of scholars showed that CHS was indispensable for an analysis of the two most important political questions of the day: the rise of fascism and the origins of communist revolution. These scholars made a space for CHS in the academy, and in that space others with less immediately relevant concerns could thrive. If we want to protect the next brilliant George Steinmetzes, we have to nurture the next Barrington Moores.

A policy direction can bring the force of comparative historical methods to bear on important social problems, can lead to more intellectually interesting analyses than are currently being produced, and can broaden the subdiscipline by appealing to external audiences - and it is clear that many in the subdiscipline want to move in this direction. And yet, in reading these replies from prominent sociologists, I was reminded that one reason graduate students have such a hard time conducting policy-relevant research is that faculty advisers can be so eloquently dismissive of it. Nor is there much support from the section for scholars who want to adopt a policy orientation. Indeed, in re-reading this debate I see we do not even have a precise language for what we are discussing (policy-relevant, policy-oriented, problem-solving, real-world, engaged, “big questions”) and that may be the cause of some of the disagreement. The discussion this year is meant to address these problems. In the next issue of the newsletter, we present advice from several scholars who have successfully conducted policy-oriented comparative historical sociology.

Meanwhile, if you would be interested in helping us think through what an infrastructure of support and guidance for CHS scholars doing policy relevant research would look like, email me (m-prasad@northwestern.edu). And if you are a grad student interested in doing problem-solving sociology of the kind I describe above, but find yourself with an adviser who discourages this, also feel free to email me. We’ll work through your interests and figure out a way for you to approach your topic that will excite your advisers. And they never need to know that you are trying to save the world.

What do you think?

The debate continues online at: http://policytrajectories.asa-comparative-historical.org/2016/01/should-chs-save-the-world/

If you are interested in having your thoughts on this debate appear in the next issue of *Trajectories*, please email Matt Baltz (mjbaltz@ucla.edu). If you would like to be involved in the "Can CHS Save the World" effort, email Monica Prasad (m-prasad@northwestern.edu).

Book Symposium

What Unions No Longer Do

Harvard University Press

Jake Rosenfeld

Editor's Note: The following text is based on an author-meets-critics session that was organized by David Brady for the Annual Meeting of the American Sociological Association Annual Meeting in August, 2015. My thanks go out to Arne Kalleberg, Kim Voss, Evelyn Nakano Glenn and Jake Rosenfeld for agreeing to contribute their comments to the newsletter.

Comments on Jake Rosenfeld's *What Unions No Longer Do*

Kim Voss
University of California, Berkeley

Jake Rosenfeld's book, *What Unions No Longer Do* (henceforth *WUNLD*) accomplishes the difficult feat of being simultaneously rousing about the achievements of the American labor movement and realistic about its future. Too often books about American unions are either overly optimistic or relentlessly negative. In contrast, Rosenfeld's book strikes a near perfect balance in demonstrating both why unions have mattered so much for U.S. society and exposing the full, sobering consequences of labor's decline. Although I find the book to be bleaker in the end than is fully warranted by some of labor's recent campaigns, it is a major achievement that deserves widespread attention and debate.

WUNLD takes up critically important but rarely asked questions about the *consequences* of unionization: what effects have high and low rates of unionization in the U.S. had over the past 110 years? Many scholars have probed the reasons for labor's high and low points but Rosenfeld turns his considerable talents to investigating why labor's rise and decline *matters*. The book clearly and persuasively demonstrates that when union density levels were high (particularly in the private sector), the United States was a country of greater income equality and more racial equality in wages. It was also a country where immigrants were assimilated economically. In addition, strong unions produced a significant political voice for non-college educated workers.

Notably, *WUNLD* goes beyond just identifying the consequences of high unionization rates; it also pinpoints the *mechanisms* by which high levels of union density produced such positive outcomes. First, collective bargaining - backed by the threat of strikes - directly raised wages of union members in the private sector *and* had a spillover effect on the wages of non-union workers. Second, unions politically incorporated their members, elevating the turnout of workers in elections. Jointly, collective bargaining and workers' political incorporation combined to connect productivity gains to rising wages. Finally, unions provided a prominent and effective voice for economic justice in the U.S. In labor's heyday, unions used their voice to shape cultural understandings of what constituted fairness in the workplace, they used their political leverage to move policy in directions that addressed the needs of average workers, and they used their bargaining clout to deliver tangible rewards to non-managerial, nonsupervisory workers.

I found these arguments rousing because they are so well done. The heart of the analysis is a rigorous investigation of data from the Current Population Survey (CPS) for the years 1973-2009, yet Rosenfeld contextualizes the trends he observes with a masterful comparative-historical analysis. Especially noteworthy is the way he uses comparisons to reveal what other analysts miss. For example in the chapter on deunionization and racial inequality, he fully acknowledges the shameful history of racism in many unions, but he goes on to demonstrate how much worse it generally has been for blacks in unorganized workplaces. I also found it very refreshing that the book shows the profound equalizing effects of things that sound dry and technical like collective bargaining and bureaucratic rules. Rosenfeld makes the best case I've ever seen for the progressive effects of all kinds of unionism, even business unionism. Finally, Rosenfeld highlights the importance of labor's normative role, something that is too often missing in investigations of unions and their impacts.

But while the book is rousing in its excavation of the effects of high rates of union density, it is quite sobering in its analysis of union decline. Rosenfeld makes it abundantly clear that unions in the private sector today are far too small to serve like they once did as the core equalizing institution in American society. The power of the strike has collapsed and labor's political clout has fallen precipitously. The effects have been devastating for black Americans, for Latino immigrants, and for workers who lack a college education.

Moreover, Rosenfeld offers no hopeful pathway for turning things around. Indeed, he systematically counters the hopeful prospects offered by others. Many progressives and labor academics, for example, have argued that the relative success of unions in the public sector (where the unionization rate today stands at 36%) might counteract some of the worst consequences of labor's decline in the private sector. But Rosenfeld shows that the success of unions in the public sector seems to have plateaued with the recent attack of Republican governors on public sector unions. Moreover, his analysis of CPS data over the past three and a half decades reveals that a labor movement tilted to the public sector acts very differently from one dominated by the private sector. First and foremost, the public sector accounts for only a fifth of the American labor force, and given current political realities in the U.S., it is unlikely to grow. The public sector is also made up of a more highly-educated and better-paid labor force, which alters unions' historical role of representing those with comparatively low education and income levels. And tellingly, public sector workers are less effective at improving the working conditions and wage gains of their members. Most devastating for income inequality in the U.S., unions' influence in the public sector does not spill over beyond its own members to other government employees. Additionally, Rosenfeld shows that a labor movement tilted toward public sector workers is one that has fewer effects on political incorporation and voting.

Rosenfeld also undercuts the other great hope of labor activists and scholars: that the unionization of Latino workers might stem union decline. He dashes such hopes by revealing that the source of progressive hopes, the Justice for Janitors campaign, yielded only limited gains in union density. He notes that today, janitors are unionized at an even lower percentage than they were at the time of the first Justice for Janitors campaign. Additionally, he reports that far from being the leading edge of union revitalization, Latinos today are less likely to be union members than non-Latino workers. This is largely because the kinds of jobs most Latinos hold are structurally very difficult to unionize. Additionally, he argues, Latinos typically arrive with less pro-union attitudes than immigrant workers in the first half of the 20th century.

In his final chapter, Rosenfeld attempts to be uplifting, noting that this is not the first time in U.S. history that the labor movement has suffered such widespread decline. He draws a parallel between today and the 1920s, when employers relentlessly fought unionization, replacing striking workers with impunity, and the judiciary issued injunction after injunction. That was a period of enormous income inequality in America, too. Yet, a decade later, following the passage of the National Labor Relations Act, unionization increased immensely. Still, Rosenfeld finds little on the horizon today in terms either of what labor is doing or what's happening politically that will yield the kind of institutional arrangements that supported a robust labor movement in its heyday. As much as he tries to conjure up a reason for hope, the future looks very bleak at the end of Rosenfeld's book.

I'm not yet ready to succumb to this bleakness, however. Rosenfeld doesn't talk in this final chapter or much anywhere else in the book about *recent changes in labor's repertoire of contention*. As many social movement scholars argue, successful movements change tactics in response to changing targets. This has happened in the contemporary labor movement. As unions have moved away from the strike as a key tactic for all the reasons Rosenfeld highlights, at least some unions have moved in the direction of corporate campaigns and community alliances. Yet, corporate campaigns and community alliances as a source of leverage over private sector employers are not considered in the book. I'm curious about Rosenfeld's view of labor's changed tactics. Do corporate campaigns, for example, have any of the potential that strikes once had? Are they at least a part of a plausible path forward?

Similarly, I'm wondering what Rosenfeld thinks of the recent success of $15Now and other citywide and region-wide campaigns to

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raise the minimum wage. I'm interested in his answer in general and - more specifically - I'm wondering what he thinks about the central role of the Service Employees International Union (SEIU) in these campaigns. As I see it, SEIU is a union dominated by public sector workers that is building on its strengths and innovativeness to help improve the working conditions and wages of low-wage private sector workers through the $15Now campaigns. Do the successes of such campaigns in Los Angeles, San Francisco, Seattle, across the University of California system, and elsewhere change Rosenfeld's view of the potential of public sector unions to rescue the labor movement? This is one place where many of us find hope. Does Rosenfeld?

These campaigns also raise another issue I have with Rosenfeld's account. He notes that as unions declined in the private sector and ceased having a large effect on the wages of nonunion workers, they came to be seen more and more as a narrow interest group of privileged workers, which left unions vulnerable to political attacks. I believe that this is accurate to a degree but I also think that the critique of labor as a narrow interest group is more long-standing than the late 1970s, when private sector density began declining rapidly. Instead, the claim that labor is only a narrow interest group has been used repeatedly against the labor movement over the last century in the U.S. The reasons why is the subject of some debate, as a recent article by Barry Eidlin (2015) demonstrates. But labor itself no doubt has some responsibility for it. The arguments that labor unions articulate for themselves, how they act on those arguments, and the extent to which they tie the struggles of any particular group of workers to larger themes of inequality and democracy are important. Jennifer Chun (2009) calls such struggles "moral dramas." Making such arguments and engaging in moral dramas are agentic actions that unions can control. Rosenfeld begins his book by gently critiquing those of us who have tried to identify organizational blueprints that might alter labor's fate in the contemporary anti-union climate, suggesting that both the recent failure of social movement unionism to stem the tide of labor's decline, as well as the international picture, point to forces beyond unions' control as the reasons for labor's sorry state today. I have a hard time accepting this. Even Rosenfeld's own focus on the normative role of unions suggests that labor's fate might at least partially be in its own hands.

Over a hundred years ago, a member of the Knights of Labor made an argument to the U.S. Congress that may be truer today than ever, *"The political structure of this country is resting on a sand heap, owing to the degradation of labor."* Rosenfeld's book makes an invaluable contribution in demonstrating the key role unions have played in shoring up American democracy and fostering economic equality. It also is crucially important for documenting the multiple forces undermining unions' ability to play the same role today. Yet, as the Knights' member suggested long ago, if there is no organized force to counter the degradation of labor and no voices willing and able to make the link between labor's place in society and the political health of the nation, our democracy is indeed "resting on a sand heap." Let us hope that Rosenfeld's conclusions turn out to be bleaker than need be.

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Comments on *What Unions No Longer Do*

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As its title indicates, *What Unions No Longer Do* is about the decline of labor union membership. But it goes far beyond analyzing the *causes* of the decline by examining the *consequences* of the decline for American workers more broadly, and disadvantaged groups especially. The study is based on meticulous and sophisticated multivariate analysis of the Current Population Survey and other large data sets. Yet the text is deliberately written to make the findings understandable and compelling to non-quantoids, such as myself.

Jake's overall argument is that the decline in private sector union membership has had negative effects beyond worsening the wages and working conditions of private sector workers. In their heyday in the mid-20th century, unions were the main equalizing force to get some of the productivity gains that were achieved to be reflected in higher wages for all workers, not just unionized workers. Without union power to leverage political support, the government has retreated, leaving the field to employers, who have enjoyed record profits while reducing workers' share. Moreover, the decline of private sector unions has hurt the most disadvantaged workers the most, namely minority, female, non-college educated workers. For example, racial gaps in wages between white women and black women, which had shrunk to almost nothing by the late 1970s have widened considerably.

Jake elaborates on the consequences in five areas, each spelled out in a chapter:

1) Public sector unions do not have the same equalizing effect as private sector unions. Public sector employees tend to be more educated, and their gains don't lift wages for less educated private sector workers.

2) The political power of workers has diminished as unions have lost their biggest weapon against employers: the strike. Strikes and the threat of strikes gave workers great clout during the heyday of union membership. Today, strikes are rare and mostly unsuccessful.

3) The decline of private sector unionization has hit black workers especially hard because they were just starting to benefit from unionization when unionization started collapsing.

4) Labor unions play less of a role in fostering immigrant incorporation: In the heyday of unionization, when European immigration was also at its peak, unionized jobs gave new immigrants opportunities to assimilate and move up. Today's non-professional immigrants have little opportunity beyond the low paid labor market.

5) The decline in private sector union membership is related to diminution in civic participation and voting: unions used to mobilize workers to vote, but are less able to do so today.

As a scholar of labor, race and ethnicity, I would like to focus on two of these chapters, one on Blacks and the other on Latinos.

The chapter entitled "The Timing was Terrible" makes the point that after years of being excluded from major, i.e. white controlled, labor unions, African Americans were finally able to join unions during World War II because of the demand for labor in the defense industries. Their membership grew further in the 1970s with the enactment of fair employment laws. Once the racial barriers were broken, African Americans went on to have higher unionization rates than workers as a whole to the present.

Jake tests two general explanations for Blacks' high participation in unions:

1) A *positional theory* says that rates of union membership is affected by a group's position in the labor market. In this case, positional theory would argue that blacks are more concentrated in occupations/industries with high unionization rates (e.g. semi-skilled jobs in auto manufacturing.

2) In contrast, a *protectionist theory* explains higher African American unionization rates as a product of Blacks' desire for protection against discrimination, and dismissal, because of their

history of discrimination and insecurity.

In a rather Herculean analysis, Rosenfeld controlled for more than 50 variables that affect unionization rates and found that Blacks had higher unionization rates than whites net of controls. Rosenfeld attributes this unexplained portion of the variance to protectionism, and he concludes that that protection was a significant factor in explaining higher unionization rates among black workers, especially black women, who historically have suffered the greatest discrimination.

Chapter 6, entitled "Justice for Janitors," examines differences in unionization rates for Hispanics vs. non-Hispanic whites. The issue of unionization is critical because of the phenomenal growth of the Hispanic population; today, Hispanics constitute 1 in 7 American workers. Their increase has come at a time of decreased unionization (another instance of bad timing?)

Jake finds that in the 1970s and 1980s, Hispanics had higher rates of unionization than non-Hispanics, but that by the 1990s, they had lower rates of unionization. As in the analysis of variation in black rates of unionization, Jake tests the positional theory of group variation in unionization rates, but in the case of Hispanics, the contrasting explanation is "solidaristic" theory. Solidaristic theories posit the importance of class or group solidarity that immigrants may disrupt or augment. In this analysis, Jake divides the Hispanic population by citizenships status, national origin, time since immigration, and generation. He also controls for a variety of positional variables and any remaining variation is attributed to solidaristic factors. The findings confirm that after controls, third generation Hispanics have higher rates of union membership than the overall rates, while non-citizen Mexicans have lower rates than overall rates. He attributes these unexplained variations to the development of solidarism over time and generations.

I was intrigued by the differing emphases in these two chapters. The chapter on Hispanics starts with a case study of the successful organizing efforts of Mexican immigrant Eliseo Medina, who began his career with the United Farm Workers in the 1970s and went on to lead organizing drives for the American Federation of State, County and Municipal Employees (AFSME) and the Communications Workers of America, and finally, the Service Employees International Union, where he orchestrated the unionization of 74,000 home health care workers in California in 1999 and the Justice for Janitors victory in Los Angeles in 2000. Jake relates Medina's career to a longer history of immigrant organizers and union leaders.

In contrast, the chapter on African Americans makes no mention of the history of black labor organizing and militancy. Relevant examples that I know of range from the formation of the Colored National Labor Union in 1869 and the Atlanta Washerwomen's strike of 1881 (which involved 3,000 laundresses) to the formation of the Brotherhood of Sleeping Car Porters and Maids in 1925, the interracial Southern Tenant Farmers Union of the 1930s, and the fifteen-year unionization campaign initiated and led by African American hospital workers at the Duke University Medical Center in the 1970s and 1980s. I put the question to Jake as well as to the other panelists: Is the history of black labor organizing relevant to their current propensity to join unions? If not, why not?

I also mention these black-initiated and led efforts because they were fueled and sustained by community mobilization and articulation of the connection between economic empowerment, civil rights and social justice. These black-led efforts resonate with current-day Latino, Caribbean, Filipina -led labor movements, including the present day efforts to organize domestic workers and homecare workers, and environmental justice movements to fight the placement of toxic dumps and plants in minority communities. As Karen Sacks (2014) argues in a recent article, these movements mobilize large constituencies around interracial solidarity, civil rights, and economic citizenship and may represent the future of labor organizing beyond the workplace.

A second point of juxtaposition has to do with the issue of timing the decline of unionization with the integration of Blacks and incorporation of Hispanics into the U.S. labor force. The chapter on Blacks is titled "The Timing was Terrible." It might alternatively be titled, "Blacks get a ticket to ride after the gravy train has left the station." I suggest this alternative title because the timing of black entry and union decline is unlikely to be coincidental. I'd like to put the question to Jake and the other panelists: What do you think about this timing issue? What do you think are the economic and political forces at work in the growth of inter-

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racial organizing and the decline in union power, given the historic division of the labor force along race lines? For example, up until their integration into labor unions, Blacks had been hired as substitutes who were fired once a strike was settled and about the time that Blacks were increasingly integrated in unions, employers began to break with established understandings, such as not replacing striking workers with permanent substitutes rather than temporary strike-breakers. Similarly, the influx of immigrants from Mexico and other parts of Latin America and the Caribbean has coincided not only with the decline of private sector unions, but also the spread of subcontracting, precarious, and part-time employment. Surely this is not the result of "bad timing" by immigrants, but part of the same global economic processes that are affecting us all, but especially racial minorities and racialized immigrants.

In closing, *What Unions No Longer Do* is a thought-provoking and illuminating book: it is an important and timely intervention in labor studies. If you haven't already done so, please read it.

Coda: If labor unions have finally become multiracial, why is the field of labor studies so white?

Reflecting back on this ASA author-meets-critics session, perhaps the aspect I found most striking was the overwhelming whiteness of the audience. I perceived only one person of color (an Asian American woman in the audience of 50+ people.) This audience did not reflect the composition of the membership of ASA and was very different from that of the audiences at two other sessions in which I spoke. Why would this be? Perhaps it is the fact that labor scholars are steeped in and inspired by "the good old days" of labor militance, the heyday of which was a period when major unions excluded black workers, whom they viewed as undercutting the wages and status of white workers. Meanwhile, black workers struggled to organize on their own behalf, especially in those fields where they constituted the main labor force. Thus white and black (as well as Latino, Asian American) labor histories developed as separate streams, with the former being constituted as "labor history" and the latter being viewed as part of "civil rights history." I suggest that the legacy of this separation continues today in the different lenses that whites and people of color bring to their perspectives on labor and labor struggle, and thus the seeming lack of identification by sociologists of color in "labor studies."

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Comments on *What Unions No Longer Do*

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Jake Rosenfeld's *What Unions No Longer Do* is a timely and useful discussion of the consequences of the diminishing power of unions in the past four decades in the United States. Unions were the main equalizing institution in the U.S. during the three decades following World War II and a key driver of the expansion of equality that has been described as the "Great Compression" and of the growth of the middle class. The subsequent weakening of unions, on the other hand, was a major contributor to a host of pressing social, political and economic problems of our time, including: increasing income inequality; the growing disadvantages of racial minorities and lower-skilled immigrants; and the declining political voice for lower-income workers.

The book presents an historical analysis of the multidimensional repercussions of the decline of unions, especially in the private sector. I have little to quibble about in terms of what he has done in the book. He accomplishes his goals very well. His arguments are well grounded and he provides good statistical support for them. The book also provides a nice counterpoint and update to Richard Freeman and James Medoff's classic 1984 book, *What Do Unions Do?* from which the title of Jake's book draws inspiration.

In this brief essay, I highlight some of the central issues related to the decline of unions that are raised by the book and discuss a few of the policy and research issues related to these questions.

Some Issues Raised by the Book

There is no doubt that the rise of income inequality is a major and pressing issue, and that unions have had a great deal to do with generating economic inequality. The power of unions during the three decades after World War II enabled workers to benefit from rises in productivity as well as provided a check on the ability of employers to obtain high rents. In addition, weak union power enabled employers to establish a shareholder model and facilitated political policies that permitted them to acquire such rents. At the same time, the decline of unions is only part (albeit an essential one) of the explanation for the increase in economic inequality since the 1980s. While it is true that the wages of middle and working class Americans have been stagnant for many years, there is now considerable evidence that the biggest increase in economic inequality is due to the separation of the top 1% (or at least 10%) due to factors such as financialization that were enabled by political processes of liberalization such as the deregulation of markets. Forces of neo-liberalization were present in all industrial countries, albeit in greater or lesser degrees depending on the strength of unions and other labor market institutions. While equalizing labor incomes would go a long way toward addressing problems of inequality, then, other changes (such as in tax structures, capital market incentives, etc.) are also needed to accomplish this goal.

For many, the high level of union density in the public sector (i.e., 40 percent of the workforce in the public sector was unionized in 2009, compared to only 8 percent in the private sector) raises the question of the future role of public sector unions. Rosenfeld argues that the "government is not the answer," by which he means that organizing the public sector will not compensate for the major decline of unions in the private sector, due to the constraints on the ability of public sector unions to do what private sector unions do. Moreover, there is a growing assault on public sector unions, especially in the traditionally strong unionized states of the Midwest. These are both, of course, true. Nevertheless, we should not take this to mean that we should give up on public sector unions, especially in view of the growth of jobs in services, many of which are in the public sector. In addition, the distinctions between public and private sectors are becoming increasingly blurred, as witnessed, for example, by the privatization of formerly government functions such as private security and prisons.

Unions declined around the world during the period 1973-2009. This has a number of implications that could be explored more than they are in the book. For example, the general decline in unions suggests that there is not a strong link between weaker unions and greater economic competitiveness. One also wonders how much the worldwide pressures toward greater economic liberalization will result in a fundamental shift from labor toward capital such that an upsurge in unionization in the U.S. is unlikely or even possible in the modern world economy.

What is to be Done?

I'll now turn to a discussion of some of the issues that Rosenfeld addresses in his last chapter, "The Past as Prologue: The Labor Movement Pre-New Deal, Today, and Tomorrow." Here he raises questions such as: what is to be done? And, what is the future of unions and worker power?

Rosenfeld compares the current situation in the U.S. with that existing in the 1920s and 1930s, another period of great economic inequality that preceded the huge increase in private sector unionization in the 1940s and 1950s. He points out a number of important differences between these two periods, such as: The Great Depression was much more severe than the recent Great Recession; there has now been a change from a manufacturing to a service economy; there is presently a weaker connection between labor and the Democratic Party; and the 21st-century economy is characterized by greater globalization, which makes employers less dependent on the domestic market.

While he discusses briefly some hopeful signs, he doesn't provide much hope for the future. To be fair, Rosenfeld's book is not really concerned about the future, but is more of a counterfactual argument focused on the past, outlining what might have been the situation for workers in the United States had unions not declined. And, he is very actively concerned and involved as a public intellectual with issues regarding the future of the labor movement.

What seems clear is that any new resurgence of labor will differ in key ways from the previous organizational forms. Rosenfeld suggests that union movements need to involve broad-based social movements. This is of course true and there are some notable successes in this regard. But new union movements also need to address other key transformations in the nature of work and the economy. One such secular change is the growth of new employment relations, such as independent contracting with its tendency to misclassify employees (e.g., Uber and other examples of the share economy) as well as the growth of temporary and contract workers that are characterized by triadic employment relations among workers, a client organization, and an employing temporary agency or contract company. Another concern is the failure of our labor laws to keep up with changes in employment relations, having been enacted in periods where the normative form of employment was the standard employment relationship that was predicated on a male breadwinner - female homemaker model. Unions also need to organize workers across national borders, in order to avoid a continued "race to the bottom."

When I think about the obstacles to reversing the decline in unions over the past four to five decades, I too get a bit weary and pessimistic. But there are hopeful signs of progress. For example, there is considerable ongoing experimentation with a variety of organizational forms of worker power, including various types of social movements, worker centers and other "alt-labor" groups, and partnerships between the AFL-CIO and various reform groups. Unions have historically changed their tactics and modes of organizing in response to challenges produced by changing economic, social and political environments and labor gains often occur in leaps or "upsurges," as Clawson (2003) and others have argued. In addition, there are have been notable successes made by the SEIU (which has a large number of public sector workers and is doing a lot of different things such as targeting both private and public sector workers and linking up with social movements) and Fast Food workers ("Fight for Fifteen") in their struggle for a living wage. Moreover, Occupy Wall Street and similar movements have put the issue of income inequality squarely in the national political conversation. Unfortunately, these successes haven't dramatically increased the number of union members or the resources available to unions and they have not yet provided unions with weapons as effective as strikes were in the past.

The prospects of a revitalization of the labor movement in the U.S. also point to research agendas for sociologists and other students of labor and social movements. One key issue is what we can learn from the experiences of unions in other countries; while there has been a general pattern of union decline, unions have fared better in some countries than in others, underscoring the importance of political and institutional factors in shaping employer-worker relations. Another central issue for research is understanding better the implications of a shift from the traditional study of unions and "industrial relations" to a more general study of work and employment relations, including new institutional forms and modes of organizing.

What seems clear is that any new resurgence of labor will differ in key ways from the previous organizational forms. Rosenfeld suggests that union movements need to involve broad-based social movements. This is of course true and there are some notable successes in this regard. But new union movements also need to address other key transformations in the nature of work and the economy.

Jake Rosenfeld's book has underscored what we have lost due to the decline of unions in the United States and has set the stage for efforts to consider new forms of worker power that are likely to be effective in the 21st century. We are fortunate that ASA president Ruth Milkman and her program will focus on many of these issues at the 2016 American Sociological Association meetings in Seattle!

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Reply to Critics

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It is an absolute honor to discuss and debate my book with Professors Voss, Glenn, and Kalleberg. Their scholarship has left a deep impression on my own, and its imprint is evident throughout *What Unions No Longer Do*. Unsurprisingly, they raise a series of probing questions about the book. I address their major themes below.

Union decline and the rise in inequality

The collapse of organized labor is absolutely central to one of the defining issues of our time: rising inequality. From workers' wages to presidential elections, labor unions once exerted tremendous clout in American life. In the immediate post-World War II era, one in three workers belonged to a union. The fraction now is close to one in ten, and just one in twenty in the private sector—the lowest in over a century.

Yet labor's losses obviously do not explain all aspects of rising inequality, as Kalleberg emphasizes. This is especially apparent in the rise of top-end incomes, a development that Kalleberg suggests is linked more to the financialization of the economy and deregulation of markets than to falling membership rolls. I wouldn't disagree, although I would suggest that this particular topic – the possible interconnections between the collapse of organized labor and the rise of the 1% – remains relatively unexplored. Emerging research actually points to a larger role for unions than previously assumed. For example, Taekjin Shin (2014) examines pay disparities between rank-and-file workers and their CEOs, finding that unions exert downward pressure on top-end compensation. The decline of labor, then, may have helped untether executive pay from preexisting compensation structures.

The financialization of the economy, meanwhile, didn't unfold naturally – it involved political power struggles with interest groups aligned on competing sides. Research by Ken-Hou Lin and Donald Tomaskovic-Devey (2013) finds that even ostensibly non-financial firms such as General Electric grew reliant on financial channels for their profits in recent decades. This development shifted earnings toward elite workers at the expense of the rank-and-file. It's hard to imagine that a powerful labor movement wouldn't have combated such a development, given financialization's impact on the pocketbooks of members. Yet labor's diminished role in fighting off the growth of the finance sector is a story that is largely untold.

As Kalleberg points out, union decline was a global phenomenon. It also was one that occurred unevenly, with some labor movements emerging from the political and economic changes of the late 20th century relatively unscathed, while others, like our own, grievously injured. Rising top-end inequality is also not unique to America. For comparative scholars, the interrelationships between top-end inequality and the strength of labor movements remains fertile research terrain.

Union decline and the fate of racial/ethnic minorities

In her remarks Glenn highlights the book's focus on historically disadvantaged populations. I am glad she does. One of the core goals of the book is to enter into debates about the interrelationships between the labor movement and minority groups, such as African-American and Hispanic workers. Regarding African-Americans and organized labor, I argue that much of the existing scholarship is dated, historical, or both, casting unions as racist and exclusionary organizations bent on protecting predominantly white, and predominantly male, workers. That was certainly true. But as I demonstrate, this common stereotype of organized labor has not held for some time. Since at least the 1970s no group has been more overrepresented in organized labor than African-Americans. This is especially true of African-American women, whose organization rates were twice as high as white women's for decades, helping to narrow black-white wage gaps.

And as Glenn highlights, the particular form of decline suffered by the U.S. labor movement has hit disadvantaged groups the hardest. In the book I reveal how the broad benefits of unionization – ranging from direct wage gains, to establishing norms of fair pay for nonunion workers, to encouraging otherwise disaffected citizens to show up to vote on election day – were strongest in the private sector. And union decline has been a story of declining memberships in the private sector, where the vast majority of American workers are employed.

As a result of union declines in the private sector, black-white wage gaps expanded dramatically among women and held steady among men. And the decline removed a key ladder toward a solid middle-class lifestyle for recent arrivals to the U.S. In the early decades of the 20th century, organized labor successfully incorporated millions of new immigrants and their offspring, even as union leaderships (disproportionately foreign-born!) fought for restrictionist laws along the nation's borders. Today, leadership has moved away from its past policy stance, embracing positions including a pathway to citizenship for the nation's undocumented population. Policy stances aside, the weakened state of the labor movement means that unions do not play the same role incorporating newcomers economically that they once did.

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In the book I recount the story of Elise Medino, a Mexican immigrant, who rose through labor's ranks during the 1970s, 1980s, and 1990s to become one of the most powerful labor leaders in the U.S. Medino – along with numerous others who diversified a leadership disproportionately comprised of native-born, white men – both benefited from and contributed to labor's changing attitudes toward organizing immigrants and immigration policy. His story also exemplifies the crucial role broad-based social movements that encompassed unions and a range of other organizations played in scoring dramatic victories for immigrant workers, especially those from Latin America.

Glenn questions why the book lacks a similar exploration into the history of African-American-led organization efforts and fights to integrate labor's ranks. As she suggests, these campaigns, some of which date all the way back to the second half of the 19th century, also drew their strength from multiple alliances, and their leaders often emphasized the overlap between economic independence, civil rights, and social justice.

This is a valid critique. The hard-fought campaigns to integrate unions racially established crucial ties between civil rights and labor organizations, and a narrative emerged from these efforts emphasizing the connection between racial and economic justice. During the second half of the 20th century, these ties and that narrative, along with numerous court battles, overwhelmed the racist stance of many locals, leading to representation rates among African-American men that topped 40% in the private sector (and nearly 25% among African-American women). My overwhelming focus in the book was on detailing the dramatic change in membership rates among African-American workers, and analyzing whether rising rates reflected changes in African-Americans' labor market locations (a "positional" explanation) or represented black workers' desire for the protections that unions provide (a "protectionist" explanation). I find support for both theories in the data. But the protectionist explanation rests on the hard work of African-American activists convincing rank-and-file workers that unions could, indeed, offer them protection. That story awaits a fuller treatment.

Glenn also questions the curious timing of African-Americans' ascent in organized labor, which coincided with labor's decline. Were these opposing trends coincidental? I would suggest not entirely – for one, as Paul Frymer (2007) has demonstrated, for many locals the costs of racial recalcitrance in the form of lawsuits that helped integrate their ranks were substantial, and came at a time of growing employer opposition that required a unified and well-funded response. But again here the international picture is important. Unions in the U.S. as elsewhere faced a similar set of challenges posed by globalization and economic stagnation. These challenges weakened nearly every labor movement, even ones not facing efforts to racially integrate the ranks. Other developments that put labor on the defensive, such as the deregulation of certain highly unionized industries, do not have an obvious connection to the diversification of the American labor movement.

Maybe government is the answer?

In the book I investigate the compositional differences between public and private sector union memberships, as well as differences in union effects on members in the public and private sectors. I find that a labor movement anchored in the public sector, as ours increasingly is, looks and acts differently from one concentrated in the private sector, as ours once was. Government employees, unionized or not, tend to be better educated and compensated than their private sector counterparts. And unions that represent them tend to be less effective in delivering wage and benefit gains, and at encouraging their members to participate in politics, given the already high participation rates of public sector workers.

I believe these differences are important for understanding what it is that unions once did, and what a union movement disproportionately comprised of government employees is unlikely to do. Voss and Kalleberg suggest that the sectoral division I emphasize is overdone, especially during an era where the demarcation between the sectors is increasingly blurry. Kalleberg points to the substantial overlap between public and private sector work, such as the increasing privatization of many public sector jobs. Voss highlights how the same union often represents government employees and workers in the private sector. The Service Employees International Union (SEIU), meanwhile, which counts among its membership base thousands of government employees, is leading many of the minimum wage campaigns for private sector workers that have gained momentum – and secured hard-fought victories – in many cities and states. We are also in a period where public sector unions face unprecedented attacks, most notably in the wake of Wisconsin's Act 10, which has decimated memberships in the Badger State. A Supreme Court ruling in the Friedrich's case (*Friedrichs v. California Teachers Association et al*) is due this summer and could change the entire public sector to "right to work." Thus, while their growth trajectories were very different, with public sector memberships expanding during recent decades as private sector memberships declined, current opponents of labor aren't drawing any boundaries between the sectors – they want to see unions eradicated in both.

What comes next?

In her comments, Voss suggests that my assessment of union decline largely absolves unions of much blame for their losses. Evidence for this contention rests, in part, on the comparative picture that Kalleberg highlights. Labor's losses across a range of institutional environments point to underlying causes that go beyond the particularities of any one organizing strategy and style of union leadership. Yet organized labor's losses in the U.S. outstrip those of many peer movements overseas. And it is certainly true that the U.S. labor movement's core leadership was caught off guard by the challenges posed by economic and political developments during the 1970s and 1980s, not to mention the employer opposition that gained strength during the same period. Evidence for this sense of complacency is not hard to find, most famously in the words of former AFL-CIO leader George Meany, who remarked in 1972: "I used to worry about…the size of the membership. But quite a few years ago I stopped worrying about it, because to me it doesn't make any difference" (Rosenfeld 2014: 10).

As the leader of a shrinking set of organizations, Meany should have worried. But how much would his worry have done to reverse labor's decline? The employer resistance was so ferocious (and effective) that even the most forward-thinking leaders with the most innovative tactics would have faced an uphill battle in maintaining labor's base. This employer offensive coincided with a rightward turn in the polity, and the resulting distancing between unions and their political allies rendered labor without the requisite political power to rebalance existing labor laws. All the while broader economic forces shifted employment away from labor's historical base – manufacturing – and toward sectors that for a whole host of structural reasons are just more difficult to organize.

Why does labor's own strategies and leadership during past decades matter for what comes next? Arguments that a sizable portion of labor's decline was labor's own doing suggest that unions themselves have the agency to arrest the turnaround. Voss argues that I fail to focus on "recent changes in labor's repertoire of contention." She is absolutely right: Unions have largely abandoned the strike, a weapon that increasingly backfires as employers have perfected their counterattacks during work stoppages. Instead, proactive unions have embraced the corporate campaign and community alliances, among other strategies. Some of these efforts have paid big dividends. My concern is what these new efforts require in terms of resources. Put simply, paying for them – both in terms of money, manpower, and the years spent planning and developing alliances –

is incredibly difficult, costly work. Doing so on such a scale as to raise membership rates significantly seems out of reach, at least for the near future.

Yet the near future for organized labor is brighter than it was when I wrote *What Unions No Longer Do*, at least in certain respects. First, the bad news, from labor's perspective: membership rates have not turned around, and the concerted attack on public sector unions is bearing fruit for labor's opponents. The book's bleak conclusion reflect this reality, but as Voss and Kalleberg mention, I overlooked many recent efforts to combat inequality and poverty in which unions have played starring roles. These include the Fight for 15. This campaign originated as a minor labor organizing drive in the Pacific Northwest, and has mushroomed into a nationwide fight to raise wage minimums. The Fight for 15, funded and supported organizationally by unions, has scored victories in major cities and states, including such unexpected places as Arkansas. Concerted pressure by the union-backed "Our Walmart" campaign helped convince the nation's largest private employer it was time to raise wages for its lowest-paid workers. Paid sick leave ordinances have passed in New York, Seattle, and dozens of other cities, often spurred on by union pressure and lobbying.

> ***...proactive unions have embraced the corporate campaign and community alliances, among other strategies. Some of these efforts have paid big dividends. My concern is what these new efforts require in terms of resources. Put simply, paying for them – both in terms of money, manpower, and the years spent planning and developing alliances – is incredibly difficult, costly work.***

These union victories – and unions should call them that, for that is what they are – have improved the lives of millions of working Americans, the vast majority of whom do not belong to a labor union. And here remains the issue. Sustaining these fights requires enormous expenditures in time and money. Often, existing members provide the time, and their dues provide the money. Without replenishing union ranks, these resources will run out. Voss points to the successful community alliances unions have forged with other progressive organizations in recent years. In many cases, these other organizations are accustomed to being on the receiving end of union generosity. What the broader progressive community should understand is that this generosity relies on the strength of a movement that is under extreme threat, and that without a broad coalition to help labor counter ongoing attacks, the recent victories for American workers will prove short-lived.

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Policy Brief

Why the United States Selective Service System Should be Scrapped

Dorit Geva

Central European University

This piece was written as part of a new policy briefs initiative begun within the Comparative and Historical Sociology Section. Policy briefs will address practical implications of comparative-historical studies and aim to foster exchange between comparative-historical social science and broader academic and non-academic audiences. For more information about this initiative and how to contribute, please see the information box on page 50. Please note that the views expressed in this policy brief are those of the author, and do not necessarily reflect those of the American Sociological Association.

Supporters of the draft typically argue that an all-volunteer army means that the less privileged shoulder the greatest burden in defending the nation. They claim that America might be more cautious about flexing its military muscle if the children of the elite were just as likely to be at risk as everyone else. Some also argue that the draft is an important institution for achieving equal citizenship, and that therefore women ought to be integrated into draft registration. A related argument suggests that an active draft would enable young women and men to recognize the diversity of class, race, gender, ethnicity – and now sexuality – which together make up the American social fabric, and which can produce a new social contract.

These arguments, however, are based on a rose-colored view of how the Selective Service System, the federal administration organizing the draft, actually worked during the twentieth century. Partisans in this debate need to confront two legacies of the Selective Service System: its inherently non-universal nature, and its administration through autonomous local boards with enormous discretionary power.

America never organized its conscription system as other countries did (see Geva 2011, 2013, 2015). Broadly, there have been two "worlds of conscription" throughout the twentieth century.[1] The first is the world of French or Prussian-style universal conscription. Although never really universal, such conscription systems required most men to provide mandatory service when they reached the age of adulthood (Geva 2011b, 2014). The United States, however, has belonged to the second world of conscription. Drawing from the British tradition, Anglo-liberal countries such as the United States, Canada, Australia and New Zealand avoided institutionalizing continuous, universal, military conscription systems (Flynn 2002; Geva 2013, 2014). But keeping true to its exceptionalist reputation, the United States was even more radical in creating an especially un-universal military conscription method.

Up until the First World War, there was no unified national and federal draft system. When the United States declared war on Germany in 1917, President Woodrow Wilson, together with military leaders and lawmakers in Congress, had to quickly figure out how to recruit enough manpower to match the big conscript armies already fighting in the Great War (Chambers 1987). The result was the creation of the Selective Service System, a federal administration deeply shaped both by the Civil War draft experience and a reluctance to shoulder the hefty price tag of social supports that would need to be offered to draftees' dependents. During the Civil War, President Lincoln had eventually called for a Union Army draft. That draft was designed to maintain local autonomy and created the organizational blueprint for local "enrollment" boards. Draft resistance in the North was high, with an estimated twenty-one percent of men refusing to report to local enrollment boards (Levine 1981. See also Wheeler 1999). The Union Army's draft experience was still fresh in the minds of policymakers in 1917. By that point in the war, policymakers had also taken note of the heavy price tag close allies such as Canada and Great Britain had paid in providing social supports to conscripts' dependents since the start of World War I. American policymakers therefore resisted implementing a more universal conscription method.

As its name makes clear, the Selective Service System centered on a single, overarching logic: to *select* only some men for mandatory service (Geva 2011a, 2015b). Men's universal obligation was to register with local draft boards so that the government could classify men for fitness for service and have a repository of potential draftees. If registrants were doing valued work, or if they were a main source of financial support for dependent wives and/or children, then local boards were to give them deferments. In line with federalist principles, Selective Service delegated the role of selecting men for service to civilian-staffed local draft boards who could apply their own discretion in classifying men and determining who should be drafted or deferred, while national headquarters would issue statewide quotas and loosely defined deferment classifications.

These classifications can be viewed as a hierarchy of whose life was of greater or lesser worth (see Geva 2015b, p. 190 for a sample of the WWI classification list). For example, from my research on the First World War draft, I found that African American men were drafted at higher rates nationally than other groups of men, with 34.1% of African American registrants inducted, as compared to 24.04% of white registrants (Geva 2013, chapter four). A post-war government report estimated that 50.65% of African American registrants from the draft-liable registrations were placed in Class I, the classification for men immediately available to be drafted, as opposed to 32.53% of white registrants (Provost Marshal General 1918, p.459. See also Hickel 2000, Keith 2004, Shenk 2005).

Since WWI, the draft and registration system have undergone reforms that have added a gender dimension to the problem, while leaving its non-universal nature intact. The WWI draft halted at the end of WWI, but Selective Service was revived in 1940 and continued to register and draft men through to 1973. Although a lottery system was introduced in 1969 during the Vietnam War, the lottery did not replace the classification system. The draft was then unceremoniously cancelled in 1973, and was replaced by the All Volunteer Force. Registration was cancelled in 1975. In the midst of Cold War anxieties, President Jimmy Carter asked Congress to reinstate registration in 1980, but this time also requested that a revived Selective Service would register women too. Although Congress allowed for the renewal of men's registration, and although the all-volunteer army was already actively recruiting women, Congress could not fathom women's registration, and argued that given women's exclusion from combat roles, it did not make sense to register women for the draft. The Supreme Court deferred to Congress and upheld the constitutionality of male-only registration in 1981 (See Geva 2015a for a review of the case). Since then, registration persists to this day for almost all men living on American soil between the ages of 18-25, including undocumented immigrants.[2] Recently, however, the Pentagon announced that it has cancelled all combat exclusions for women. The basis of the Supreme Court's 1981 decision looks to have lost ground. This development has led many to speculate that women's draft registration is inevitable (Lamothe, 2015). Already, officials at the Pentagon are reportedly working with Congress to review the Military Selective Service Act.

The problem lies with Selective Service's non-universal nature, and its administration through autonomous local boards with enormous discretionary power in making decisions about whose life should be put at risk.

Expanding the registration system to include women could be a step toward making Selective Service more equitable. But the lessons learned from comparative-historical hindsight suggest the system ought to be scrapped altogether. Why? The problem lies with Selective Service's non-universal nature, and its administration through autonomous local boards with enormous discretionary power in making decisions about whose life should be put at risk. This design makes the draft particularly susceptible to racist, heterosexist, and sexist ideas about whose life is of greater worth, whose labor is valued, what is a "real" family or marriage, who is a "real" breadwinner or dependent, even who is a "real" man or woman. Introducing more women, African Americans, ethnic minorities, or LGBTQ citizens as local board members would, perhaps, alleviate such tendencies. But my research suggests that this would likely fail to be a panacea. For the same reasons, I remain skeptical that women's integration into draft registration would be good for gender equality.

Compared to the Selective Service System, the All Volunteer Force (AVF) seems to be a better solution. There is little evidence that the AVF has an over-representation of African Americans and other ethnic minorities. However, class does a play a role, as members of low-income families are more likely to join the AVF than the children of higher income families (Lutz 2008). While these inequalities ought to be taken seriously and are in themselves problematic, they are preferable to a state-sanctioned system of valuing the life of certain citizens (and residents) over others.

Current considerations of the "woman question" and the draft are an opportunity to reevaluate the Selective Service System altogether. Instead of asking a more narrowly-focused question regarding whether or not women should register for the draft, we should consider scrapping the Selective Service System altogether. The debate should instead pivot around the maintenance of the All Volunteer Force, and/or the creation of a new, genuinely universal, national service.

Endnotes

1. Thanks to Myra Marx Ferree for suggesting this phrasing.

2. For a comprehensive list of who is required to register: https://www.sss.gov/Portals/0/PDFs/WhoMustRegisterChart.pdf

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The Policy Brief Initiative

Is it possible to learn from history and the experience of other countries? We think that it is. Although comparative-historical research is seldom motivated by policy considerations, it can often inform policy design.

For comparative-historical scholars/researchers, this initiative is an excellent opportunity to promote one's research and make it attractive to general audiences and funding organizations. Policy briefs should be no longer than two single-spaced pages and should defend a specific policy action that arises from the author's research. Please submit your policy briefs or any questions about this initiative to Natalia Forrat (forrat@u.northwestern.edu) and Jensen Sass (jensen.sass@yale.edu).

Section News

News and Announcements

Policy Trajectories Blog Launches

Submitted By: Basak Kus

The Comparative-Historical Section has a new blog! *Policy Trajectories* was born out of the "Can Comparative Historical Sociology Save the World?" initiative. It is a platform where sociologists can engage with the critical questions and events of our time, offer ideas, and tackle policy challenges.

In its fall issue (Vol 27; No 1), *Trajectories* featured insightful essays that gave voice to the issues and intentions that inspired us to create the blog. Monica Prasad reminded us that "comparative-historical sociology has always been most intellectually vibrant when it has been most explicitly engaged with questions of public purpose." Jensen Sass pointed to the ways in which comparative-historical sociologists can contribute to policy, one of the most important of which, I believe, is by alerting policymakers to the dangers of unintended consequences. Josh Pacewicz made the point that one of the main reasons why comparative historical sociologists lack influence is that "we comprise a tiny social network adrift in a vastly complex society, one with few ties to centers of policy debate." Jason Jackson talked about the importance of encouraging comparative and historical-minded scholars to engage in policy-relevant discourse and debate.

We hope that *Policy Trajectories* will be a venue for us to start being part of a larger public conversation. We invite all members of the section to contribute. Please visit our webpage (www.policytrajectories.org). You will find posts on inequality, the Paris attacks, globalization, climate policy, taxation, the Syrian refugee crisis, and a lot more! And please "like" us on Facebook to keep up to date with all the new posts.

Missing Pieces of Section History Digitized

Submitted By: Matt Baltz

Last year, lifetime section member Thomas D. Hall (Professor Emeritus, Depauw University) contacted *Trajectories* with an offer. He had noticed on the section's website a request for old issues of the newsletter and informed us that he had in his possession a trove of newsletters and section-related sundries dating as far back as 1982. This was the year when the section held its first organizing session at the San Francisco Hilton on September 9th (Tom also noted that he was, in fact, an attendee of this meeting).

The section's council voted to allocate a budget to digitize Hall's archive, and council member Sarah Quinn (University of Washington) took the lead in organizing the effort. Tom then sent his materials by mail (at his own expense as a donation to the section) to Reed Klein, Sarah's former research assistant. Reed then started the job of turning what amounted to 242 pages of material into searchable pdf files (see Reed's notes on the process in the item below). For his part, Tom found UW to be an especially appropriate destination for his materials since this was the very institution where he earned his Ph.D. back in 1981!

Reed has since completed his task, and Tom's archive of newsletters and section materials will all be available on the section website soon. For now, I wish to extend once again my thanks to Reed Klein, Sarah Quinn, and, above all, Tom Hall, for his careful stewardship of these important relics of the section's history.

Notes on CHS Newsletter Digitization Project

Submitted By: Reed Klein

All previous newsletters of the Comparative and Historical Sociology Section, as well as several founding documents, have been scanned and will soon be available as searchable files on the section's website. They were scanned using a Scannx Book Scanner, and then touched up and converted into searchable .pdf files using ABBYY Fine Reader 12, a file conversion program. In all, 242 pages were scanned.

These early newsletters, some of which were produced solely on a typewriter, are a far cry from the full-color 80 plus page newsletters the section regularly publishes today. And yet, much of the content from these early newsletters still seems relevant. The fall 1995 newsletter, for example, features a debate between Jeff Goodwin and Charles Tilly as to whether then-current trends render the concept of "state-defined societies" anachronistic in comparative-historical research. Twenty years later, when our globalized world is facing a resurgence in geopolitics and nationalism, revisiting Tilly and Goodwin's debate seems more relevant than ever.

I feel lucky to have been tasked with digitalizing this material, as it has provided a window on the discipline's rich debate that I otherwise would not have known. As Rosemary Hopcroft points out in the Spring 1993 issue of the newsletter, "By overturning our conditioned ways of thinking and revealing the possibilities in social life, comparison helps us revise the very questions we ask." Hopefully, this older material will contribute to this ongoing process of revision and reflection, and help fuel the debates that have characterized the section since its beginning.

Coming up in
the next issue of

Trajectories

Can Comparative Historical Sociology Save the World?
The debate continues in April

New book symposia:
War, States, and Contention
by Sid Tarrow

Expulsions
by Saskia
Sassen

And
Much
More!