Trajectories

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How Comparative Historical Sociology Can Save the World

As part of this year's "Can Comparative Historical Sociology Save the World?" discussion, in this issue scholars whose own work has used comparative historical methods to address policy-relevant concerns give advice on how to do this kind of research.

***For previous discussions, see "Can Comparative Historical Sociology Save the World?" and "Should Comparative Historical Sociology Save the World?" that have apeared in prior issues of the newsletter* (Trajectories, vol27:1-2).**

Four ways to turn good sociology into policy-relevant sociology

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With the questions of whether comparative historical sociology can or should save the world safely settled, I'm grateful we can now turn to the practical question of how to go about it! Obviously I joke — as I sat down

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to write this piece in response to Monica's request, I first re-read the views exchanged in the pages of the two prior issues of *Trajectories* and was reminded anew of how differently many of us feel about the possibility and desirability of policy-relevant sociology. Of course it's not entirely possible to bracket these questions when thinking about the techniques I would recommend to sociologists who, like me, have decided that policy relevance is a worthy goal. Fortunately, diving into the mechanics of producing policy-relevant research may help clarify whether such projects are possible or worthwhile. If the techniques that are necessary to make our work policy relevant are distasteful, we might back away from the conclusion that this is such a good idea in the first place. Or, if it turns out that we are all deluding ourselves that there is such a thing as policy-relevant sociology, we might reconsider whether it's worth making such a fuss about its ethical foundations.

And even if I can't persuasively answer whether comparative historical sociology can or should save the world, Monica's request that I provide some thoughts on how to go about producing policy-relevant research came with a specific question that I found entirely compelling on its own. She noted that these concerns often have particular resonance for younger scholars, who often identify the gulf between policy-oriented work and the type of research that gets published in journals as a major obstacle to doing policy relevant sociology.

When it comes to the question of how to get policy-relevant scholarship published, I think there is really only one answer: it has to be good sociology. That's simply the only criterion that matters for peer-reviewed scholarship. In other words, you don't get extra points just for writing about the issues that show up in policy debates. Luckily, the relationship between policy-related empirical concerns and sociological significance is not a zero-sum game. There's nothing inherently fruitful about the policy world as a source of compelling evidence — at least no more so than workplaces, families, social movements, refugee camps, board rooms, or anywhere else sociologists seek out data. On the other hand, the years I spent working on tax policy before I entered graduate school gave me a detailed, technical background on these issues, and this knowledge of policy problems generated puzzling and productive questions. Why do states vary so much in the types of taxes that compose their revenue systems? What accounts for the fact that ballot initiatives have been a barrier to adopting new taxes in some states, but not in others? What explains why states that all use similar revenue tools develop very different tax reliance patterns, for instance, with the income tax structured as a flat tax in some states and a progressive, graduated tax in others?

At their most successful, my attempts at answering these questions have unfolded very much like the hypothetical case that Monica described in her piece in the Winter 2016 newsletter, in which "an orientation to solving the problem forces the analyst into a deeper confrontation with the issue." Because I knew something about tax structures and state budget processes – and was already familiar with the comparative landscape of state tax systems – I was able to ask questions about this world that wouldn't have occurred to me otherwise. My policy experience also made me more attuned to understudied venues and actors, such as governors and state legislatures, which proved fertile ground for answering long-standing sociological questions about the relationship between revenue generation and the welfare state.

But I want to provide more specific guidance, if I can. Focusing on the practical side of this issue may be particularly helpful for early-

career scholars looking for a road map. Here are four "how-to" moves I would recommend to comparative historical sociologists who want to aim for policy relevance while building a publication record.

First: seek out opportunities to take your research to policy audiences. I suspect that a key reason my research is seen as policy relevant is because I made an effort to write about it in blogs and opinion pieces or go on podcasts or television shows to discuss it. In other words, if my research is policy relevant, it's as much because of the things I did around it as for the content of the work itself. This is harder than it looks — it goes beyond just using accessible language to present your conclusions. Policy audiences are often less concerned with your careful articulation of your findings than with how your research might advance their point of view, and they are happy to rephrase your conclusions to fit their arguments – sometimes out of genuine enthusiasm for their understanding of your results, and sometimes with less benevolent intent. And, to be fair, I have sometimes found myself tempted in these contexts to push or stretch my conclusions to speak to broader issues of public concern.

Ultimately...it's hard to do policy-relevant work without drawing policy actors into your research process and finding out what questions are relevant to them, and I have found that these exchanges usually improve my scholarship by prompting me to think more creatively about my own work.

I think the key here is to pay attention to whether you agree with yourself in these moments of boundary-pushing — are the theories you are developing really congruent with the conclusions policy actors want to draw? If not, why not? Are they asking a more interesting question? What would it take to answer their version of the question, or to be able to come up with an answer to the point they are trying to prove with your research? Instead of being nervous at these moments, I try to switch on the creative, evidence-gathering side of my brain and not worry so much about policing the ways that people are talking about my work. There are limits to this creativity; there's a big difference between talking to journalists about your research – when you really want your conclusions represented precisely and accurately – and being in a room full of social movement activists where you have the opportunity to trade ideas in a more free-flowing fashion. Ultimately, though, it's hard to do policy-relevant work without drawing policy actors into your research process and finding out what questions are relevant to them, and I have found that these exchanges usually improve my scholarship by prompting me to think more creatively about my own work.

Second, and relatedly, take every opportunity to get your writing edited by professional communications staffers and editors. I rarely hear other sociologists say this, but I think it substantially improves your writing to be edited by smart communications professionals and editors. It is immensely helpful to see how someone transforms your prose when they are just focused on strengthening your arguments. There are professionals out there who don't have patience for academic turns of phrase, and who will shorten your sentences, make your claims bolder and more declarative, and restructure your paragraphs to actually put your arguments up top and the warrants below. This exercise also forces you to examine your own claims and conclusions in a way that is good for your argument, not just your writing. I

accomplished this by saying yes to any request to write a blog post, white paper, or opinion article, even if I winced at the time it would take to put these things together. Eventually, developing a policy voice and an instinct for the right cadence – which is easier when you see the "track changes" unfold in real time – pays off as you can finish pieces for popular or policy consumption more quickly, even if the first time you write a 500-word blog post it takes all afternoon.

Third: where they exist, seek out supportive academic communities that care about the policy issues your research touches on, not just about the theories or methodologies you employ in your work. For me, the support of the fiscal sociology community I encountered through the workshop organized by Monica, Isaac Martin, and Ajay Mehrotra – and then continued to engage with through annual sessions at SSHA – made a big difference as I sought to develop my voice as a scholar. Sometimes these communities will be located inside the discipline or the academy, but sometimes they won't. Whenever I found someone who liked talking about taxes or state governments or the fun facts buried in 1960s governors' biographies, I tried to touch base every semester to catch up and hear about their projects. Even if you don't share a methodological or disciplinary perspective, hearing from someone else that your research is prima facie interesting because it touches on a shared set of policy concerns is a huge morale boost that I was repeatedly grateful for during graduate school. And a key part of doing policy-relevant comparative historical sociology is continually reminding yourself that this is important, exciting work!

Finally, to the extent that your research lends itself to quantitative analysis, developing those skills can boost both your sociological scholarship and your policy relevance. This point might already be obvious to many in the comparative historical subfield; examples abound of compelling scholarship that uses archival materials to construct original datasets or employs new technological tools to make sense of large quantities of archival data. I started to use some of these techniques only when I was in the later stages of my dissertation writing, as I tried to make sense of a large trove of letters that constituents in four states had written to governors about their views on proposed sales taxes – but I quickly found this project to be one of the most intellectually promising aspects of my research. Policy audiences are always eager for conclusions that have numbers attached. More than that, I have found that the quantitative skills I have developed through my dissertation research have allowed me to draw on useful skills when I participate in the policy world as a practitioner (not just as a sociological researcher). For instance, in my current role as the ASA Congressional Fellow, working as a staffer in a congressional office, I rely on these skills frequently.

While these tips point to practical ways that comparative-historical sociologists can produce policy-oriented, academically-rigorous work, I think it would be disingenuous to imply that there is a straight line from policy-oriented commitments to policy-relevant research and then to policy impact. Ultimately, if comparative-historical sociologists want to be policy relevant, they have to seek out the policy world in some fashion – perhaps expanding beyond observing this world to participating in it and contributing to it. As I've sought out opportunities to do policy as a sociologist, I've concluded that on the whole – and unlike fields like economics and law – our discipline lags behind when it comes to structuring opportunities for scholars to move diachronically between these worlds. The more that sociologists with ambitions of policy relevance can find ways to translate our work for policy audiences or address questions on

the minds of policy actors, the more we might hope that our expertise will be in demand by the policy world – perhaps by serving in an administration, working as a policy advisor, or participating on a high-profile commission. When this happens, I hope sociology as a discipline will encourage, applaud, and reward these moves, even if considerable – and deserved – ambivalence remains about whether comparative historical sociology can or should change the world.

Strategies for saving the world

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> "Philosophers have hitherto only interpreted the world in various ways; the point is to change it." Marx, *11th thesis on Feuerbach*, 1845.
>
> "If I knew for a certainty that a man was coming to my house with the conscious design of doing me good, I should run for my life." Henry David Thoreau, *Walden*, 1854

Thesis 11 and Thoreau's distaste for do-gooders represent polar markers for a discussion of intentional efforts to positively transform the social world. Thoreau's intellectual descendants are less numerous, but they may more thoroughly share their forbearer's conviction, while the confidence of those hoping to use Marxist theory to change the world has been shaken by the onslaught of history. The endurance of Thoreau's perspective in modern social science is nicely exemplified by James Scott's *Seeing Like a State*, with its famous subtitle – "How Certain Schemes to Improve the Human Condition Have Failed."[1]

The debate can also be turned on its head, making the question not whether comparative historical sociology might destroy or save the world, but rather whether trying to save the world might destroy comparative historical sociology. This might be considered a more immediate concern. Comparative historical sociology's likelihood of having a significant impact on the transformation of global society (at least in the short run) is small, whereas one can easily imagine a world in which sociology (especially comparative historical sociology) is reduced to a position that echoes its status in the dark days of the mid-twentieth century Soviet Union.

My own position is that efforts to save the world, properly understood and strategized, are not a threat to comparative historical sociology and, in some contexts, might even contribute to creating a more sociology-friendly environment. It would be quixotic in any case to try to extract the "save the world gene" from the intellectual DNA of most sociologists (including those who share James Scott's antipathy to the efforts of others to save the world). Trying to suppress this propensity or pretend it doesn't (or shouldn't) exist is probably more dangerous to sociology than trying to figure out how to make the best of it. There are many ways to use sociology to try to save the world. Some are much more misguided than others. If we can't stop sociologists from trying to save the world, we should think about what kinds of strategies are more promising.

I will begin by borrowing some ideas from Michael Burawoy. Burawoy creates four inter-related ideal types of sociology: public, policy, professional and critical. He doesn't denigrate any of the four, but public sociology is clearly his preferred vehicle for sociology's "saving the world."[2] The key is establishing a dialogic relation with "publics," which is to say constituencies outside of academe.[3] The

connection may be simply that publics read and engage in conversations about the issues and theories raised by "public sociologists" (what Burawoy calls "traditional public sociology"). More interesting is "organic public sociology" "in which sociologists work in close connection with a "visible, thick, active, local and often counter-public" creating [2005:9] "a dialogic relation between sociologist and public in which the agenda of each is brought to the table" (Burawoy 2005: 9). Typical publics might be "a labor movement, neighborhood associations, communities of faith, immigrant rights groups, human rights organizations." Burawoy's conception of "organic public sociology" elevates sociologists to co-equal status with collective political actors trying to "save the world." At the same time, Burawoy argues that the interaction of publics and sociologists creates possibilities for "mutual education" that enable organic public sociology to enhance sociology as an intellectual endeavor, regardless of its efficacy in saving the world.

The plot thickens further when we add "policy" sociology, which is "sociology in the service of a goal defined by a client." Here Burawoy sees sociologists as having more of a subordinate than a co-equal role: "Policy sociology's raison d'être is to provide solutions to problems that are presented to us, or to legitimate solutions that have already been reached" (Burawoy 2005: 9). Sociology becomes a handmaiden to "saving the world" rather than a co-equal partner and risks becoming an instrument of groups that are not saving the world at all.

Despite his limited enthusiasm for policy sociology, Burawoy remains steadfast in defending the value of sociology's engagement with publics outside of academe, asserting that "few would argue for a hermetically sealed discipline, or defend pursuing knowledge simply for knowledge's sake," and in arguing that "[t]o defend engaging extra-academic audiences, whether serving clients or talking to publics, is not to deny the dangers and risks that go with it, but to say that it is necessary despite or even because of those dangers and risks." In short, Burawoy constructs a conceptual frame in which the essence of a sociological project of saving the world is building connections with constituencies beyond sociology and collaborating with them in the construction of an intellectual and political agenda.[4]

While he does not disparage the value of the more inwardly looking professional and critical sociology, directly engaging "counter-publics" with whom collaborative relationships are possible is clearly Burawoy's preferred mode of saving the world. I am happy to sign on to this program, but I would like to see a wider definition of the "publics" with whom "organic" as opposed to "traditional" public sociology relationships are possible.

In particular, I think that many groups who would appear in Burawoy's framework to be available only as "clients" for "policy" sociology are in fact "publics," people who are engaged in conversations with each other and with whom sociologists can collaborate in the construction of new frameworks and understandings. Expanding our vision of the potential interlocutors for "organic public sociology" is important in a variety of substantive realms, but none more than one of the central topics of comparative historical sociology: the role of the state and the consequences of the evolution of this role for social transformation.

Burawoy implies that policy makers or political leaders will define the goals of sociological analysis in advance, thereby short-circuiting the interaction as "a dialogue, a process of mutual education." States may indeed employ sociologists and pre-define intellectual agendas, but the inhabitants of the state are a

variegated group. Some may be "counter-publics" within the state in search of ideas and information that will legitimate alternative projects.

I would argue that key subsets of people working within state apparatuses see themselves as beleaguered minorities in search of allies, not just for political support but as collaborators in the mutual construction of new understandings and interpretations. For example, for those working inside the state unconvinced that restructuring policy to give more power to markets is a good way to save the world, the kinds of counter-narratives that comparative historical sociology has developed are attractive. Figuring out ways to translate these narratives into practical initiatives is a potential collaborative project.

In my own research on the computer industry in Brazil (Evans,1995), I found exactly this sort of "counter-public" both within the state apparatus and connected to it. My dialogues with them shaped my understanding of the process of technological change that was going on in Brazil at the time and the ideas that were part of these dialogues were in turn employed in debates within this "public" and between them and their adversaries. Obviously, the structural forces shaping the evolution of the global computer industry were more powerful than the narratives of this minority of local policy makers, but the Brazilian computer industry still ended up looking different because of their efforts than it would have otherwise.

None of this is to deny that the impact of ideas on larger processes must always be assumed to be extremely modest at best and hard to assess even then, even if ideas are embedded in a process of organic public sociology. This doesn't mean abandoning hope that ideas will have an impact or giving up engaging whatever publics take an interest in them. For every comparative historical sociologist who decides that trying to create dialogic relations with even sympathetic publics is too uncertain or unrewarding, there will be plenty of other happy intellectual warriors eager to engage, often in pursuit of very different agendas. The story of the construction of neoliberal ideas and the role of these ideas as handmaidens in the transformation of the late 20th century is such an obvious example that it has become a cliché. Hayek and Friedman labored for years building ties with their own set of publics in what seemed like very unfruitful intellectual vineyards before the confluence of structural change enabled them to play a role in "saving the world" from Keynesianism and the welfare state (Evans and Sewell 2013).[5]

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Some would argue that the apparent role of neoliberal theorizing was chimerical and that the shift to more market-dominated logics was over-determined by the resurgent political power of capital and its political allies, but this road has serious pitfalls. It leads to a position in which ideas play no role – not even that of the flapping butterfly in chaos theory – in the actual processes of change. We may be

skeptical regarding the importance of the role of ideas in transformative change, but when it comes to issues like the role of the state, it seems very unlikely that ideas are irrelevant.

James Scott, no friend of "schemes to improve the human condition" and their embodiment in the structures of the state, makes a strong case for the role of ideas. He pins the blame for the construction of these misguided schemes and their subsequent failure on the attractiveness of simplifying "high modernist" ways of seeing the world and the predilection of state bureaucrats to adopt and oppose them. Scott would disavow aspirations to "save the world," but, while he has been content to restrict himself to traditional public sociology, he has been amazingly effective as a traditional public sociologist. *Seeing Like a State* has amassed a readership of many thousands. If it is hard to assess the role of the ideas that it puts forward in dislodging the hegemony of "high modernism," it is equally hard to believe that Scott's incisively formulated critique has not played some role.

I have tried to build an argument with two facets, using the substantive issue of the role of the state as a concrete fulcrum. One facet supports Burawoy's argument that organic public sociology is the most promising mode for sociologists trying to save the world, but argues that we should be more catholic in our consideration of the groups that are potential "publics." The example of debates on the role of the state illustrates why a broad definition of potential publics is particularly important for some of the issues most central to comparative historical sociology.

The second facet of the argument circles back to the more basic question of whether any strategy that relies on ideas as instruments for saving the world is credible, suggesting that the burden of proof falls on those that would dismiss this possibility. But, this circling back should not be allowed to distract from the fundamental point that ideas without publics lack effectiveness, just as publics without a coherent set of ideas to work with are likely to flounder in their efforts to save the world.

Let me close with an example of a broad-gauged project aimed at building publics both inside and outside of academe that takes Burawoy's ideas a step further. Burawoy suggests that sociology can participate in the creation as well the transformation of publics. He also suggests that our students are "our first and captive public," drawing attention to the fact that creating active dialogues among sociologists themselves should be an important complement to collaboration with publics outside of academe.

The Scholars Strategy Network (SSN) is the brainchild of a comparative historical sociologist (Theda Skocpol) and offers a concrete architecture for the pursuit of public sociology.[6] It begins from a reconstruction of "traditional public sociology." The reconstruction has two faces. First, all members produce "briefs" – that is, short (2 pages), clearly written, jargon free expositions summarizing a key aspect of their research – that are available on the website. Collectively, the briefs, which will soon approach a thousand in number, represent an innovative way of doing "traditional public sociology," but they are also embedded in a concrete organizational structure that creates the possibility of organic public sociology.

The SSN's 650 members[7] are organized into more than two-dozen local chapters or "regional networks." SSN chapters not only give members in a particular locale a chance to collaborate with each other but also generate opportunities for scholars to engage directly with a variety of local publics around issues on which research interests and expertise intersect with the concerns and agendas of local groups.

These publics range from local groups organizing undocumented, informal workers to congressional staffers looking for research than is relevant to policy debates.

Will the SSN be able to mitigate the exclusion of sociology and other social sciences from participation in policy debates and increase the ability of local publics to marshal social science ideas on their behalf? Comparative historical sociologists will appreciate the impossibility of making this judgment on the basis of the SSN's short, five-year life span. But, the SSN certainly illustrates the continued creative evolution of efforts to better deploy sociological knowledge in the service of saving the world.

Endnotes

1. I am not going to worry here about defending disciplinary boundaries or excluding non-sociologists. Scott's work, for example, is completely relevant to comparative historical sociologists, regardless of the fact that his degree and his academic appointment are in Political Science.

2. Burawoy has written dozens of articles on public sociology. To simplify the discussion, I am drawing only on the published version of his original ASA Presidential address "For Public Sociology" (Burawoy 2005).

3. "Public sociology brings sociology into a conversation with publics, understood as people who are themselves involved in conversation"(Burawoy, 2005:7).

4. To be fair, Burawoy (2005:10) is insistent on the necessity of strong norms and practices internal to the discipline: "There can be neither policy nor public sociology without a *professional sociology* that supplies true and tested methods, accumulated bodies of knowledge, orienting questions, and conceptual frameworks. Professional sociology is not the enemy of policy and public sociology but the *sine qua non* of their existence—providing both legitimacy and expertise for policy and public sociology."

5. For a fascinating and original "revisionist" version of how a counter-intuitive set of publics in support of neoliberalism was constructed, see Johanna Bockman's *Markets in the Name of Socialism: The Left-Wing Origins of Neoliberalism* (2011).

6. See: http://www.scholarsstrategynetwork.org. Once again, disciplinary and sub-disciplinary affiliations are beside the point – SSN membership includes a gamut of social scientists.

7. The exact number of members and chapters is a moving target with more members joining each month and new chapters being formed.

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How comparative historical sociology can change the world (for the better)

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I take the topic of this symposium to mean, How can Comparative Historical research (CH) change the world for the *better*, and I will understand "better" to mean "freer of social domination and injustice." So I will take my remit to address how CH research can help move the world toward more just and humane social arrangements.

Of course CH can contribute a great deal to the pursuit of social justice. There are three questions involved here – what is the relation between historical research and social change, second, should it pursue such ends, and third, how might it effectuate them? As to the first, it has a great deal to contribute, but not

necessarily more than other methodological divisions within sociology. The basic approach that it would need to take contains two steps. First, to identify patterns of interaction, or social norms, that are deemed in some way normatively relevant – in other words, social facts that are considered either conducive to justice, or in some way undermine its pursuit. Examples of the former would be the rise of democracy, labor organizing, greater equality in gender relations, the rise of social democracy, etc.; examples of the latter would be the consolidation of racialized states, the shift to less redistributive policies, inequities in the labor market, etc. Facts such as these are selected because these are social outcomes that we feel we need to understand, either as phenomena that we need to bring about, or to dislodge or dissolve in some way.

The second step is then to understand the causal processes by which such outcomes are brought about or sustained. By this I mean that we seek to identify that mechanisms that we think are responsible for the phenomenon to occur or to persist over time. So, if your interest is in understanding the persistence of racial oppression in modern societies, your research strategy will be to first identify a case of such oppression that endures over time, and then to uncover the mechanisms that maintain it. And this is where the comparative component of CH comes in. The biggest problem with the identification of causal mechanisms is the danger of latching on to spurious ones – ones which hang around without contributing to the outcome. For historical and qualitative research setting up relevant comparisons is one of the most reliable ways of screening away spurious causal factors, and thereby raising one's confidence on the candidate factor that one has settled upon.

This sounds a lot like conventional social scientific practice. What, if anything, sets CH apart? What makes CH distinctive is simply that it is able to pose questions that other methodologies might not. So, for example, quantitative sociologists might very well be interested in changing the world, in the sense I have taken it to mean. But the particular constraints on statistical research makes it harder for them to investigate certain issues, since many of the most interesting questions from a normative standpoint don't lend themselves to these techniques. And even when the questions might lend themselves to quantitative analysis, many of the most promising hypotheses available for testing come out of historical or comparative research. In my view, a carefully conducted, closely researched CH study has no match in the search for the causes behind important social phenomena. Hence, it is hard to imagine the success of the spate of quantitative research on the rise of democracy by people like Daron

What makes CH distinctive is simply that it is able to pose questions that other methodologies might not. So, for example, quantitative sociologists might very well be interested in changing the world...[b]ut the particular constraints on statistical research makes it harder for them to investigate certain issues, since many of the most interesting questions from a normative standpoint don't lend themselves to these techniques....In my view, a carefully conducted, closely researched CH study has no match in the search for the causes behind important social phenomena.

Acemoglu, James Robinson, Carles Boix, and Adam Przeworski without the antecedent work of Barrington Moore, John Stephen, Evelyn Stephens, Dietrich Rueschemeyer and others.

What does not set CH apart, and what should not, is that it might somehow be unique in embracing such an agenda. What sets CH apart is methodology, not its goals. And indeed, the methodological distinctiveness of CH cuts both ways – just as it is able to pose certain questions that other methodologies are not, so it is also limited in its own ways. This means that CH should ideally work in a coordinated and complementary fashion with other methodologies, all of which should be motivated by the same basic goal of changing the world. In other words, changing the world should be a fundamental goal of all social sciences. And why not? It is hard to see what other reason there might be for doing social science at all. If the goal of science is to understand the causal structure of the world, it seems morally outrageous to not direct this ambition at uncovering the mechanisms that perpetuate unjust social arrangements.

Won't this undermine objectivity?

We come now to the second issue, viz. should CH be in the business of changing the world? A call to embrace an explicitly normative agenda often raises concerns that in so doing, we might undermine the objectivity of our scientific practice. I think this is a very valid concern – but it is based on a confusion of objectivity with *neutrality*. Objectivity requires only that we respect the integrity of scientific work, of the findings it generates, even if we don't like them. But neutrality is very different. What it requires is that we abjure any judgments about the moral or political significance of the practice in which we are engaged. It demands that we not make any judgments about the issues that we take up in our research, whether it pertains to our motivation or to our findings. The worry is that if we let our judgments influence either end of our practice – its motivation or the outcomes – we would be tempted to distort the work to suit our ends.

But it is a simple enough matter to see that the worry, while germane, is unfounded. It is of course possible that scientists let their views bias them. But there is no reason to believe that the embrace of normative commitments must lead to such an end, or even that it significantly raises the likelihood of it. Indeed, I would urge that the very opposite is the case – that a commitment to changing the world is a recipe for *better* scientific practice. A social scientist with the commitments I endorse is no different than an epidemiologist embracing the goal of eradicating a disease, or a mechanic seeking to build a better engine. All three are cases of scientists committed to a normative goal – reducing the incidence of an illness, or making a more efficient automobile, or reducing unemployment – and generating a research agenda that serves this purpose. We do not often worry that the epidemiologist's passion will interfere with his science. Indeed, we typically take his commitment to his cause as laudable, as a source of energy and dedication to his vocation.

A sociologist seeking to understand the causes of long-term unemployment is, in principle, no different from the epidemiologist. And if the commitment is genuine, then not only will it fuel her pursuit of the goal, it will encourage her to *more fully* respect the facts of the matter, not distort them, precisely because she actually *wants* to know the truth, so that she might effectively intervene in the social world. It is of course true that particular individuals might let their passion interfere with their objectivity – but this is not built into the fact of being passionate. The worry that social commitments will undermine objectivity depends on the corrosive effects of normative commitments being a predictable consequence of holding to

the latter. If all it means is that in some cases these commitments might intrude, it is no more worrisome than any other occasional slip in scientific rigor.

Effectuating change

The upshot of the argument this far is that, far from being in tension, moral commitment and scientific objectivity are in fact natural bedfellows. But how will this bring about change? Naturally, for ideas or knowledge claims to actually have any traction, they have to find anchor in some kind of social force. In the case of social science committed to the pursuit of social justice, that force cannot be the power centers that serve to uphold the status quo, or the groups that benefit from it – for reasons that should be obvious. This is not to say that research laid at the feet of elites cannot help change things. It most certainly can on those issues that do not touch the basic power and prerogatives of dominant groups. Where the implications of committed research do not undermine dominant interests, there is reason to suppose that the latter groups might use it toward desirable ends. But in cases where elite interests would suffer, the recommendations issuing from committed research will either be ignored … or land the researcher into some degree of discomfort.

…if it really wants to change things, CH needs to understand that there is only limited utility in "Speaking truth to power" … because "power" isn't listening.

So if it really wants to change things, CH needs to understand that there is only limited utility in "Speaking truth to power" … because "power" isn't listening. Since many of the truly unjust social institutions are directly maintained by elite groups, if CH or any other stream of research wishes to actually change the world, it will benefit from turning its attention to those groups and organizations that have an interest in the same ends. The most obvious way of course is to be directly connected to institutions and organizations committed to social justice. For decades, this was a common practice among socially committed intellectuals. They were members of political parties, worked with trade unions or activist organizations, published in their magazines, etc. The degree of their proximity and the intensity of their relationship varied of course. Some became full-time researchers for the organizations while others maintained a friendly, but serious, conversation. Nevertheless, for such intellectuals, such political groupings provided a moral anchor as well as a potential vehicle for the ends to which they were committed. But a direct involvement with these organizations is just one, albeit most intense, manifestation of such an orientation. It can also take more indirect forms – such as adopting research agendas that might be useful to them, publishing in a language they can use and disseminate, helping train their functionaries, etc.

All of these strategies are ones that academics engage in routinely without thinking twice – when they are connected to elite institutions. They apply to establishment research institutes, appear on the corporate media, struggle to get op-eds published in the establishment newspapers, etc. What I am suggesting is that for scholars interested in making the world better, there is a greater utility to turning toward the smaller, more marginal, and less prestigious institutions of social change.

It is my impression that many, if not most, of the people within CH are in fact quite open to changing the world in the sense suggested here. The problem is that within sociology, CH is today a somewhat marginal field, and

becoming more marginal by the year. This is in part because the discipline itself is returning to a kind of narrow and quite blinkered research agenda. We do not see the "big questions" taken up in anywhere near the proportion that they were a generation ago, and CH has always been associated with "big question" research. But another dimension of it is the intense speed-up that is being pushed in the leading departments, where graduate students are being pressured to finish up in five or six years. The shortening of time-to-completion structurally discourages CH research, which inevitably takes longer than the typical ethnographic or quantitative dissertation. It might not be an exaggeration to say that at this moment, the most pressing question is not how CH can save the world, but how it can maintain its weight and presence within the discipline of sociology.

Sent for: how to engage public policy

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It was a Tuesday in September of 2013 when José Quiñonez, an Ashoka Fellow who founded The Mission Asset Fund,1 headed to the US House of Representatives to explain the consequences of not having a credit score. The hearing was slated for the Rayburn House Office Building, described by *The Washington Post* as "Middle Mussolini, Early Rameses, and Late Nieman-Marcus." Today's hearing would be about building credit scores for people who pay their bills on-time but remain invisible to credit card companies, banks, employers, landlords, and other service providers who make life-altering decisions about who gets access to what under what terms. Some in attendance viewed credit visibility as just one more manifestation of a financialized economy sustained by predatory consumer credit. Like social reformers before them, Quiñonez along with Congressmen Keith Ellis (D-MN) and Congressman Michael Fitzpatrick (R-PA), the principal sponsors of the legislation, understood credit as justice. This notion is not a new one, particularly if we look to the experience of the Feminist Federal Credit Union in the early 1970s as well as the set of hearings sponsored by Senator William Proxmire pushing for the Consumer Credit Protection Act of 1968. The National Urban League and other civil rights organizations have long recognized the role of credit for participating in social and economic life in the US (Hyman 2011; Prasad 2012).

I, as a sociologist, needed to take on the role of a doctor at my patient's bedside, explaining an alternative course of treatment.

Quiñonez had invited me down to DC to get a feel for the players in these ongoing debates. This was a natural extension from the field site where I was shadowing staff of the Mission Asset Fund and sitting in on staff meetings. He also offered some useful advice for how best to engage with his organization, reminding me exactly what they are up against. Organizations serving low or moderate-income families are confronted by a broad set of assumptions about the poor and their money. More damaging, those holding such assumptions may be policy makers and foundation program officers who bracket family relationships along with cultural and moral concerns in order to focus on incentives and cognitive biases merely in need of a nudge. While thinking about the psychological foundations of decision-making proved useful, there remained too little attention paid to the relationships shaping cognitive processes.

I, as a sociologist, needed to take on the role of a doctor at my patient's bedside, explaining an alternative course of treatment. I couldn't get bogged down in the details of molecular biology or the nuanced differences among the various clinical studies justifying the course. I needed metaphors for relationships and how they work, narratives generated from practical engagements with comparative-historical materials, ethnographic investigations, and interview-based studies where the individuals in question are busy doing relational work—marking the meanings of their relationships through earmarking, colorful narrative accounts, and emotionally laden attempts to establish, maintain, honor, or sometimes dissolve important social ties (Bandel 2012; Zeilzer 2012; Wherry 2016).

We have to position ourselves to be sent for. If a policy maker wanted to talk with an economist, she need not look far. So we should stop trying to be economists (those of us in the trying business) and offer up what we know in a manner that policy audiences can know it. We don't need to save the world; we simply need to explain it.

With this alternative explanation of household finances, I went to the White House in the fall of 2015, sat in the Roosevelt Room, and interacted with members of the National Economic Council and representatives from the financial industry, nonprofit service providers, think tanks, and maybe two other academics (maybe). The invitation arrived after my op-ed in the *New York Times* on payday lending started making the rounds in policy circles. There was no time for citations, throat clearing, or the usual academic nuance. Historical evidence and modal stories illustrating the meanings of money and credit had to be clear and lean. Sharp contrasts had to be drawn to get us away from thinking about people as merely in need of financial education to evaluate the payoffs to action. Surprisingly, those with the most on the line (economically speaking) seemed most drawn to comparative-historical accounts and ethnographic meanings. They already have well trained staff to establish empirical patterns. They have fewer opportunities to understand what those patterns mean. They were looking to interpretive social scientists for compelling explanations emanating from practical wisdom.

In my days in the masters program of public affairs at the Woodrow Wilson School, I heard the adage, "Nobody wants somebody nobody [sic!] sent for." We have to position ourselves to be sent for. If a policy maker wanted to talk with an economist, she need not look far. So we should stop trying to be economists (those of us in the trying business) and offer up what we know in a manner that policy audiences can know it. We don't need to save the world; we simply need to explain it.

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1. See: http://missionassetfund.org/

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"Sociologue engagée" : a contribution to a debate on "can comparative historical sociology save the world?"

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Although I entered academia through political philosophy and Marxism, I chose sociology as my field of research because I thought that political theory was too remote and did not make enough of a difference. I am all for "free inquiry" and research that is independent of "social impact" concerns but my commitment to social change trumps all. I would rather create knowledge that provides alternatives to the ambient utilitarianism, individualism, and psychological/economistic babble of our times than do just about anything else. It still keeps my blood flowing. I made this particular choice when I was twenty years old and it still works for me.

This choice has taken several forms over the last fifteen years. First and foremost, in 2002 I took the lead of the Successful Societies Program, which I co-direct with political scientist Peter A. Hall, and which is funded by the Canadian Institute for Advanced Research. This program has focused on the various dimensions by which one can assess collective well-being, ranging from recognition and social inclusion (how does it work, really?) to the social incorporation of immigrants and low infant mortality. Our first book, *Successful Societies: How Culture and Institutions Affect Health* (2009) aimed explicitly to engage epidemiologists in order to get them to consider broader social dynamics (including cultural repertoires) that influence health outcomes, and the health gradient in particular. Our second book, *Social Resilience in the Neoliberal Era* (2012) pursued a similar goal, but focused precisely on the challenges that come with neoliberalism, such as the dominance of market fundamentalism and the privatization of risk. This book considered how institutions and cultural repertoires can provide buffers and scaffoldings to human capabilities in the specific context of this neoliberal era. We demonstrated our argument with (often comparative) case studies written by social scientists from a range of disciplines (including political philosopher Will Kymlicka, sociologists Peter Evans, Ann Swidler, and Ron Levi, historian William Sewell, Jr., political scientist Jane Jenson, and others).

It is in the context of these Successful Societies projects that I started work in 2006 on a massive comparative study of how members of stigmatized groups respond to ethnoracial exclusion. This coauthored book, titled *Getting Respect: Dealing with Stigma and Discrimination in the United States, Brazil, and Israel*, is to be released in August by Princeton University Press - the other authors are Graziella Moraes Silva, Jessica Welburn, Joshua Guetzkow, Nissim Mizrachi, Hanna Herzog, and Elisa Reis. This book considers how African Americans, Black Brazilians, and three Israeli groups (Arab Palestinians, Ethiopian Jews, and Mizrahim) experience ethnoracial discrimination and respond to it. After developing a very detailed analysis of variations across groups (What is the most frequent type of incident mentioned by each group? Who confronts most? Which group promotes individualist versus collectivist response?), we explain these variations by how the cultural repertoires that prevail across contexts empower some responses rather than others. We also consider groupness, i.e., the extent to which our four hundred plus interviewees understand the incidents as having to do with their ethnoracial membership or with other dimensions of their identity (e.g., being stigmatized as lower class). We find quite contrasted patterns, with two relatively strongly bounded groups (Arab Palestinians and African Americans) and two more weakly bounded

groups (Black Brazilians and Mizrahim). One of the contributions of this book is to provide a sociological analysis of how ordinary people fight for recognition and how various social policies, ideologies, and interventions can facilitate more inclusion and broader definitions of cultural membership (which we view as important measures of societal success). As such, this book is both a contribution to scholarship and an intervention in how to change the world, i.e., how to activate levers for greater agency for citizens who are not actively involved in politics or in social movements. In my view, the production of knowledge for the purpose of change is unconceivable without an effort to understand social reality from the perspective of rigorous and systematic empirical analysis of social dynamics. You have to get it right if you are going to be effective, even if the reality you unearth is not to your liking.

I want to return to our books *Successful Societies* and *Social Resilience in the Neoliberal Era*. These two interdisciplinary books did make a difference and captured the imagination of some scholars ranging from experts on the quality of government (such as Bo Rohstein) to scholars interested in human flourishing (such as Bryan Turner and Philip Gorski) and related questions. Another measure of the influence of our ideas is that the Australian Sociological Association will hold its 2016 annual meeting around the theme "Cities and Successful Societies," that the 2016 meetings of the Council for European studies has "resilience" as a theme, and that the 2014 meetings of the German sociological association focused on crisis and social resilience.

In the United States, our agenda also attracted the attention of the Robert Wood Johnson Foundation as it was launching its new funding program on "Cultures of Health." Together with fellow comparativist Mabel Berezin, I was invited to co-edit a special issue of the journal *Social Science & Medicine* (almost forthcoming as of this writing) with the vice president of research of the Foundation on the topic of "Solidarity and Health Cultures." This issue features essays on mutuality, mobilization, and messaging from fellow cultural sociologists and social movement experts Christopher Bail, Abigail Saguy, Michael Schudson, Steven Epstein, Irene Bloemraad, and others. Our goal is to stimulate novel conversations in the field of health by bringing to it "new" ideas such as narratives, scripts, repertoires, and institutions (see also Lamont and Small 2008). Will this project make a difference? Probably, if the Foundation embraces a more multilayered understanding of the causal paths that feed collective well-being, as an alternative to a dominant individualist approach focused on choices and incentives. And this thinking in turn can affect funding priorities. So in small ways, I do feel that I am saving the world one small step at a time. I do this both as a comparativist and as a plain old generalist.

In my view, the production of knowledge for the purpose of change is unconceivable without an effort to understand social reality from the perspective of rigorous and systematic empirical analysis of social dynamics. You have to get it right if you are going to be effective, even if the reality you unearth is not to your liking.

This ameliorist aspiration of my scholarship also comes to light in my mentoring. I have been working with a number of graduate students who are bringing the analytical constructs of cultural sociology into fields that

have thin views of culture or are still operating with the Parsonsian view of norms and values. Although these students more often than not are working on domestic issues, they do contribute to a reformulation of questions that matter. For instance, in a paper titled "How Judges Think about Racial Disparities: Situational Decision-Making in the Criminal Justice System" (forthcoming in *Criminology*), Matthew Clair and Alix Winter analyze interview and ethnographic data on the beliefs of state court officials concerning criminal justice processing and jury selection. Caitlin Daniel studies the interaction between low income parents' economic resources and their ideas about food. She recently published a much noted paper in *Social Science & Medicine* on why parents defer to their children's preferences. Also, Monica Bell's paper "Situational Trust: How Disadvantaged Mothers Reconceive Legal Cynicism" reveals specific ways that poor African-American mothers - often depicted as either disdainful of police or as manipulators who unfairly call the police on their relationship partners and children - understand and strategize around criminal justice in the age of heavy policing. This article, which received a number of awards is forthcoming in the *Law & Society Review.*

As argued and shown by medical anthropologist Iain Wilkinson and Arthur Kleinman in his recent book *A Passion for Society: How We Think about Human Suffering*, our discipline has always experienced pendulum swings between critical detachment and abstraction on the one hand, and a desire to change the world on the other. Both dispositions are equally necessary in my view. Bad scholarship too often comes out of the over-politicization of academia. Yet, how can we maintain the attitude of the detached scholar as right-wing populism is spreading fast and furious in the United States as well as in Europe? Most of us became social scientists as we were moved by a variable mix of intellectual passion and moral conviction. We respond to this vocation by focusing our attention on what truly matters most to us, and by looking where we can find self-realization and satisfaction. While I do not claim that this approach is ideal or should be emulated by others, buried inside of me remains the view that academics should empower social change and contribute to collective well-being by all means necessary. To not do so is to abdicate in the face of inequality and human suffering and to indulge ourselves in the comfort of our privileges, including the increasingly luxurious freedom of speech and thought. This is not all about us, after all…

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Strategy or serendipity?

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

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

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

On the way to addressing that question, I advance three claims. First, comparative and historical sociologists should not avoid policy-relevance on principle, even as we recognize the dangers in research driven too directly by our own policy preferences or those of state authorities and corporate funders. History and comparison play important roles in policy debate; we all have a stake in the quality of that scholarship. Second, scholarship and policy operate on different clocks. Therefore, comparative historical sociology is likely to be at its best in "problem-illuminating" rather than "problem-solving." But because the most powerful scholarship identifies *how* things come to be, such research also contributes, at

least potentially, to the wisdom of those engaged in present-day problem-solving. Finally, in order to produce scholarship that illuminates key problems of the extended past, present and future – what some San Francisco thinkers term "the long now"[2] – decisions about research need to go beyond existing theory debates and methodological concerns to address problem choice. Comparative historical sociology is often defined by its commitment to "big questions," but we need to think hard about the nature of those big questions for our own time.

When policy science turns to history and comparison

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

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

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

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

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

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

The future, of course, is unknowable. It may well be that the most policy-averse historical sociologist is currently immersed in a topic that is about to be made salient by events. In the 1980s heyday of comparative research on revolutions and state-formation and welfare policies, who would have suspected that the solo scholar digging deeply into the shifting and schismatic politics of the Crusades would have so much to say to policy two decades later, in the aftermath of 9/11? Problems of empire have attracted new generations of scholars as they have become unavoidable in rethinking global politics. As we look at the unexpected character of the 2016 presidential primary season, we can all benefit from comparative-historical research on populism even as many in the United States imagine that

"it can't happen here." Given the different tempos of politics, policy, and scholarship, it would be short-sighted to tie our work too closely to present concerns *even if* one took the position that all scholarship should have some policy relevance (a position that I am absolutely not advocating). The key point is that historical sociology, whatever the initial motives of specific researchers, provides a store of topically-relevant hypotheses, provisional models, and theoretical generalizations that are ready to hand as we try to make sense of the present and future.

Deep analogies and where we might find them

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

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

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

Such a research-based conversation would make all of us wiser when it comes to thinking seriously about the implications of new energy technologies. Historical geographers, climate scientists, and economic historians are already pursuing such topics. Arguably (and, really, one does not need to argue terribly hard on this

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

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

Endnotes

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

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

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

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

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

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

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

economic interests were responsible for the ultimate abolition of the transatlantic slave trade. He concludes that it was only the coincidence of all three of these forces, none more primary than the others, that led to the abolition of the trade. The book is among other things a pioneering study of social movement outcomes, and the argument is notable for its sophisticated treatment of conjunctural causation, for its recognition of contingency, and for its use of explicit counterfactual reasoning. (DuBois argued there was a missed chance to abolish the slave trade at the time of the American Revolution, and supported this argument with a comparison to the Civil War.)

How well does this book stand up? The sources and methods available to historians of the slave trade have improved dramatically in the 120 years since DuBois finished his dissertation, and we now know better on many points. Historical demographers will note that he misjudged the extent of smuggling in the mid-19th century because he overestimated deaths and underestimated births in the enslaved population. (This point is the subject of a truly great footnote in David Levering Lewis's biography of DuBois.) Still, as a work of scholarship, this book holds up rather better than Émile Durkheim's dissertation, which your colleagues still teach. Consider assigning this one as comparative historical sociology's answer to that one.

My exhibit B is *Black Reconstruction in America, 1860-1880* (1995 [1935]). This book uses comparative reasoning to argue that Reconstruction was a missed opportunity to achieve social democracy in America. The book also illustrates the power of unexpected historical analogy to force a rethinking of abstract concepts. DuBois argues that the Civil War was decided by a "general strike" of enslaved laborers who collectively walked off the job and thereby deprived the Confederacy of resources. He also argues that the Reconstruction governments amounted to a "dictatorship of the proletariat." These are deliberate conceptual provocations that draw our attention to unexpected similarities across time and space. The use of surprising—and debatable—analogy is one of the characteristic intellectual moves of comparative historical sociology, executed here to great effect.

Black Reconstruction also seems methodologically innovative in its use of biographical detail to support a sociological argument. DuBois devoted hundreds of pages to the lives and achievements of Black legislators in the South during Reconstruction. It is easy to misread this part of the book as nothing more than a vindication of a few great Black men. DuBois certainly was an elitist. But in this text, his is best understood as *methodological* elitism, in service of a sociological point: he takes the elite to be interesting because following people of unusual ability allows him to reveal the social limits on human achievement, in much the same way that glass ceilings only become visible when you climb high enough to bump into them. The book's use of personal biography to reveal both contingency (what if Lincoln hadn't been killed?) and structure (what stopped a talented politician like Hiram Revels from becoming another Lincoln?) exemplifies DuBois's distinctive approach to sociology as the scientific search for "the limits of chance in human conduct" (DuBois 2000 [1905]).

This book stands up surprisingly well. Maybe for this reason it is one of very few of DuBois's books still under copyright that is still in print in an inexpensive paperback edition. If you could only read one book about the history of Reconstruction, you would probably not want it to be *Black Reconstruction*, but instead Eric Foner's *Reconstruction: America's Unfinished Revolution, 1863-1877* (1988)—only because that book is, basically, an update of *Black Reconstruction*. But if you want a classic comparative-historical text that uses

Reconstruction to exemplify a sociological approach to war, revolution, the creation of labor markets, and the question of why there is no social democracy in America, you will want to choose DuBois.

Why don't more of us teach DuBois already? Sociology, like other fields of academic production, is organized around a distinction between pure and applied scholarship that is also a distinction between high-status and low-status positions. Twentieth-century comparative historical sociologists positioned themselves on the pure end of the spectrum. DuBois was impure. Morris (2015) argues that it was not only the racism of academic gatekeepers, but also their aversion to DuBois's activist orientation, that shut him out of the best libraries, grants, and graduate teaching opportunities. His long career of scholarship produced no dissertation advisees employed in departments of sociology. No surprise, then, that when a handful of early-career sociologists convened in 1979 to discuss the comparative historical scholarship of their own teachers—in the conference that led to the canon-making *Vision and Method in Historical Sociology* (1984)—they left out any mention of DuBois and his legacy. They weren't his students.

[DuBois] never claimed that comparative historical sociology could save the world, but he certainly thought that the world needed saving, and hoped his historical sociology could contribute something to that project. He did not save the world, but he changed it.

But we can still claim him as a teacher. Although the philosophical interpretation of DuBois is vigorously debated, I read him as a pragmatist, and the pragmatist case for putting DuBois on the syllabus does not rest on the historical significance of his works. Life is too short to read every book of historical significance. Instead, it rests on the *usefulness* of reading his works for our own collective projects of understanding and changing the world. He never claimed that comparative historical sociology could save the world, but he certainly thought that the world needed saving, and hoped his historical sociology could contribute something to that project. He did not save the world, but he changed it.

Indeed I sometimes think that we do not read DuBois because he was so effective at changing the world that his works now seem dated. He spent an extraordinary amount of scholarly effort marshalling comparative historical evidence to refute racist theories that are now long discredited. Some of the hundreds of pages he spent arguing with racists are, as they say, of merely historical interest. You can skip those parts, of course, just as you may have skipped a few pages of Marx's screeds against this or that forgettable left Hegelian. But as you skip these pages, reflect that it was DuBois who discredited those discredited theories. It would be ironic if he went unread just because he won the most important argument in the history of American sociology.

Endnotes

1. Not all of his works fall into this category: he spent much of his long career without the security of an academic appointment, he wrote quickly under conditions that were not always ideal for careful scholarship, he sometimes lacked access to the best or most recent sources, and he often deliberately mixed genres in ways that make his writings hard to classify or evaluate. The same was true of many of the greats, of course, and we preserve their reputations for greatness in part by reading them selectively and ignoring their weaker efforts. (Who now remembers *Herr Vogt*?) I propose that we extend the same respect to DuBois.

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How to do policy-oriented social science

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Guiding Strategies and Policies

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

but extended to more moderate causes. As historian Robin Blackburn (2011) pointed out, Marx diagnosed that in the historical development of the wage labor system, extra-economic exploitation outside the system like slavery was instrumental to strengthening capitalists' hands in their containments of free workers' struggles. He concluded in many of his newspaper writings that the international working class movement had to throw their support behind the US abolitionist movement to maximize the freedom of Europe's free workers to struggle, even though the abolitionist movement was led by the American bourgeoisie. Marx even sent a letter to Lincoln on behalf of the International Workingmen's Association to express their support of the North. The US ambassador in London returned to Marx, confirming the President had read Marx's letter and was thankful. Marx's position on the American Civil War, as derived from his analysis of the historical development of wage labor, was no small thing. In the early stage of the American Civil War, many liberals and leftists sympathized with the Confederacy. To them, supporting the South seemed to be a more natural position from either a free trade or self-determination standpoint. Marx's and the International Workingmen's Association's support of the North contributed to shifting Europe's public opinion to become more favorable to the Union. The hesitation of Europe in general and UK in particular in aiding the Confederacy, which expected that such support could lend them victory over the North, contributed a great deal to the eventual triumph of the Union and the final dissolution of slavery in America.

After Marx, generations of intellectuals followed his spirit to count on historical-comparative analysis to guide their advocacy and practice for liberating social changes, even though they are not necessarily the revolutionary type like Marx. For example, Charles Tilly (1978), through his classic works on nineteenth-century rebellions and social movements in Europe, theorized that organizational resources and political opportunities brought by cleavages within the ruling elite circle were critical for the rise of contentious politics. These insights inspired many analysts and practitioners of social movements to see the importance of organization building and seeking alliance with elite sympathizers. In contrast, Frances Piven and Richard Cloward (1978), based on their investigations in the history of popular movements in twentieth-century America, concluded that movements that were not connected to the power elite and not constrained by formal organizations were usually more effective in bringing progressive changes, as they dared to be more disruptive and hence more powerful in intimidating the ruling elite to yield. These opposite views on social movements are both historically grounded. Only more historical sociology, not less, can resolve their differences.

Historical-comparative analysis is essential to the discovery of general conditions or universal dynamics underlying social movements and revolutions, as their emergence and success usually did not transpire in numbers large enough for effective quantitative analysis at any given time. Instead, social movements and revolutions usually occurred in waves and were distributed unevenly across time. To look for cases for comparison and generalization, one has to choose historical instances at different times and places. Movements and revolutions, to be sure, are just two examples of this type of social phenomenon. Another object of study for which comparative historical analysis is particularly pertinent is the process of development, industrialization, or industrial upgrading. Like social movements and revolutions, successful take-off of developing economies does not happen all the time in great numbers, and analysts need to look at historical cases in different parts of the world to compare

and to unearth the conditions conducive to successful development.

For example, Andre Gunder Frank (1966) compared economic growth in different periods of Latin America and found that most Latin American countries grew rapidly when their economic linkages to the metropoles in North America and Europe were severed by wars or depression, while their growth slowed down when their linkages with the metropoles revived. He therefore formulated the famous "development of underdevelopment" thesis, arguing that trade and investment links with rich countries were detrimental to development in poor countries. Policy makers and politicians influenced by this school of thought experimented with different regimes that restricted trade with and investment from rich countries as development policies. For another example, scholars who compared the postwar development of East Asia and Latin America reached a different conclusion, contending that what determined successful or unsuccessful development was not linkages to wealthy countries, but the autonomy and capacity of the state (e.g. Evans 1995). As such, the most effective policy to promote development is not to sever ties with rich countries, but to strive to build a capable, resourceful and development-promoting bureaucracy. Again, these two contrasting views and the corresponding policy prescriptions are both grounded on comparative-historical analysis, and their resolution will require more, not less, historical sociology.

Recovering Repressed Agencies and Processes

Comparative historical sociology also contributes to progressive social change by helping recover historical processes and agencies that were crucial in the causal chains of historical development, but were repressed in contemporary historiography for political or other reasons. One good example is the work that reconstructs the contribution of slave rebellion in the Caribbean to the end of slavery in the world capitalist system. In the hegemonic historiography, the abolition of the slave trade and the rise of the abolitionist movement that led up to the American Civil War were mostly enacted by white Christians who detested slavery as an immoral system and fought for its end. In popular culture, we saw a lot of glorifying narratives about William Wilberforce and Abraham Lincoln. In these narratives, slaves of African descent were little more than passive victims waiting to be emancipated by conscientious white men, and the agency of the black resistance rarely entered the picture until Martin Luther King.

There has been a spate of sociological works showing that what set the abolitionist movement in motion or created the condition for its success in Europe and the United States was the wave of slave revolts at the turn of the nineteenth century - those in the Caribbean in particular (Patterson 1970; West and Martin 2009; Silver and Slater 1999). These revolts culminated in the slave revolution in French Saint-Domingue in 1791-1804. The Black Jacobins who led the rebellion there were inspired by the French Revolution. They overthrew the slavery system on the island and founded the Haitian Republic. The army of former slaves not only defeated Napoleon's army which tried to reinstall French rule and slavery, but also expelled the British navy which tried to conquer Haiti for fear that the Haitian Revolution would spread to British Jamaica. The triumph of the Black Jacobins sparked a wave of slave revolts in the American South, forced France to leave North America, and made the Louisiana Purchase possible. It created great anxiety among slave owners that slaves imported from Africa could no longer be assumed to be docile. This wave of slave revolts set the stage for the victory of the British abolitionists in banning the slave trade in 1807, and contributed to the rise of the

US abolitionist movement that moved the North to a staunchly anti-slavery position. These revolts also fostered transnational networks of solidarity and exchanges among the African diaspora across the Atlantic Ocean that persisted through the nineteenth and twentieth centuries and helped connect African anti-colonial struggles and the many currents of the civil rights movement in postwar America (see West et al eds. 2009).

While historians might unearth some forgotten events repressed in hegemonic collective memory, it is often historical sociologists who rediscover the systemic significance of these events, the agencies embodied therein, and the inter-connectedness among these agencies and processes. The recovery of these silenced voices can shed new light on our interpretation and understanding of contemporary movements like Black Lives Matter and contribute to our formulation of effective strategies in those movements.

Another example illustrating how comparative historical sociology rediscovers repressed historical processes is the new sociology of neoliberalism. Dominant interpretation of the rise of neoliberalism according to the monetarist orthodoxy asserted that neoliberal reforms in the US and UK in the 1980s were necessary, technical, and successful responses to the stagflation crisis caused by the failure of Keynesianism in the 1970s. This interpretation of history has spread fear of inflation in most advanced capitalist countries and strengthened the call for austerity even at times of economic crisis, making many believe that major increases in government spending and money supply would bring the stagflationary 1970s back. Niall Ferguson has been predicting that fiscal and monetary stimuli under Obama's White House and Bernanke's Federal Reserve would foster the nightmare of double-digit inflation. Though this prophecy has never been fulfilled, it lent support to the Tea Party agenda of defunding the state. It also fuelled the rise of austerity politics in Europe.

Thanks to the new sociology of neoliberalism (e.g. Prasad 2006; Martin 2008; Krippner 2011), we now know that this orthodox interpretation of the rise of neoliberal reform is a myth. Instead of a technical and impartial remedy to economic failure, the liberalizing reforms in the US and UK in the 1980s stemmed from capitalist and middle class mobilization aimed at taming the power of organized labor and rolling back progressive taxation. More, inflation in the 1970s was driven by powerful organized labor, wage growth, and high employment. Inflation at that time devalued debt and lowered inequality, and it was bad to owners of capital but not necessarily bad for debtors and workers. The stagflation crisis was therefore more a crisis of capital than a crisis of labor (Hung and Thompson forthcoming). The subsequent three decades of neoliberalism has shifted the

While historians might unearth some forgotten events repressed in hegemonic collective memory, it is often historical sociologists who rediscover the systemic significance of these events, the agencies embodied therein, and the inter-connectedness among these agencies and processes. The recovery of these silenced voices can shed new light on our interpretation and understanding of contemporary movements like Black Lives Matter and contribute to our formulation of effective strategies in those movements.

balance of power between capital and labor heavily to the former's favor. Its consequence is a runaway financial bubble, escalating inequality and underemployment, and more recently, deflationary pressure. It follows that the prevalent worry about inflation among economists and policy makers today is excessive, and deflation is in fact a bigger risk. What we need now are bold public policies that re-empower the working class, revive the social state, and reverse the rise of inequality.

History is too important to be left to historians. Comparative-historical sociologists are bestowed with the obligations to generalize and theorize from important historical processes across different parts of the world, as well as to uncover historical processes and agencies that have been repressed by ideologies. These are not only important for our better understanding of the world, but are also important intellectual building blocks for viable social movements and public policies that could bring us a better world.

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What do you think?

The debate continues online at: http://policytrajectories.asa-comparative-historical.org/2016/04/how-chs-can-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

War, States, and Contention

A Comparative Historical Study

Cornell University Press

Sidney Tarrow

Editor's Note: The following text is based on an author-meets-critics session that was organized by Antonina Gentile for the Annual Meeting of the Social Science History Association in November, 2015. My thanks go out to John Hall, Bill Sewell, Lis Clemens, Dan Slater, Antonina Gentile, and Sid Tarrow for agreeing to contribute their comments to the newsletter. I would also like to thank Jeff Isaac and James Moskowitz at* Perspectives on Politics *for allowing an extended version of Dan Slater's comments to appear in* Trajectories*, and Adam Hirschberg at Cambridge University Press for arranging the necessary permissions.

Deep History and the Present Moment

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Before proceeding to my own comments, let me offer a summary of Sidney Tarrow's book. *War, States, and Contention* is a complexly structured history charting the dialectic of states and contentious politics as they shape and are affected by war. Professor Tarrow focuses on the West from the French Revolution through the 21st century, and the concluding chapter considers post-World War II colonial struggles for independence to demonstrate the analysis's wider relevance. Fundamentally, Tarrow asks, "what is the long-term effect of the relations between war and contention on the development of modern states?" (27). Analytically, he draws especially on the contentious-politics perspective that is the central legacy of Charles Tilly. Conceptually, he is especially informed by Michael Mann's distinction between despotic (recast by Tarrow as "hierarchical") and infrastructural power. And in historical terms, Tarrow addresses how the Schmittean exception of emergency powers that states frequently invoke in wartime has changed over time. A central issue is whether emergency powers recede in the aftermath of war or "ratchet" up to new and enduring constellations of state power.

War, States, and Contention is divided into three parts. First, "War and Movements in the Building of New States" maps diverse *emergences* of modern states in relation to war and contentious politics by examining three cases – the French Revolution, the US Civil War, and Italian fascism. Everywhere is irony. For France, Tarrow describes the contradictions between the Declaration of the Rights of Man and the Reign of Terror, which initiated the French state's enduring and often heavy-handed dealings with inhabitants deemed insufficiently lacking in the qualities of citizenship. For the US Civil War, Tarrow narrates the ante-bellum complexities of

Western expansion and abolition social movements. Given wartime emergency measures (such as suspension of *habeas corpus*), he concludes, "The Civil War … was the fountainhead for the future emergency powers of the American state, both in war and in war's wake" (70). Italy in the early 20th century underwent a different process. Facing strong regional differences and incomplete democratic integration, the state moved strongly toward a wartime footing, right down to disciplining workers in factories. Italian fascism subsequently built on the wartime "exceptions" (97). In Tarrow's analysis, Mussolini managed to have a party take on the form of a militia, but one inspired as a movement that became a harbinger of Nazism (98).

Part II, primarily focusing on the US, traces the transformation of modern states via the emergence of "Endless Wars" resulting from globalization, internationalization, and technological change. The old distinction between intrastate, interstate, and extra-state wars no longer works. A new kind of war emerges, "composite war," war in which social movements fight against states, not within states but on an international scale. The War on Terror is a novel extension of tendencies in the US building over the 20th century toward the consolidation of a permanent National Security State that has increasingly articulated with private corporations in what Tarrow usefully dubs "military Keynesianism."

Over the course of the 20th century, along with US neo-imperialist institution-building came suppression of political dissent – from antiwar movements during the Great War and its extension in World War II to the Cold War and the Vietnam War – all before 9/11. Internment of Japanese and Japanese-Americans, McCarthyism, FBI surveillance and harassment of the Civil Rights movement and the anti-Vietnam war movement – these were not isolated events. Rather, Tarrow shows, they manifested a general extension of state power to control political dissent.

Such developments mushroomed in the post-9/11 "War on Terror" – through torture and its tortuous legal justification, increased surveillance, press manipulation, and subordination of corporations in electronic surveillance. In the US the exercise of state power increasingly exceeds any democratic capacity at controlling it, yielding what British politics scholar Colin Crouch (2000) has called "post-democracy." Part III then shows that in the 21st century, movements such as al Qaeda and ISIS have become transnational, while internationalism, once heralded as a liberal process, has developed a "dark side" – a war on terror pursued through multilateral networks. We now begin to approximate an "international state of emergency."

In his conclusion, Tarrow paints an understandably bleak picture of the contemporary situation. The connection between citizenship and war, a cornerstone of the modern state, has eroded through the development of professional armies and private-contractor military operations. Earlier state propensities to *subvert* the law through emergency rule have been supplanted by rule under the guise *of law* in a post-liberal version of what political scientist Harold Lasswell once anticipated as a "garrison state." States have become ever more effective at maintaining acquiescence within civil society, and practices of war have diffused from the sites of conflict to other social domains, e.g., in the militarization of local policing.

However, war is a crucible of the dialectic between protest and repression in which new forms and scripts of resistance are forged, new alliances established, new opportunities unveiled. Tarrow is no Pollyanna. Yet *War, States, and Contention* clearly demonstrates

that across diverse historical moments from the French Revolution to the "endless wars" of the 21st century, contentious politics have shaped broader historical developments, sometimes in decisive ways. Tarrow concludes, "Civil society activism often fails. At best, it is hard, slogging, and frustrating work. But it is the only recourse for those who believe in the defense of rights against expanding state powers" (259).

A critical engagement

I have already tipped my hand concerning *War, States, and Contention* in a blurb on its cover:

> In the face of our epoch's wrenching conflicts, once seemingly solid understandings of the modern world are at risk of melting into air. Sidney Tarrow's lively and compelling study … establish[es] a new rubric and substantive analysis of modern history and the shifting geopolitical dynamics of the 21st century. *War, States, and Contention* is a necessary book for any serious discussion of our global future.

With Sidney Tarrow's framework, we can ask how things might be different, what our limitations are, and what we can do to shape the future. To begin to speak to those questions, as is our political obligation as scholars in the present historical moment, I will raise two issues – how to give *War, States, and Contention* a deeper historical footing in European history of what used to be designated as the medieval and the "early modern" periods, and how to build out Tarrow's analysis of the present historical moment in a more configurational theorization.

First, the deeper European history. Tarrow begins his study with the French Revolution and its aftermath, pointing to its invention of the modern citizen army (32) and the "first movement state," which gave rise to the first modern state (33, 258). To be sure, any developmental history must start some place to avoid an infinite historical regress. Nevertheless, we can gain analytic purchase by understanding the deeper origins and parallel dynamics of war, state, and contentious politics in earlier European history. I would suggest three "moments" for further inquiry.

First, long before the emergence of modern states, the consolidation of the Roman Church beginning in the tenth century heralded what might be called an "Empire of (Western) Christianity" as a post-Roman Empire civilization of world-historical importance. Through administrative centralization, the Church gained temporal power relative to feudal monarchs and lords (e.g., by eliminating lay investiture), and in the Crusades it became capable of mobilizing both nobility and commoners to military and "policing" actions – the suppression of "internal" dissent among Albigensians, Othering Jews, and financing and undertaking international conquest against Muslim hegemony in the Levant. As a "theocratic empire" in formation, the Roman Church demonstrated a striking capacity to mobilize both *hierarchical* power toward feudal powers and *infrastructural* management of believers (in what Weber called "hierocratic" domination, controlling people by channeling pursuits of salvation).

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The Roman Church did not succeed as a theocratic empire. But it initiated certain features of modernity that remain in play, notably in the modern dynamics of geopolitics,

war, and contention. The Empire of Christianity was a complex geopolitical assemblage encompassing a not omnipotent but temporally powerful church hierarchy in relation to both secular powers and supposedly subordinate but difficult to control church orders (like the famed Knights Templar, with its semi-autonomous military and financial powers). Comparing this (very) early modern complex to the present geopolitical situation, I warrant, could deepen understandings of "composite" and "endless" war under conditions of internationalization and globalization, thereby helping us consider future trajectories in relation to contemporary geopolitics.

Which brings us to my second sketch. A number of heretical and heterodox religious sects challenging the Roman Church anticipated Protestant sectarian movements beginning in the 16th century. As Robert Wuthnow (1989) and Anthony Marx (2003) have argued, the Reformation took hold (or failed) in diverse ways within different European societies. Trajectories of religious reformation – some of them violent, others not so much – were highly significant for how European monarchs consolidated state power in the 16th and 17th centuries. In Tarrow's framework, the Reformation was a (religious) social movement that transcended individual societies. It spread in part because individuals, groups, and ideas moved across national boundaries.

Consider the Reformation as it unfolded in the Puritan Revolution on the isle of Great Britain. Tarrow rightly points to the French Revolution as a signal event in the fusion of social movement and state power. But in the Puritan Revolution, the formation of the New Model Army as an ideological military force oriented to revolutionary consolidation of state power anticipated the "citizen army" that Tarrow (241) treats as originating in the U.S. and French revolutionary wars. As Michael Walzer (1965: 277) held concerning the Puritan Revolution, "Those who fought for God would have to *know the reasons*; only then could army regulations and religious fervor come together in a new discipline." In effect, the French Revolution transferred the fervor of sacred struggle from a religious to a (still quasi-religious) secular dispensation. Nationalism and citizenship became sacred. As for the international dynamics, England's long crisis of state legitimacy was only resolved in the Glorious Revolution of 1688, with the ascension of the invading William of Orange to the throne. In Great Britain and more widely, the Protestant Reformation was an international movement that inspired and gained strength from the contentious politics within societies, where political unrest could be decisively influenced by the engagement of foreign powers. Tarrow's powerful analytic framework, pushed back in time, can help us further understand the emergence of modern dynamics of contention and power.

My third sketch is, well, sketchy, but it consolidates a point left largely implicit in the first two. What I have described as relations among contentious politics, states, and war in the unfolding Protestant Reformation are conventionally characterized as Europe's "Wars of Religion." In these wars, whatever the specific rapprochement between state, Reformation, and Catholicism, there was one general outcome, one "ratchet" effect. Religion became "contained": states took over from the Catholic Church and contending Protestant confessions the "religious" function of determining the boundaries of acceptable doctrine and practice, defining state hegemony over religious deviance in a way that has continued to be important to our day (Hall 2009: 105-6).

This general development shifted contentious *religious* politics to the *secular* domain (albeit with continued religious overtones that are

especially apparent in the French Reign of Terror and its repercussions in subsequent episodes of "terrorism" carried out by secular "true believers"). However, it is precisely secular containment of religion that contemporary movements of what Tarrow (103) characterizes as "Islamist radicalism" contest. Tarrow rightly notes that Islamist movements share many dynamics with non-religious movements (115). Yet it is also worth

Tarrow has shown how war and contention – in conjunction with wider social and technological changes – have increased the power of the state over the past two centuries. If we describe the array of factors detailed by Tarrow as a configurational whole, it constitutes the emergence of a different sort of world order as such.

wondering whether – compared to nationalist, ethnic, and class contentious politics – the radical jihadist movements of al Qaeda and ISIS, like Reformation Protestantism, are transformational hinges that have the potential to refigure the geopolitical social order in more fundamental ways.

Which brings us to the contemporary geopolitical situation of endless wars and permanent emergency. Tarrow has shown how the pursuit of war is driven by domestic considerations, and once undertaken, war has consequences both for domestic politics and, sometimes, for the institutional arrangements under which politics proceeds. He wants us to ponder "whether the 'new wars' of the twenty-first century are merely an intensification of earlier trends or war-making against transnational clandestine violent movements has initiated a new wave of global contention?" (6). The answer depends on if and how contentious politics scale up to an international arena. The issue of whether emergency rule becomes a "permanent emergency" hangs in the balance.

One way to address that issue is to develop a broader theoretical description of what 21st century emergency rule might entail for global society. Tarrow has shown how war and contention – in conjunction with wider social and technological changes – have increased the power of the state over the past two centuries. If we describe the array of factors detailed by Tarrow as a configurational whole, it constitutes the emergence of a different sort of world order as such.

That global order looks something like the "Empire of Modernity" that I described in *Apocalypse* (2009: 129-31). One element, imperialism – in the previous era, the project of one or another nation-state – is becoming a transnational project of Western states and their allies. The other element, modernity – which Jean-François Lyotard characterized as ideologically formed around visions of progress through science and democracy – has articulated a new relationship with imperialism. In "post-modern" modernity, what Habermas described as the "colonization of the lifeworld" has intensified through a concatenation of bureaucratic and computer capabilities, in governmentality practiced both by actual government agencies and by corporations. The "Empire of Modernity," an emergent configuration that fuses imperialism with bureaucratic and technological governmentality, increasingly overwhelms civil society and the public sphere and develops new social technologies for "containing" contentious politics.

Overall, a theoretical typification of Modernity's Empire as an emergent social formation would include the following features:

• Eclipse of the nation-state and its subunits as territorially bounded arenas of sovereignty.

• Attempts to differentiate between what Martin Coward (2005a; 2005b) calls "zones of pacific civility" subject to policing ordered by the rule of law versus liminal and contested zones where the "universality and civility" of Modernity's Empire are challenged by people who are treated, as Coward puts it, as "barbarian Others."

• Military operations undertaken by coalitions of nation-states against regions or movements of rebellion against the Empire of Modernity.

• The decline of nation-states' conventional modern (Weberian) capacity to monopolize the means of violence within their borders.

• Increasing amalgamation of power undertaken to promote "security" such that the military policing of "barbarian Others" no longer is restricted to peripheral zones but instead is mapped across the entirety of the Empire of Modernity.

• The shift – in parallel with blurrings of geographically defined social orders – to new networked and nodal organizations and exercises of power that we as yet lack an adequate theoretical vocabulary to describe.

• The decline in political significance of democratic elections relative to legal-rational bureaucratic and technocratic administration in diverse public, semi-public, and private bodies of issue-oriented stake-holding, political action, and governance.

• In the face of these changes, the increasingly complex and problematic character of citizenship and human rights both within and across domains of governance.

This is only a brief, provisional typification. But it underscores the importance of Tarrow's analysis and suggests where his research might lead us next. First, although the title of his book suggests the centrality of states, his developmental analysis of globalization shows that contentious politics will need to continue their recent trajectory toward collective action in transnational circumstances. Second, Tarrow has argued that emergency rule has become permanent. If this is so, we need to develop a deeper understanding and critique of this new dispensation. I have taken a first step toward characterizing a post-democratic, post-liberal Empire of Modernity. What, we now must ask, is its overarching governmental logic? What are its consistent and contradictory principles?

At the beginning of *War, States, and Contention* (3), Tarrow has Western observers describing the conflict in the Ukraine as the "worst foreign policy crisis since the Cold War." Today, ISIS holds considerable territory, it draws recruits from around the world, and it carries out or inspires violent actions in the core regions of the Empire of Modernity. Meanwhile, in Syria, other insurgents battle Bashar al-Assad's regime, which now enjoys military support from Russia, yielding a flood of some half million refugees reaching Europe, a development now routinely called Europe's worst crisis since World War II. Add to the mix Israel and Palestine, Iraq, Iran, and Afghanistan, and throw in climate change for good measure. You don't have to reach for the apocalyptic (as analysts increasingly do) to recognize that the old (neo-)liberal order of modernity is under extreme pressure in ways that are having global consequences. In his compelling book, Sidney Tarrow offers us a "history of the present." Today, we must find our callings in relation to convergent crises of global society. None of us has been here before. But we can and must seek to understand how we got here, and what alternative directions and pathways open out to the future. *War, States, and Contention* is a key point of departure that should serve as a touchstone of our endeavors.

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Comments on *War, States, and Contention*

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Sidney Tarrow's *War, States, and Contention* strikes me as two books folded into one, or perhaps as a second book striving to free itself from the husk of the first. The first book, about war, states, and contention, is an homage to Charles Tilly. It strives to complete the triangle established on the one hand by Tilly's famous claim that while states make war, wars also make states and on the other by Tilly's numerous works showing a dialectical relation between states and contention (Tilly 1990, 1995). In this book, Tarrow demonstrates that, in addition, contention makes wars and wars make contention.

The second book, which, on my reading, gets most of Tarrow's real passion, is about how wars induce states to inhibit contention and enforce conformity. They do this by declaring states of exception, by whipping up patriotic fervor, by rewriting or reinterpreting the laws, by suppressing civil liberties, and by expanding both the despotic and the infrastructural powers of the state. Worse, these increases in state oppressiveness tend to have a ratchet effect, declining only partially when peace is restored and rising yet higher when the next war arrives. The clear source of passion driving this second project is Tarrow's alarm about the consequences of the early twenty-first century American "Global War on Terror" – a war declared by George W. Bush in 2001 and continued in a moderated fashion by Barack Obama. This second book accounts for four of Tarrow's eight substantive chapters, and departs from the Tillyesque theoretical framing of the first book – relying much more on Michael Mann's work despotic and infrastructural power and on Kim Scheppele's work on anti-constitutional ideas and the state of exception (Mann 1986-2013; Scheppele 2006, 2010).

Let me begin with Tarrow's Tillyesque "book one," which examines the relationship between war, contention, and states. The essential point of Tilly's highly influential *Coercion, Capital, and European States* was that in Early Modern and Modern Europe, war-making and state-making formed a mutually confirming spiral (Tilly 1990). States made wars, of course, but in order to continue to make wars they had to build up their capacities, especially their capacities to extract and administer taxes, which, when successfully done, in turn enhanced their ability to make wars. Tilly did point out more or less in passing that the tremendous ratcheting up of taxes in European states touched off a major round of contention, especially in the seventeenth century – the English Civil War of the 1640s, the mid-century French Fronde, and revolts of the Catalans, the Portugese, and the Neapolitans against the Spanish monarchy all pivoted around taxes (Tilly 1990, 99-102). But discussions of the relation between war-making

and contention were only at the margins of Tilly's argument.

In Tarrow's book one they take center stage. Chapters on the French Revolution, the American Civil War, the rise of Italian Fascism, and American wars of the twentieth century all examine the relation of war and contention. Tarrow shows that contentious political movements can help to push states into war: the abolitionist movement in the United States was a major source of the Civil War and there were important pro-war movements in Italy that precipitated that country's declaration of war against the Central Powers in World War I. Popular movements can galvanize war efforts: when the French Revolution's war against the Prussians and Austrians went badly in 1792, a surge of defenders of the revolution staged a levée en masse that turned the tide in 1793. Wars, especially wars that rely on the draft, can stir up powerful bouts of contention: the wars of the French Revolution, the Civil War, and the War in Vietnam all touched off major bouts of contention against the draft. Unsuccessful wars can feed post-war contention: the disastrous experience of the First World War in Italy was a major contributing factor both to a wave of socialist agitation and to the Fascist movement that helped put down the socialists and eventually took over the state. And although Tarrow doesn't discuss the cases, failure in the First World War also led to revolution in Russia and Germany.

But already in "book one," Tarrow particularly emphasizes the state's repressive responses to real or imagined popular resistance to war – the French revolutionaries' criminalization of dissent in the Terror, Lincoln's suspension of habeas corpus and generalization of military justice in the Civil War, the Wilson government's suppression of free speech in World War I and the Red Scare in its immediate aftermath, McCarthyite anti-communist persecutions during the Cold War, and the dirty tricks of J. Edgar Hoover and Nixon during the Vietnam War. My impression is that, even in book one, Tarrow may be less interested in developing a framework for explaining the relationship of contention to war than in tracing the way that nominally democratic states – especially the American state – has responded to wartime dissent of any kind, contentious or not, by undermining the right to political expression and thus endangering an essential feature of democracy. In this respect, Tarrow's historical examples have a strongly present-minded thrust.

The main focus of Tarrow's "book two" is the American state's conduct of the "global war on terror." Although there were huge demonstrations against the proposed invasion of Iraq in February 2003, both in the United States and in Europe, these soon dwindled once the war began. (The fact that the US had eliminated the draft was surely a major reason that American popular resistance was so much weaker than during the Vietnam War.) The political contention that interests Tarrow in

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these chapters was aimed not so much against the Iraq war or the "global war on terror" themselves as against the way the war was conducted by the American state. Especially under the presidency of George W. Bush, the American government engaged in wholesale evasion of constitutional limits on the power of the president, the military, and the intelligence agencies; it kidnapped and tortured suspects in open defiance of national and international law; and it undermined American citizens' rights of privacy and free expression. It also outsourced both war-making and intelligence to a vast array of private firms, creating a kind of security para-state. And the contentious politics that Tarrow discusses in book two was not a matter of popular protests against the war but of attempts by various "civil society" groups, lawyers, and individual hackers to push back against the Bush and Obama administrations' abuses of power.

Tarrow's discussion of contention against the American state's abuses of power is trenchant and informative, and I basically agree with his arguments. It occurs to me, however, that the "global war on terror" of the 21st century could provide some very good fodder for the arguments developed in book one about the three-way relationship between war, states, and contention. But this would require moving the focus from the US to the Middle East. The launching of the Iraq war destroyed a state, but it also provoked a huge wave of contention, both contention against American forces and contention pitting Iraqi Shiites against Sunnis. Although the "war on terror" eventually decimated the leadership of al Qaeda, it also inspired jihadist movements throughout the Muslim world and it appears to have emboldened both jihadist and liberal opponents of despots across the Middle East. Hence the spectacular movement wave of the Arab Spring and the turmoil that has followed. In this case, contention, among other things, touched off in a phenomenally destructive civil war in Syria, which became a magnet for diverse jihadists from all over the Middle East and beyond. Indeed, a contentious jihadist movement actually formed a new state – the so-called Islamic State, which controls a sizable territory in Syria and Iraq and claims to be a new Islamic caliphate. In the contemporary Middle East, states make war, war makes and unmakes states, war makes contention, contention makes war, states make contention, and, most strikingly in the case of the Islamic State, contention makes states. It seems to me that the contemporary Middle East provides a splendid (if deeply unsettling) argument for a fully reciprocal Tillyesque triangular relationship between war, states, and contention.[1]

Endnotes

1. These remarks were delivered at the Social Science History Association meeting the day before the Islamic State's horrendous terrorist attacks in Paris. These attacks, a particularly lethal form of contention that were branded acts of war by French President François Hollande, are clearly having the kind of effects on the French state that Tarrow's argument would predict.

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Configurations of Power: Reflections on Tarrow's *War, States, and Contention*

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In his compelling new book, Sid Tarrow looks across the disciplinary boundaries that organize political analysis to recognize the world-shaping interactions among state-making, war-making, and contentious politics. He makes his argument in two registers: "it has always been so" and "it is becoming ever more so." The accomplishment of the book is to document this relationship in varying forms across a wide variety of cases and periods. The promise – although it is a dark promise – is to understand how these dynamics inform the threats posed by contention, in the form of either protest or war, to democratic values and institutions.

Tarrow's attention to variation across space and time forces reflection on key concepts for his argument: Mann's distinctions among forms of political ordering. In Tarrow's reading, the key difference is between infrastructural and hierarchical power: "[a]uthoritarian states exercise control largely through despotic means – I call this *hierarchical power.* In contrast, more liberal states use a combination of repression and what Michael Mann (1986) calls *infrastructural power*, which controls civil society from within" (*WSC*, 5). Determining the precise meaning of "infrastructural power," however, is no simple task.[1] The undeniable potency of the phrase derives from how much Mann packs into it. Starting with the concept of "power," Mann offers multiple modifiers in the opening pages of *The Sources of Social Power*: collective vs. distributive power, extensive vs. intensive power, authoritative vs. diffused power, plus four circuits of power (ideological, economic, military, and political; 1986, 6-9). The first of these distinctions emphasizes that "*Authoritative power* is actually willed by groups and institutions. It comprises definite commands and conscious obedience. *Diffused power*, however, spreads in a more spontaneous, unconscious, decentered way through a population, resulting in similar social practices that embody power relations but are not explicitly commanded" (1986: 8). Here the central distinction lies in the quality of being intended or not.

This first theoretical imagery is often keyed by the use of a noun rather than an adjective modifying power; literacy, coinage, and markets are examples of "universal infrastructures" (1986, 23). A second variant of infrastructural power invokes a balance of power imagery (1986, 171), followed by a quick shift to highlight state capacity: "*Despotic power* refers to the range of actions that the ruler and his staff are empowered to attempt to implement without routine, institutionalized negotiation with civil society groups . . . *Infrastructural power* refers to the capacity to actually penetrate society and to implement logistically political decisions" (1986, 169-70). Mann's focus on state capacity also comes in two variants. The first highlights *control* exercised over civil society by the state (1986, 477). A second foregrounds the advantages that come from *centrality* within a network, from positions of brokerage within systems of collaboration and competition (1986, 481).

These imageries of infrastructural power – condition for coordination, balance of power, control over diffused networks, and site for coordination – are powerfully interconnected. Yet they have different implications for how war, state-formation, and contention shape the character of democratic politics and the rights of citizens. Shifting terms from Mann's *despotic* to Tarrow's *hierarchical*, the central question of *WSC* might be stated as: "In the making of war, what shapes the configuration of hierarchical and infrastructural power? How

does the configuration of wartime powers influence the possibilities for resistance to expanded state authority?" The answers lie not only in the balance between hierarchical and infrastructural power, but in the relations across the multiple meanings and dimensions of infrastructural power.

Configurations of Power

WSC is an extended conversation with the challenges of the present as well as the legacy of the late Charles Tilly and his claim that the war-time expansion of states through the construction of hierarchical power requires concessions to citizens that ultimately strengthen rights and the prospects for democracy (*WSC*, 12). Many questions follow. Where such concessions are not forthcoming, is resistance to state expansion more likely to take the form of contentious politics? Do the answers depend on whether states are constructed substantially with infrastructural power, allowing non-state actors to constrain the extent of war-induced expansions of hierarchical power? What matters for all these questions are specific architectures of infrastructural power.

A quick tour through the history of American wars suggests how periods of conflict facilitate shifts in the configuration of infrastructural and hierarchical power.[2] Emerging from a revolution that pitted loose networks of colonials against the world's most powerful empire, American political culture was marked by an almost allergic reaction to political hierarchy. Worked out through decades of competition over the power of "the general government," by the onset of the Civil War there was a clearly articulated understanding of the dangers that military mobilization posed to core political values. The title of one influential pamphlet captured the challenge: *How a Free People Conduct a Long War.* Singling out the British and Russian military for their despotic organization, Charles Stillé (1863) championed a different vision based on the deployment of infrastructural power in the form of the voluntary contributions of citizens. As another commentator reflected, by sustaining the war through popular, voluntary mobilization, the people would hold *their* army close to them, preventing a despotic combination of bureaucratic authority and military power. The result would be "to make the armies of the world *the armies of the people and not of kings*."[3]

Interpreted through the lens of "infrastructural power," the key feature of Civil War mobilization lay in its balance of power. The army and the federal government depended upon the contributions of citizens – through military service, through donations and home production, and the voluntary purchase of war loans – in order to conduct the war itself. Thus the key relationship was not simply consent, but active co-production. The years of war generated a ratchet toward greater hierarchy both in civil society and in government, but the legacy of that first modern or industrial war for hierarchical state power was surprisingly sparse.

...the central question of WSC might be stated as: "In the making of war, what shapes the configuration of hierarchical and infrastructural power? How does the configuration of wartime powers influence the possibilities for resistance to expanded state authority?" The answers lie not only in the balance between hierarchical and infrastructural power, but in the relations across the multiple meanings and dimensions of infrastructural power.

This configuration of infrastructural and hierarchical power reappeared during the First World War. Two key resources – fighting men and funding – were secured by harnessing civic networks to state purposes. Although the federal government established a national draft, the message to prospective draftees began with "greetings from your neighbors" and decisions about individual cases were made by boards of local notables (Geva 2015). Dependent on voluntary donations and loans for sixty percent of the funding for the war (Ott 2011, 55), the financing operation ran through networks of voluntary associations and municipal fund-raising drives that bore a strong resemblance to Civil War efforts and which simultaneously took care of much of the pressure – and at times violent coercion – exerted on those who sought to express opposition to the war by withholding donations.

If the First World War provides a "proof of concept" for preempting the ratchet effect by which "wars make states," what changes with the mobilization for the Second World War and the onset of the Cold War followed by the conflict in Korea? As Tarrow convincingly argues, this period marks an important shift in the configuration of infrastructural and hierarchical power. This shift was fueled by the combination of the New Deal's domestic implementation, the declining dependence of states on citizens with respect to war-making, and increases in the fiscal autonomy of the federal government with the adoption of the income tax. With the greatly expanded governmental capacity that was a legacy of the New Deal, the work of "penetrating" civil society involved a densely connected network of federal, state, and local governmental efforts or at major worksites managed by private firms. Citizens were subjected once again to intense voluntary fund-raising campaigns, but these served as much to dampen consumer demand as it did with financing conflict (Sparrow 2011). This upward ratchet in the fiscal power of the state sustained strong ties to private industry through an expanded pattern of military spending. Under the guidance of a new breed of management consultants, new agencies were now constructed as "docking stations" staffed only to the level necessary to write and monitor contracts with private firms (McKenna 2006, 104-105). These shifts made it ever more difficult for citizens to complete the political arithmetic of modern democracy. "What are my tax dollars doing for me?" The structural complexity and opacity of the post-war welfare-and-warfare state made that question impossible to answer, problematizing democratic accountability.

From Cold War to Global War on Terror

The reconfiguration of power driven by the Cold War contributed to the erosion of rule of law and democratic accountability that has become so unmistakable in the aftermath of 9/11 and the open-ended Global War on Terror. Hierarchical power – particularly with respect to fiscal capacity – was greatly expanded and infrastructural power increasingly worked through private corporations and state/local government rather than through voluntary organizations of citizens. The result was the "fragmentation of state structures and . . . confusion of jurisdictions" that emerges so clearly from Tarrow's discussion of the era of GWOT (*WSC*, 170). Thus the troubling developments of the past decade or more have been long in the making. But what has made them so much more threatening in recent years and seemingly immune to efforts to roll them back?

Tarrow identifies one key to the story in the changing character of infrastructural power organized through networks of private industry. Firms grew on the basis of government defense contracts, lobbied furiously to secure those contracts, and still more furiously to maintain the level of spending whenever relative peace broke out. Just as private prison corporations lobby for intensified incarceration, so too

defense firms invest in expanding government security budgets and steering those expenditures through contracts where private profits can be made. This has led to a particularly encompassing form of "state capture": "The great advantage of the American state has always been that its links to its citizens go in both directions – upward and downward – even in times of war. It is an elusive power because it uses relationships that Americans – and those who study it – see as part of a *pluralistic* system. But the problematic property of pluralism is how easily its logic can be inverted" (*WSC*, 176; see Wolin 2008). These relations also create the conditions for still greater incoherence of infrastructural power as firms have greater control over the experts necessary to conduct the analyses and operations of warfare (*WSC*, 181-86).

More insidiously, the reforms introduced to contain the security apparatus in response to the protests and scandals of the Viet Nam era provided openings for what Tarrow describes as the shift from "rule of law" to "rule by law" (*WSC*, 244). Here, in Albert Hirschman's terms, is the rhetoric of perversity in which resistance to the expansion of state power produces new opportunities for further expansion and the evasion of democratic accountability. State actors drive a "horizontal ratchet" in which potentially objectionable activities – torture, rendition, etc. – are relocated to the jurisdictions that are least particular about legal matters and then the precedents are transported back, shifting the bar for what is and is not legally-protected state action.

Against the grim inertial force of these developments, Tarrow holds out the optimistic reading of Edward Snowden's principled – if quite possibly illegal – decision to leak large quantities of information on the activities and organization of the national security state. Stressing the incoherent complexity of the contemporary American state, Tarrow reassures us that "The same links that enable states to operate within civil society afford civil society groups opportunities and resources to oppose war-making and its domestic effects" (*WSC*, 252). Of Snowden as precedent, we need to consider whether his capacity to engage in resistance represents a feature – or a bug – in the evolving configuration of state power. Whereas the war-fueled expansion of hierarchical state power was restrained by widely-held commitments on the part of citizens and leaders alike during the Civil War and the First World War, what are the odds that another such "organic critic" will emerge within a system of contracting that – absolutely no doubt – has tightened its psychological screening and employee monitoring since Snowden's revelations?

Reflecting on Tarrow's compelling analysis of the problems of the present, I am struck by how relatively little of the "protest" piece of contentious politics has figured in my discussion. This may well express my own sense of the ongoing erosion of the specific mechanisms by which democratic citizens have restrained the translation of war-making into the consolidation of hierarchical power: the loss of the ability to withdraw resources needed for the co-production of war; the institutionalization of alternative channels for the raising of revenue; the end of a mass draft; and the constraints on the ability of individual soldiers to provide information about the conditions of battle. Certainly the lesson of the protest against the invasion of Iraq in Europe was that even unprecedentedly large demonstrations were unable to shift foreign policy. The "party in the street" that reconvened inside conventional politics with the election of a Democratic majority in Congress and then President Obama has been repeatedly and unpleasantly surprised by the ability of the security apparatus to resist efforts

at rollback (Heaney and Rojas 2015). Thus the narratives of resistance return to a feature of the anti-statism of the early republic, its principled opposition to durable configurations of state power. Yet now rather than being central to a broadly-shared political culture, such values appear as moral commitments of individual actors, typically isolated (indeed, evidence of any connection to a movement or membership in the ACLU might well prevent them from gaining access to the positions from which their resistance can be made substantively consequential). Thus mass protest functions here as a means of educating the conscience of potential actors at a distance and potentially altering their calculus of risk and opportunity. Citizens massed in opposition to state expansion thus have little power to withdraw necessary resources for the co-production of war, but they can hope that someone – the right someone – is watching.

Endnotes

1. On this point, Dan Slater and I read Mann's work differently. A more extended theoretical genealogy would be required to sort out this disagreement which appears to be driven, in part, by the commentator's focus on either the first or later volumes of *The Sources of Social Power*. The first volume, I would argue, offers a more capacious approach to infrastructural power which has been particularly influential among historians (e.g. Novak 2008; Balogh 2009), whereas in Mann's engagement with comparative political scientists, infrastructural power comes to be understood more instrumentally, as a modality deployed by a neo-Weberian "state as actor."

2. This historical sketch draws on my nearly-completed book manuscript, *Civic Gifts: Benevolence and Voluntarism in the Making of the American Nation-State*.

3. "The Far-Reaching Influence of the Sanitary Commission," *Our Daily Fare*, no. 5 (June 13, 1864), p. 36.

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Comments on *War, States, and Contention*

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Where there's smoke there's fire.[1] This is the essence of the argument lying at the heart of Sidney Tarrow's sweeping, bracing new book manuscript. War is the fire in this analogy, and contention is the smoke – or vice versa. It is not Tarrow's purpose to show that war generates contention or that contention generates war in any unidirectional sense. It is to call his readers' attentions to the critical historical realities that where there is war there is usually contention; that where there is contention there is very often war; and that states are usually altered in dramatic ways amid these violent interlocking processes. Most books in

contemporary political science try to draw precise causal arrows pointing from cause to outcome. *War, States, and Contention* provides more of a causal swirl. Almost everything points, at one juncture or another in Tarrow's argument and analysis, to almost everything else.

This might be considered a drawback if everything Tarrow analyzes were not so bloody interesting. As with most ambitious works in the comparative-historical tradition, the power of Tarrow's volume cannot be neatly distilled into any single causal claim, or even a particularly concise set of claims. Its glory lies in the author's historical breadth, comparative imagination, normative mission, and steadfast commitment to addressing some of the most interesting historical questions in some of the most interesting historical cases. For anybody who loves political history, *War, States, and Contention* is above all else a good old-fashioned riveting read. Crisscrossing both the globe and the centuries, Tarrow shows how contention fed into war and vice versa in contexts ranging from revolutionary France to the American Civil War, and from fascist Italy to America's "War on Terror." Intermingled among these fascinating histories are a series of thought-provoking theoretical novelties. This review cannot possibly do justice to these loosely related analytical moves, each of which is worthy of sustained attention in future research. What might be more fruitful is to try to tie a tighter thread through these many themes than the fact that they are all somehow connected to wars, states, and contention.

Recall the smoke-and-fire metaphor that launched this review. In these conflagrations of contention and war, what exactly gets burned up? The main answer, Tarrow convincingly if somewhat implicitly demonstrates, is *rights*. More than anything else, this is a book about how wars give states extraordinary latitude to restrict citizens' rights, while also providing citizens with extraordinary incentives and opportunities to fight for their rights. In this respect, Tarrow's book is not only a timely homage to his late compatriot and coauthor Charles Tilly. It is also a profound and resounding corrective. For Tilly, wars tend to expand rights by giving citizens new leverage over their rulers as indispensable warriors and taxpayers. Tarrow does not disagree. But he brilliantly exposes the flip side of this process, in which wars give states the perfect pretext to trample on rights by declaring states of emergency.

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In fact, rights play such a leading role throughout Tarrow's book that *War, **Rights**, and Contention* would have been a more fitting title than *War, **States**, and Contention*. As a book about war, a book about rights, and a book about contention, Tarrow's work succeeds mightily. But as a book about *states*, my reaction is much more mixed. To understand why, it might be helpful to put Tarrow's book in direct dialogue with my 2010 book, *Ordering Power*.

On the one hand *War, States, and Contention* makes a perfect follow-up, accompaniment,

and corrective to mine. If I had chosen a Tarrow-like title for my book, it would have been *Contention, States, and Regimes.* Note the absence of war. This absence is because, more so than Tarrow, I wrote my book to be a corrective to Tilly's work on the state. Specifically, I was convinced from my research on Southeast Asia that internal conflict (i.e. contention) could build the state as surely as international war. To my mind this was a corrective worth offering.

But here's the problem Tarrow's book exposes: war is all over the place in *Ordering Power.* This doesn't mean I was wrong to correct Tilly on internal vs. external conflict. War didn't make the state in Southeast Asia: but *war made contention, and contention made the state*. I am explicit that World War II created the possibility for massive contention by annihilating colonial states. But I do not theorize war, much less the war-contention dynamic. War matters in *Ordering Power* as an exogenous shock; for all intents and purposes it could have been a meteor strike. Tarrow's book has totally persuaded me that war doesn't simply generate contention as an exogenous shock, but as a recurrent, theorizable historical pattern. My book would have been better if Tarrow had written his first. It is a major improvement to mine on the question of war and contention.

Not every theoretical step Tarrow takes on the state is a step in the right direction, however. In fact I would argue that* War, States, and Contention *threatens to halt or even reverse the progress that I tried to make in* Ordering Power, *and that several colleagues have made over the past decade as well, at furthering a comparative research agenda on Michael Mann's concept of infrastructural power.

Not every theoretical step Tarrow takes on the state is a step in the right direction, however. In fact I would argue that *War, States, and Contention* threatens to halt or even reverse the progress that I tried to make in *Ordering Power*, and that several colleagues have made over the past decade as well, at furthering a comparative research agenda on Michael Mann's concept of infrastructural power. To be blunt: infrastructural power is my very favorite concept. It has been the most important concept in shaping my research career. I also consider it perhaps the clearest important concept in the social sciences. Until now I had never seen it defined as anything other than *the state's capacity to implement policy throughout its realm.*

In my fifteen years of writing about infrastructural power, I have never once encountered anyone offering an alternative definition, or questioning my definition of it. I also had the pleasure of joining a 2008 edited volume of *Studies in Comparative International Development* devoted to advancing empirical research on infrastructural power. In our workshop the issue editors and article contributors debated how to *apply* the concept of infrastructural power, but never how to *define* it. In his invited conclusion to that special issue, Mann himself never takes umbrage at how my co-contributors or I define infrastructural power. In a series of excellent book monographs over the past decade by the likes of Daniel Ziblatt, Scott Straus, Matthew Lange, and Hillel Soifer, infrastructural power as the capacity to implement plays a starring role.

Tarrow's conceptual take is starkly different. Without reference to this growing and highly

unified literature on infrastructural power, he refers to my book as the only application of Mann's concept, and suggests that I define it "in a narrower sense" than I should (262). Unsurprisingly, I beg to differ. When Tarrow explicitly defines infrastructural power, he relies upon an article Mann wrote in 1987 instead of his 1988 or 1993 books. Quoting Mann (1987, 114) directly, Tarrow defines the concept as "the power of the state to penetrate and centrally coordinate the activities of civil society through its own infrastructure" (17). The key word here is "its." Tarrow seems to interpret it to mean "society's," as in, the state coordinates society through *society's* own infrastructures. But by 1993, Mann had make it perfectly clear that the "its" he has in mind is not society's but the state's. To quote Mann in full: "Infrastructural power is the institutional capacity of a central state, despotic or not, to penetrate its territories and logistically implement decisions. This is collective power, 'power through' society, coordinating social life *through state infrastructures*" (Mann 1993, 59). Mann strikes me as extremely consistent on this point throughout his writings, including his conclusion to the *SCID* special volume in 2008.

For Tarrow, infrastructural power most fundamentally consists of a state's "ties within society" (25). Yet such social ties may or may not improve a state's capacity to implement its policies and objectives. Tarrow implicitly acknowledges as much in his awkward formulation that the Civil War allowed "financial capital….to control the state's infrastructural power for generations to come" (75). By this definition, infrastructural power is not power at all: it is just infrastructure, or just state-society ties. If a narrow segment of society is controlling the state, that is state capture, which I consider pretty much the exact opposite of state infrastructural power.

To the extent Tarrow is talking about power and not just infrastructure, he is offering a hypothesis about which kind of states can accomplish what they want: i.e. those with strong ties within society. This is a plausible claim but a familiar one. It quite closely echoes Peter Evans' notions of embeddedness, state-society synergy, and shared projects as well as Philip Gorski's concept of state disciplinary power (which drew heavily on Michel Foucault's notion of "governmentality"), in fact. Both Evans and Gorski are very clear that they are making a claim about where infrastructural power comes from. They quite explicitly do not conflate embeddedness with the infrastructural power they hypothesize that it produces.

Happily this allows us to end on a positive, forward-looking note. Once this confusion over definitions is resolved, Tarrow's book can be understood as an important new contribution to existing debates on how states' social ties (as opposed to their internal features or "autonomy" from society) might enhance their power to accomplish their objectives. Tarrow, Evans, Gorski, and Foucault are all probably right that the very strongest infrastructural states are those with robust social ties. But we cannot even ask and assess if that argument is true if we treat social ties as the very definition instead of one of many possible sources of infrastructural power.

Endnotes

1. An abbreviated version of this review appears in a symposium published in *Perspectives on Politics*, Volume: 14 Issue: 1 (March 2016) p 164-165. Copyright © 2016 American Political Science Association. Reprinted with the permission of Cambridge University Press.

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Afterword: Building out from *War, States, and Contention*

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With *War, States, and Contention* we find Sidney Tarrow yet again mastering new fields of study (war), sources (legal), and literatures (International Relations) in a handful of years, and then prodding, poking, and even provoking scholarly communities with his concepts, analysis, and ever-broader interdisciplinary conversations. Inspired by Charles Tilly's *Coercion, Capital, and European States* (1992) and dedicated to its author, *WSC* pushes well beyond traditional Tillyan concerns and sets the scene for a number of scholarly debates and new research agendas. Among the former, Tarrow's conceptualization and use of Michael Mann's "infrastructural power" will likely feature prominently, as Dan Slater's and Elisabeth Clemen's contributions signal emphatically. And among the possibilities for further research, John R. Hall and William Sewell provide us with a neat list, beginning with Sewell's appeal to apply Tarrow's triangle to the contemporary Middle East, and Hall's, to push our comparative framework back into medieval and early modern European history. The study of Urban II's call for a Crusade alone might draw to the surface teleologies submerged in Tilly's foundational works on war and the European state, and on repertoires of contention – foundations upon which the work of most of us in the Contentious Politics program lie.

In this critique, I will focus first on the "infrastructural power" debate, and then suggest one particular research program that arises from what Sewell calls Tarrow's "second book" – the study of a hegemon-led international system and its implications for contentious politics.

My take on the "infrastructural power" issue is simply this: Tarrow was brokering a meeting between (early) Mann and (late) Gramsci. Antonio Gramsci – his critique of civil society and his concern with wars of position and wars of movement in liberal capitalist states – has accompanied Tarrow throughout his long academic career. Gramsci stood steadfastly by Tarrow's side in *Peasant Communism in Southern Italy* (1967), in *Democracy and Disorder* (1989) and in the earlier editions of *Power in Movement* (1994); and then, in *The New Transnational Activism* (2005) and *WSC*, he receded into the shadows. But, *nota bene*: recede, not abandon, as Tarrow expanded his conversational circles to include scholars in mainstream International Relations.

The New Transnational Activism and *WSC* are marked by disparate historical moods – one was penned during the post-Cold War period of liberal euphoria, which needed tempering, and the other, penned as Tarrow apprehended the "War on Terror" and its ratchet effect on citizen rights and protest. But in both books, civil society, civil rights, and liberal internationalism enter Tarrow's work full force; and in both, Tarrow invites liberal scholars of international and transnational relations to consider the place of contention in their schemes. And, more subtly, to interrogate their liberal precepts. Subtly, but nevertheless crucially. If a reader of *WSC* should (understandably) find herself flummoxed by Tarrow's ambiguity about liberal internationalism – "Is he arguing that the War on Terror has *inverted* liberal internationalism or that liberal internationalism is *inherently contradictory*?" – reflection on Tarrow's Gramscian shadow helps to settle any doubt in favor of the latter. It also, I suggest, provides the key to Tarrow's (re-)conception of "infrastructural power."

Tarrow's "infrastructural power" is consistent with Gramsci's conception of civil society – the bourgeois state's inner defense system, which rises to the fore in times of crisis and threats to the state (Gramsci 1971:235). A major theorist of "political opportunity structures" (Tarrow 1994), Tarrow has long sought to identify contradictory spaces within the liberal state and opportune moments in its political processes from which contentious politics might emerge. This search is a matter of considerable urgency in the half of *WSC* that is focused on the US Administration's "War on Terror". The US state is relying more and more on civil society's resources while simultaneously rolling back citizens' rights; capital ("private firms" in civil society, in Tarrow's usage) and the national security apparatus are becoming more closely enmeshed through a process of military and intelligence outsourcing; and the media have been "embedded" in this war-making machinery to foster popular consent, or at least, resignation to ever-longer states of emergency.

Tarrow does not label such state-society interpenetration as "despotic," as Sewell would prefer; and he resists describing capital's alignment with the national security state as "state capture", as Slater would prefer. Instead, Tarrow searches for contradictions in what is ultimately Gramsci's liberal state of the West. Under threat, the liberal US state digs furiously into and draws heavily upon its reserve "system of fortresses and earthworks" to extend its hegemonic control of society (Gramsci 1971:238). To be sure, Tarrow notes that, under threat, the US state increases its "hierarchical power", but, he stresses, it also expands its society-based infrastructural power (Tarrow 2015, table 1.1). By so doing, the state provides space for a counter-hegemonic response:

> The central paradox in this expansion of the government's infrastructural power is that, although it provides the state with outposts in civil society, the power is so poorly controlled, so extensive, and so easily inhabited by individuals who are uncomfortable with the state's growing hegemony that it can both increase profit-making at the cost of the public and open space for resistance to the expanding state. (Tarrow 2015:185)

And the agents who might exploit such contradictions to mount a counter-hegemonic response? Having begun his career as a specialist of political parties, especially of communist parties (Blackmer and Tarrow, 1975), Tarrow knows that Gramsci's Prince/party is of little relevance here – the USA is and has always been devoid not only of a socialist party, but of mass, membership-based parties of any stripe. This reduces the range of organizational vehicles that might redirect a politics of war and leaves largely those civil society organizations and movements that are a characteristic feature of the liberal state. As organizational vehicles, these organizations and movements are not only complicit in the process of hegemonic ordering, but they may also be appropriated, mobilized and driven by contenders of hegemonic ordering.

In his culminating chapter, "Contesting Hegemony," Tarrow locates potential agents of a counter-hegemonic response in *liberal* civil society groups, such as pro bono lawyers and civil libertarians; in immigrant associations that exploit their *liberal* rights; and, notably, in the hactivism of one Edward Snowden, a high-tech employee born of the state-capital alliance forged by military outsourcing, and who is imbued with *liberal* consciousness. Tarrow suggests that Snowden and individuals like him have potentially ushered in a whole new repertoire of contention, which might in turn boost the efforts of more traditionally collective actors. Where Marx's gravedigger of the

capitalist state was the class conscious and organized proletariat, Tarrow's gravedigger of the *national security* state is, one might argue, the "conscious" and (not always?) organized liberal citizen. In the quintessentially liberal polity that is the United States, and which is mobilizing consent for an "endless war" and permanent state of emergency, this is Tarrow's bet and hope.

Moving on to one particular research agenda that Tarrow's "second book" opens to students of Contentious Politics. Whereas Tarrow's analysis of the War on Terror focuses on its dynamics within the United States, he pointedly asks, what happens when we move away from the US and into a world in which the US has the disproportionate power—*as the hegemonic state of the international system*—to upscale and internationalize its norms and preferences, including its preference for states of emergency and the expansion of national security networks? Can movements intent upon countering repression exploit the same contradictory spaces? Tarrow's worried and worrying reply: In the US, civil libertarians can access US institutions and occasionally manage to "hold an aggressive administration's feet to the fire", but

> [w]hen, under U.S. pressure, the USA Patriot Act was transposed into international resolutions, the source of the policy was unreachable by ordinary citizens, who were told by their leaders that they had no choice but to adopt sanctions imposed from abroad. (Tarrow 2015:238)

Tarrow's book largely leaves to other researchers the task of investigating the impact of up-scaled hegemonic ordinances on extra-US contention. I would ask: How and to what extent do/might citizens of other states bring pressure to bear on their own states and on the hegemonic state to roll back war and national security regimes? How does/might such contention vary across the world of states? And, what role do/might the citizens of the hegemonic state itself play in related transnational campaigns? That of boomerang thrower? Or of the catcher?

By introducing the US as the hegemonic state in the international system, Tarrow potentially expands the research agenda of those working in the Tillyan tradition of comparative historical sociology exponentially. Theoretically, it requires that we revisit the program's Tillyan premise of a *pluralist* international system of national states. I have written elsewhere on the Weberian origins and limits of Tilly's pluralist state system (Gentile 2011), and will thus here suggest instead some theory-expanding empirical work. First, we might start by revisiting our own classic areas of study—national campaigns waged by civil rights, labor and peace movements, episodes of political violence, and transnational campaigns of all types—but, this time, factoring in hegemonic state and society. To resource our auto-revisions, we now have a number of state and organizational archives at our disposal, some even online!

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My second suggestion takes up from another aspect of international hegemony raised in *WSC*. Tarrow suggests in his final chapter, "The Dark Side of Internationalism," that there is a qualitative leap between the age of hegemony by *bilateral* relations and the age of hegemony by *multilateral* relations, especially given the shift in "the focus of internationalism from the ministries traditionally dedicated to foreign relations to the executive and the clandestine world of national security" (Tarrow 2015:219). Perhaps. Multilateralism may well have affected the speed and geographical scale of hegemonic ordering, but the claim of a change in its quality requires concerted empirical investigation. Bilateral relations often covered a much broader spectrum of activity than is implied by Tarrow. When the US State Department was in Cold War mode, it developed an extraordinary capacity to coordinate its activities across a variety of US government departments, including intelligence and the military, as well as to ally itself with elements of US civil society and labor. It in turn used that policy coordination power and its infrastructural power to penetrate national polities, potentially altering the trajectories of entire national political systems.

To illustrate my point: My own investigations of post-WWII US intervention into the union movements of (future) OECD countries is signaling to me that the US State Department and its embassies were structured such as to specifically target party-government systems, parties of labor, and union movements. The Labor Attaché Program was piloted in Chile during WWII and then instituted in all of Western Europe in the immediate aftermath of the war before being fully internationalized. Following the State Department's turn to the policy of containment in 1946, labor attachés increasingly communicated with various arms of the US state, including national security, and became a key institution for both the surveillance of and bilateral exchange with the political and industrial wings of national labor movements.

Intervention by US labor attachés and allied US civil society organizations into national labor movements took many forms – from identifying potentially sympathetic union and party leaders and sponsoring their trips to the US in order to witness the "benefits" of apolitical, workplace-based unionism, through to financial aid *and* covert funding that helped to split anti-communist factions away from unitary union movements and to form competing unions, or that boosted the resources of friendly unions and labor party factions against left wing factions. Labor historians who have availed themselves of the thirty-year rule to gain access to US government files have narrated many such post-WWII stories (e.g., Carew 1987; Filippelli 1989; Romero 1992). A social science reading of these works alongside now available archives, however, suggests that: (1) both hierarchical and infrastructural power were deployed such as to construct pro-US hegemonic consent within potentially insurgent labor movements and labor parties (2) from Norway to New Zealand, (3) with long-term implications for the de-politicization of labor movements, and (4) the "varieties of capitalism" that would emerge (Gentile 2015; 2016).

The difference between the era of hegemonic ordering by bilateral relations and that by multilateralism might not be as stark as it at first appears. I also wager that, with the War on Terror, the ease with which multilateral institutions served to upscale the US-preferred security regimes that Tarrow notes was not independent of earlier, bilateral waves of hegemonic ordering. To gauge this, however, we need to shift away from the national unit of analysis that has traditionally rooted the contentious politics program and to move beyond "campaign time." By so doing, we might also engage more with a nearby school

of historical sociology, World Systems Theory (e.g., Arrighi and Silver, 1999), whose forte, by contrast to the Tillyan tradition, is its analysis of hegemonic orders but which, conversely, is lightly equipped for gauging political contention.

With *WSC*, Sidney Tarrow, a founder of the Contentious Politics program, has paved the way for historical sociologists of the Tillyan tradition to study a hegemonic state-led international system of states and society, complete with the central place of war and contention in the development of and resistance to such an order. Forever the broker of exchange across scholarly fences, Tarrow has, by doing this, paved openings to not only interdisciplinary dialogue, but also to deeper *intra*-disciplinary dialogue.

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In Response to My Critics

Sidney Tarrow
Cornell University

Nobody who has been around as long as I have will fail to know how easy it is to trash a book or – even worse – demean it by faint praise. This is why I am so grateful to my critics in this Symposium for the time and effort they have put into evaluating *War, States, and Contention*.

Thanks to all five critics and especially to Antonina Gentile, for organizing the Social Science History Association author-meets-critics panel on which the Symposium is based.

My critics come at this book from different angles of vision and from their own experiences as comparative historical scholars: John R. Hall's sweeping historical reactions go back to the medieval Catholic Church and to the Wars of Religion in Europe and come forward to the transnational empire of the present; Bill Sewell's draws on his own experience with 1789 and all that but also on his wisdom as a historical sociologist. Antonina Gentile uses her work on global hegemony to help me to better see the transnational setting of the American case I tried to study in Part Three. Dan Slater evokes both the social theory of Michael Mann and his own comparative work on authoritarian states in Southeast Asia. Lis Clemens draws on both her historical work on turn-of-the-century America and on her current work in progress on voluntarism in the United States. I cannot hope to cover all the rich and varied reactions to my book in the essays my colleagues have contributed. The least I can say is that if I were to write another book – which at this stage of my career is by no means certain – there is grist for five different books in their contributions!

John Hall does me the great honor of both summarizing *War, States, and Contention* effectively and for suggesting how the book could have been enriched by delving deeper into European history. John begins by noting the book's Tillyan heritage, especially from "the contentious-politics perspective that is the central legacy of Charles Tilly." But he also notes the book's debt to Michael Mann's distinction between "despotic" and "infrastructural power," a distinction that was strangely missing from Tilly's *oeuvre*, to which I will return below. John captures the key claim of my book when he writes that "war is a crucible of the dialectic between protest and the repression of dissent and it is one in which new forms and scripts of resistance are forged, new alliances established, new opportunities unveiled."

John also captures something that I failed to articulate sufficiencly in my book: that major changes often take place "not through state crises, revolutions, colonialism, and long-term economic development," but through " a much wider variety of social fields, their institutional patterns, and lifeworldly practices – from political shifts, social movements, and economic change to organizational and interorganizational network structures, social fields and their dynamics and interactions, and shifts in patterns of action and culture." The changes I tried to trace in *War, States, and Contention* come not only from dramatic breaking points, but also from incremental changes in the nature of war-making, in the long, slow expansion of the US security state after World War Two; and – in the realm of contentious politics – from the shift from set-piece demonstrations against the Vietnam war to the digital whistleblowing of Edward Snowden. Big changes in institutions often result from capillary processes, as the Mahoney/Streeck/Thelen branch of historical institutionalism has been insisting in political science.[1]

John has only two complaints, first about the present conjuncture and then about the past:

The second half of my book focuses analytically and empirically on the United States in the decade and a half since 9/11. John has no problem with that but he urges me to expand the horizons of the contemporary analysis to what he has elegantly called "the empire of modernity" – the fusion of imperialism with bureaucratic and technological governmentality," which – he submits – has "shifted the character, remit, and

effectiveness of democratic politics, and become highly effective in "containing" contentious politics" (also see his *Apocalypse* 2009).

John is of course right: if internal politics in the United States looms too large in the picture I drew it was in part because it was missing from the picture that Tilly drew of state-building, war and contention in his *Coercion, Capital and European States* (1990), the book that inspired mine. This lacuna left the United States looking like little more than a side-product of European state development, and neglected how the American national security state has expanded through a dialectic of rollbacks and ratchet effects during and after each of its many wars.

John also thinks that my book would have profited from a deeper historical footing in earlier epochs of European history, and of course he is right. As I have recently learned from Steve Pincus's imposing book on 1688 in Britain (2009), the reformation, state building, social movements and even transnationalism can be found much earlier in European history than the French Revolution, which introduced Part One of my book. Even earlier, as John reminds us, the Puritan New Model Army was "an ideological military force oriented to revolutionary consolidation of state power" anticipating the citizen army that I saw dating from the US and French revolutions. My only defense – as I learned from Bill Sewell long ago - is that the French were the first to combine the explosive quality of popular insurrection with the mobilizing message of popular sovereignty (Sewell 1990) .

This makes for an easy transition to Sewell's typically thoughtful assessment of my book. Bill observes – he is not the first to do so – that the book is really TWO books: the first, building on the work of Tilly on the historical relations between war and state-building in early modern Europe, but adding contentious politics to the mix; and the second on the troubling slide of the American state in wartime – and especially since 2001 – from a state of rights to a state of surveillance. Part One of my book tries to make explicit what Tilly only left implicit: that contention was *everywhere* in the relation between war and state building in the creation of the modern state in European history. To the extent that I have something to add to critiques of the post-9/11 surveillance state in Part Two, it is to see that state growing not through the direct array of the instruments of "despotic power," but through the far more invidious mobilization of "infrastructural power" within American society – a theme to which I will return below.

Part One of my book tries to make explicit what Tilly only left implicit: that contention was* everywhere *in the relation between war and state building in the creation of the modern state in European history. To the extent that I have something to add to critiques of the post-9/11 surveillance state in Part Two, it is to see that state growing not through the direct array of the instruments of "despotic power," but through the far more invidious mobilization of "infrastructural power" within American society...

In between the two parts of the book is Chapter Five, which attempts to link "book one' to "book two". That "bridging chapter" argues that the American wars of the early 21st century are part of an international trend towards composite conventional/

unconventional wars that began after the conventional war that ended in 1945. These "new wars" (Kaldor 2012) are combinations of conventional and unconventional warfare; they combine military and political tactics; and they exploded in the 1990s with the civil wars in the former Yugoslavia and in Africa. They can be traced back to the anti-colonial movements of the 1950s and 1960s and to the IRA insurgency against British rule from the 1960s to the 1990s.

Sewell seems unconvinced by this "bridging chapter." He sees "book one" as a corrective for Tilly's insufficiently contentious account of historical state building and "book two" as an effort to show "how wars induce states to inhibit contention and enforce conformity." Although those efforts at rights restriction do appear in "book one" – from the Terror in the French Revolution to Lincoln's abridging *habeas corpus*, to the Italian government's arrest of antiwar activists – Bill is correct that the major thrust of "book one" was to add contention to the dialectic of state-building and war-making, while the focus of "book two" is "the American state's abridgement of rights in the conduct of the 'global war on terror.'"

***However rights are defined, they are part of the modern state and their rollback in wartime changes the relations between the state and its citizens. That is – or should have been – a clear relationship between "book one" and "book two" of* War, States, and Contention.**

"What happened to state-building with this focus on rights," Bill rightly asks, and I answer – not clearly enough – that the modern state is in part a state of rights, and the fluctuation of rights in and after wartime has profound implications for the shape and extent of the state. What is special about war is that it provides an excuse (state elites claim it is a "need") to change the built sense of which rights inhere in citizenship and to reconstruct the social and legal sense of rights while claiming that this reconstruction is part of the inherited American creed.[2] However rights are defined, they are part of the modern state and their rollback in wartime changes the relations between the state and its citizens. That is – or should have been – a clear relationship between "book one" and "book two" of *War, States, and Contention*.

But Bill is correct that there is something surprisingly missing from *War, States, and Contention*: the catastrophic effects of the Iraq War and the Global War on Terror in helping to trigger "a huge wave of contention, both contention against American forces and contention pitting Iraqi Shiites and against Sunnis" in the Middle East. "Indeed," continues Sewell, "a contentious jihadist movement actually formed a new state – the so-called Islamic State, which controls a sizable territory in Syria and Iraq and claims to be a new Islamic caliphate."

To leaving open this major lacuna I plead partly guilty and partly innocent: I am guilty because my preoccupation with what has been happening in the United States since 9/11 led me to elide the long-term effects of America's warmaking on contention and states in the Middle East; and I am partly innocent because a major claim of my book is that the lack of clear boundaries of the "new wars" of the late 20th and early 21st centuries have the different effects than "old wars" between states. There is growing evidence that these "new wars" – like the civil war in Ulster that I examined in chapter 5 and the French/Algerian war that I touch on in the Conclusions – have created

transnational conflicts linking domestic contention and international warmaking. Future historians may etch sharp lines among the Iraq and Afghanistan wars, the global war on terror, the Arab Spring, the civil wars in Libya and Syria, and the Islamic State's depredations across the Middle East, but from the vantage point of 2016, they look like parts of a hybrid conflagration.

This takes me to Antonina Gentile's lively commentary on my book. Although she modestly titles her comments an "afterword," Gentile has more original things to say than I can possibly respond to in these brief remarks. So I will focus on only two of them: first, her critique of my ambiguity about the nature of American liberal internationalism; and, second, on the links between Mann's concept of "infrastructural power" and Gramsci's concept of hegemony.

In Chapter 10 of *War, States, and Contention*, I shifted the book's attention from the domestic political effects of the global war on terror to its projections abroad. I focused on what Antonina correctly calls the use of the hegemonic power of the United States to "upscale and internationalize its norms and preferences, including its preference for states of emergency and the expansion of national security networks." That this was no new phenomenon emerges from Antonina's own work on what she calls the "hegemonic ordering" of the international industrial relations system by the State Department, the CIA, and the American labor movement in the years after World War Two (Gentile 2015). But it has been vastly expanded with the shift from largely bilateral to multilateral ties through the working of the institutions created under the umbrella of liberal internationalism. Americans, as Samuel Huntington long ago observed, attempted to use the international system in the same ways they had developed their own political system – as a set of liberal, plural institutions (Huntington 1973).

There is a narrow and a broad way of interpreting my claim of the "dark side" of internationalism. The narrower way is to note, as I did in Chapter 10, that "when, under U.S. pressure, the USA Patriot Act was transposed into international resolutions, the source of the policy was unreachable by ordinary citizens, who were told by their leaders that they had no choice but to adopt sanctions imposed from abroad" (*WSC*, 238). Under cover of the UN Security Council's Resolutions 1267 and 1373, states as diverse as our European allies, Russia, and China now had the sanction of multilateral institutions to repress their residents in the name of joining the war on terror (Ibid., 232).

But Gentile is inclined to interpret the new internationalism in a broader register as well when she asks whether the global war on terror has merely "*inverted* liberal internationalism" or whether "liberal internationalism is *inherently contradictory*." Seeing the U.S. as the hegemonic state of the international system, as she does, she chooses the darker model, and sees multilateral international institutions for enforcing order as the basic mechanism for the workings of hegemony in the 21st century. But she also asks "to what extent do/might citizens of other states bring pressure to bear on their own states and on the hegemonic state to roll back war and national security regimes?" In other words, have the new multilateral mechanisms for the exercise of hegemony created political opportunities for anti-hegemonic contentious groups around what I once called the "coral reef" of international institutions (Tarrow 2005).

Antonina has a second string to her bow and this too circles around the concept of hegemony and its relation to the contested concept of "infrastructural power." Even before the completion of Michael Mann's imposing "quadrilogy" on *The Sources of Social Power*, social scientists began to interrogate the meaning of his concepts of "despotic" and

"infrastructural" power. Some – including the later Mann – appear to have interpreted the term narrowly, to mean effective implementation of public policy in society. In *War, States, and Contention*, I argued for a more structural distinction between the two, with despotic power as the power of the state over civil society and infrastructural power as its power *within* civil society. That was close, as Antonina carefully observes, to something resembling the Gramscian concept of hegemony: "the bourgeois state's inner defense system, which rises to the fore in times of crisis and threats to the state" (Gramsci 1972: 235).

In my analysis of The French revolutionary wars, the American civil war, and the fascist revolution in Italy, the concepts of despotic and infrastructural power were seen as polar constructs. But in my analysis of the United States, after 9/11, I argued, *contra* authors like Giorgio Agamben and Kim Lane Scheppele, that the danger to American democracy lies not in its becoming a despotic state, but in the use of the state's infrastructural power within civil society for the exercise of despotic powers. But because infrastructural power is inherently interactive, I also argued that it provides political opportunities for civil society groups to advance their interests and, potentially, to defend the values of democracy. This was how Gramsci saw the "system of fortresses and earthworks" around the core of the bourgeois state (ibid., 238), as Antonina observes. Thanks, Antonina, for reminding me of my gramscian roots!

I am still mulling over the implications of the reciprocal nature of infrastructural power and how its uses in wartime may have contributed to the shift from the state of rights to the surveillance state in post-9/11 America. But it is worth noting that another of my critics – Dan Slater – sees infrastructural power as a narrower form of power: as "the state's capacity to implement policy throughout its realm." In support of his reading of the concept, Dan quotes its originator – Michael Mann – who, in 1993, wrote that "Infrastructural power is the institutional capacity of a central state, despotic or not, to penetrate its territories and logistically implement decisions." For Dan, nothing could be clearer, when he quotes Mann (1993: 59) writing "This is collective power, 'power through' society, coordinating social life *through state infrastructures*."

Alas, Mann was not as consistent as Dan would have it in defense of his reading of the great social theorist. While the later volumes of *The Sources of Social Power* employ the narrower definition of infrastructural power, both the first volume of Mann's quadrilogy and his key article, "The Autonomous Power of the State" offer, as Lis Clemens observes, "a more capacious approach to infrastructural power which has been particularly influential among historians, whereas in Mann's engagement with comparative political scientists, infrastructural power comes to be understood more instrumentally."[3]

Perhaps the difference between Dan and myself boils down to the fact that while he employed the concept of infrastructural power to help him understand how contentious politics led to the formation of authoritarian states in Southeast Asia (Slater 2010), my use of the term is meant to understand how a liberal state – the United States – employs the instruments of infrastructural power in wartime and how it provides civil society groups with instruments for resistance. In this respect, I differ from other critics of American power – like Giorgio Agamben (2005) – who saw the post 9/11 American state careering dangerously towards new forms of despotic power. My view is that the American state works in more complicated – and more contradictory – ways; more complicated because rather than forswear infrastructural for despotic power, wartime has

led to an increasing blend of despotic and infrastructural power.

This has two critical correlates: first, it means that through these infrastructural ties, civil society groups can exercise contained forms of influence over the state, and Chapter Nine of *War, States, and Contention* is at pains to outline both the forms and the limits of this influence. It ranges from the capacity to demonstrate against war-making (as millions did on February 15th, 2003) and to lobby and use the courts to defend constitutional rights (as non-profit groups like the American Library Association did in response to the Patriot Act), to the capacity of whistleblowers (like Edward Snowden) to reveal the extent of of government spying. Whatever its venue, infrastructural power is reciprocal.

This takes me to the generous comments of Elisabeth Clemens – an authentic comparative-historical Americanist and not, like me, a trespasser on American soil. Lis pays me the best compliment a critic can pay an author: that his work has helped her to think about her own work. Lis Clemens does not need my help in plumbing the intricacies of state/civil society relations in the United States, as her past contributions have shown, and as her forthcoming book, promises to reinforce (Clemens 1997 and in progress). She too takes up the concept of infrastructural power, and shows how infinitely complicated the concept can be.

She takes the concept from war to war, which my more episodic account failed to do. Beginning with the role of the Sanitary and Christian commissions in the Civil War, she moves through the role of voluntary group mobilization in the First World War to the more business-centered relationships of the Second World War and the Cold War, to the ingrained role that weapons acquisition has given to military/technological firms in recent decades. "Under the guidance of a new breed of management consultants," she writes, "new agencies were now constructed as 'docking stations' staffed only to the level necessary to write and monitor contracts with private firms."

In her commentary, Lis makes clear what was only implied in my book – that different groups have differential power to use the reciprocal relations of state and civil society groups to their own advantage. Second, she argues that *non*-warmaking state development – like the New Deal – trickled over into war-related developments, so that by the time the NSA and other three-letter agencies began to mushroom after 1945, the growth of the national security state passed almost without notice. Finally, Lis lays out what my book only tiptoed around – the essential failure of contentious forms of protest against the growth of the national security state. "Thus, mass protest" she closes depressingly, "functions here as a means of educating the conscience of potential actors at a distance and potentially altering their calculus of risk and opportunity."

Although my book...shows how mass protest against the Iraq War evaporated after 2006, educating the conscience of potential actors and altering their calculus of risk and opportunity may affect the long game in the relations between states and citizens and help to shape new forms of contentious politics.

But is this such small beer as it may seem? Although my book, following Heaney and Rojas' sparkling work on *The Party in the Street* (2015), shows how mass protest against the Iraq War evaporated after 2006, educating

the conscience of potential actors and altering their calculus of risk and opportunity may affect the long game in the relations between states and citizens and help to shape new forms of contentious politics.

True, in the wake of the Snowden revelations, and spurred by the rise of ISIS and its enormities, American decision-makers are sliding back to new forms of surveillance. But through Snowden's revelations both the public and the big Internet firms have been alerted of the dangers of unencrypted communication. "Educating the conscience of potential actors at a distance and potentially altering their calculus of risk and opportunity" is the slow, halting, and sometimes successful way that democracy can make progress in the American state.

Endnotes

1. I refer here to the important strand of work by Streeck and Thelen and their collaborators (2005) and Mahoney and Thelen and their collaborators (2010) on institutional change.

2. This is the core message of the recent book of my friend and colleague Joseph Margulies in his important book, *What Changed When Everything Changed* (2013)?

3. See Lis Clemens' contribution to this Symposium as well as a work of mine in progress, "The Vertical Axis of Pluralism: Infrastructural Power and American National Security" (2016).

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Book Symposium

Expulsions: Brutality and Complexity in the Global Economy

Harvard University Press

Saskia Sassen

Editor's Note: The following text is based on an author-meets-critics session that was organized and chaired by Patricia Ferndandez-Kelly for the Annual Meeting of the American Sociological Association in August, 2015. My thanks go out to Alejandro Portes, Michaeline Crichlow, Joseph Blasi and Saskia Sassen for agreeing to contribute their comments to the newsletter. I would also like to thank Patricia Fernandez-Kelly for all her help in making this feature possible.

Review of Saskia Sassen's *Expulsions*

Alejandro Portes
Princeton University & University of Miami

Sassen's *Expulsions* is a great lament about what happened to the old liberal welfare project of progressive inclusion and protection of people in a world of de-regulations, globalizations, and the absolute domination of financial capital.

Expulsions occur when former government employees lose their jobs without any alternatives and are forced into poverty; when factory workers are laid off from plants moving abroad and compelled to accept casual, low-paid work in the informal economy; or when young people in American cities are arrested and incarcerated even for minor offenses and are subsequently removed from society and from the labor force. The main insight of the book is that these and other forms of expulsion are systemic and have to do with the reversal of the Keynesian project of social and economic inclusions and the rise of corporate and financial profitability to the status of categorical imperative.

To save the multinational banks, millions are evicted from their homes and governments become bankrupt; no Work Progress Administration to alleviate the plight of those made redundant through no fault of their own. The financiers who have caused so much harm mostly escape consequences of their actions, and return with renewed vigor to the same destructive practices. And all of this with the blessing of world leaders who sanctimoniously proclaim, following Thatcher and Reagan, that "there is no alternative".

The global rise of inequality is well known by now and one can say that there is even now a flourishing industry based on exposing the growth of this or that form of inequality. What makes Sassen's book valuable is the painstaking documentation of a systemic shift encompassing multiple fields and a consistent focus on the precise moment when unsuspecting people suddenly find themselves thrown out – deprived not only of jobs and homes, but also of their dignity. The book overlaps at many points with Piketty's analysis

of capitalism and inequality in the twenty-first century, but it is more sociological in the analysis of the internal dynamics of the finance industry and the processes of globalization that have made the present situation possible.

Inside Sassen's book, one finds real nuggets, such as the commodification of prisoners in the United States whose work is appropriated through various subcontracting mechanisms by well-known multinational corporations and whose very bodies become valuable and tradeable for the benefit of the fast-growing private corrections system. We are informed, for example, that sheriff departments in Louisiana trade prisoners for the sake of keeping jail beds full and per capita payments by the Federal government coming. The section on foreclosures in chapter 1 is particularly powerful and should be read in conjunction with the creation of the financial instruments in chapter 3 that made that social catastrophe possible.

Chapter 3 makes clear that it was not the interest paid by families on mortgages that the financial industry was after, but rather the proliferation of mortgage contracts that could then be spliced, packaged, and sold to eager investors. To secure those contracts, mortgage companies dispensed with down payments and credit reports. In a country like Spain, banks even gave people 10 and 20 percent over the value of their home in order to get their signatures on paper, setting the stage for what was to come. The financial crisis was triggered less by massive defaults on mortgages than by generalized uncertainty in the financial world about the actual value of those packaged derivatives. This social uncertainty was what made them "toxic."

I have only a few comments for expansion and strengthening of future work. First, there is a need for greater attention to the entire class structure. Most of the contemporary literature on inequality tends to focus on what happens at the "heights" – the top 1 or 2 percent of the population – and the "depths" – the bottom 10 or 20 percent. This leaves out the majority of the population who are not engaged and profiting from financial engineering, but who are also not at the "systemic edge." One may argue that these middle four-fifths or so of the population represents the core source of economic and political continuity and stability, and the reason why the entire system does not come crashing down.

What makes Sassen's book valuable is the painstaking documentation of a systemic shift encompassing multiple fields and a consistent focus on the precise moment when unsuspecting people suddenly find themselves thrown out – deprived not only of jobs and homes, but also of their dignity.

In past work, I have tried to develop maps of both the American and Latin American class structures, provide definitional criteria for membership in each class as well as numerical estimates. My map of the American class structure criticizes previous Marxist analysis of the same topic for stopping with the proletariat as the bottom of the class structure, neglecting the rapidly growing classes of petty informal entrepreneurs and redundant workers. The latter, in particular, consists of workers pushed out of employment by dint of dated skills, past union militance, or economic downturns and unable to get a foothold back into the system. This is precisely the social class on which *Expulsions* focuses attention and correctly so. It is important, however, to consider the economic situation and political outlook of the classes above it and how they interact, both

among themselves and with the top 1 or 2 percent. Otherwise, we lose sight of what is happening in reality, because the massive middle classes and their actions hold the key for long-term systemic continuity.

A second observation for future work is driven by current events. It concerns precisely the political behavior of that top 1 percent and its ramifications on American democracy. The recent spectacle of the Koch brothers convening a conclave of billionaires to decide what Republican presidential candidate to back, i.e. to buy, is a worrisome development. It attests both to the hubris of the dominant class, empowered by free markets and the weakness of government, and the looming transformation of the American political system into what President Carter also recently labeled an "oligarchy". A government by and for the top 2 percent (at most) is what we may be confronting as a counterpart of the growth of the redundant classes. This political corollary to the analysis of the modern capitalist system in *Expulsions* certainly deserves attention in the future.

A final consideration is what can be done about all of this. In a sense, Margaret Thatcher was right and "there is no alternative." It would be useless and counterproductive to try to set the clock back to the time of national hegemony, strong unions, and generous national welfare regimes. Capitalist globalization is a *fait accompli* and is here to stay. However, as Karl Polanyi knew, the "disembedding" of markets from society and the catastrophic consequences that it brings inevitably triggers new forms of popular mobilization and state activism seeking to bring these forces under new forms of control.

Pierre Bourdeiu was resolutely anti-globalization in his defense of the popular conquests and welfare protection achieved within national borders by France and other developed countries. However, this is not the likely path for future effective mobilizations. The global power of the billionaires must be counteracted by international alliances of activists and workers. The plight of those made redundant by the expulsions that Saskia so vividly describes has to elicit solidarity and protest beyond their national borders. Put differently, the long-held assumption that "capital is global, labor is local" needs to be replaced by an increasing global alliance and mobilization by the subordinate classes.

We already have some examples of these counter-movements in the environmental protection field (Greepeace, etc.); in cross-country mobilizations against labor exploitation by the multinationals in third world countries; in the women's rights movement; and in the increasing and well-documented transnational activism of immigrant communities and their home country counterparts. These and other incipient forms of "globalization from below" are worthy of attention as potential means of achieving some form of re-balancing, some manner of Polanyian "re-embedding" where the hold of runaway markets and the hubris of seemingly all-powerful billionaires can be brought under control.

Discussion of Saskia Sassen's *Expulsions*

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In this short text, Saskia Sassen deploys the concept of "expulsions" to perform two related tasks. The first is to make visible particular destructive socio-economic phenomena that have been trending since the 1980s (more or less) in the wake of the untrammeled emphasis on growth. The second is to gesture to a methodological imperative - namely, to unthink

or rethink macro-concepts that may be serving to obscure the dynamics of post 1980s devastations occurring across the globalized world economy.

For Sassen, expulsion is a productive concept for ferreting out the subterranean *"savage sorting of 'winners and losers,'"* (Sassen 2010) that have occurred in the wake of the increasing ontological and spatial complexity of economic globalization manifest in the expansive and intensive devaluing of human social economies and ecologies. Thinking of these material and socio-political processes through the notion of "expulsions" demonstrates the need for, as Sassen put its, "empirical research and conceptual recoding," given that the destructive forces under analytical scrutiny, "cut across our conceptual boundaries-the terms and categories we use to think about the economy, the polity, the diversity of nation-states." (*Expulsions*, 215) Using a variety of case studies Sassen explores in detail four manifestations of this *"*savage sorting*"*: 1) the emergence of shrinking economic spaces; 2) the land grabs occurring mainly in Africa; 3) the relentless financialization of just about everything; and 4) the devastation of the biosphere in ways that make its capacity for self-healing doubtful. In all these cases, irrespective of their location and regardless of the state forms - whether socialist, communist, or capitalist - a single pattern woven paradoxically from the complexity of the system emerges with the common locus being the myriad "expulsions" of people, places and nature in brutal fashion.

This systemic unmooring which takes off during the 1980s is coincident with the onset of the period commonly deemed *neoliberalism*, (though never named as such in the text). It is implicitly contrasted with the earlier Keynesian order which ushered in the bounteous yield, through the visible hand of the state, of growing economies and importantly expanding middle-classes, incorporating perhaps significant elements of minorities, including women. Keynesianism also created the spaces for the flourishing of working class organizations, job creation, redistribution policies and other social protections accorded by *strong welfare states*, at least in the Global North. This era of state management for growth and development seems to represent for Sassen a golden age, but she certainly acknowledges its share of disorders such as the marginalization of women and minorities.

Contrasted with the present moment, given Sassen's cataloging of the devastations wrought across the Global North and South, one would be hard pressed not to read the liberal state as somewhat of an aberration from an ideal counterfactual of alternative world orderings. But I will return to this point later.

Many others have analyzed the growing disorders of the present mode of global capitalist accumulation, with Harvey (2004), for example, pointing to a heightened process of capitalist "accumulation by dispossession," Piketty (2014) decrying the sharply growing income inequality, Moore (2010) depicting a world-ecological crisis, and various others positing the end of growth, more or less. Sassen's text is unique in that it operates at a middle range engaging in what she refers to elsewhere as a de-theorizing process that seeks to recover processes and projects overshadowed by the sacred metrics of growth. Drawing diverse cases from across the globe, Sassen maps the precarity of place as expressed in the housing crisis brought on by the proliferation of subprime mortgages, the excess incarceration especially endemic in the US's privatized prisons, the growing detrimental effects of global warming, and agricultural degradation in areas as diverse as China, Kenya and Bangladesh. In an extensive chapter called "Dead Land, Dead Water,*"* she discusses the anthropocentric recklessness producing the growing swaths of land and water that have

died through the excessive use of agro inputs ranging from pesticides and fertilizers to overdoses of antibiotics - all in the name of producing higher yields and profits. Of course, among nature's revenge are the superweed, the body's resistance to antibiotics, and other existential backlashes.

Many of these alarming disorders are hidden within such reassuring goals and metrics like "Growth and Development." One illustration is the idea of land sales in Africa - otherwise known as land grabbing - which appear within Growth and Development metric. What is not immediately graspable under the notion of land sales, and perhaps hinted at in the idea of "land grabbing," is the sociocultural and economic implications of such sales. Such sales, especially those that lead to the cultivation of industrial crops like biofuels, invariably involves the eviction of fauna and flora, the displacement of villages, of food economies, the ramping up of poverty and hunger, the expulsion of people who end up in cities – all under some other obfuscatory category. Elsewhere, Sassen similarly asks what relationships a concept like urbanization really conveys. Certainly it does not convey the aggressive gentrification that repurposes neighborhoods, the struggles between the incoming (and invariably younger) tenants and the previous (and usually older) residents, or threats to the particular traditions of place. Thus, these projects of expulsion positioned at, and constituting, the systemic edge provide for Sassen a window onto these *predatory formations*.

Those disappeared, or, as she put it, "expulsed" in the Global North or Global South, no longer count, thus leading to constricted formal economies. This makes Sassen wonder "if this brutal restructuring was undertaken precisely in order to achieve a smaller but workable [formal] economic space that would show growth in GDP according to traditional metrics- even if it necessitates the expulsion from the economy, and its measures, of significant shares of the workforce and the small business sector" (*Expulsions*, 43).

In this timely work, what are the alternative options that Sassen gestures toward? Her valuation of the regulatory dimensions of the liberal welfare state leads her to argue that despite its shortcomings, it nonetheless is a much needed enabler of spaces for the marginalized to struggle for rights. It furthermore presided over an accumulation process that was rooted in capitalist formations based on real and not fictitious (production of) goods and services, and performed key regulatory functions reining in the market. In this way, Sassen seems to implicitly suggest fixes by way of a return to the liberal welfare state – a state which balanced, more or less, its pivotal components i.e., the market and civil society, in ways which Somers (2008) similarly deems necessary for a rebirth of the social.

But given Sassen's own methodology adopted in *Territory Authority and Rights* (2008, henceforth *TAR*), it would be safe to assume that the seeds for accumulation by savage sorting, of the types that she exposes were already sown within the welfare state, despite its regulatory alertness. For using the logic of *TAR*'s argument (concerned with how historical assemblages of territory, authority and rights have been reworked and remade and how complex systems change) one could well argue that the seed for contemporary expulsions was one of those dis-ordering continuities from the 18th and 19th centuries that remained lodged within the governing structures even of the redistributive state.

Can it not be said then that the root of today's savage sorting, lay in the fact that the political and economic rationalities of the 20th century enshrined a belief in an unbounded expansive capitalist production? Is the complexity that

Sassen analyzes by the numbers not simply the evolved dynamic of high accumulation growth models sustained by optimistic Keynesian planners and by discourses about the benefits of a model of development rooted in crude but seductive promises of the needful destructive transitions from tradition to modernity, and about the benefits of particular forms of rationally-calculative hegemony for progress? In other words, while Keynesian policies articulated the need for checks and balances against the very possibility of the reckless unleashing of autonomous market logics, could it not be said that given the geopolitical commitment to a modern market economy, regulatory devices would be too re-active, always too late, to forestall the growth of these brutal complexities? What then is necessary to thwart the relentless charge of this systemic edge? What really sustains the presence of systemic edges? And, finally, can well-intentioned institutional fixes carried out by say, a post-welfarist state, remedy the situation-that ultimately returns a social that has been savaged since the post-1980s at least?

No doubt the critical purchase of* Expulsions *is its revelations of development's unsavory underbellies - the "systemic edge" that Sassen discloses. But this edge is an effect of thriving predatory tendencies, something that seems built into the system and perhaps is the fuel that sustains it.

To begin to address the sort of queries that are provoked by the rich detail of layers of crises confronting us in the global capitalist order, one needs to explicitly ask what kind of politics is sustained by institutions and elites in creating this systemic edge. Is there another theoretical twist to the story that might be told here, as Sassen suggests, that takes us beyond what we have grasped in the discourses already available on crises and modern social spaces? Marxists, for example, point to the intrinsic nature of capitalism aided and abetted by states that have always been sympathetic to the needs of capital. For them, *exclusions* are intrinsic to the system that thrives on class-based exploitation of one sort or another. But what of the modes of violence held to be present in primitive accumulation? Could that also be a basis for contemporary forms of predatory formations - veritable carry-overs from its earlier iteration in the 19th century? I refer here to forms of coloniality as "states of exception" that apparently never died out with the demise of formal projects of colonialism. Certainly Escobar's path breaking text, *Encountering Development* (2011) chronicles the projects and discourses through which the then "Third World" were *included* further within the capitalist world economy. Today, the devastating outcome has not only been the destruction of their food self-sufficiency (Africa was self sufficient up until the 1960s), but also, as Sassen's statistics make palpably clear, their disproportionate membership in that highly-prized group of HIPCS, (Highly Indebted Poor Countries) now paying 20 to 25% of their export earnings toward debt service.

No doubt the critical purchase of *Expulsions* is its revelations of development's unsavory underbellies - the "systemic edge" that Sassen discloses. But this edge is an effect of thriving predatory tendencies, something that seems built into the system and perhaps is the fuel that sustains it. The edge, then, is structural. The diverse sets of elites - whether in government, the financial sector, or in computer software operations - are caught up wittingly or unwittingly in producing processes that engender these catastrophes. But is there a single or plural logic that explains these

developments, this systemic edge? Is the onset of complexity at fault? Other analysts speak to the onset of the extremes of neoliberalism, and certainly the text gestures to this given its timing and its dating of the onset of these expulsive formations. Yet nowhere in the text is this concept named. Neoliberalism is not mentioned. Therefore, in this theoretical silence, one is either led to consider afresh or to seek to disclose what is the subterranean logic propelling us towards these specters of death, and destruction. Why, for example, is systemic complexity retrospectively so malignant? And how can we escape this not-so-opaque journey to the systemic edge, given our lived history of the present? Is this contingent and non-necessary historical trajectory partly a function of the logic of a particular modern philosophical underpinning that makes expulsion the silent metric of our modern power and rational faith? For example, the idea of *Homo Sacer*, the eponymous title of Agamben's book (2003), argues that modern sovereignty is undergirded by the production of *bare life*. Is there a link here, then, between "unrestrained profit maximization," in the economic sphere and the production of racialized spaces?[1] Of course, such racialization is tied to bodies as well as spaces, but not always the same visual cultures mark such bodies as Sassen's work palpably demonstrates in a wide array of situations, geographies and people. For example, we may think human trafficking, though now referred to as the new slavery, casts a wide net over all kinds of bodies in a way that differs significantly from the chattel slavery of say the 18th and 19th centuries or earlier. Differences include the easy disposability and cheapness of bodies and the responsibility of the abused for their own reproduction (not to mention the enormity of the profiteering).[2]

The "Development Project," though also unnamed in the text, seems to be another underlying theme here, with an implicit thesis that development a la growth seems to have run its course. Accumulation by savage sorting or accumulation by dispossession is its systemic edge. The systemic edge is thus the price for pursuing infinite growth for a finite planet. Given this, and the brutish natural selective logic attending such metrics as GDP, the text implicitly suggests that one should also be moving away from these ways of assessing our progress, as Philipsen argues in his recent book, *The Little Big Number* (2015). In short, a hidden message in Sassen's text seems to be that we perhaps need to think more in terms of the principles underpinning the call for a post-development world even though governments and industry continue to channel those 20th century solutions toward re/solving current planetary catastrophes.

In *Expulsions*, what Sassen seems to gesture to is the idea that the complexity of primarily the economic system emerges out of a simplistic belief in unlimited growth. There is an ineluctable certainty that such a model assumes. It is a model that has been in many ways imposed on the Global South by state-sanctioned agencies like the IMF and the World Bank and also routed through parastatal and non-governmental organizations (NGOs) the latter of which have (and I will be generous here) un/wittingly taken on the language of neoliberalism - suggesting that neoliberal reason has taken over all our sensibilities more than less. For example, the new organizations of banana farmers (most owning less than 5 acres of land) that have emerged in the Windward Islands to fill the void of retreating states and given the WTO mantra of "free trade," also speak the language of neoliberalism and refer rhetorically to the irrelevance of states (Crichlow, 2003).

Therefore if complexity is built on these simplistic singular models of certainty then it seems that our salvation lies in plural models of uncertainty. What I am gesturing to here, to use

Sassen's own words, is *incompleteness*. Might we instead find solutions in uncertainty-being content with incompleteness? Might we consider then incompleteness as a productive tension, that would lead to what Henrietta Moore refers to as more "collaborative experimentation," more incomplete learning, more social political and economic experimentation in order to build prosperity on a broader understanding of human nature?[3] In Sassen's words in *TAR*, and elsewhere, we need to break the path dependency and *jump tracks* on this model of growth, given "the fierce urgency of now"![4] Sassen has produced a wonderful and concise book that brilliantly outlines, to recontextualize and readapt the Irish poet William Yeats, the shape of the rough beast, "its hour come round at last, slouches towards everywhere, already born." Things fall apart.[5]

Endnotes

1. "Racialized" here is used in a broader sense than is generally understood in the U.S., and refers to a more generic production of a politics of the abject, though it encompasses U.S.-recognized divides.

2. See, for example, the comparisons between chattel slavery and "modern day slavery" made by Barnes (2004).

3. Henrietta Moore, in the program "The End of Development," argues emphatically for a pluralist approach to development, whereby there are different kinds of approaches to building human capabilities. She urges us to look at the various feasible experiments now taking place in the Global South. For example, the experiments in agro-ecology involving some 500 million people in those geographies. Listen here:

http://www.bbc.co.uk/programmes/p02l0c1r

4. This phrase, "a fierce urgency of now," is appropriated from Reverend Martin Luther King's sermon, " Beyond Vietnam: A Time to Break Silence." See: http://www.hartford-hwp.com/archives/45a/058.html

5. This is a slight adaptation of William Butler Yeats, "The Second Coming."

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Comments on *Expulsions*

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Saskia Sassen's book, *Expulsions*: Brutality and Complexity in the Global Economy, examines four areas of savage sorting that results in mass expulsions of persons from a sustainable society. The first area is shrinking economies where the losers are the unemployed, especially women and minorities, who are sidelined by both structural adjustment programs pushed by international economic institutions and facilitated by the monopolization of complex knowledge and technologies by those who concentrate their ownership and control at the

top. The second area is the world market for land where the losers are the farmers, cohesive villages, native peoples, and factories with roots in communities where free trade agreements and open borders and globalized land markets result in people being expelled from their own land and places of work. The third area is financialization where new technologies resulted in a financial collapse that made homeowners the losers by a group manipulating complex knowledge with the new technology of financialization. The fourth area is environmental destruction where the environment makes all citizens the losers as a result of violent massive new mining technologies.

Sassen's principal contribution is to assemble her erudite combination of powerful argumentation and solid empirical evidence to persuade the reader that her use of such harsh terms as "expulsion" (to express being thrown out forcefully), "losers" (to express the notion that some other group is winning in a zero-sum situation), and "brutality" (to express an everyday savage and cruel attitude towards millions of persons), are all, well, entirely accurate. A lot of empirical research is mobilized in this persuasion. Is Sassen exaggerating and selecting these terms for shock value or it this a substantially new insight? As a group, I think that humans like to gild the lily and see the glass half full and accept the social construction of reality as ok rather than contemplate the sharp edge of disaster. Sassen asks: What if the evidence simply does not justify an antiseptic way of looking at the social violence in our world? She does this by looking at the "systemic edge" where the system is coming apart for large groups of people. She deconstructs the tame language of everyday acceptance of brutality with facts showing that no exaggeration is taking place. Based on the evidence, *Expulsions* is not an exaggeration but a creative and new analysis to challenge us to heavily discount our own socialization to buy into the current social construction of reality. I was reminded of a front page article in the Amherst, Massachusetts newspaper on a recent visit that said, "Amherst to plug homeless meal gap." No, Sassen will simply not allow such dishonesty and escape from the brutal naming of social violence for what it really is.

Her contribution goes far beyond fitting blunt terms to systematic empirical evidence. She has elucidated some distinctively new social processes in our post-modern world. One of her ideas is that, while *inclusion* of more and more citizens in the economy used to be profitable and build economic growth, now the *expulsion* of large numbers of persons from the economy creates more economic growth for those who own the economy. The notion that economic growth actually benefits from expulsions is a game-changing insight and also very depressing. Another idea is that brilliant people, who we would love to have at our dinner parties and marry our sons and daughters, are the creators of a new science that uses financial technology, computer

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technology, mining technology, and what I will label "neo-liberal economic technology" (which manipulates the economy), in order to plan and execute the actions that make these expulsions happen and expand exponentially. The notion that brilliance and knowledge - not ignorance and lack of science - is the new source of the Frankenstein of neo-liberalism is very disheartening, although carefully exposed in this book. The intellectual and scientific violence underlying the technologies and the reasoning behind these expulsions has a lot of what some in universities and many in the world tell students and children to be: discover the new edge, be interdisciplinary, be empirical, be daring, be unique, "have an impact on your field." But it surely lacks the norm of human dignity. I emphasize this to also underline that it is easy - as Sassen sometimes does - to say "the 1%" is behind the problems that she elucidates, but it is really the 20% and the 20% is an uncomfortably broader group. The problem of Sassen's book is how to reclaim the human dignity to drive the project of society in the future with new social and economic and political structures to serve as home bases for individuals whose efforts are compromised by the current institutions.

Sassen puts a heavy finger on the people behind the process and calls them "predatory elites" who combine in "predatory formations." For example, in the area of finance it would include tech experts and financial engineers, and, unfortunately for us academics, many of our colleagues in economics and finance and computer science departments! It is this "army of the smart" arraigned against the working class and the middle class that Sassen calls a "phalanx," that are people at our cocktail parties, again, people we want our sons and daughters to be or to marry, and yet, it has an overall predatory effect. The subtext of this book is how does rational science and corporate management now add up to the new brutality when these people think they are all about economic progress? The overall result of this process, according to Sassen, is a "capacity to concentrate wealth." As a globalist, she focuses on intra-country and inter-country inequality in some enlightening discussions. I think that the moralizing about these individuals and "elites" and "formations" begs the point of whether there are institutions to contain their activity that would be more beneficial for society.

Sassen's melding of these diverse patterns of power, status and rewards/resources is a new sociology for our connected, knowledge-glutted world where intellectuals think they are on the side of the angels but somehow get caught up in the predatory phalanx without even knowing it. Her synthesis *is*, in my view, unique, creative, novel, and, as I said earlier, smashing of the linguistic conventions that would allow us typically to talk about all of this in a comfortable way. Realizing her insights is very upsetting, it is very hard to read this book, and one regularly wants to escape to the claim of exaggeration to not have to deal with all of her well-arranged data. The book is worth the patience.

An abiding strength of Sassen's book is how she weaves together her social analysis of the economy from various corners of society to make her point. Some observers might look at this as observations about disconnected elements, but that is far from the case. Sassen persuaded me that these various social spheres are all a homogeneous part of her story: the "enclosure" happening by financial firms in how they have designed mortgage-backed securities and control the design of the tax system to their benefit, the stunning social policy consensus that less rather than more social benefits are needed, the related worldwide focus on contracting government expenditures ("an economic version of ethnic cleansing"), the fact that the so-called unemployment rate is a lie and excludes people

pushed out of the workforce, and the growing prison population. Nevertheless, while the description and analysis is helpful, identifying clear causality across all of these spheres, remains a limitation of the work. What Sassen observes is not simply a function of the "complex technologies" and "predatory formations" that she describes in each of the four areas of her analysis. These four areas are well described but the description does not constitute a persuasive causal analysis of what underlies them. A causal analysis identifies where pressure must be applied to stop the causation and change the dynamic under discussion. To illustrate, the solution is not to deemphasize scientific and managerial elites or to stop developing technology. This is where the next book and the future stage of Sassen's work begins. If she brings the same careful analysis to that, her next work will help map out future social institutions.

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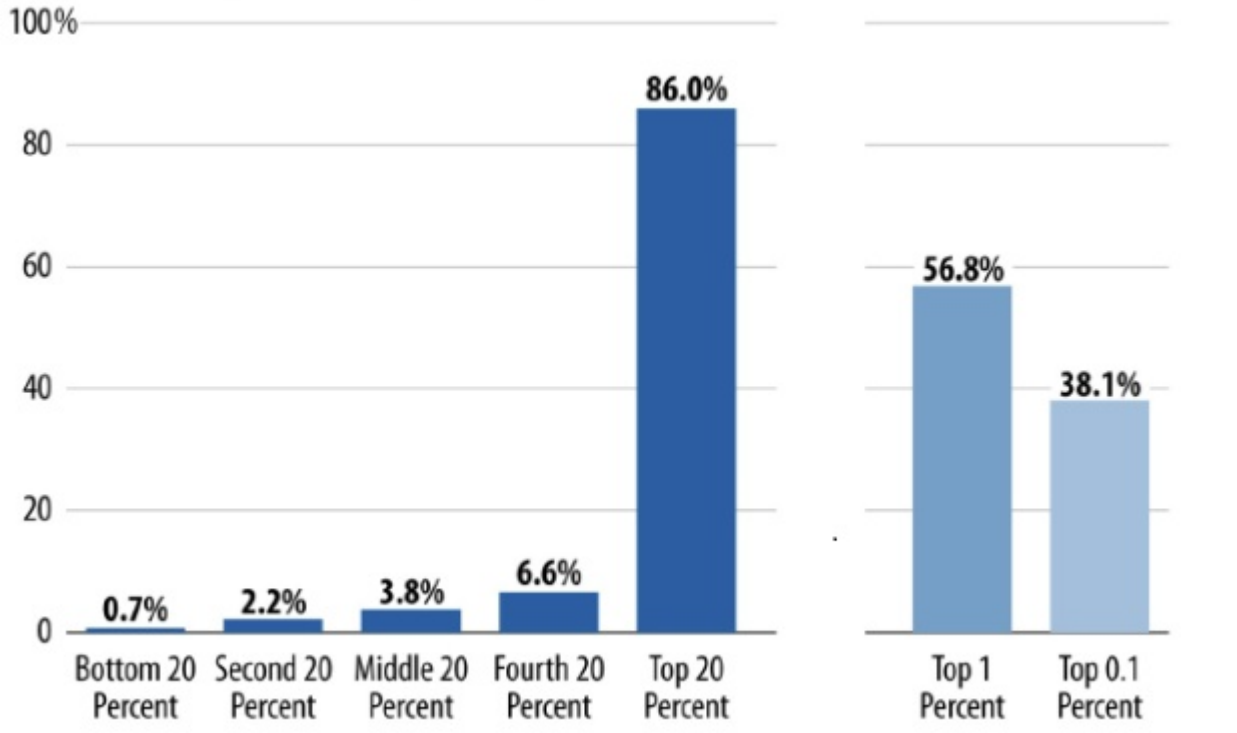
Every grand project like this requires criticism so there are some issues that I would like to raise. Sassen, for all of her attack on neo-liberal economics, seems to not get beyond one of its main strictures, namely, the focus on wages and wage growth and wage inequality as the central story of this economic inequality. The work of Thomas Piketty (2014) underlines that, while we know that real wages have been generally flat adjusted for inflation since 1989 and low relative to economic productivity, that the real story of inequality is the stunning increase in the concentration of both capital ownership and capital income. Both Sassen and Piketty emphasize the outsize share of both in the hands of the top 1%. However, it is really the top 5%, 10%, and 20% that is the giant squid-like "predatory formation" that Sassen assails, not just the 1%. One empirical fact will make this crystal clear: according to the Urban Institute and the Brookings Institution Tax Policy Center, 86% of all capital gains and capital income such as dividends and interest on equities and bonds is in the hands of the top 86% of individuals in the United States. Yes, it is true that the top 1% holds 56.8% within this 86% and that the top 0.1% holds 38.1% within this "capital formation," but Figure 1 (located on next page) shows that the true divide happens where the middle class starts with the fourth quintile of the U.S. population that has only 6.6% of capital income. I am sorry to say that many of us social scientists writing about the 1% are actually solidly in this top 20% "predatory formation" and benefiting from it nicely, as are the creators of the science and technology whom Sassen assails. Bluntly, it is the concentration of both capital ownership (amply covered by Piketty) and capital income (shown in Figure 1, next page) - not just wage income - that is behind this new wealth dynamic.[1] The group in control is really the upper middle class, not just the 1%. This high concentration of capital ownership and capital income needs to be addressed for the causation to be reversed. It leads to a domination of the political system by elites and corporations and hollowing out of the middle class (Madland, 2015).

One needs to ask why capitalism has led to

Figure 1: Distribution of capital income by income percentile, 2011



Note: Figures are for calendar year 2011, current law. Capital income includes taxable and non-taxable interest income, income from dividends, realized capital gains or losses, and imputed corporate tax liability. The cash income percentile classes used are based on the income distribution of the entire population and contain an equal number of people, not tax units.

Source: Urban Institute - Brookings Institution Tax Policy Center

such high levels of concentration. While shrinking economies, the world market for land, financialization, and environmental destruction all play a role in this concentration, underlying the concentration is that the technologies to which Sassen alludes are more productive than human labor, they are replacing human labor, their owners are compensated more than the human labor involved with them, and these technologies are owned and largely controlled by the same 1%, 5%, 10%, and 20%. Sassen's modern version of Charles Dickens's dank factories are hyper-productive, tightly-owned mining operations whose dirtiness has similar patterns of capital ownership and capital income concentration as the "clean" humming computers of financialized Wall Street. The problem is that workers and citizens do not own enough capital and receive enough capital income from these operations. Concentrated capital runs the "shrinking economy" which Sassen describes; this capital is driving both wages and benefits down worldwide. The evidence strongly suggests that technological unemployment and underemployment is becoming a reality as part-time Uber drivers in the U.S. replace full-time unionized workers with the once-vaunted solid pay packages of ever increasing wages and expanding retirement and healthcare plans of much of the post-WWII period. Underneath all of this is that computers and technology and robots are becoming more common, more productive, more controlled by an elite, less related to middle class wealth, and less a development machine of middle class jobs (Frey and

Osborne, 2013; Ford, 2015). It is important to explore who will own the computer science algorithms and robots of the future.

I will say more on how this affects Sassen's analysis of the prospects for reform below. But before I do so, I would like to quibble for a moment with Sassen's discussion of mortgage-backed securities (MBSs) and credit default swaps (CRSs). I have praise and criticism. As a sociologist of finance, I admire the accurate, fine-grained, well-informed, grounded-in-economic-research discussion Sassen lays out. It is superbly done. The financial collapse did in fact come down as she recounts it, but there is a certain Luddite-ism in her portrayal of all securtitization as bad, as the following quote illustrates:

> We all need debt, whether we are a firm, a household, or a country. But do we need this level of debt? More important do we need such complex instruments to finance basic needs for firms and banking loans? (p 146)

Sassen has not proven that well-regulated MBSs and CRSs could not have general benefit to society. The fact that Wall Street currently controls elements of the political system that regulates them does not establish that a reasonable version of regulation cannot exist. The question is whether financialization can be harnessed rather than abolished.

Limitations of space restrain me from going into more detail about Sassen's discussion of the shrinking economies, the world market for land, financialization, and environmental destruction other than to say that reading Sassen's detailed account is well worth the pleasurable effort. Sassen distinguishes herself with a vast interdisciplinary command of concepts, a truly admirable knowledge of many far-flung facts, and an exquisite sociologist's art for naming the essence of how all of this adds up to a particular distribution of the power, prestige, and rewards pie. I would like to focus on the needed next step in her analysis. First, it would make sense to draw out the linkages between the four areas under discussion. We are persuaded that land acquisitions drive environmental destruction, but does financialization play a role in this process? Sassen's prescription is often "do the opposite." This approach works for raw material markets where she solidly makes the case that many practices must be ended. But it is not clear how many 1950s-type middle class jobs can be created that way, nor is it clear that expanding the welfare state or, as noted above, pursuing the opposite of financialization (such as outlawing MBSs and CRSs), would be the right prescriptions for reform.

Admittedly, *Expulsions* focuses on the tragic outcomes of what Sassen calls "the systemic edge," not the policy reforms. That is for Sassen's next set of lectures and her next book. For now, I would like to suggest some directions for further thinking about reform. Let's take MBSs as an example. Before the financial collapse, mortgage lenders were predatory of low income persons, they broke the law with bad record-keeping and illegal approvals of bad mortgages, they compensated officers for loan volume vs loan quality, they used this volume to drive up stock prices so executive compensation could balloon based on grants of equity and profit sharing to the 1% of America's corporate elite, and they persuaded so-called independent third parties to rate their MBSs with excellent bond ratings. This sector is ripe for corporate governance reform more than abolishment. Whether the current elites are capable of having truly independent board of director elections and a vastly different corporate governance system is a separate question. But Sassen can look to countries in the European Union that do corporate governance differently and even make room for workers on corporate boards and works councils.

Sassen has an underlying perspective that everything that goes wrong deserves to be called "capitalism." I would propose that what she is really observing is feudalism; namely, an economic system where status, not performance, determines power and rewards. Capitalism was supposed to be performance-based, replacing an entrenched feudal elite. It was supposed to expand economies, broaden real estate and property ownership, and make raw materials and finance drive progress. Sassen labels the brutalities but does not call out many so-called "capitalists" who are actually feudal lords.

As part of her reform discussion, Sassen is now well-positioned to invent some new social measurement statistics. For example, one can imagine a World Expulsion Index, a Middle Class Index measuring its growth or decline, and many others suggested by her analysis. The test of an elegant social analysis of a social phenomenon is often the ability to measure it, track it, and thereby, study it more carefully into the future. Moreover, Sassen's initial ideas on reform are worthwhile but need to be vastly expanded. Yes, national laws are insufficient to regulate multinational corporations, and yes, a global coordinating body of national Securities and Exchange Commissions is probably necessary, but it is not clear that such reforms will allow corporations to broaden wage income and wealth in the material economy for the masses. Sassen can now turn to articulating a UN corporate reform agenda. This is not to be sneezed at. Look at what Eleanor Roosevelt accomplished with the UN Declaration on Human Rights.

Sassen's reforms need to confront the political sociology underlying her analysis. She points to an "enfeeblement of local democracies" and a shift away from social and economic inclusion. What reforms are needed to channel the power of democratic majorities? On one hand, she seems to be pointing to a certain determinism, namely, that political power simply is incapable of overcoming globalized corporate power. On the other hand, if she finds a way to broaden capital ownership in the economy, this new ownership force could perhaps tame politics and corporate power more effectively. In the future, she will need to figure this out in a lot more detail.

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As a result, a large challenge for Sassen is to articulate what the next economic system should look like. If it is not failed communism and unbridled capitalism (I would say, it is now, essentially, reconstituted feudalism in capitalism's clothing), well then, what will it look like? Once democratic polities are willing to regulate corporations more and once corporate governance is reformed and once we measure the Expulsions really well so we are not forgetting the brutality under our very feet, how is property ownership and work life to be organized in post-modern society? I will briefly suggest two divergent paths that she has to sort out. (Perhaps the solution is a combination of the two but that requires careful analysis.) One is to grow and expand and feed the state so that it takes care of the middle class and the working class better. One version of this is Thomas Piketty's annual global tax on wealth and annual high tax on incomes to finance this expansion. Another might be to broaden capitalism so that greater numbers of people have access to capital ownership and

capital income through broad-based worker ownership or citizen ownership of citizen's trusts or wealth funds that pay income to individual citizens worldwide. For example, might populist sovereign wealth funds be a possibility? Examples are Norway's oil fund and the Alaska Permanent Fund that invest their cash in a diversified basket of non-carbon assets and use the capital gains and dividends to fund (in Alaska's case) direct payments of about $2,000 a year to every citizen and (in Norway's case) to stabilize the welfare function of its state budget.

Part of this future discussion has to do with Sassen's ultimate prescription for how sociology must change to catch up with her insights. One uncomfortable implication of her analysis is that sociology itself requires reform and revision. As I have alluded to in this review, Sassen confronts us with the uncomfortable and highly inconvenient truth that a lot of science and research and technology are being formulated by middle class elites to create the expulsion society. She challenges sociology to develop a more critical analysis of this phenomenon. In the recent past, sociology has also become dominated by identity sociology, identifying, measuring, analyzing and articulating the bad distributions of power, prestige, and rewards for different minorities. This is correct, it is right, and it is just, and it is evidence-based, and these analyses suggest what must be changed about our society. But what if our research and arguments about who is excluded get way ahead of our ability to recast and recreate institutions that are actually capable of broadly distributing power, prestige, and economic rewards? My point is that modern sociology and sociologists have not invested enough study, empirical investigation, theory-building, and policy analysis to figure out what kinds of social and economic systems might expand power, prestige, and rewards for more people, including the aggrieved minorities. By Sassen's own analysis, the combination of the "shrinking economy" and the fact that the middle class itself is becoming a giant minority, suggests that we have to focus simultaneously on studying exclusion and expulsion while studying more inclusive societies and economies that are alternatives to the current social and economic institutions.

This new sociology needs to ask a lot of new questions that it has not traditionally been good at asking. Given Sassen's concern about the evils of finance, the sociology of finance as a sub-discipline needs to be meaningfully developed. Here are some of the new questions that have to be asked: Is the reform of the current wage system enough to rescue the working class and the middle class? Will there be enough jobs and work organizations to employ everyone with robotization? What might social life look like if many citizens are supported by either government programs or Alaska-type dividends and economic support is not work-based? Under such circumstances, can forms of social interaction be developed that are not based on work and wages but based on pro-social behavior where people with a lot of "free time" help the dispossessed, work with the elderly, pay attention to children and adolescents, etc.? In the end, Sassen's major contribution is to shake up sociology itself so that the discipline can conduct a more accurate and meaningful measurement of society and analysis of society and evaluation of policy alternatives for the reform of society.

Endnotes

1. For detailed data on this, see also, Mischel, Bivens, and Gould (2012) and related updates: http://www.stateofworkingamerica.org/.

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At the systemic edge: an author and her critics

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The point of inquiry in this book is the systemic edge.[1] The key dynamic at this edge is expulsion from the diverse systems in play - economic, social, biospheric. I conceptualize the systemic edge as the point where a condition takes on a format so extreme that it cannot be easily captured by the standard measures of governments and experts and thereby becomes conceptually and analytically invisible, ungraspable. Each major domain has its own distinctive systemic edge - thus the edge is constituted differently for the economy than it is for the biosphere or the social realm. This edge is foundationally different from the geographic borders in the interstate system.

The core hypothesis is that we are seeing a proliferation of systemic edges originating partly in the decaying western-style political economy of the 20th century, the escalation of environmental destruction, and the rise of complex forms of knowledge that far too often produce elementary brutalities. It is in the spaces of the expelled where we find the sharper version of what might be happening inside the system in far milder modes and hence easily overlooked as signals of systemic decay. In this regard, I posit that a systemic edge points to the existence of conceptually subterranean trends because we cannot easily make them visible through our current categories of meaning.

In earlier work (Sassen 2007; 2008) I developed methodological and conceptual elements to cut across the weakened categories of the inter-state system. There I identified a variety of vectors that allow one to track processes whatever their geographies. Thus the intent here was not to contest the weight of interstate borders, but rather to study how a given process scales globally. What are the instruments - of law, the economy, the social, the cultural - that have been and continue to be developed to enable the making of cross-border processes?

In *Expulsions* (2014) I develop an additional conceptual instrument - the systemic edge. This project does not override or contest the earlier (2007; 2008) work. On the contrary, it often builds on that earlier work and takes it further both theoretically and empirically by calling for the need to de-theorize - to go back to "ground level" - in order to re-theorize. For instance, I compare a highly polluting industrial complex in Russia and one in the US, and ask: What matters more to understand the current period? that one has a long communist trajectory and the other a long capitalist trajectory, or that they both have vast capacities to destroy the environment?

Inserting the environmental question serves to triangulate what is otherwise a mere comparison. Thereby it helps generate a variable that can go beyond traditional comparisons: we leave behind the cold war and organize our research and interpretation in terms of the environmental question. This kind of third dimension takes on specific contents and meaning depending on the domain or variables I focus on. For instance, I explore the growth and privatizing of prisons in the US and the growth and privatizing of refugee camps. Both are growing, and both have private sector

interests increasingly at work that can profit from prisons and camps. My question becomes: Are these two very diverse formations, with such different specifics, actually systemic parallels, each adapted to its particular environment? I find in this mode of interrogating complex conditions a methodological and interpretive practice that runs through *Expulsions*.

In what follows I address the comments and analyses of Professors Crichlow, Portes, and Blasi. Each wrote extensive and smart pieces that will stay with me for a long time.

Crichlow gets at the heart of the matter in *Expulsions* when she writes that I aim at "a methodological imperative - namely, to unthink or rethink macro-concepts that may be serving to obscure the dynamics of post 1980s devastations…across the globalized world economy." Yes, that is very much the case, as it is when she observes that for me "expulsion" is a productive concept" for getting at the *"savage sorting of winners and losers*" that marks the current epoch, one insufficiently captured through standard categories such as inequality and poverty.

What I think of as *conceptually* subterranean conditions is one way of describing the fact that there is a reality that we are not capturing well through our existing master categories - categories rooted in pre-1980s historical periods. By using the notion of subterranean I am signaling that much, though far from all, of this conceptual work exists, but is not known or is being contested by those deploying well-established traditional categories. In my reading, Crichlow has developed categories for analysis in her own work (Crichlow 2009) that I would describe as fitting this argument.[2]

These are research practices I see as critical for studying and theorizing a new period, or epochal transformations, or periods where the old conditions become increasingly unstable. I found Crichlow's observation that using the category "expulsions" demands empirical research and conceptual recoding very useful: the notion that some categories push us to do the work of discovering and interpreting while other categories do not, or even keep us form doing so.

To illustrate, critical in my work is the need to negotiate between established paradigmatic categories and the fact that the empirical ground on which they rest or from which they were generated is unstable. For instance, well-established categories such as the national state, the middle classes, the economy, have all served us well for decades, but today they are all unsettled or weakened by the instability of the conditions they seek to capture. Mostly, none of the conditions these categories seek to capture can today be confined to the national, and, even if they can, they have each undergone radical, even if partial, change.[3]

In the book I argue that the destructive forces I seek to analyze, or to subject to critical scrutiny, cut across our familiar and/or dominant conceptual boundaries. One empirical way of putting it is that these forces and dynamics cut across the terms and categories we use to think about the economy, the polity, nation-states. This brings to the fore the distinction between conceptual structure and the conditions at ground level. I appreciate Crichlow recognizing that I am not arguing that these destructive forces are all interconnected, and that my point is rather different: the conditions at ground level cut across some of our well-established *conceptual boundaries*. That is the issue for me.

Crichlow captures this when she says that this is "manifest in the expansive and intensive devaluing of human social economies and ecologies." I detect and/or construct conceptual spaces within which we can aggregate (not connect) conditions that our categories of

analysis keep separate. Such a conceptual aggregator can be thought of as space that allows us to cut across traditional conceptual boundaries.[4]

An example of such a conceptual aggregator used in the book is that of two highly polluting industrial operations: one is the vast nickel-producing complex in Norilsk, Russia, and the other is one of the major gold mining companies in Montana. One has a deeply communist history and the other a deeply capitalist history. But today, what matters most, I would argue, is their extraordinary capacity to destroy the environment. We can think of this as a way of interpellating the older categories (Western democracy and Russian Communism), making visible their limits in today's epoch. This means not taking them as givens – as we so often do with terms such as the economy, the state, etc. It takes us beyond our conventions, and becomes a way of unsettling the ground on which those established categories sit.

An issue that recurs in both Crichlow and Portes, is my position regarding the Keynesian period. Perhaps I should clarify not only my position regarding this period but also what I see as the key features marking that period. I do not see it as a golden age nor is this book a lament about its demise. Both Crichlow and Portes list the positives I invoke, but both also recognize that I have critical elements. Indeed, Crichlow posits that I allow for the seeds of the post-keynesian capitalism to be already present in the Keynesian period given the methodology I developed in *Territory Authority and Rights* (2008). In that book I interrogate diverse historical periods to understand how complex systems change. They do not change via erasure of the preceding period but rather by the fact that capabilities developed in the earlier period (in this case Keynesianism) are repositioned in different organizing logics.

As Crichlow puts it, "it would be safe to assume that the seeds for accumulation by savage sorting, of the types that she exposes were already sown within the welfare state." Yes, I do find that at its best, that Keynesian period brought good things to many, even if not to all. Further, and as noted by Crichlow, particular negative components of the post-1980s phase of capitalism began to grow already in that earlier period. For instance, inequality was already growing at that time in particular ways: thus the income of corporate executives in major firms was increasingly growing distant from that of workers. But, unlike what is the case today, the income of workers in those corporations was also growing, only at a slower pace.

The second feature is that the working classes often were well organized, with unions that could confront corporations. Today both of these features are either severely weakened or simply gone. This tells us something about the importance for a larger society of enabling some measure of power to contest and make claims by the more modest classes. That is the best that liberal democracy can offer - not equality, but some voice, and having that voice requires specific material conditions. Without those conditions, there is no voice.

And yes, as Crichlow writes: "In all these cases, irrespective of their location and regardless of state forms - whether socialist, communist, or capitalist - a single pattern woven paradoxically from the complexity of the system emerges with the common locus being the myriad 'expulsions' of people, places and nature in brutal fashion." And one instantiation is what I refer to as "economic cleansing" – the elimination from what is measured as "the" economy of growing pieces of the economy that are in deep trouble. The result is a formal economic space that can show economic growth, even as it rests on some serious economic cleansing. Why bother with

this? It creates a safe zone for investors and politicians and real estate speculators, and such.

Mine is then not quite a "lament" as Portes puts it, and, I imagine, with a smile. It is a project that wants to get at the innards of the Keynesian period and the ensuing "post-Keynesian" present. In a way, this comparison serves as an empirical staging for my insistence that we need some new categories for analysis. Portes puts it well when he describes it as "…the painstaking documentation of a systemic shift encompassing multiple fields and a consistent focus on the precise moment when unsuspecting people suddenly find themselves thrown out – deprived not only of jobs and homes, but also of their dignity." The project I pursue in the book includes the work of tracking *conceptually* subterranean processes in order to detect the extent to which what has been categorized as very diverse and distinct conditions, often for good reasons, may actually share key features that take on special importance in the current period. For instance, those who are confined to camps for the internally displaced, on the one hand, and those in long-term imprisonment, on the other, can also be seen as different instantiations of a similar basic fact: they are both expelled.

Portes's comment leads me to address a second, easily confusing matter: the fact that my transversal focus on very diverse conditions (that may share some key structural element) is decidedly not predicated on *connections* across these differences. In his comment, Portes recovers the diverse empirical moments in the book. And in fact, in many ways it is empirical conditions that enable the larger conceptual project. They do so, as Portes writes, in two different ways. One was finding empirical aspects that supported my hunches and the second was to mark or feed the specifics of my conceptual work.

I need to emphasize this given the locus of "connections" as one of the all-dominant terms in discussions about globalization: everything is connected now. It is not at all. It is rather about how we the social scientists have gone about understanding our world. Much good has been done conceptually and empirically in detecting specificities. This accumulated knowledge enables me to ask a transversal question: not more detail about the differences or similarities, but rather detecting a systemic condition that recurs across very diverse domains which we, in turn, have constructed as very different "bodies" of knowledge. The outcome is silos. But what if, to repeat an earlier example, long-term imprisonment, long-term homelessness, and long-term displacement camps can be conceived of as all sharing a key feature no matter their significant differences? I name that feature expulsion.

The project I pursue in the book includes the work of tracking* conceptually *subterranean processes in order to detect the extent to which what has been categorized as very diverse and distinct conditions, often for good reasons, may actually share key features that take on special importance in the current period.

At the other extreme we might find a case that also shows a shared feature across enormous differences: the increasingly international class of top-level economic actors in the global economy - executives, lawyers, managers, and such. We can select their diverse nationalities and emphasize that type of internationalism, as is typically done. Or we can, as I would argue, find that at that level, regardless of national differences, they are also, and perhaps above all, a global corporate actor. In both examples I

seek to interpellate the conventional category in order to get at some basic ground level that is emerging. This is also the analytic operation in the comparison of Norilsk and the gold mines of Montana.

In short, it is not so much about sharing and connecting as it is about stepping into a specific kind of operational space - the space of those who are expelled, in the first case, and the space of those who are running much of the global economy, in the second case. It is more about a spatial formation than the features of the individuals: many who could easily have become prisoners or displaced wound up avoiding it and many who could have been in that top global capital space are not. My emphasis is on the making of such spatial formations and what it tells us about the usefulness of some of our categories for analysis in the social sciences: these often focus on the attributes of individuals and assume a somewhat fluid open space of upward and downward mobility. Mostly western economies have functioned in that mold, albeit with multiple types of visible and invisible walls. Today we see new kinds of alignments, and these are not easily captured with our existing categories of analysis – perhaps especially in the case of the most extreme alignments, notably the spaces of the expelled and those of leading economic deciders, investors and innovators.

Finance is often seen as a major instance of connection across sectors, places, and such. But in my own research I find little interconnection among the clients of finance (governments, investors). Finance circulates its instruments and advice across multiple borders. But this does not mean it institutes an interconnected space for its clients. In fact it does not. It can install its preferences in diverse financial centers but does not necessarily promote connections among its clients (whether governments or investors). Thus in *Expulsions* I conceptualize finance as marked by a logic of extraction.[5]

In his conclusion, Portes calls for work that can expand and strengthen the analysis of our current period. He notes that a population sector left out of the many discussions about class is the majority of those "who are not engaged and profiting from financial engineering, but who are also not at the 'systemic edge.'" He further adds that "one may argue that this middle four-fifths or so of the population represents the core source of economic and political continuity and stability, and the reason why the entire system does not come crashing down." I agree with this. In *Expulsions* and elsewhere in my work I have argued that the top 50 or more of the residents in a city who are doing very or reasonably well are a key force in the upgrading of our cities and the general sense of an expanded prosperous population. They also keep us from noticing the other half that is losing ground. Portes is right: the more modest success of this top 50% is a significant force that keeps our economies going and keeps our societies from collapsing. Portes has made significant contributions to the debate about class in today's US and Latin America (Portes 2010). And yes, I also agree with his call for examination of where the political system is going, a subject that led me to a 9-year research project (Sassen 2008). One central issue for Portes in such a revisiting is that "the long-held assumption that 'capital is global, labor is local' needs to be replaced by an increasing global alliance and mobilization by the subordinate classes."

Blasi's comment is long, picky, brilliant. It deserves an extensive set of responses, and the conversation will, thus, have to continue beyond the space of this text.

Blasi gets at just about all the key efforts in the book. It would not be possible to comment on

them all here. I will focus on a few. First, a framing proposition, and I quote, "the notion that brilliance and knowledge not ignorance and lack of science is the new source of the Frankenstein of neo-liberalism is very disheartening, although carefully exposed in this book." He notes my effort to lay bare "the intellectual and scientific violence underlying the technologies and the reasoning behind these expulsions." He criticizes the far too easy invocation of "the 1%" as the source of many of the problems I analyze in the book, and that the focus should be on the top 20% , which he describes as "an uncomfortably broader group."[6]

I agree with all of this. And let me add that I actually often speak of the top 20% in my work. I am far less in the top 1% camp that remains dominant. Most of the charts in the book show distributions across different levels of income. But the most important aspect of Blasi's statements is that they are spot on.

This does not preclude disagreement with some of Blasi's observations. Thus he writes that "for all of her attack on neo-liberal economics," I do not seem to "get beyond one of its main strictures, namely, the focus on wages and wage growth and wage inequality as the central story of this economic inequality." I would disagree with this. I think of myself as a systems researcher, probably most evident in *Territory, Authority, Rights* (2008). I see wages as making visible larger worlds, and want to get at those larger worlds. I would agree that concentration at the top is very important, but my focus is on a larger set of dynamics that go well beyond income concentration.

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For this reason, I give a "yes" to Blasi's quote here, but would also add that it is not enough: "The work of Thomas Piketty (2014) underlines that, while we know that real wages have been generally flat adjusted for inflation since 1989 and low relative to economic productivity, that the real story of inequality is the stunning increase in the concentration of both capital ownership and capital income…Both Sassen and Piketty emphasize the outsize share of both in the hands of the top 1%." I agree with this, but I also agree with what Blasi says next, and that is in fact closer to how I work the evidence: "However, it is really the top 5%, 10%, and 20% that is the giant squid-like 'predatory formation' that Sassen assails, not just the 1%."

Most related to the effort to specify a larger setting than the 1% is my emphasis that the working category for me is predatory formations, not simply predatory elites - even though the latter are certainly present worldwide. The argument I make is that even if we managed to get rid of the very rich - which is not a realistic option - we would not succeed in crashing the current system.

These formations include mixes of elites, technical capacities, global networks, laws, accounting rules, government policies. Capital owners and managers matter, but by themselves they could not have achieved the extreme concentration of wealth and unaccountable power they now have across the world. This mix produces massive capture at the top, environmental destruction on a scale we have not seen before, and the growing expulsion of

people from survival options even in rich countries. Getting rid of one element in this mix is not enough.

One feature of these predatory formations is a roving capacity for liquefying and capturing what there is to be captured, all done with a minimalism of sorts. This is not the old imperial mode: no interest in controlling vast territories, just extracting what is needed. This efficiency requires complex tools.

A major supplier of such tools is high finance. These are tools that are well beyond violations of the law (as in the Libor scandals, for instance). They involve a far more difficult to combat autonomous effect of interacting electronic networks. Yes, the decisions are taken by individuals and the algorithms are constructed by physicists. But when they are sent into electronically interacting markets, there are unexpected outcomes. To this we need to add to that computer driven high-frequency trading and the new types of private trading networks referred to as "dark pools" - all subjects engaged in *Expulsions*.

Here is an instance of a predatory formation at work whose details we now know thanks to a freedom of information request by Bloomberg News: the secret "necessary" use of US$ 7 trillion of taxpayers money to support the global financial system in order to rescue the economy. While the US legislature was having a passionate public debate about whether to give US$ 300 billion to the major US banks, the Fed was designing a "facility" to pass on those US $7 trillion to US and foreign banks. Blasi's slightly wicked observation seems especially relevant here, and I cannot resist quoting it at length:

> It is this "army of the smart" arraigned against the working class and the middle class that Sassen calls a "phalanx,"[7] that are people at our cocktail parties, again, people we want our sons and daughters to be or to marry, and yet, it has an overall predatory effect. The subtext of this book is how does rational science and corporate management now add up to the new brutality when these people think they are all about economic progress?

This observation also segues to Blasi's notion of the need for a new sociology, which I of course endorse: "Sassen's melding of these diverse patterns of power, status and rewards/resources is a new sociology for our connected, knowledge-glutted world where intellectuals think they are on the side of the angels but somehow get caught up in the predatory phalanx without even knowing it."

Blasi also makes a critical observation, which I fully agree with, about technologies and robots that are not only replacing human labor and "owned and largely controlled by the same 1%, 5%, 10%, and 20%" but are also "becoming more common, more productive, more controlled by an elite, less related to middle class wealth, and less a development machine of middle class jobs." To this I would add that I also bring a range of additional variables we might summarize under the notion of a massive loss of habitat partly produced by these developments and the enormous demands generated by the technical/electronic revolution on land and water - to develop mining to get at the new components demanded by electronics,

I find that the language of "climate change" is almost too beautiful. I want language that captures the brutality of the condition, and what it took to get at this vast level of destruction.

water to keep cooling all those machines, and so on. There is a parallel history in the making that gradually replaces the older histories of environmental destruction. This became a crucial part of the book. I named the chapter, the most intense one for me, "Dead Land, Dead Water." I find that the language of "climate change" is almost too beautiful. I want language that captures the brutality of the condition, and what it took to get at this vast level of destruction.

Finally, Blasi finds that I do not document causality. This is correct. Establishing causality requires a specific process and given the mix of variables I focus on it would have meant a whole other book. Further, I was not quite ready to sacrifice elements which are almost impossible to establish causally. My whole effort to de-theorize in order to re-theorize in a way was in tension with the notion of sacrificing empirical elements in order to demonstrate causality. I have not yet resolved this issue in my own mind. I do find Blasi's way of describing such work very compelling: "a causal analysis identifies where pressure must be applied to stop the causation and change the dynamic under discussion." On this point, I am in basic agreement that the solution is not to give less weight to "scientific and managerial elites or stop developing technology."

I am not against knowledge, though I could object to the notion we need knowledge in the shape of elites. But the point about causality brings me to what is a key logic in my work: discovering trajectories, whether these are marked by causality or not. I do not prioritize causality, partly because the complex and long-term trends I tend to examine are often marked by multiple causalities depending on what point in time one examines. What runs through the book *Expulsions* is a logic of discovery and of detecting the limits of some of our key categories when it comes to explaining some major emergent conditions.

Endnotes

1. I want to start by expressing my deep gratitude to Professor Fernandez-Kelly for organizing and chairing this panel and to Matthew Baltz for inviting us to publish the session in ASA's Comparative and Historical Sociology Section newsletter, *Trajectories*.

2. See also the debate about *Expulsions* that appeared in *Cultural Dynamics* (2015, Vol. 27,No.1)

3. Elsewhere (2008), I have developed an analysis that posits that change of, and in, complex systems functions mostly by shifting old, established capabilities to new organizing logics. Thus in the case of these three categories, they are still valid but the ground, and hence key organizing logics with which they function. have changed. Thus much of the familiar is still there, but it inhabits or is shaped by a different organizing logic.

4. I see the detecting of such a specific transversal conceptual space also in diverse interpretations that Patricia Fernandez-Kelly makes in her recent book (2015).

5. I also address this point elsewhere (Sassen 2013).

6. This is also a subject that Blasi and his collaborators (2014) develop.

7. I don't think I do, though I could have. I stick with the concept of predatory formations, which is harsh enough.

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Essays

Identities

Editor's Note: The following essays continue the revival of the newsletter's "Identities" feature. These are short autobiographical essays where section members reflect upon what drew them to comparative and historical sociology and how the latter has subsequently shaped their professional identities and influenced their research agendas. My thanks go out to Anne Kane and Tom Hall for contributing essays, and to past newsletter editors for hatching the original idea. Tom's essay also marks the debut of the "career" Identities feature which allow emeritus section members additional space to reflect on their careers as comparative and historical sociologists.

Conjunction and contingency were on my side: on becoming a historical sociologist

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Conjunction and contingency explain how I became a historical sociologist. Though certainly not determined, I was on an early path to this identity. History and social studies were my primary academic interests in middle and high school; growing up in a military family and living "around the world" may have seeded the fascination with and compulsion to understand how and why various societies, and their histories, were different than mine.

My father retired from the Air Force and moved our family to California my last year of high school, an event of some consequence for me. I chose to attend college at UC Santa Cruz, beginning in 1974, not because it was a leftist hotbed (at the time I naively did not know that) but because it was so beautiful. On May 1, 1975, I went to my early morning course," Female Physiology," taught by Laurie Garrett, then a graduate student. Written across a very large blackboard were the words "Vietnam is Free!" Amidst the celebration, I became a bit unhinged, and spent a good part of the day in deep reflection about what I thought I knew about the world. Memories of B52s constantly flying overhead as they departed the base on Guam where I lived for two years filled, and hurt, my head. What was that really about? The day became a turning point in my education and developing world view. Many courses in history and macro-sociology followed, based largely on some form of Marxist theory (I was now attending the "University of Moscow" according to my father). My senior thesis, developed in Walter Goldfrank's comparative sociology course, "Development and Underdevelopment," explored the "roots" of the Vietnam War, focusing specifically on agriculture and peasants. I think while researching and writing the paper I "became" a historical sociologist in heart and mind.

In 1983, five years after graduating from UCSC, I entered graduate school at UCLA. The beginning was not auspicious – my academic mind was woefully out of shape, and the Sociology graduate program was under reconstruction and in some disarray. I floundered at first, but conjunction and contingency were on my side. In the second

quarter of my first year I enrolled in Robert Brenner's "The Origins and Development of Capitalist Economy." Though a struggle for me, Bob's patience was great, and my historical research and analytic skills slowly started to rebuild. Then in the third quarter, mostly on a lark, I took Jeff Alexander's "Cultural Sociology." Eureka! I found the missing link in historical explanation. Structures and imperatives are important, but so is meaning. Thus, I began my journey to incorporate culture into historical sociology, a course only a few scholars had embarked on by the mid-1980s and a project engendering much contention in the field of comparative historical sociology. On the other hand, culture in comparative historical sociology was a project taking wing at this time, so my path was shared, in many different forms, with other scholars I've developed friendships with over the years, such as Mabel Berezin, Lynn Spillman, and Peggy Somers to mention a few.

And I was in a supportive situation at UCLA. Alexander formed a culture study group, consisting of a few graduate students[1] interested in cultural analysis, who met monthly to read and discuss each other's work. Known affectionately as the "Culture Club" (now the Center for Cultural Sociology at Yale), we collectively began constructing what is now known as the Strong Program in cultural sociology, and developed concepts such as "culture structure" and "concrete and analytic forms" of the "relative autonomy of culture." At the same time, Brenner began the Center for Social Theory and Comparative History. As a graduate student I listened to some of the best scholars in comparative history discuss (and discussion, often heated, was the name of the game) their work.

One of these scholars, Michael Mann, came and decided to stay at UCLA. Once again, I benefitted from contingency. Though finished with my course work by the time he arrived, Mick hired me as a research assistant. For more than two years I learned from one of the leading, and most generous, comparative historical sociologists. While working with him on a chapter of *Sources of Social Power, Volume II* (1993) on nineteenth century European peasant politics (of course),[2] we co-authored an article on the same (Kane and Mann 1992). Though Mick's framework was not (too) cultural, the comparative research on peasant political movements helped me devise my dissertation project. An agricultural /peasant-based movement could be the historical event which would allow me to forge my theoretical framework for bringing culture to historical analysis. But which one?

Maybe due to my ancestral connection, and historical documents in English, I began to survey Irish history and found the ideal historical event to study, the Irish Land War. A major tenant farmer and nationalist movement spanning several years, 1879-1882, the Land War had been analyzed primarily from socioeconomic and political perspectives. It was begging for a strong cultural analysis, and I found that the hundreds of movement events, and discourse therein, were recorded in detail and verbatim in local and national newspapers as well as police reports. By this time, I was

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incorporating Bill Sewell's "eventful temporality" framework for historical analysis with a cultural emphasis into my own. Following his lead, I saw that I could chart and flesh out an emerging ideological culture structure (later to be reformulated as national identity) that I hypothesized explained why the Irish Land movement had succeeded in building a strong alliance of diverse and contentious groups when previous movements had not. Once again, contingency encouraged me. I called (email was in its infancy) Samuel Clark, the one sociologist who had published a book on the Land War, analyzing it from a resource mobilization perspective (Clark 19790. After I nervously (fearing he might tell me to take a hike) explained my intention to study the Land War with a cultural framework, Sam enthusiastically supported the plan, saying "it's needed." As my research progressed, Irish scholars welcomed me into the fold, and have been immensely helpful along the way, sharing their good humor and knowledge about everything from the complexities of Irish history to how to be first in line at archives in Dublin.

The database that I fashioned from textual analyses of nearly a thousand articles and reports documenting mass meetings, eviction demonstrations, legal proceedings, parliamentary debates, and editorials allowed me analyze and designate crucial symbolic concepts and codes, embedded narratives, discursive struggle, and patterns of symbolic transformation. On the way to publishing my book on the Land War (2011, many years post-dissertation), I mined this archive over and over to develop a model of meaning construction in social movements, chart the narrative construction of national identity, and incorporate the emotional dimension of meaning and action into the explanation of movement and historical processes.

More than twenty years on, I am still studying Ireland and still refining that culture in historical sociology theoretical framework. I've begun to dip my toes into contemporary social issues (I use that metaphor because water is the problem), but I am a cultural historical sociologist. And it is not a dual identity, though at one time it may have been. After all, what is history without meaning?

Endnotes

1. The initial members included Eric Rambo, Laura Edles, Elaine Chan, Hannah Kully and me. We were joined within a couple years by Phillip Smith, Ron Jacobs and Agnes Ku; the second generation at UCLA included Lisa McCormick, Isaac Reed and Jason Mast.

2. Chapter 19 (Mann 1993).

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A long strange trip: reminisces of an academic career

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I know this title from Robert Hunter's "Truckin" by Grateful Dead is overused, but my intellectual development feels like that. It is appropriate since my "trip" started at UC Berkeley in the 60's and subsequently entailed a lot of moving, so lot of trucking. Here is how I came to write this essay. I came into contact with Matt Baltz late in 2015 when he and

others were looking for old Comparative Historical Newsletters (details in *Trajectories*, 27:2, Winter 2016). I am now several years into retirement and was chipping away at old files, etc., so the request for Newsletters was opportune: I had them all. Sending them to *Trajectories* for archiving helped the CH section and relieved me of a couple of inches of file space. As those of you who have cleaned files – at retirement or earlier – know, it often sends you down memory lane.

I came to the social sciences and sociology "by way of Robin Hood's barn" as the old saying has it. I started in '64 at UCLA in physics and engineering, in '66 moved to Berkeley, and a few years into that I jumped to anthropology. The full story is too tedious to recall in detail; a gloss is I wanted to figure out why people fought rather than help figure out better ways to blow each other up. I thought I would go into physical anthro to study human evolution, an interest I've never entirely left, but was quickly seduced into cultural anthro. One the first courses I took was Elizabeth Colson's "North American Indians." Much later I developed a similar course, "Native Nations of North America." One of the first ethnographies I read in Prof. Colson's class was Clyde Kluckhohn and Dorothea Leighton's *The Navaho* (1962; current convention is to spell Navajo with a j). Ironically I later taught on the Navajo reservation, and written about them a number times.

After graduation, having strung undergraduate studies out a couple of years, I applied to graduate anthro programs. Late that August, 1970 I had called home to check on mail. Two letters had arrived. One was official notice that my draft number was too high, so I did not face an imminent trip to Southeast Asia or Canada. The other was acceptance to U. Michigan graduate school. The slip said sociology *not* anthro. So I packed my VW bug and left for Ann Arbor and crashed with an old high school friend. It turned out that I actually was in Anthro. The error on my acceptance was caused, I think, because I had listed my interest as socio-cultural anthro.

At UM I worked with Joseph Jorgenson. Through his methods class I got a research job helping analyze family composition data among Turkish sheep herders for a senior graduate student. With a background in physics and engineering, I did not find statistics and methods a major undertaking, as some graduate students did. This was a time when the quantitative – qualitative debates were raging across the social sciences. I found the debate silly then, and still do.

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My "outside reading" included Vine Deloria's *Custer Died for Your Sins* (1969), an important book and still worthy of reading. Prof. Jorgenson encouraged me in that pursuit and thought Deloria's discussions were valuable for any beginning cultural anthropologist. Deloria's Chapter 4: "Anthropologists and Other Friends" opens with the following: "Into each life, it is said, some rain must fall. Some people have bad horoscopes, others take tips on the stock market. McNamara created the TFX and the Edsel. Churches possess the real world. But Indians have been cursed above all other people in history. Indians have anthropologists." The chapter continues in that vein with harsh

critiques for the "my village" mentality and instances where the money some anthropologists received to study an Indian group could have ended their poverty by giving them that money. I resolved not to be one of "those anthros." That year Eric Wolf was giving teach-ins about peasants to help students become more informed about the Vietnam quagmire.

I finished my MA in one year - at that time MAs were consternation prizes for those who did not immediately go forward into the doctoral program. And I was broke. So the bug and I went back to Berkeley where I crashed with another high school pal and started applying for Community College positions in anthro. I was invited to Navajo Community College (NCC, now Diné college) in Tsaile, AZ for an interview. I was offered the position from a stupendous applicant pool of two. It was a temporary position which replaced the Navajo anthropologist who returned to work on a doctorate. I took the position thinking that this was great: I could work *for* the Navajo, not "on" them. Looking back, the two approaches - on or for - are not as incompatible as I thought then. I think it is possible to combine good research with activism. I also relished teaching anthro to Navajos. I often used Anglos (a generic term for white people throughout much of the southwest) for my ethnographic examples.

NCC was the first "tribal college" in the United States, plowing uncharted territory for academe. There are now over 35 such colleges. NCC had the usual community college goals, preparing students for various vocations and/or for moving on to four year universities or colleges. But its main mission was really preserving and enhancing Navajo culture, and preparing Navajos who had lived mostly on the reservation [now called nation] most of their lives. There were courses in Navajo Language, in culture, courses on traditional religions, and practical subjects like agriculture, animal husbandry, silver smithing, and rug weaving. I took several courses in the Navajo language, and eventually achieved the fluency of a three year old.

That was also the year of the OPEC oil boycott and consequent shortage and rise in gasoline prices. The boycott also changed the relative value of Navajo resources. Oil and especially coal increased in value. Increased value led to increased royalties, some of which supplemented federal support for NCC. There was tremendous debate among Navajos about sale of resources, and efforts to get better royalties. Others, more traditional, want to limit or ban resource exploitation because the damage to the earth. These debates and intense politics permeated the Nation, even at the Chapter level (a political unit roughly akin to a county or township). Observing these controversies, and co-teaching a course on contemporary issues (akin to a Navajo Social Problems course), led me to develop an interest that has been a driving force in my intellectual pursuits. I became fascinated by how very local political struggles are shaped or influenced by international and global processes, and how historical processes shaped these. Sheep herding, silver smithing, rug weaving are crafts that in the late 20th century were thoroughly Navajo. But all originated in contact with Europeans, mainly Spanish colonists in the 1600's, 1700's, and 1800's.

By the spring of 1973 my temporary job ended, as NCC was preparing to build at its permanent site in Tsaile, AZ, literally a stone's throw from the New Mexico border. They were then contemplating building a satellite campus in Shiprock. So I moved back to LA and searched for a community college position. I was also moving away from the "my village" approach in anthro because of my interests in historical and global connection. At the time I did not realize the "my village" approach was already

on its last legs. The only community college position I found was as an adjunct anthro instructor. I had a couple of near misses, all of which wanted me to also teach sociology. I worked fixing copy machines for a major corporation and attended Cal. State LA in sociology, intending to prepare for dual-discipline teaching. Over the next couple of years the marxists, the functionalists, the quantitative types and the qualitative types kept pushing me to go on for a Ph.D. I Applied all up and down the west coast, and ended up at the University of Washington [aka U-Dub] because it offered the best support, and because my first spouse had a favorite aunt who lived in Seattle.

I intended to pursue my interests in historically-rooted studies of local – global interactions combined with further training in quantitative methods. I jumped right into Herb Blalock's advanced seminars, and discovered world-system theory (as it was called then) in classes with Michael Hechter and Daniel Chirot. Many conversations with Pierre van den Berghe helped me understand the artificiality of the anthro – soc "divide." They became my doctoral committee, eventually chaired by Dan Chirot, and worked on ethnic persistence and social change in what is often called the "Greater Southwest," meaning southwestern part of the current U.S. and the northwest of what is now Mexico. My dissertation eventually morphed into *Social Change in the Southwest, 1350 – 1880* (1989). I explored how local peoples, various native nations, and Hispanic colonists responded and resisted forces impinging from the outside world. I came to see that it is impossible to understand even the most localized social changes and ethnic politics from a solely local approach. Much change and politics is driven by external pressures, often poorly understood initially, but eventually resisted with a modicum of success in the efforts of local peoples to gain some control over their own destinies. I also saw that processes that I came to call incorporation into the modern world-system had a far wider range than a simple dichotomy of in or out. I was inspired by Eric Wolf's *Europe and the People without History* (1982) to examine incorporation processes from the peripheral perspective, and see how peripheral peoples took a very active role in continuing to shape their own destinies.

All of this I tried to explore through multi-tiered comparative strategies. I made comparisons among and between sedentary groups (Pueblo nations and others), nomad foragers (Apaches, Comanches and others) and groups that practiced both (Navajos). Over time, all had experienced interactions with various outsiders. First the weak, but significant, interactions with the native states in central and southern Mexico, then with Spanish colonizers, then with an independent Mexican state, and finally with the conquering, semiperipheral capitalist state, the U.S. The book has held up reasonably well for two and a half decades. Its weakest point today is that the first period, the archaeological, pre-European contact time, which is now woefully out of date. The then debate about whether there were contacts between "the southwest" and central Mexico has now been settled: there were. Still under intense investigation is how much, how long, and to what effect.

From this initial foray, my interests in nomad-nomad, sedentary-sedentary, nomad-sedentary and all three with New Spain, Mexico, and the United States developed. Also I found an interest in what kinds of pre-European or precapitalist world-systems existed and how they gave rise to the contemporary global system. I have been interested in how social change, broadly construed, simultaneously takes place on several time scales from the quotidian through the millennial – and yes, Braudel was lurking in the wings. I also developed an abiding interest in nomads and

pastoral nomads, indigenous peoples and their struggles. Another interest is how all these processes interact with geography, or space, both physical and human. Clearly, humans make their own history, but as Marx noted long ago, they do not make it any old way they please.

The morphing of my dissertation to a book was greatly helped by Scott McNall. I was, and am, greatly pleased to have been one of the first books in his series with University Press of Kansas. I wish the series could have continued; it has been a pleasure to be in the company of the other writers in the series. And, after such a long-winded prologue, Scott is how I came to be involved with the Comparative Historical section, from its founding forward.

Not surprisingly, the first section I joined in the ASA was the Political Economy of the World-System. That section has taken up the lion's share of my professional life. I have been part of its evolution from world-system theory, to world-system perspective, to world-systems perspectives, and now in world-systems analysis. My work has been heavily influenced by my association with Christopher Chase-Dunn, which began in 1982 as I was riding BART from Berkeley to San Francisco. I noticed across the car a guy thumbing through the ASA program, so went over to chat. From that chat grew a very productive collaboration.

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For the most part, our joint work centered on a shared interest in pre-modern world-systems. During that time Chris wrote what I still consider the best review of my *Social Change in the Southwest in Contemporary Sociology* (1990). It is "best" not because of its praise – tough, to be sure, that was nice – but because out of nearly thirty reviews he most clearly understood, and described, what I tried to do. I was pleased too that many historians who reviewed the book were generally positive. I attribute that to my strategy in the introduction to the book to draw on Charles Tilly's *As Sociology Meets History* (1981) to lay out what I was trying to do, and making clear I was *not* writing a history of the Greater Southwest but pursuing theoretical and conceptual issues using many types of comparisons.

Some of my first papers with Chris centered on how to compare world-systems and how older world-systems evolved into the modern world-system. We edited a collection on the topic, and in 1997 published *Rise and Demise: Comparing World-Systems*, and further explored scales in 2012.

During these years we worked with archaeologists, world historians, civilizationists, and others who had some overlap in interests or had collected information relevant to our research. One of our other contributions was to recognize that world-systems were bound at four levels, vaguely concentric: bulk goods, political-military interaction, luxury or prestige goods networks and information networks. This conceptualization rendered studies of incorporation into world-systems and mergers of world-systems richer and more complex. Examination of prestige goods or luxury trade underscored the role of long-distance trade in the evolution of world-systems.

In the mid-90s Chris and I were involved in an ASA panel where James V. Fenelon presented a

paper discussing the history of the Lakota peoples with the U.S. using some of our concepts. That quickly gave rise to my extended collaboration with Jim on a number of presentations, papers, and a book on indigenous peoples. We worked on developing a framework, again comparative, of processes of incorporation and change when indigenous societies encountered colonizers who made strong efforts to wipe out their cultures, their identities, and far too often, their lives. For me, some of our key findings were that many of these processes were similar across all types of states and world-systems. Too often, the disastrous consequences of those interactions have been attributed to capitalism, or a focus on European views and beliefs. For sure capitalism and European world-views played a significant role in these encounters. However, there were some broad similarities in encounters with Rome, China, South Asian states, and even the Aztec (aka the triple alliance) and the Inka. All this underscores a core issue that in the broadest terms could be described as conflicts between state and nonstate societies. The other "finding" (it was and is, in fact, old news) was the incredible staying power of nonstate peoples in the face of intense efforts to eliminate them.

Much of what we have found in our work was not new, but a recasting in broader terms. Too often indigenous peoples and nonstate societies are glossed over in reporting and explaining processes of interaction and are compartmentalized: studies in nonstate societies to anthropology; and more contemporary minority peoples to sociology. Also, studies of indigenous peoples are too often compartmentalized to specific modern state boundaries, or regions – even when interactions long pre-date the construction of those states and continue to cross state boundaries. For example, studies of indigenous peoples, whether historical or archaeological, all too often divide along the contemporary border between Mexico and the U.S., in spite of the relatively recent formation of that border between 1848 and 1853 (e.g., McGuire 1980, McGuire and Villalpando 1989).

This last issue and my research in the Greater Southwest are major drivers of my continuing interest in frontiers, especially when studied comparatively. Much of that work is summarized in two articles in the *Journal of World-Systems Research* in 2009 and 2013.

In 1990 I met P. Nick Kardulias, an archaeologist who specializes in the study of Mediterranean Greece, at a conference where he gave a paper on the North American fur trade that used some world-systems ideas (1990, 2007). Over the years we organized panels on many of these topics at Anthropology and Archaeology meetings, and occasionally some sociology meetings. One of our major arguments or pleas has been for those who have done detailed studies, both localized and regional, to use their findings to address issues in world-systems analysis, whether to contradict, elaborate, emend, or critique world-systems analysis. One of our major frustrations is that such people cite Wallerstein's first volume of the *Modern World-System* and nothing else since and then dismiss it at not applicable to their areas of research and remain woefully ignorant of world-systems research

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relevant to their studies. On the more positive side are the numerous studies that do use world-systems analysis, especially the work on precapitalist world-systems. Many of those works have been useful in rethinking and revising earlier statements. Our 2011 paper in J*ournal of Archaeological Research* (Hall, Kardulias, Chase-Dunn) summarizes many of these issues and the large body of work on them. Kardulias also pulled together two edited collections on these topics. The first, *World-Systems Theory in Practice* (1999), critically examines the use of world-system thinking in a variety of anthropological topics. The second, *The Ecology of Pastoralism* (2015), examines pastoralism comparatively both historically and geographically. Nick asked me to write the final comparative chapter (Hall 2015).

Along the way, via an email requesting comments on the draft of a book, I came to meet Peter Turchin. His manuscript applied findings about animal populations in his *Complex Population Dynamics: A Theoretical/Empirical Synthesis* (2003a) to human societies. That book eventually became *Historical Dynamics: Why States Rise and Fall* (2003b). What intrigued me about Peter's work, even though his mathematical derivations went beyond my faded memory of calculus, was his theoretical explanation of cycles among states. In particular, some of his hypothetical graphs were reminiscent of evidence and ideas that Chris and I had written about in *Rise and Demise*. This sort of thing is not unusual in the physical sciences where someone working in one area derives hypotheses that ultimately apply in other areas. This started another collaboration that led to several papers and Peter's joining discussions among world-systems analysts.

Later I joined with Peter in an attempt to examine what might be taken from animal and plant population dynamics in conditions of minimal contact (2003). The basic argument is that a very small connection between almost isolated populations could bring their cycles into synchronization. Further, if the cycles derived from chaotic processes – in the mathematical sense – there would be some synchronization, but it would fade in a few cycles. However, if we are thinking of long cycles in world-systems, 200, 300, or 600 years, a few cycles of synchronization would cover a large segment of history. This offered some insight into why trade in luxury or prestige goods and cultural or informational contacts might have consequences far beyond the intensity and volume of trade. This harked back to Jane Schneider's (1977) critique of Wallerstein's concentration in bulk goods trade. It also pointed a way to how and why very early contacts via the silk roads could simultaneously affect Rome and China, something that had puzzled world historians for many years. Peter and I had a few more papers exploring these possibilities.

We also collaborated with anthropologist Johnathan Adams (2006) to empirically examine Jared Diamond's (1997) argument that diffusion was easier along east - west axes than along north – south axes. Broadly, our crude measurements confirmed Diamond's surmise. We also argued that the one major exception, along the Andean corderilla, in fact is a confirmation, since spread was north – south along the same elevations and hence analogous to general east – west axes. So the general principle is that diffusion is easier along similar biomes than across them. We further note that north – south was more common when traders were pursuing "exotics" that could not be produced at home. These patterns seem to explain much of the expansion and merger of world-systems, especially in precapitalist settings.

One further comparative strategy that could be of use would be to make comparisons across comparable phases of cyclical processes rather

than always at similar chronological times. Sing Chew has done something like this in his study of dark ages (2007, 2008). Among comparative historical sociologists one the most nuanced examples of such comparison is found in Julian Go's *Patterns of Empire* (2011) in which he compares Britain and the United States at similar phases of the hegemonic cycle: ascent, maturity, and decline or competitive. Although it was not his major objective, he does, essentially, destroy all arguments of "American Exceptionalism" since nineteenth century Britain exhibited many of same, supposedly unique, processes in U.S. history.

In my own work, I had hoped to do something similar in comparing frontiers (2009, 2013). I framed my general discussion in terms of a "puzzle": why it is that scholars who study frontiers at first see broad similarities, but gradually uncover many differences? This rather common sequence seems to be embedded in the structure of frontiers: a handful of variables, with only a few different categories, will generate an immense number of potential comparisons. The underlying variables account for the initial impression of similarity; the myriad of "types" of frontiers readily gives the impression that every frontier is unique. Obviously, this latter impression is in some sense correct. However, it hides underlying processes and similarities. For me the purpose of sociological research is to dig out various similarities in processes, while remaining cognizant that each case is unique in some ways. Further, the second article comparing the "Greater Southwest" with southwestern China (approximately the area of Yunnan) demonstrates that there are important similarities that seem to be embedded in world-system position and dynamic cycles, and undermines claims that all frontiers are shaped solely by mercantilism, capitalism, and European colonialism. It is worth noting that most frontiers are in peripheral areas where incorporation of new peoples and new territories is most active. Thus studies of frontiers and borders that focus solely on the 19th, 20th, and 21st centuries can go somewhat astray because some key variables do not vary much in that time span. Also important is that by now, the world-system is planetary, and thus curtails many previously common frontier processes.

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My two papers are only first steps in exploring frontier dynamics. Alas, it remains to be seen whether I will be able to study them more in retirement. I do hope others will take up that task.

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Interested in telling your own story?

If you would like to contribute your own "Identities" essay for publication in a future issue of *Trajectories*, please email the newsletter editor, Matt Baltz (mjbaltz@ucla.edu).

Book Review

Max Weber: Modernisation as Passive Revolution

A Gramscian Analysis

Haymarket Books

Jan Rehmann

Reviewed by:

J. I. (Hans) Bakker
University of Guelph

This is a translation from the German-language original (Rehmann 1998). Max Henninger has done a good job of taking difficult prose and making it accessible to an English-speaking readership. One great value of the book is that the German-language publications Rehmann relies on provide a broader review of the literature than a book originally written in English.

The key question is whether a "Gramscian Analysis" of Weber can remain analytical rather than purely rhetorical. While there are many argumentative passages it is nevertheless also true that Rehmann attempts to give credit where credit is due. He even argues in places that Marx's thinking and the Marxism of the 2nd International often make use of something epistemologically akin to Weberian ideal types. This will be a difficult read for anyone in social science who does not have a fairly good background in German philosophy, particularly Kant, Fichte, Schelling, Hegel and Left Neo-Hegelians as well as Neo-Kantians. Having worked through Windelband, Rickert, Dilthey and Lask I appreciated the insights found in Rehmann. All of the major Weberian themes are dealt with, for the most part in a reasonable, although critical, manner. What is particularly relevant is that Rehmann has worked his way through some of the *Gesamtausgabe*. What is surprising in that respect is that he discusses "prebendalism" but he does not discuss Weber's writing on small-scale, indigenous Patriarchy (not to be confused with an essentialist and Feminist Patriarchy) and the two major forms of traditional legitimate authority that Weber labels Patrimonialism (i.e. Patrimonial prebendalism and Patrimonial feudalism). There is no discussion of the Asiatic Mode of Production in Marx or "Oriental Despotism" more generally. The term "Orientalism" is only noted as having reference to Eurocentric thinking. The discussion of traditional authority in both Marx and Weber is therefore not as complete as one would wish, given the use of the term "traditionalism" to refer to the influence of landed aristocracies (e.g. Prussian Junkers) and Weber's arguments about parliamentary governance having to trump bureaucracies. (In many countries the bureaucratic civil servants are ultimately answerable to the parliamentarians, just as in the U.S. the military bureaucracy is ultimately answerable to the executive and legislative branches.)

Only another 400+ page book could possibly deal with all of the detailed arguments found in Rehmann. It would be impossible to use in any but the most advanced graduate seminars. Almost all undergrads would hate it ("Will that

be on the exam?"). But having taught classical and contemporary theory for forty years and having read much of both Marx and Weber in both German and English I learned a great deal about some of the subtle nuances of the contributions of both thinkers. Since it is a German book by a German author there are a number of major English-language authors who are ignored (Randall Collins, Eric Olin Wright, Jonathan Turner, Stephen Turner, John Rawls, Charles Taylor, etc). But, having an insider's account of the internal German debates more than makes up for that. Some of the nuances that divide some German authors are made explicit. The Lutheran Church background is also clarified (i.e. mandatory "church" versus voluntary "sect" membership).

A Gramscian scholar will have to comment on the extent to which Rehmann uses Gramsci's ideas correctly and the validity of the argument that Weber was an "organic intellectual" of the haute bourgeoisie. I myself tend to think the argument is over-stated, but it has inspired me to read Gramsci more carefully.

A Gramscian scholar will have to comment on the extent to which Rehmann uses Gramsci's ideas correctly and the validity of the argument that Weber was an "organic intellectual" of the haute bourgeoisie. I myself tend to think the argument is over-stated, but it has inspired me to read Gramsci more carefully. Much of what Weber wrote was similar to Thorstein Veblen when it comes to values pertaining to the skilled working class as "industrialists." (Veblen is not mentioned, despite his clear relevance for any comprehension of the U.S. system and rapid industrialization.) The rhetorical use of "passive revolution" needs to be considered in terms of the actual, real-life historical results of "active revolutions" and revolts (e.g. Paris Commune of 1871, Russia in 1917 and the C.C.C.P.). Perhaps the term "evolution" could have been considered in more depth. Rehmann frequently mentions an overlap between Weber and Gramsci that I had not suspected. It is certainly not emphasized in the secondary literature, much less textbooks.

Overall, read this book, but with caution. Ultimately the author has a Neo-Marxian, "Gramscian" axe to grind and rejects all forms of "value freedom" (*Wertfreiheit*) and even the distinction between "value relevance" (*Wertbeziehung*) and value freedom. The claim to work for the liberation of all human beings not "just" historically-situated classes and status-power groups does not seem to have been provided an adequate philosophical basis. Sometimes statements about similarities between aspects of Fascism and aspects of Weber's *verstehende Soziologie* may cause a superficial reader to think that Rehmann thinks Weber had some latent fascist tendencies, which is actually not what he is claiming at all. Why Rehmann does not seem to fully comprehend the cogency of Kant's conceptualization of the *Ding-as-Sich* as a secular answer to Roman Catholic and Lutheran theology is not clear to me. This book will provoke some deep thought and tough questions. The Haymarket paperback price makes it affordable.

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New Publications

Books and Edited Volumes

Autocracy and Redistribution: The Politics of Land Reform

Cambridge University Press, 2015

Mike Albertus

When and why do countries redistribute land to the landless? What political purposes does land reform serve, and what place does it have in today's world? A longstanding literature dating back to Aristotle and echoed in important recent works holds that redistribution should be both higher and more targeted at the poor under democracy. Yet comprehensive historical data to test this claim has been lacking. This book shows that land redistribution – the most consequential form of redistribution in the developing world – occurs more often under dictatorship than democracy. It offers a novel theory of land reform and develops a typology of land reform policies. Albertus leverages original data spanning the world and dating back to 1900 to extensively test the theory using statistical analysis and case studies of key countries such as Egypt, Peru, Venezuela, and Zimbabwe. These findings call for rethinking much of the common wisdom about redistribution and regimes.

Inequality, Democracy, and the Environment

New York University Press, 2016

Liam Downey

The world currently faces many severe social and environmental crises. Using a novel theoretical argument developed by the author, *Inequality, Democracy, and the Environment* sheds new light on the structural causes of these crises and explains how they are linked to each other. Specifically, Downey argues that these crises are to a significant degree the product of organizational, institutional, and network-based inequality, which provides economic, political, military, and ideological elites with the means to develop and control organizational networks and undemocratic institutions that they use to achieve environmentally and socially harmful goals in the face of resistance from others. To demonstrate the validity and widespread applicability of his theoretical argument, Downey presents a series of case studies that (a) highlight several of the world's most important elite-controlled organizations, institutions, and networks and (b) show that these organizations, institutions, and networks play a key role in shaping some of the world's most critical human, social, and environmental crises. These case studies further demonstrate that undemocratic and elite-controlled organizations, institutions, and networks as diverse as the World Bank, agricultural commodity chains, policy planning networks, the military, and the news media belong to a single category of social mechanism that is responsible for much of the social and environmental devastation the world currently experiences.

Antecedents of Censuses from Medieval to Nation States: How Societies and States Count

Palgrave Macmillan, 2016

Rebecca Jean Emigh, Dylan Riley & Patricia Ahmed

Antecedents of Censuses from Medieval to Nation States, the first of two volumes, uses historical and comparative methods to analyze how medieval population counts and land surveys, starting about one thousand years ago, were the precursors of censuses in the United States, the United Kingdom, and Italy. The authors show that the development of censuses depended on the interaction between states and societies. Censuses developed as early and comprehensive solutions to state administrative problems where social actors had extensive knowledge that states could use and where social actors advocated for their adoption.

Changes in Censuses from Imperialist to Welfare States: How Societies and States Count

Palgrave Macmillan, 2016

Rebecca Jean Emigh, Dylan Riley & Patricia Ahmed

Changes in Censuses from Imperialist to Welfare States, the second of two volumes, uses historical and comparative methods to analyze censuses in the United Kingdom, the United States, and Italy, starting in the nineteenth century. The authors argue that censuses arose from interactions between government bureaucracies and social interests, and that censuses constituted public, official knowledge not where they were insulated from social pressures, but rather where intense social and political interaction surrounded around them.

Contested Embrace: Transborder Membership Politics in Twentieth-Century Korea

Stanford University Press, 2016

Jaeeun Kim

The incongruity between territory, citizenry, and nation has long preoccupied students of international migration, nationalism, and citizenship, including the state's transborder relationship with its "external" members (e.g., emigrants, diasporas, and ethnonational "kin"). This book is a comparative, historical, and ethnographic study of the complex relationships among the states in the Korean peninsula, colonial-era Korean migrants to Japan and northeast China and their descendants, and the states in which they have resided over the course of the twentieth century. Despite a widespread, deeply-entrenched, and quasi-primordial belief in Korean ethnic nationhood, the embrace of these transborder coethnic populations by the Japanese colonial state and the two postcolonial states (North and South Korea) has been selective, shifting, and recurrently contested. Through analyses of transborder membership politics in the colonial, Cold War, and post-Cold War periods, the book explores under what circumstances and by what means the colonial and postcolonial states have sought to claim (or failed to claim) certain transborder populations as "their own," and how transborder Koreans have themselves shaped the making, unmaking, and remaking of transborder ties as they have sought long-distance membership on their own terms. Extending the constructivist approach to nations/nationalisms and the culturalist/cognitive turn in recent theorizing on the modern state to a transnational context, the book demonstrates that being a "homeland" state or a member of the "transborder nation" is not an ethnodemographic fact, but a precarious,

arduous, and revocable political achievement, mediated profoundly by the historically evolving and mutually interlinked bureaucratic practices of the state.

The book is based on the dissertation that won the Theda Skocpol Dissertation Award in 2013.

The Making of an Indian Ocean World-Economy, 1250-1650: Princes, Paddy fields, and Bazaars

Palgrave Macmillan, 2015

Ravi Palat

To counter Eurocentric notions of long-term historical change, *The Making of an Indian Ocean World-Economy, 1250-1650* draws upon the histories of societies based on wet-rice cultivation to chart an alternate pattern of social evolution and state formation and traces inter-state linkages and the growth of commercialization without capitalism.

England's Great Transformation: Law, Labor and the Industrial Revolution

University of Chicago Press, 2016

Marc Steinberg

With *England's Great Transformation*, Marc W. Steinberg throws a wrench into our understanding of the English Industrial Revolution, largely revising the thesis at the heart of Karl Polanyi's landmark *The Great Transformation*. The conventional wisdom has been that in the nineteenth century, England quickly moved toward a modern labor market where workers were free to shift from employer to employer in response to market signals. Expanding on recent historical research, Steinberg finds to the contrary that labor contracts, centered on insidious master-servant laws, allowed employers and legal institutions to work in tandem to keep employees in line.

Building his argument on three case studies—the Hanley pottery industry, Hull fisheries, and Redditch needlemakers—Steinberg employs both local and national analyses to emphasize the ways in which these master-servant laws allowed employers to use the criminal prosecutions of workers to maintain control of their labor force. Steinberg provides a fresh perspective on the dynamics of labor control and class power, integrating the complex pathways of Marxism, historical institutionalism, and feminism, and giving readers a subtle, yet revelatory new understanding of workplace control and power during England's Industrial Revolution.

The Anatomy of Revolution Revisited: A Comparative Analysis of England, France, and Russia

Cambridge University Press, 2014

Bailey Stone

This study moves far beyond Crane Brinton's 1938 classic of comparative revolutionary analysis, *The Anatomy of Revolution*. It utilizes the latest research and theoretical writing in history, political science, and political sociology to compare and contrast, in their successive phases, the English Revolution of 1640-60, the French Revolution of 1789-99, and the Russian Revolution of 1917-29. Venturing beyond both Marxian "class" analysis and "revisionist" stresses on short-term, fortuitous factors in revolutionary causation and process, this book seeks ways to reconcile state-centered or "structuralist"

explanations of the three major European upheavals with "postmodernist" accounts accentuating the centrality to these revolutions of individual actors' agency, discourse, ideology, mentalities, and political culture.

The Fall of the Turkish Model: How the Arab Uprisings Brought Down Islamic Liberalism

Verso, 2016

Cihan Tugal

Just a few short years ago, the "Turkish Model" was being hailed across the world. The New York Times gushed that prime minister Recep Tayyip Erdogan and his Justice and Development Party (AKP) had "effectively integrated Islam, democracy, and vibrant economics," making Turkey, according to the International Crisis Group, "the envy of the Arab world." And yet, a more recent CNN headline wondered if Erdogan had become a "dictator."

In this incisive analysis, Cihan Tuğal argues that the problem with this model of Islamic liberalism is much broader and deeper than Erdogan's increasing authoritarianism. The problems are inherent in the very model of Islamic liberalism that formed the basis of the AKP's ascendancy and rule since 2002—an intended marriage of neoliberalism and democracy. And this model can also only be understood as a response to regional politics—especially as a response to the "Iranian Model"—a marriage of corporatism and Islamic revolution.

The Turkish model was a failure in its home country, and the dynamics of the Arab world made it a tough commodity to export. Tuğal's masterful explication of the demise of Islamic liberalism brings in Egypt and Tunisia, once seen as the most likely followers of the Turkish model, and provides a path-breaking examination of their regimes and Islamist movements, as well as paradigm-shifting accounts of Turkey and Iran.

Articles and Book Chapters

Fox, Cybelle. 2016. "Unauthorized Welfare: The Origins of Immigrant Status Restrictions in American Social Policy." *Journal of American History* 102(4):1051-1074

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Section News

News and Announcements

New Master's in International Development

The Barcelona Institute for International Studies (IBEI) offers a new Master's in International Development. This Master's is a one-year full-time degree intended for future professionals in the area of development research and practice. Its innovative approach combines both academic rigour and policy relevance, offering a wide range of specialised courses on key topics of international development such as ongoing trends in socioeconomic inequalities, global governance, economic growth and sustainability, conflict over limited resources, and new emerging powers and social movements. This advanced and interdisciplinary programme, taught in English, is tailored towards students considering a high-level career in government, the private sector or non-governmental international organisations, or wishing to pursue an academic career.

For more information please consult http://www.ibei.org/ or write to the academic coordinators Matthias vom Hau (mvomhau@ibei.org) and Pablo Astorga (pastorga@ibei.org). The 2016-2017 edition of the International Development Master's starts in mid-September 2016. The application deadlines are as follows:

1 July 2016: Non-EU applicants

25 July 2016: EU applicants

Newsletter Archive Now Available Online

In the previous issue of *Trajectories* (Vol 27:2) it was announced that Thomas D. Hall's archive of section newsletters and documents stretching back to 1982 had been digitized thanks to the efforts of Reed Klein and Sarah Quinn and with the support of section funds. Now, Hall's entire archive is available online as searchable PDFs at the following address:

http://asa-comparative-historical.org/newsletter_archive.php.

Thanks are due once again to Tom, Sarah, and Reed for all their help in making the digitization of our section's history a success, as well as to our section's webmaster, Sahan Savas Karatasli, for his work posting the archive online.

Awards

Emily Erikson's book, *Between Monopoly and Free Trade: The English East India Company, 1600-1657* (Princeton University Press, 2015) has won the Social Science History Association's 2015 Allan Sharlin Memorial Award. This honor was shared with co-winner, Mara Loveman, for her book *National Colors: Racial Classification and the State in Latin America* (Oxford University Press, 2014)

Coming up in
the next issue of

Trajectories

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

New book symposium:
Conscription, family, and the modern state
by Dorit Geva

And
Much
More!