-----. 2007. "Useless Tilly (et al.): Teaching Comparative-Historical Sociology Wisely." *Trajectories: Newsletter of the Comparative and Historical Sociology Section of the American Sociological Association*. 19(1):14-17.

-----. 2013. "The Structural Transformation of Sociology." *Society* 50(2): 156-166.

Durkheim, Emile. (1908) 1982. "Debate on Explanation in History and Sociology." Pp. 211-228 in his *The Rules of Sociological Method*. New York: The Free Press.

Manza, Jeff and Michael A. McCarthy. 2011. "The Neo-Marxist Legacy in American Sociology." *Annual Review of Sociology* 37:155–83.

Prasad, Monica. 2015. "Can Comparative Historical Sociology Save the World?" *Trajectories: Newsletter of the Comparative and Historical Sociology Section of the American Sociological Association.* 26(2):1-2.

Smith, Christian. 2014. *The Sacred Project of American Sociology*. New York: Oxford University Press.

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

Greta Krippner
University of Michigan

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.

References

Bell, Daniel. 1976. *Cultural Contradictions of Capitalism*. New York: Basic Books.

Goldthorpe, John. 1987. "Problems of Political Economy after the Postwar Period." Pp. 363-407 in *Changing Boundaries of the Political: Essays on the Evolving Balance between State and Society, Public and Private in Europe*, edited by C.S. Maier. New York: Cambridge University Press.

Hirschman, Albert. 1980. "The Social and Political Matrix of Inflation: Elaborations of the Latin American Experience." Pp. 177-207 in *Essays in Trespassing: Economics to Politics and Beyond*. Cambridge: Cambridge University Press.

Krippner, Greta R. K. 2010. "The Political Economy of Financial Exuberance." Pp. 141-173 in Michael Lounsbury and Paul Hirsch (eds.), *Markets on Trial: The Economic Sociology of the U.S. Financial Crisis. (Research in the Sociology Organizations 30B)*. Emerald Publishing Group.

-----. 2011. *Capitalizing on Crisis: The Political Origins of the Rise of Finance*. Cambridge, MA: Harvard University Press.

Streeck, Wolfgang. 2011. "The Crises of Democratic Capitalism." *New Left Review* 71: 5-29.

Weber, Max. 1946. "Science as Vocation." Pp. 129-156 in *From Max Weber: Essays in Sociology*, edited by Hans Gerth and C. Wright Mills. New York: Oxford University Press.

Problem-Solving Sociology

Monica Prasad
Northwestern University

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