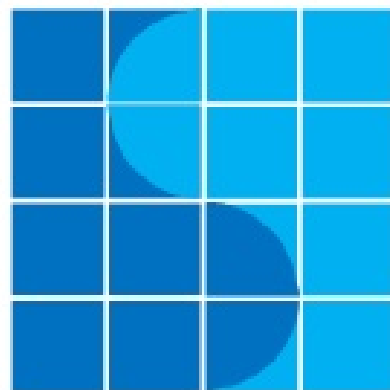


Trajectories

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2015 ASA Conference Report

Can Comparative Historical Sociology Save the World?

Editor's note: In this feature, Monica Prasad, Jensen Sass, Josh Pacewicz, and Jason Jackson report on what they learned from attending panels on this year's conference theme.

Monica Prasad
Northwestern University

The most important lesson I took away from this year's panels is that the elements that make a piece of scholarship intellectually

exciting are the very same things that make it policy relevant, namely (1) a well-argued statement of cause and effect or (2) a completely new way of looking at a problem. These two paths were exemplified by two brilliant papers on the "Genocide" panel.

Robert Braun (a new faculty member at Northwestern) showed how to make a statement of cause and effect: he used multiple kinds of evidence to

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argue that it is religious minorities who are most likely to shield potential victims of genocide. It's not the ideology of the religion that matters, as much as the fact of the believers being in the minority. Braun argued that being in the minority makes it easier for them to coordinate, and increases their empathy with potential victims. This opens up important new insights into how to intervene in genocides, and with whom to partner when attempting to save victims.

Aliza Luft (a graduate student at Wisconsin, on the job market this year—grab her if you can!) exemplifies how to give us a completely new way to look at a problem: she studied the Rwandan genocide and makes the point that perpetrators of genocide are not always perpetrators. In some situations they are simply witnesses, and on other occasions they are actually protectors. When we reify the categories of perpetrator and protector, we miss the situational factors that lead to killing. This too opens up important perspectives into the situational factors that we could target when intervening in genocide.

Along with the other papers on the genocide panel, these two papers suggest that genocide is emerging as the next frontier of policy-oriented comparative historical research.

I also learned that with these exceptions, by and large comparative historical sociologists are not seriously attempting to conduct policy-relevant comparative historical sociology, even when they focus on a “real world problem.” Good efforts are being made in defining and measuring modern slavery, such as Kevin Bales and Monti Datta's Global Slavery Index, but this work is on the margins of our subdiscipline today. On the other issues—Israel/Palestine, global poverty, and climate change—there were some thought-provoking papers, but I found three tendencies that limit the relevance of the work to policy,

and also make it less intellectually exciting than it might otherwise be. First, scholars often limit themselves to describing the state of affairs, e.g. simply describing variation in how different countries approach a problem, or how something has evolved over time, without trying to provide an explanation for the differences or the changes. Second, where a causal statement is attempted, scholars will often blame “capitalism” for the problems without making a serious effort to assess whether non-capitalism really would resolve the problem. And third, instead of actually analyzing the main question, sometimes scholars dance around it, or are unable to link their narrower investigation back up with the main question.

Doing research that can actually provide insights into policy is hard, and there is currently no infrastructure in our subdiscipline that would help a scholar trying to move forward in this way. So I salute all the efforts that scholars who try to address real-world problems are making, and over the course of this year the section will be doing more to try to set up such an infrastructure (including dissertation proposal development workshops for graduate students....stay tuned).

Jensen Sass Yale University

Most sociologists would accept that human societies are complex systems and that the effects of policy interventions are thus hard to predict. A policy intervention which aims to resolve an economic problem can have unpredictable social, cultural, and political consequences, each of which have their own effects, on each other and, iteratively, on economic affairs. The problem that complexity poses for policy should be especially obvious to comparative-historical sociologists who are

wont to perform multifactorial analysis of large-scale and long-term processes of social change. But if comparative-historical sociologists are especially aware of social complexity, they need not refrain from policy discourse. Indeed, this awareness could lend us a competitive advantage over other social scientists. The 'Can Comparative-Historical Sociology Save the World?' panels at ASA 2015 suggest that our subfield can contribute to policy in a number of ways, three of which became apparent to me. Comparative-historical sociologists can: (1) alert policy makers to the dangers of unintended consequences; (2) they can massively expand the scope of participation in policy-making, and; (3) they can enlarge our collective understanding of political opportunity. In what follows I briefly consider each of these contributions with references to research presented at Chicago.

In this newsletter Monica Prasad discusses Robert Braun's excellent work concerning the fate of Dutch Jews during the Holocaust. Braun shows that most Dutch Jewish survivors were saved by religious groups that were minorities in their immediate surroundings. Braun argues that empathy, and so a willingness to protect, was greatest among those who had previously feared persecution. Braun's policy prescription is that we bolster minority groups, in both resources and numbers, so that where genocidal conditions emerge, the empathy of these groups can be put to good use. It is a clear and striking prescription and yet, as comparative-historical sociologists, we are aware of the need to carefully consider the knock-on effects of any recommended intervention. In this case, there is a real and ironic risk that by bolstering minority groups, their members will no longer fear persecution. The implication is that our support for minority groups would erode the foundation of their empathy and so wreck the very mechanism that can save lives.

But even if we discount this risk, our capacity to support minority groups may well be limited or may have other perverse consequences, as suggested by research presented on the same panel by Hollie Brehm. Brehm's paper examined the social and political factors associated with genocide—it is an extension of long-standing efforts by the United Nations to produce an early warning system which could alert the international community and facilitate humanitarian intervention. One factor associated with genocide which Brehm discussed was the growth of an unchecked executive, its relationship to the abuse of state power being obvious enough. Though it seems counterintuitive, one pathway to 'unchecking' executive power is bolstering civil society. How so? The recent demolition of independent civil life in Russia was caused, in part, by the suspicion of Russian authorities that civil society organizations were being propped up, and directed, by foreign governments—a belief formed in light of the Ukrainian revolution and longstanding efforts by the U.S. State Department and private foundations to deepen Ukrainian democracy. In this regard, any attempt by foreign organizations to shore up minority groups, for whatever purpose, must be weighed against its wider effects. Indeed, if executive power is already concentrated in a country, opportunities to bolster minority groups may be limited, as in Russia today. But even if executive power is not concentrated, actions driven by our moral convictions must be carefully considered because their consequences will seldom be discrete. If this awareness of social complexity seems to encourage inaction, it is worth noting that interventions which directly prevent genocide will seldom generate unintended consequences worse than genocide itself. More generally, comparative-historical sociology should not merely warn that there are always unintended consequences; it should identify

their recurring forms, such as runs on banks, and it should reveal where their likely occurrence demands that policy makers tread carefully.

There are strong epistemic justifications for public participation in policy-making processes. Where states act without input from ordinary citizens they are prone to overreach and policy failure, a pattern comparative-historical scholars have extensively documented. Encouraging participation is thus a clear and familiar prescription for policy makers, but this insight can be radically extended by advocating for the direct participation of historical-comparativists in the policy-making process. In their transnational and transhistorical study of social, political, and economic affairs, comparative-historical sociologists can convey the experience and insight of peoples from across a massive swathe of human society, past and present, which is to say that comparative-historical sociologists can be a vehicle of popular participation. This, of course, is a somewhat abstract conception of participation but it surely bears consideration if there is something to the idea of learning from history. Work presented on the climate change panel at Chicago is indicative of the form such scholarship could take. Climate change is an enormously complex policy problem—one with no precedent and the capacity to paralyze policy-making processes. But work presented on this panel demonstrated that while the substantive challenge is new, its political and institutional dimensions enjoy extensive precedent. The panel's papers described successes and failures in achieving environmental goals by various legal and political means and, in doing so, it condensed historical and institutional experience into a form that might guide future action.

In his discussion for the Global Poverty session, Samuel Cohn suggested a related idea

about the contribution comparative-historical sociology might make to the formation of policy. He sketched an image of comparative-historical sociology which facilitated what he termed 'curatorship.' The nub of his idea is that social scientists can recover forgotten historical episodes that can widen the horizons of current policy thinking and increase the sense of political opportunity held by ordinary citizens. Though it was not presented at Chicago, recent scholarship on state-led but radically decentralized programs of agricultural research during the New Deal, as recovered by Jess Gilbert, is a clear case in point. Gilbert describes a policy episode that pushed against the centralization and privatization of agricultural knowledge which has characterized the past century and which for many observers is a matter of deep concern. Gilbert's work suggests another way, one now inspiring a new movement of agricultural activists and researchers.

These three kinds of policy contribution lack the elegance of formal models and their clear and confident prescriptions. But they make up for this in their realism and their refusal to wish away, via the magic of *ceteris paribus*, the vagaries that define complex societies. They each afford an important role for comparative-historical sociology and the kinds of knowledge and insight our field can offer to a policy-making process rightly understood as the science of muddling through.

Josh Pacewicz
Brown University

Due to my substantive research interests, I was also predisposed to think about the question of how CHS can change the world in terms of policy impacts first, but took a somewhat broader view of "world changing": essentially, I reflected on how styles of historical-

comparative reasoning could inform policy-making rather than trying to identify aspects of our reasoning that will actually make us more policy-relevant.¹ This is because the question of what types of historical-comparative claims can influence ongoing policy debates is an empirical one, and I am not confident that I know the answer. I am fairly certain that just redirecting CHS towards a narrow focus on making counterintuitive, causally-specified claims would do little to endear us to policy-makers, and—more generally—I see no warrant for holding up this style of historical-comparative work as a policy-relevant ideal. Consider two examples.

At one extreme, we have accounts like Greta Krippner's of 1970s-era financial deregulation: advocates of de-regulation gained currency because they were well-positioned to give answers that policy-makers wanted to hear, not because their answers were right and produced the outcomes that they expected. My own work focuses partially on municipal finance, and I have watched several reform efforts unfold—for instance, California's recent liquidation of its redevelopment agencies (these agencies issued 14% of the United States' municipal bonds at their peak). I spoke with a couple people involved in this reform and have talked to a few individuals who work for policy think tanks, in part to kick the tires of a potential project on policy influence. The case of redevelopment is germane here, because the academic literature on the topic was written mostly by scholars trained as economists and virtually all of it mobilized sophisticated models to answer the question, "do these agencies promote growth?" These studies went ignored by legislators. Meanwhile, state policy think tanks focused on the diffuse consequences of redevelopment agencies for local democracy, their indirect costs to other municipal governments, and conducted case studies that nicely illustrated how redevelopment works on the ground.

Legislators seemed to more readily take up the latter. Indeed, the knowledge claims that seemed most influential were precisely opposite those most valued by the self-avowed scientist: anecdotes.

Such cases lead me to think that, first, policy-influence has more to do with knowledge-producers' structural position—that is, their ability to get the right information to policy-makers at the right time—than the content of what they say and, second, that the type of content that can influence policy is broader than what we may assume.

Stated differently, I see the simplest explanation of historical comparativists' lack of influence as rooted not, to borrow a distinction from Aristotle, in material causes (i.e., the kinds of knowledge claims we produce), but formal causes: we comprise a tiny social network adrift in a vastly complex society, one with few ties to centers of policy debate. We would likely have little collective influence on policy no matter what kinds of claims we make—although, perhaps, the smallness of our field may provide an individual comparative advantage in some cases. Insofar as this is a structural problem, the solutions are practical ones that involve building closer ties with worlds of policy-making. I have no particular insight into whether or how historical comparativists should do these things, and focus instead on what kinds of useful insights historical comparativists could contribute should anyone with policy influence ever ask.

Historical sociologists have many politically-relevant insights, but I will highlight just one: that political elites' commitments to ideologies, policies, and worldviews are shaped by historical processes. This was especially evident to me while watching the Israel-Palestine panel. I found the panel instructive, because all the papers—although flawed in

some ways—nicely illustrated the centrality of historical contingency in sociologists' thinking about political elites. As Daniel Hirschman and Elizabeth Popp Berman tell us, even economists, presumably the most world-changing social scientists of all, are not influential primarily because their advice carries the day in any particular policy debate, but rather when policy makers adopt economists' styles of analyzing, measuring, and ultimately seeing the world. Similarly, the Israel-Palestine panel showed that sociologists make useful tacit assumptions when analyzing political elites, even when their work may have other flaws. The panel consisted of three papers. The first, Ian Steven Lustick's "Fatal Attraction: Four Constructions of the Holocaust in Israeli Society," provided a historical analysis of Israeli politicians' framing of the Holocaust. The other two papers—Andy Clarno's "Neoliberal Apartheid in South Africa and Israel/Palestine" and Shai Dromi and Gülay Türkmen-Derviřođlu's "Reversal of Fortune: The Trauma of the Displaced Founding Elite in Israel and Turkey"—relied on unexpected comparisons: between post-1994 South Africa's treatment of blacks and Israel's treatment of Palestinians and between Israeli and Turkey's founding elites' reactions to electoral defeat.

Lustick's paper shows that Israeli politicians have framed the Holocaust differently over time. Israel's founders initially presented it as a Zionist Proof Text, or as justification for their efforts to create a permanent state. Others subsequently presented it as a Wasting Asset, or as card that Israel had to play quickly for leverage in international affairs, or an objective lesson in human rights. Later, the rising Likud Party framed the Holocaust as a template for Jewish life, or the Jewish experience par excellence, which illustrates that extermination by gentiles is a constant and immanent possibility. Lustick's paper is steeped in historical detail, which illustrates that

politicians adopted framings of the Holocaust because they were contextually useful in catalyzing coalitions in favor of statist and—in Ben Gurion's case—apparently Leninist-inspired statecraft, advancing politicians' own careers, or in justifying other immediate goals. No less important were historical events like Britain's interdiction of the SS Exodus, the Eichmann trial, and Israel's unexpected success during the Six Day War, which reshuffled the fortunes of politicians and their version of the Holocaust. Despite such contingencies, Lustick shows convincingly that politicians subsequently took their own narration of the Holocaust as natural and true as did the general public, particularly after narratives were introduced into school books, state financed trips tours of concentration camps, and the like.

In similar ways, Dromi and Türkmen-Derviřođlu and Clarno's papers focus on the historical processes that influence political elites, producing unintended results. Dromi and Türkmen-Derviřođlu's paper shows that Israel and Turkey's founding elites experienced the trauma of losing power similarly, and even went through similar grieving stages over time: they initially claimed that voters acted irrationally, then accused the ascendant party of polluting democracy, and so on. In showing political elites behaving badly in similar ways, the paper convinced its audience that coming to terms with losing power is a process with definite consequences for elites' political imagination. Clarno's paper too illustrated the surprising emergence of "neoliberal apartheid" in both Israeli society and South Africa after the formal downfall of the apartheid regime. One would expect the trajectory of the two nations to diverge after 1994, but each nation's elites instead began framing problems of structural inequality as ones of individual dysfunction like black crime or Muslim terrorism, justifying apartheid-like

policies on this basis. The paper therefore invites reflection on contemporary political economy, and particularly the aspects of it that encourage politicians who ostensibly embrace different state-building projects to pursue similar policies.

Each of these papers could have been more successful on its own terms. For instance, Lustick's paper presents one version of the Holocaust—as template for Jewish life—as hegemonic in Israel and Jewish Diaspora communities, but I doubt that is entirely true. To paraphrase Marshall Sahlins, the world is in a constant state of becoming, not being, and it would have been nice to see systematic consideration of the political coalitions, educational policies, and other factors that lock-in one version of the Holocaust as authoritative in Israeli public life. Similarly, Dromi and Türkmen-Dervişoğlu's paper is arguably less about the fortunes of founding elites than the nature of parties within electoral systems, and more explicit attention to how dynamics of party competition promote framings of electoral outcomes would have been welcome. Finally, Clarno's paper raised many questions about the aspects of Israeli and South African political economy that lead elites to understand structural inequality similarly, ones which his subsequent work will hopefully answer. Throughout all the talks, I found myself wishing for more insights into how contemporary political leaders strategically mobilized historical conflicts, not accounts of how historical conflicts produce an essentially static present. But such shortcomings notwithstanding, the papers as a set communicated convincingly that political elites embrace ideologies and policies for reasons other than their substantive merits.

This key insight is one that historical comparative sociologists could contribute to contemporary policy debates, by - for example - presenting policy-makers with theoretically-

informed case studies of how and why political elites reacted in particular ways to certain historical circumstances. A critical reader might object that policy-makers would likely not see such insights as a solution, much less a useful one, but I suspect that this reaction belies an overly-narrow understanding of how policy influence works.² My limited exposure to policy making leads me to think that those who are truly influential approach policy as a process more than a one-shot affair: the case of expert showing up, prognosticating on what should be done, and having their recommendation adopted is rather the Hollywood version. In many cases, the scenario is more akin to a back and forth wherein policy makers (or, realistically, their staff) approach experts with a problem (or vice versa), experts respond in ways that shift the former's understanding of the problem, and the process repeats, often eventually producing an initiative that is radically transformed from policy-makers' initial ideas—or, more frequently, drops off along the way. I have heard more than one policy professional joke that 95% of policy-makers initiatives never become laws and that 98% of them probably should not (I have not independently verified this claim, but the proportions strike me as plausible). That historical comparativists are not involved in such processes of separating wheat from chaff is, I think, a product of the structure of our political system more so than the type of knowledge we produce. Certainly, it is not hard to think of cases—austerity measures during The Eurozone Crisis, anti-immigrant policies, and the like—where one wishes that some brand of expert would introduce more historically-informed introspection into the policy making process.

Endnotes

1. I am grateful to Monica Prasad and Dan Hirschman for generous and thoughtful comments.

2. Crazier things have also happened in the past; for instance, Stephanie Mudge shows that left-leaning parties once relied on Marxist theoreticians (!) for policy expertise.

Jason Jackson

Wharton School

The focus on policy relevance in the Comparative Historical Sociology series "Can CHS Save the World" is a valuable and important step for encouraging comparative and historical minded scholars to engage in policy-relevant discourse and debate. I am convinced that there is significant opportunity for interventions from CHS scholars and hope that this marks the beginning of a movement towards promoting engagement with the policy world. In my scholarly and professional experience (doing research on economic policy in India and as a research economist and consultant to a range of development organizations in the Caribbean and South Africa), many policymakers, particularly at local and municipal levels in industrialized countries and at the national level in developing countries, are quite skeptical of the status quo approach to policy formulation that primarily rests on technical economic analysis. This is not to say that they are entirely dismissive of economic approaches. However, scholars who utilize alternative research methods and modes of reasoning should not be too quick to cede the high ground of policy legitimacy. There is significant potential for scholars from other disciplines to contribute effectively and productively in policy debates. In particular, scholars who use comparative historical methods and modes of analysis are well equipped in a number of key ways.

I would suggest two strengths of CHS scholars that would augur well in policy discussion. First, policymakers are drawn strongly to historical reasoning and logics in at least two

ways: by recognizing the importance of 'deep' historical structures, including those that shape belief systems and worldviews; and being strongly influenced by lessons from recent (and often short-run) historical experiences. As such, policy spheres are fertile ground for scholarly work that relies heavily on historical explanation. Second, and relatedly, I've found that policymakers are strongly drawn to comparative case examples where strong similarities can be articulated between successful experiences and episodes where a positive policy outcome was generated. For example, in small as well as large developing countries, policymakers are deeply drawn to mechanistic causal explanations that promise to produce growth, particularly at a sectoral level and more so than an abstract macroeconomic level where measurement is a function of numerous factors that are cognitively difficult to disentangle, and hence where technical macroeconomic models tend to dominate. This is particularly important given that policy action largely takes place at the sectoral level through line ministries and departments, not simply in Finance or the Treasury where broad-based macroeconomic strategies are defined. This is an important element of the policymaking world that is often overlooked by scholars across disciplines. As such, whereas I certainly agree that there are important challenges for CHS engagement in the policy domain, however construed, I am far from pessimistic.

I would make two suggestions in order to strengthen CHS engagement in the policy domain. The first rests on CHS scholars having confidence in making causal claims based on their empirical research. This does not mean abandoning nuance and contingency, which are signature strengths of high quality CHS work, but it does suggest relegating these to the background in favor of foregrounding simpler causal relationships. Policymakers are deeply enmeshed in the real world and are all

too aware of the importance of contingent factors. This is an advantage for CHS scholars, as the background contingency and ever-present nuance can be deployed in response to challenges in the course of debate in a manner that strengthens rather than weakens foregrounded causal claims. Second, an analytic advantage that comparative historians may have over other policy-engaged social scientists lies with the use of mechanisms that provide clear causal pathways and linkages to outcomes of interest. Mechanistic explanations have powerful intuitive appeal, often more so, for example, than evidence that comes from studies that rest on correlation. Specifying mechanisms also is valuable in providing a roadmap for policy action. Highlighting the role of mechanisms in generating outcomes is thus an important tool in the CHS scholars' policy engagement toolkit.

Turning to the CHS panels at this year's ASA meeting, the subject areas of the 'Save the World' panels addressed issues of major policy importance; however, the content and discussions on the panels could have gone much further in explicitly addressing policy issues. This report discusses two of these panels: Israel-Palestine and Modern Slavery.

First, even though the first panel was billed as Israel-Palestine, the papers seemed to focus more on the former than the latter. Andy Clarno's "Neoliberal Apartheid in South Africa and Israel/Palestine" perhaps came closest, while Ian Lustick's "Fatal Attraction: Four Constructions of the Holocaust in Israeli Society" provided a fascinating analysis of political culture but might have said more about Palestine, particularly given the role that conceptions of Palestine likely play in the evolving idea of Israel, especially in the last two decades. Shai Dromi and Gulay Türkmen-Derviřođlu's "Reversal of Fortune: The Trauma of the Displaced Founding Elite in Israel and Turkey" fell in the middle, using the

Israel-Palestine issue to highlight internal Israeli political dynamics, particularly to explain the weakness of the pro-peace left-wing and its failure to generate support among the religious non-European segments of the Israeli population.

More fundamentally, while the core comparison in Clarno's paper on neoliberal apartheid was appealing (particularly having lived in South Africa myself), more could have been done analytically to make clear the work that the term 'neoliberalism' was doing in the comparisons. What does neoliberalism mean, and what are the implications on outcomes of interest, as well as for policy? For example, if part of the objective is to critique conceptions of neoliberalism as a set of institutional arrangements between state and market, how do the institutions that constitute 'apartheid' in these countries challenge our conception of neoliberalism? One of the more compelling elements of the paper was the focus on internal elites, particularly Palestinian and black South African elites (in addition to the expected Israeli and white South African counterparts). This is certainly one of the more striking features of contemporary South African society, and it is a relatively recent development that increased in pace during Thabo Mbeki's ANC administration through the combination of black empowerment policies and what was widely termed as the "neoliberal" macroeconomic strategy under the "Growth, Employment and Redistribution" (GEAR) policy package. However, since part of the goal of the theme was (perhaps) to point to policy relevant findings, if not propose policy solutions (thus illustrating how CHS might save the world), it would have been interesting to hear perspectives from each of the authors in their area of analysis. For example, we would want to learn more about the processes that generated the rise of this new black economic elite (as well as its Palestinian counterpart). What were the mechanisms that

led to what the author describes as "a reconfiguration of apartheid from racial apartheid and class-based apartheid"? And crucially from a policy perspective, does the identification of these mechanisms provide opportunities for intervention, either by actors working within the political system or social movements operating outside of it? Similarly, what are the implications of Lustick's compelling analysis of political entrepreneurship for Israeli politics that might generate desirable policy outcomes? Are there implications for generating mobilizing political ideas that might lead to alternative policy outcomes?

The Modern Slavery panel focused on an issue that is at the forefront of national and transnational policy discussion, particularly around labor migration and human trafficking. However, even though the policy relevance was clear, the presentations were not explicitly oriented toward providing concrete policy implications in specific contexts. This is not to say that the content was not applicable to policy debates. Allain's paper was helpful in providing a working definition of modern slavery, particularly in making the distinction between *de facto* and *de jure* slavery (around condition vs. status) that emphasized the importance of power and control. Similarly, Kevin Bales and Monti Datta's work on constructing a Global Slavery Index is a useful step towards creating a policy relevant dataset, though it raises questions about survey methodology across diverse social, political and cultural contexts. This is a methodological critique that has been raised by some who work in this field and is part of a wider critique of the increasing dominance of indicators as governance tools.

Most importantly, however, one got a strong sense with this panel that an opportunity for engaging in policy debates was not fully exploited. For example, the discussions on the

definitions of slavery and categories of forced labor provided opportunities to debate existing interventions as well as potential alternatives, such as whether defining labor conditions as slavery generates effective policy responses by the national and supra-national policy organizations (and the plethora of non-governmental organizations) that seek to address them. The surprising answer may be that it doesn't. As legal scholars such as Janie Chuang at American University suggest, once defined as "modern slavery," the policy response has focused on the use of criminal law through an abolitionist response that centers on criminal law. This may satisfy the outrage that the idea of modern slavery generates, especially in wealthy countries, but it does little to address the underlying structural issues that generate these outcomes for the people involved. By contrast, Chuang and others argue that a focus on labor law might provide a range of policy solutions that are foreclosed by criminal law approaches.

This brings us back to the importance of highlighting mechanisms in comparative historical work and bringing these to bear in engaging with policymakers and in policy debates in the public domain. The use of these types of tools that are a central component of the CSH scholar's toolkit would be a powerful and effective addition to the policy realm.

The Cultural Revolution at the Margins: Chinese Socialism in Crisis

Harvard University Press

Yiching Wu

Editor's Note: The following text is based on an author-meets-critics session that took place at the Social Science History Association Annual Meeting in November, 2014. My thanks to Joel Andreas, Michael Kennedy, Marc Blecher, Xiaohong Xu, and Yiching Wu for agreeing to prepare their comments for the newsletter.

Testing the boundaries of the Cultural Revolution

Joel Andreas
Johns Hopkins University

The focal point of Yiching Wu's new book about the Chinese Cultural Revolution is the dynamic and troubled relationship between Mao Zedong, who issued the call to rebel, and the disparate groups that took up his call. The central narrative involves Mao introducing the issues, rebel groups taking these issues beyond the boundaries he set, and Mao ultimately suppressing his more transgressive followers. By focusing on rebels who tested the boundaries of the Cultural Revolution, Wu is able to elucidate these boundaries with particular clarity.¹

The three main empirical chapters treat episodes central to the development of the movement. The first of these chapters examines the early skirmishes, starting in the

spring of 1966, between Red Guard groups in elite Beijing middle schools and their debates about "bloodline theory," a high-stakes battle over how to define "class" that helped set the course of the movement by identifying its protagonists and targets. The second analyzes the events leading up to and following the 1967 "January Storm" in Shanghai, in which rebels seized power from municipal officials and, per Mao's instructions, established a new revolutionary committee governing structure that included old leaders and outside military cadres along with representatives of rebel mass organizations. The third examines the emergence of a particularly radical coalition of rebel organizations in Hunan Province that opposed the formation of a provincial revolutionary committee, which it regarded as returning to the old system. The suppression of the radical faction in Hunan in 1968 helped signal the end of the period of freewheeling factional contention unleashed by Mao two years earlier.

Each of these three episodes has been the subject of numerous academic accounts. The debates about "bloodline theory" among middle school Red Guards has been more extensively discussed in scholarship about mass participation in the Cultural Revolution than any other topic, and the seminal Shanghai events have been scrutinized in numerous books and articles. The Hunan conflict has

received less attention, but the famous *Whither China?* manifesto, which is the centerpiece of Wu's Hunan chapter, has been analyzed by a number of scholars and Jonathan Unger has investigated the social milieu and political conditions from which it emerged. Wu's new accounts of these episodes, however, are a welcome and important contribution for several reasons.

First, Wu makes use of a wealth of primary sources that were unavailable to or not fully exploited by previous scholars. These include materials unearthed in local archives and thousands of recently compiled Cultural Revolution-era documents, as well as newly published memoirs. Based on these materials, Wu adds important elements to our knowledge of each of these episodes, and he has produced what will likely be the authoritative account of the events in Hunan.

Second, Wu effectively challenges depictions of Cultural Revolution activists as largely pursuing instrumental, self-interested goals. This has been the dominant line of analysis in existing scholarship, with two main variants. The first has presented members of opposing factions as pursuing interests defined by their pre-existing social positions. This interpretation has been challenged by Andrew Walder and others, who have argued that preexisting position was not as important as interests that emerged as factional struggles unfolded and groups sought to gain political advantage and avoid becoming victims of repression. Wu discusses at length the grievances of social groups that became recruiting bases of the organizations he examines and he is not oblivious to the advantages of winning and the dangers of losing. Nevertheless, he objects to efforts to analyze the entire movement through the lens of instrumental concerns. Cultural Revolution activists, he rightly argues, were highly ideological, driven by ideas and ideals.

Finally, Wu uses his account of each of these episodes to develop a very insightful interpretation of the evolution of the Cultural Revolution, organized around the complicated relationship between Mao and his unruly followers. He exhibits a sophisticated understanding of the dynamics of the movement and a strong sense of the grievances and ideas that motivated the rebels. He clearly empathizes with the rebels, which helps readers better understand the thinking of the actors involved, while retaining the critical, analytical eye of a scholar.

The title of the book is somewhat misleading—it is not actually about events and groups at the margins of the Cultural Revolution. As noted above, the book examines episodes that were centrally important to the development of the

[Wu] objects to efforts to analyze the entire movement through the lens of instrumental concerns. Cultural Revolution activists, he rightly argues, were highly ideological, driven by ideas and ideals.

movement. Nor are the people marginal. The student Red Guards in Beijing and the rebel workers in Shanghai, the main protagonists in two chapters, lived in the two most important cities in China, they studied in elite high schools and worked in prestigious factories (in Mao-era China these factories were, indeed, highly prestigious places to work). They were at the very center of the socialist order and they were—for this reason—at the center of the Cultural Revolution. Even Wu's protagonists in Hunan are not best described as marginal. They put together the largest rebel coalition in the province, they had sound reasons to consider themselves the most Maoist of the local factions, and the author of

the radical *Whither China?* manifesto was the son of a revolutionary hero. Moreover, the debates that Wu dissects—about class, inequality, bureaucratic domination, and democracy—all concerned ideas at the heart of the movement. Nevertheless, here is where Wu's claim to be exploring the margins the Cultural Revolution originates, as he subjects to close scrutiny ideas and actions that went beyond the boundaries of rebellion established by Mao.

If I were writing an account of these three key cases, I would alter Wu's narrative a bit. In the first two, involving the Beijing middle school Red Guards and the Shanghai workers, Mao suppressed transgressions that would actually have led the movement in less radical, less dangerous directions. The cadres' kids in Beijing were convinced that the rightful targets of the movement had to be the conventional targets of Communist campaigns—the old elite classes. By suppressing the cadres' kids, Mao turned the movement in a much more radical, unprecedented, and dangerous direction—he insisted that the main target of the Cultural Revolution was the very parents of these Red Guards: the party officialdom.

The workers in Shanghai were different. They wanted to raise basic economic grievances and demands, demands that the CCP had always been careful to discourage and contain. It was clear, however, that at least some Shanghai officials were happy to see the movement head in this less political direction. Today, Chinese leaders certainly take this approach: Economic demands are OK, but political demands are off limits. Mao's thinking was exactly the opposite. He was certainly concerned that meeting workers' economic demands would be costly, but that was not his main objection. He was more concerned that economic demands might divert the movement from what he insisted should be its main target—the “capitalist roaders in the party.” He wanted the

target to be the communist leaders in every village, workplace, and locality. This was a much more dangerous direction, but that's exactly where he wanted the movement to go.

It was the protagonists in Wu's third case, the Shengwulian rebels in Changsha, who fully appreciated the radical direction that Mao wanted to take the movement, and pressed it in this direction beyond where Mao was willing to go. Wu highlights the specific areas in which rebel leaders went beyond Mao—their more radical class analyses and more consistent opposition to the authority of the party-state officialdom. While Mao and his rebel followers in Hunan shared a deep-seated hostility to bureaucratic authority and a fondness for the disruptive power of “big democracy,” Mao's hostility was tempered by the fact that he was in charge of the government and ultimately responsible for maintaining public order and making sure the population was fed. The Hunan rebels, especially the ones on whom Wu trains his sights, wanted to overthrow the party leadership and pursue the revolution at any cost.

Wu has put together a book that could become a classic—perhaps *the* classic—portrait of the rebel movement during the Cultural Revolution. But the book is more than a portrait, as Wu's ultimate purpose is to explore the historical significance of the events and debates he examines. He uses these events and debates as a prism through which to reconsider the 20th century socialist project, developing an assessment that is at once sympathetic and ruthlessly critical. The Cultural Revolution makes an excellent prism for this purpose because the episode was produced by Mao's own dissatisfaction with the system he had created, and the issues at the center of Wu's book involve key limits and failings of the socialist experiment. Wu's analysis of the complicated and ultimately

tragic interaction between Mao and his rebel followers powerfully illuminates these limits and failings, and this is the major contribution of the book.

In this book Wu tells the story of this troubled relationship from below, from the perspective of the rebels. I understand that in his next project he intends to reexamine these same events from the top, from the perspective of Mao, who was at once the rebel chief and the number one power holder. For me, it will be an eagerly awaited sequel.

Endotes

1. An earlier version of these comments was published in *The China Journal* No. 73 (January 2015), pp. 279-282.

A Transformational Sociology of Socialism and China's Cultural Revolution: Reflections on Yiching Wu's *The Cultural Revolution at the Margins*

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Critique does not require historical depth. Indeed there are many in China and abroad whose challenge of that nation's neoliberal turn rests on an implicit embrace of a caricature of Mao and the socialist alternative he embodied. To learn from Yiching Wu would teach everyone committed to intellectuality how dangerous such an embrace is.¹

However, to treat *The Cultural Revolution at the Margins* as critical reading only for an audience engaging China would miss the full power of this work. I'm glad SSHA already recognized that point last year and that we might build on that recognition today.

I find this volume to be deserving of the widest audience for four basic reasons:

- it exemplifies what I recognize as a Sewellian Transformational Sociology;
- it offers a critical reinterpretation of a vitally important period of Chinese history;
- it might be more substantially implicated in a larger account of socialism's transformations; which in turn
- invites us to rethink how Marxism's legacy after communist rule might be implicated in rethinking the world crisis in which we now live.

I might also say parenthetically how much I enjoyed Yiching's writing style, but let me begin my substantive comments with transformational sociology.

Transformational Sociology

After finishing Yiching Wu's book, I wrote to ask him whether he had much contact with Bill Sewell at the University of Chicago, for I could see his volume as an exercise in Sewellian Transformational Sociology. He did not, but both Yiching and Bill were part of an atmosphere at Chicago. But not only at Chicago.

Sewell's historical cultural and critical approach to social transformations resonates with many who were part of the local, and broader, network associated with the University of Michigan's Program for the Comparative Study of Social Transformations in the 1980s and 1990s. In that light, I developed what I call the Sewellian list of transformational sociology. I lay that out in my forthcoming book. This list is a good way for me to think about how change happens, but it's also good to keep in mind when reading Yiching's book. For example, consider how he portrays the January Revolution:

The early weeks of January constituted a critical juncture in which the interplay of local and national politics produced highly significant political developments, through which fragmented experiences were sifted and lifted out of their local contexts, appropriated, and strategically transformed. (p. 139).

I see Sewell's eventful sociology at work: schema transposed, the polysemy of resources exposed, the unpredictability of resource accumulation distributed, and the intersections of structures made manifest all around events through which China was transformed.

It's useful to think through this Sewellian lens, I think, for us to consider not only how to engage historical transformations better, but how to build knowledge networks that seek an alternative theory and practice in global transformations, one that is unburdened by unnecessary ideological conventions but informed by cultural political savvy. I'll return to that in the end, but for now let me refer to Yiching's work on China's cultural revolution in particular.²

Rethinking China's Cultural Revolution

I am probably the last person on this panel who might identify the critical ways in which Yiching helps us rethink China's cultural revolution,³ and to save time, I'll just say that I appreciate his Foucauldian emphasis on subjugated knowledges and his refusal to turn any particular actor into a reflection of structures distant or ideologies at work, whether Marxist or liberal. His injunction to understand the complexities of past heroism invites us too to understand this period as critically as we might the neoliberal moment. There are too many in China, I infer from the book's last two sentences, who fail to recognize the contradictions and injustices of that

socialist past: "Our critique of capitalist development in contemporary China calls for a more robust and historically grounded criticism of actually existing socialism – an unrelenting self critique, so to speak. This is the most important lesson to be learned from the now century-old history of China's revolution and socialism in general and from the experience of the Cultural Revolution in particular" (p. 238).

A Transformational Sociology of Socialism

Yiching gestures in the end of his book to a number of authors who find similar patterns of post-socialist transformation in East Europe as in China. But one might do something similar by considering the socialist period too. I propose two foci: A) around communists' modes of understanding of their own systems; and B) around the dynamics of class formation under communist rule.

A. On modes of understanding: First, I wonder how much Andreas Glaeser's (2011) political epistemics of socialism, developed most elaborately with reference to East German Stasi officers, would work for some of the hegemonic practices of Chinese communist rule. I once wrote this about Glaeser's work in defining socialism:

Glaeser characterizes this system as an "ultimately enslaving politics of liberation" (53). Socialism was to be a kind of self-fulfilling prophecy, a consciousness-driven model of social transformation but without the processes that would allow it to validate its understandings against how the world really worked. Focused more on mobilization against an enemy than understanding itself and its society, the Communist Party and its state were both constituted through mechanisms they also made. The

way in which they were made also prevented authorities from recognizing the real problems they faced (Kennedy 2012).

Here, in particular, I am struck by how much socialism across the world focuses its energies on the power of enemies in order to legitimate the essential functional integrity of its own system. I'd love to see us develop that further. In particular, I'd be curious: could the relative incapacity of Glaeser's Stasi officers (who claimed to know there was something wrong, and that something needed to be done, but their modes of understanding prevented them from knowing what could be done) be contrasted with various incapacities of Chinese communists in different eras.

Too simply put, could we contrast modes of communist understanding of their own systems, daring even to contrast how well they understood them?

I could put it this way: Poland's and China's communists in the 1980s understood their own systems better than East Germany's communists and China's communists during the Cultural Revolution.⁴ I can elaborate, but let me leave it at that just to be provocative.

B. On class: Yiching does great work around class analysis in China, but here we have an opportunity to return to a comparative class analysis with regard to socialism.

Konrad and Szelenyi (1979) and many other East European sociologists did a great deal of work on class analysis under socialism. Those works suggest some promising invitations to rethinking how class worked. Of course my own work (1991) from that period focused mostly on the Polish Solidarity movement, one that I considered in terms both of class and civil society. Without much more bibliographic citation, let me suggest a few angles.

Was the "freedom to organize" (p. 194) during the Cultural Revolution, something Yiching said was unparalleled in Chinese communist history, comparable to any other period in another communist ruled society? One might think the 15 months of self-organization associated with Poland's 1980-81 Solidarity movement comparable. While the Cultural Revolution depended on Mao, Solidarity was not at all so linked. Quite to the contrary, it depended on Poland having moved beyond a vision of change based on revisionism toward one based on civil society.

To what extent is the transformative logic that we East Europeanists have regularly rehearsed -- of movements resisting within the logic of revision evolving toward those embracing the sense of civil society -- replicated in the Chinese case? To what extent is the trajectory we see in the movement from cultural revolution to democracy wall (p. 215) analogous to what we saw from 1956 to 1980-81 in Poland?

In the penumbra of Solidarity, I argued in that 1991 book that social transformation under communist rule depended upon the proliferation of autonomous power resources whose interactions are themselves variable and historically contingent, leading to social and systemic alternatives that are not themselves defined by underlying modes of production.

We could discuss the relevance of that general observation for today, but I would rather focus here on the contrasts between Solidarity and the Cultural Revolution. Here are just a few tidbits:

- The Cultural Revolution was caught within a Maoist ideological frame, and thereby limited in its transformative power. Discourse matters. Compare, for example, how the introduction of dignity to broad popular discourse in 1979 by Pope John Paul II was, for some, a constitutive element of Solidarity's formation and

transformational power (Kennedy 1991, 43–44).

- While Yiching laments how class was “hollowed out” and made hard to read during the Cultural Revolution, during the Solidarity period of 1980-81 it was made vividly clear precisely in the sense that Yiching hopes for, I think. Yiching writes that he wishes to see

Although Yiching writes with a Gramscian accent, one that is especially laced with good doses of Laclau and Mouffe, and a hint of Raymond Williams and Karl Polanyi, I find a more conventional Marxism returning from time to time than I think I would myself promote in these times.

class as “the various ways in which marginalization, disempowerment, and domination are created and maintained and to the discursive configurations that give meanings to fragmented social conditions” (p.224). One can see Poland in that period clearly in class terms, and even more, in the complex interweaving of discursive and material conditions leading not only to class antagonisms between those who dominate and those who resist, but in terms of how class is negotiated in relational terms among the dominated and transforming. So, positively put, could one see in Solidarnosc the class analysis and practice that Yiching seeks? My student Mujun Zhou is working on a variation of this problem in her dissertation entitled *Civil Society, Public Sphere, and the Potential for Solidarity: Toward a Critical Sociology of Post-socialist Chinese Society*. Her work promises to resolve some of the quandaries this comparison invites. I will let her work speak for itself, but let me invoke a different one than Mujun raises.

- As one evokes that particular comparison between Poland and China, one therefore invites the Marxists’ nightmare question: do all roads of struggle within socialism lead to capitalism’s restoration, as it has openly in Poland and as it has in masked Chinese practice? Here is where I think Yiching and I part ways, for I would like to propose that by remaining caught in the socialism vs. capitalism problematic, we remain caught in an opposition that is the last century’s problem. We have others, and Yiching’s book illustrates some of them.

Transformational Sociology after Marxism and Socialism

Although Yiching writes with a Gramscian accent, one that is especially laced with good doses of Laclau and Mouffe, and a hint of Raymond Williams and Karl Polanyi, I find a more conventional Marxism returning from time to time than I think I would myself promote in these times.

Of course his Marxist tilt may be appropriate given that there are still very many people in China who find inspiration in Marxism and Maoism both. But rather than look for the Marxist critique of state socialism in the struggles Yiching identified in the Cultural Revolution, I would rather look to consider how those struggles actually resemble those we see today across Europe and other parts of the world today

In the final substantive chapter of my forthcoming book, I propose that critical theory is stuck because our systemic accounts are profoundly lacking in their accounts of subjectivity, and those that celebrate subjectivity are lacking in their approach to understanding the system that rules. And there’s a reason for the disjuncture and it does not rest in the lack of a proper revolutionary consciousness. I believe it rests in our failure to read properly the immanent critique and

transformational practice of actually existing protests. That is another paper, but one can read Yiching's account of the Cultural Revolution in those terms.

Indeed, I can see what Yiching does as resembling what Graeber (2011) has proposed we develop: a theory and practice of revolution in reverse. And by doing that, we might try to imagine alternatives to what exist by thinking of ways to link horizontally those movements and moments that promise a world that could otherwise be.

We might do just that by elevating those themes Yiching found in Shanghai around self-worth, dignity, and autonomy. I found the same in Solidarity in 1980-81, and we found them on the Maidan in 2013. We found them in Zuccotti Park. We see them in the 15M movement. We see them in the Arab Uprisings. We see them in Hong Kong. What happens when these expressions are taken seriously as the base with which to think about alternative classes in formation? These are not Marxist classes by any means, and they are not liberal either when they challenge the property and arrogance upheld by the power of coercive states. This IS an invitation to rethink the character of opposition in the 21st century. And after reading Yiching's book, I now wonder whether we might rethink these contemporary revolutions better by rethinking the Cultural Revolution from 50 years ago.

I don't know with what to supplant that socialist referent in Marxist accounts. But like those who Occupy, the point may not be now to name the alternative system we seek, or even the policies that will redress. The point may be to name the injustices clearly so that we can hold accountable those who claim to be responsible. Indeed, we might just be able to find "new chains of symbolic equivalence... among diverse struggles" (p. 170) if we might give up on defining our alternative in advance

and rather embracing the struggle for justice in the now. And with Yiching's book in hand, we might wage that struggle with a more just and inclusive understanding of the histories that got us here.

Endnotes

1. I appreciate all I have learned from Mujun Zhou and Yibing Shen before I prepared these remarks, as well as their comments on an earlier draft.

2. I originally named this the "Sewellian list" in Kennedy (2013). The following list is from Kennedy (2015, 22):

- *The multiplicity of structures*: Any social unit is going to be composed of a variety of structures that are unlikely to be entirely homologous or in synchrony with one another. This variety of structures can lead to conflicting claims and social conflicts.

- *Unpredictability of resource accumulation*: Enactments of schemas can produce quite unforeseen outcomes, and those outcomes, if sufficiently altering the power relations in a given social unit, lead to a transformation of structure.

- *The intersection of structures*: Structures with different schemas and different resources overlap and interact in any given setting, making their smooth reproduction always potentially problematic given the contradictions that could emerge from their contact.

- *The polysemy of resources*: The multiplicity of meaning potentially attached to any set of resources means that these resources can be interpreted in different ways, with various consequences for social transformation. Those with greater authority in interpretation, with greater knowledge, have disproportionate power in this transformation.

- *The transposability of schemas*: Actors are capable of taking schemas or rules learned in one context and apply them to another. While this capacity is also universally distributed, those with a wider knowledge of different contexts, and different rules across those contexts, should have disproportionate influence in shaping change, *ceteris paribus*

3. Joel Andreas, Xiaohong Xu and Marc Blecher offered remarkably insightful comments on his work.

4. Poland's communists understood it exceptionally well in the 1980s, and figured a path to end the system and enable their own survival in a postcommunist capitalism. Chinese communists embracing neoliberal reform understood their own

system relatively well, figuring a way to sustain their own position with even greater legitimacy than Jaruzelski et al managed. By contrast, East German authorities understood their own system's dynamics so poorly that their incompetence led to the system's collapse. Likewise, Chinese communists during the cultural revolution could not understand the system's inability to digest or embrace the cultural revolution, thus necessitating its own

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Notes on The Cultural Revolution at the Margins

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This is a wonderful book — grounded in meticulous research, full of thoughtful analysis and intriguing narrative, and of great importance in the much-needed and increasingly lively and illuminating reevaluation of China's Maoist period. At its core is an intellectual and political history of three counter-currents of the Cultural

Revolution that ultimately fell afoul of both the left and the right at the centers of power — namely, proletarian "economism" and two instances of radical left analysis: the Shengwulian and Li Yizhe. It argues that the core problem of class analysis in the Maoist period, which remained shot through with theoretical and analytical contradictions, unleashed a torrent of disputation that linked together specific material, personal and political grievances that would not necessarily otherwise have found common ground or expression, producing a revolutionary crisis that nearly destroyed the People's Republic of China. Yiching Wu also argues that none of these counter-currents delved systematically or deeply enough into the underlying structural roots of the People's Republic as a system of economic, social (class), political or ideological power, though they might have had they not been defeated and suppressed by the leadership — both "left" and "right" — in the maelstrom of the Cultural Revolution.

One can only welcome a study that takes the radical ideas at the "margins" of the Cultural Revolution so seriously. My great teacher Tsou Tang, who did so much to shape Western China studies from his base at the University of Chicago, where this book had its gestation, always stressed that we needed to treat many Chinese leaders as serious political theorists and not just politicians — much less ones with whom the US was waging a cold and sometimes even hot war.

One argument of the book, which is surely right, is that the vexatious theoretical problem of class in the People's Republic, which, as it so often does in so many times and places, had lain smoldering and only occasionally flared up, burst into a conflagration when poked at by Mao Zedong. I detect a vaguely Hegelian aroma here. A certain weltgeist — an alienated consciousness around the thorny problem of class under state socialism — had emerged in

China at the dawn of its post-revolutionary historical epoch. It produced its own dialectical oppositions that struggled against it in ways inescapably imprinted and ultimately defeated by it.

As such, in giving the Cultural Revolution some actual content, *The Cultural Revolution at the Margins* is a welcome complement to Andrew Walder's *Fractured Rebellion* (2009), which sees the Cultural Revolution primarily as a sequence of highly contingent political battles over nothing but survival that, in the context of China's Leninist, Maoist one-party state, got increasingly out of hand. Yet *Fractured Rebellion* and *The Cultural Revolution at the Margins*, despite coming at the Cultural Revolution from such opposite analytical perspectives, share a great deal. First is their emphasis on political contingency, which is surely right. Especially at a moment of crisis such as the Cultural Revolution, when existing institutions, leaders and hegemonic ways of thinking are openly under attack, politics is even more prone than usual to move along unpredictable, unanticipated and particularistic pathways. Second, and

[Wu's] move away from social science analysis toward what is essentially narrative history raises problems, both analytically and politically.

proceeding directly from this insight, is their wariness about imposing any analytical framework that would bring an unwarranted sense of an underlying pattern. In Nate Silver's terms, they eschew the signal for the noise (Silver 2012).

In particular, both books seem to want to tilt against social scientific explanations of the Cultural Revolution as grounded systematically in participants' social positions

in Chinese state socialism. Walder is explicit about this: neither the factional alignments nor the battles the factions fought out can be explained by the social backgrounds of the red guards.¹ Deploying the same metaphor of "fracture" in Walder's title, Wu also writes:

In Hunan...rebel militancy that resulted from the fracturing of mass politics may not be directly explained by the social divisions established in Chinese society before 1966, as some scholars have previously argued, according to whom the activists' political orientations and actions were shaped by their positions in the pre-Cultural Revolution status quo. Rather, the emergent positions, identities and politics of the recalcitrant rebels were the products of contingent, open-ended political processes that brought a variety of aspirations and demands into play." (188)

To be sure, his analyses of battles over working class "economism" in Shanghai are necessarily grounded in material class structures. Yet his discussions of the politics around the Shengwulian's critique of bloodline theory and around Li Yizhe don't focus on the social bases of their supporters so much as the individual protagonists (including their social backgrounds, of course) and, much more heavily, on the events swirling around them.

This move away from social science analysis toward what is essentially narrative history raises problems, both analytically and politically. Analytically, there are at least two. First, what drove the Cultural Revolution? The flight from the social base weakens our ability to grasp what I still believe is a robust argument about the signals that were operating beneath all the noise. By contrast,

Joel Andreas (2009) was able to demonstrate a great deal about the Cultural Revolution and its legacy for the structural reforms by analyzing the social — and in particular, occupational — backgrounds of the red guards. And in what I believe is still the only effort at systematic quantitative (as well as qualitative) analysis of the social bases of factional alignments during the Cultural Revolution, Gordon White and I (1979, 79) found that “class origin seems to have had the greatest independent effect on factional affiliation of all the variables tested,” which included age, sex, education, organizational and political status, and questionable political background.

Gordon White and I also found that a significant amount of the Cultural Revolution political discourse in our case masked the actual motivations of the participants. After the attack on economism, they were unable to express their anger at certain work unit leaders over material mistreatment. And they certainly could never confess to their simple desire for revenge. So they dressed up their materially- and personally-grounded attacks with the ideological tropes of the day: revisionism, the capitalist road, disloyalty to socialism and Chairman Mao, class struggle, and the like. Of course social scientists have long been aware of the complex relationship between political expression and the deeper, unspoken, often unconscious drives behind it. It's a dialectic, methodologically and even literally — one could think of the words spoken as a kind of dialect for expressing, imperfectly and indirectly, underlying meaning. And it's a dialectic, a tension, that we need to keep alive to understand the world — in this case, the world of the Cultural Revolution.

Of course one should never argue, as Walder and even Gordon White and I may seem to, that the movement's radical concepts and ideas don't matter — that they're just obligatory cant

we have to unmask to see what's really going on. Wu has made that point, to great effect, by taking so seriously the inherent problems of the continuing emphasis on class in Maoist state socialism. Equally, though, I'm concerned about studying those ideas detached from their material roots, for all the obvious reasons that go back to Marx. For example, is it possible that one reason the Shengwulian and Li Yizhe failed — or at least failed to attract more political support — is that they lacked a powerful enough social base (which the other rebels but especially the conservatives most definitely had)?

This leads directly to the second analytical problem: both Walder and Wu believe that the Cultural Revolution fractured into disarray and failure because of the state. In their explanation, society has almost completely disappeared. For Walder, the state's hierarchical, monopolistic, totalistic and ideologically charged character led to the fracturing of protest once Mao took the lid off. Factions divided for primarily contingent reasons that remain unclear but seem to have had little to do with what students actually believed; but once they did, they stayed divided and fought increasingly ruthlessly simply because, given the high stakes set by the political system, they couldn't afford to lose. For Wu, who is, happily, much more attentive to the participants' theoretical commitments, the state ruthlessly cut short and suppressed those intriguing, important and perhaps even indispensable initiatives from below.

Both arguments about the effects of the Chinese state have some purchase on what was really going on, of course. The questions, though, are: to what extent are the structures (in Walder's case) or political and ideological features (in Wu's) of the state the main cause of the Cultural Revolution's failure? And more importantly, what is their relationship to other

structural factors, such as social and economic organization and ideological hegemony? Only by keeping open significant roles for these other analytical heavy objects can we hope to grasp the Cultural Revolution in its fullness.

Moreover, this second analytical point leads to a crucial political one. If we accept that the main problem was the Chinese state, then we are putting ourselves squarely on the road to political liberalism. Walder surely believes this, approvingly. And Wu, rather less enthusiastically, nonetheless actually shows us how the radical initiatives he has illuminated so well led directly there. But liberalism isn't the only possible political future for China, and it may not be the best one. As Premier Zhou Enlai once said, the verdict on the French Revolution is still not in. And the same goes for the Chinese.

Telescoping back out, these books raise a core issue for social science history: What is the current state of the relationship between narrative history and social science? Why are two important recent books about the Cultural Revolution moving away from social scientific analysis? Is this part of a wider trend?² If so, why is it happening? Can social science help explain the flight from it? Is it perhaps a dialectical reaction to, or even — judging from Michael Kennedy's and Xu Xiaohong's comments — a reflection of postmodernism, which is hypertheoretical in its attack on social science theory, and which, of course, has its own material roots that it does not itself recognize?

Endnotes

1. Yet his book does not present a systematic analysis of the Beijing red guards' social backgrounds (which still might be possible with survey data), relying instead on close historical reconstruction of the activities of only the key players.

2. I recently came upon an article on this topic by Gerring (2012), which was urged on me by a most

eminent and theoretically sophisticated colleague.

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State, Ideological Transformation, and the Chinese Cultural Revolution: Comments on Yiching Wu's *The Cultural Revolution at the Margins*

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Revolution is a foundational topic for historical sociology. Yet, the case of the Chinese Cultural Revolution proves notoriously hard to categorize and explain according to those schemes familiar to historical sociologists. It is more than a political revolution in that it involves bottom-up mass mobilization far beyond a fringe of urban elites. It is not so much a social revolution, although it was meant to be, in that no ruling class or group was eliminated. In fact, it is even hard to speak of an "old regime" it is opposed to as scholars usually do with revolutions. This peculiarity is in part derived from the difficulty of separating it out as a historical process: is it a continuation of the Chinese Revolution ("continuous revolution"), its climax or its last breath? It is also a failed revolution, whose

revolutionary cause its successor regime has "completely repudiated." Any intellectual effort to grasp the complexity of its dynamics would face the daunting task of addressing its origins in a regime that already claimed to be revolutionary and the mechanism through which it provoked the reaction known as the Reform. Yiching Wu's book provides a provocative account reconstructing three crucial episodes in the Cultural Revolution (CR) that also sheds new light on its origins and especially its failure and consequences.

Central to Wu's interpretation of the CR is the state. Here, the state is not so much the cause of CR activism as it is the object of claims-making and critique by the activists. In a chapter setting up the stage, Wu delineates state practices of class labeling in pre-CR period, which was first put in place when the Communist Party came to power to facilitate its project of class leveling and redistributive justice. Yet, this supposedly temporary scheme of state legibility was over time ossified into a hereditary status hierarchy akin to a caste system, as the socialist state sought to secure the loyalty of the "good classes" and felt threatened by the political and ideological contamination of former elites, which Wu aptly captures as "enemies from the past." Hence, individuals born into the "bad classes" were increasingly disadvantaged in seeking education, employment, and political ambition in comparison to those of the "good classes," especially the Communist cadres.

The effect and transformation of this political semantics of class constitutes Wu's first empirical chapter. It charts the appropriation and conversion of state practices of class labeling by children of the communist cadres into a "bloodline theory" in order to monopolize the rights of rebellion. This provoked students from another camp to mount a scathing critique of the theory and to challenge the privileged Communist

officialdom. The Maoist leadership went along with and in fact took advantage of the challengers in order to unsettle the existing Party bureaucracy but fell short of supporting the latter's thorough critique of the state practices. In fact, it eventually cracked down the chief ideologues in the movement.

In the second episode, Wu examines the workers' revolt in Shanghai, the majority of whom were under precarious contract and without welfare benefits. He takes pains to unpack the contingency and complexity of this event, where political activism and demand for redistributive justice crosscut and interacted with the tactical maneuvers of the incumbent Party bureaucrats and Maoist challengers / political entrepreneurs. While the revolt succeeded and was established as a model of Maoist "power seizure" throughout the country, its demand for redistributive justice was largely dismissed as backward "economism."

In his last empirical chapter, Wu documents the rebels in Hunan, who advanced, in the name of Maoism, an all-out critique of the Communist officialdom and called for a bottom-up mobilization against all privileges conferred by the state. Repudiated by Mao and his associates as anarchists, the movement was eventually crushed.

What connects all three episodes is their shared transgressive tendency that the Cultural Revolution unleashed, which went far beyond the Maoist leadership's project of restructuring the state and societal organization. They were, as Wu calls them, the "unruly margins" of the Cultural Revolution, because they elaborated ruthless critiques of the state and called for socialist democracy and redistributive justice. They were much more than marginal, however, in that they inspired many rebels around the country and provoked the state's reaction to demobilize and routinize

the Cultural Revolution, especially after 1968. In his powerfully written concluding chapter, Wu argues that the origins of the Reform era need to be traced back, not to 1976, when Mao died, or to 1978, when Deng Xiaoping ascended to power, but to the state's agenda since 1968 to contain Cultural Revolution radicalism that these unruly margins have epitomized through technocratic modernization in the early 1970s and then marketization since 1978.

While Wu's overall theorization—more of background theorization than foreground theorization than is the norm in sociology—is grounded in Gramscian Marxism and poststructuralism, his interpretation of the heroic struggle of the CR activists and their tragic failure invites a seemingly unlikely comparison: Alexis de Tocqueville (1983). Although writing on two revolutions of a different nature and centuries apart, they both harbor deep sympathy for the revolutionaries and search for the cause of the failure of their revolutionary vision in the state, whose bureaucratic logic shapes first the form of grievance, then the form of revolutionary aspiration and eventually the suppression of that aspiration by rationalizing itself and its social control. The difference, of course, is that Wu focuses not so much on the changing dynamics of the state institutions, which Tocqueville had examined, as on the unfolding of the revolutionary events – a project that Tocqueville had promised but did not wait to finish. In historical sociology, Tocqueville's insights have been passed down primarily through two lines of thinking. The first is Theda Skocpol (1979)'s "bringing the state back" in the structural perspective of social revolutions (her incorporation of Tocqueville is almost undistinguishable from her Weberian analysis of the state as "means of administration"). The second is William Sewell's transformational sociology. Wu's analysis is much closer to Sewell's for his

emphasis on ideology, particularly the activists' ideas and critiques. This sets his work quite apart from the existing scholarship on the Cultural Revolution.

The existing literature on the Cultural Revolution is dominated by two paradigms: sociological determinism (factional formation in CR activism reflects and exacerbates the preexisting tension between activists from different class and social backgrounds, who were competing for rewards and avoidance of sanctions) and political contingency (factional formation is the effect of path dependency resulting from contingent choices made by actors of similar social profiles in response to the ambiguous and confusing signaling from the incumbent Party bureaucracy and the Maoist Central Cultural Revolution Group). Both paradigms treat ideology as epiphenomenal.

By contrast, Wu puts ideas and ideology at the forefront of his analysis. However, as Joel Andreas (2007) (and Marshall Sahlins [1976], who was Wu's mentor) point out, ideology and interests are not mutually exclusive. Rather, what begs for analysis is the changing structure of "schemas and resources" (Sewell 1992) that shapes the way in which interests are articulated and pursued. In the case of the Cultural Revolution, while factional formation surely has a lot to do with the preexisting tension between students from different social backgrounds, the paradigm of sociological determinism has little to say about the process through which this tension and interest-seeking was transformed from institutionalized mechanism of rewards and sanctions in the pre-CR period to participation in charismatic mobilization. Walder (2002, 2006) has rightly challenged the reductionism in this paradigm and pointed out that contingency played an indispensable role in the early stage of this transformative process, when participation in charismatic mobilization

was limited to students of the “good classes.” Transformational sociology would incorporate the contingency of events as well as the

For Wu, the ideological rupture only came when the Cultural Revolution took its course and was only possible because Mao unleashed it from above. This interpretation would deny the agency of Red Guard activists in creating that rupture together with Mao and miss the valorization of charismatic mobilization among students that already preceded the CR and eventually enabled their CR mobilization.

ideological transformation that the historical contingency is embedded in and helps to articulate (Sewell 1996). It is this aspect I find inadequate in Wu’s otherwise powerful analysis.

For Wu, the ideological rupture only came when the Cultural Revolution took its course and was only possible because Mao unleashed it from above. This interpretation would deny the agency of Red Guard activists in creating that rupture together with Mao and miss the valorization of charismatic mobilization among students that already preceded the CR and eventually enabled their CR mobilization. In the remainder of this essay, I propose two approaches that would refine our understanding of the process of ideological transformation that is also sensitive to group interests.

First, we need to trace this transformational process to the ideological contradictions incurred by state practices before the CR (Sewell 1985). This I have found confirmation in a project that I am doing. The pre-CR state,

more than a decade after its revolutionary founding, was on its way toward routinization but was beset between its institutional tendency to routinize and its need to shore up legitimacy through ideological justification. This was expressed in the educational practices to which the students were subjected. While the state used revolutionary historical education to justify its routinizing administration of the society (which included the students), the students, through their active appropriation of educational practices, were torn between their longing for revolutionary glamour that revolutionary historical education has conveyed and their daily necessity to compete in a routinized social order for educational opportunity and career advancement. Mao’s call for the Cultural Revolution exploded this tension, igniting a contingent process that left the students to decide how much their participation in this “unprecedented revolution” should follow the institutionalized order and how much to follow the scripts and repertoires that their revolutionary historical education has bequest. This fusion and confusion of past repertoires and present actions, which Marx has vividly accounted in *The 18th Brumaire of Louis Bonaparte* (1963), was blatantly clear in the Cultural Revolution. Surely, the “unruly margins” that Wu has documented are part and parcel of an emergent process of ideological transformation. The emergentism of this process, however, did not begin with Mao’s call for mobilization but much earlier in the ideological contradictions before the CR.

Second, this ideological transformation is partly effectuated by struggle and competition among students but not reducible to it. In this regard, I suggest that Bourdieusian analysis can be useful. From this perspective, what the CR has really opened up is revolutionary activism as a field of “generative struggle” (Bourdieu 1990). It was a field that is centered

on the struggle for revolutionary credential and is autonomous from the state's direct supervision. However, its emergence can be traced to students' competition, in the pre-CR period, for political credit, which was one of the three criteria for pursuing any educational opportunity and career advancement and also the most contentious one, given that the other two criteria—class background and academic performance—were much more stable and certain. The pre-CR competition for political credit was limited to institutionally sanctioned mechanisms such as following Party instructions and doing petty good deeds (the everyday hero). This is why children of the solid political backgrounds were the first to challenge this routinized reward system, which put them on the same footing with their competitors from the "bad classes," and claim that true political credit should go to those engaged in "great causes" of revolution. Because of their sense of natural entitlement to inherit the revolutionary tradition, they almost monopolized the participation in CR activism.

Yet, once the contingent process of juggling between past repertoires and present circumstances began and gave rise to opposing factions among students of equally good class backgrounds, an autonomous field of activism emerged and kept incorporating other students who were shut out in the beginning based on their less "red" class backgrounds. Then, the question of class and the state that Wu has dealt with so well came to the fore. While the Maoist leadership tried constantly to interfere and take advantage of different organizations by conferring symbolic recognition or sanction, the direct institutional control was no longer in place. By nature, a field of revolutionary activism is probably the most foreign to institutionalized mechanisms of containment, control, and influence that the state can muster. When competition for political credit, which was institutionally conferred, was transformed to struggle for revolutionary credential in an

autonomous field, the state found no other recourse but its coercive machineries. Hence, the military takeover, demobilization, and routinization of the CR from 1968 onward. In comparison, competition in a technocratic social order, where the state can regulate and certify, and a marketized social order, where the state is not subject to claims making, became the safe way out. Therefore, the Bourdieusian perspective may be able to help the analyst to integrate class dynamics and contingency, ideology and interests in explaining the causes and dynamics of the CR as well as the reason for its suppression and transformation into a new social order.

These comments and suggestions aside, I have found reading Wu's book a very inspiring and stimulating experience. It is both a work of penetrating thought and a work of art. In no way can this essay convey the theoretical breadth, narrative subtlety, and literary virtuosity of the book. I certainly agree with the decision that SSHA has made in 2013 to give the President's Book Award to it. The Chinese Cultural Revolution is an extremely complex historical event fraught with paradoxes. It has presented enormous analytical, political, and moral challenges that the scholarly community needs to reckon with. Events like this that do not fit easily with any existing category and framework are precisely where historical sociology can make great advance. By unraveling the critiques inherent to the experience of Chinese socialism that were developed in dialectical relationship with the CR, Yiching Wu's book has greatly enriched this recently revived scholarly literature. It will inspire many historical sociologists as it did with me. In this essay, I have also pointed out some connecting nodes where the CR scholarship and historical sociology may cross-fertilize and extend one another. I would be very pleased if it raises interest in this book and opens up further conversations.

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Response to Critics

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The Cultural Revolution at the Margins: Chinese Socialism in Crisis provides a new interpretation of the tumultuous conflicts of China's Great Proletarian Cultural Revolution, arguably the most profound crisis that the People's Republic of China had undergone. The spectacle of widespread attacks on the

communist party-state authorities, instigated by the head of the same apparatus, was extraordinary. In a letter to his wife Jiang Qing dated July 8 1966, Mao Zedong made the remark that "I feel that I possess the spirit of both the tiger and the monkey, but in the end I am probably more of the tiger than the monkey." In traditional Chinese cultural symbolism the monkey was the trickster that transgressed boundaries and defied authorities, while the tiger (and the dragon as well) represented reverence and royal power. Disclosing the Chairman's complex state of mind as he was entering his last great battle, this intriguing statement marked Mao's contradictory roles, as both the chief of China's Leninist party-state and the rebel leader, and foreshadowed the zigzagging course of the Cultural Revolution, wherein popular eruption, political containment, recentralization, or even suppression, were closely intertwined.

In *The Cultural Revolution at the Margins* I tell the story of the disobedient little monkeys, unleashed and then suppressed or herded back into the cage by none other than Mao himself. Indeed, while the numerous rebel groups that mushroomed in the Cultural Revolution looked to the Maoist leadership for political guidance, the relationships between Mao and those who earnestly responded to his call were complicated and highly fragile. With the temporary breakdown of the party hierarchy, political messages from above were interpreted differently by different agents. In responding to centrally directed policies, rebel activists responded to their own immediate circumstances. The political forces unleashed by Mao often took on lives of their own, with some young activists eventually questioning the institutional foundation of the Leninist party-state.

This book is a history of the Cultural Revolution written from the perspective of its

unruly “margins.” Exploring what may be considered a “de-centered” view of the Cultural Revolution, the book attempts to grant agency and historical visibility to those discontented, disadvantaged, excluded, or recalcitrant, who were otherwise consigned to the peripheries of the movement. With a focus on the rise and fall of transgressive currents, this book argues that the more radical political possibilities of the Cultural Revolution were pressed by young critics and activists at the grassroots who questioned the movement’s attacking individual cadres and putatively “bourgeois” ideas and life-styles. Their radically anti-bureaucratic and popular-democratic impulses were accompanied by an acute concern with the organization of political power in the socialist state. Suppressing the young rebels as early as in late 1967, Maoism cannibalized its own children and quickly exhausted its political energy, and the mass demobilization in 1968-69 became the starting point of a series of crisis-coping political and ideological maneuvers which eventually led to the momentous changes in China a decade later.

This book is a history of the Cultural Revolution written from a bottom-up perspective, as much as an inquiry into its complex historical and political legacies. The analysis provided in the book makes possible a new understanding of the historical meaning and origins of Chinese postsocialism, in that it allows us to interpret China’s post-Mao “reform and opening up” as part of a continuous process of ideological and political maneuvers to contain, suppress, and neutralize the prevalent crises that resulted from the Cultural Revolution. In contrast to the conventional wisdom that views post-Mao China’s political and economic changes as in radical opposition to Mao’s utopian “last revolution”—and dates their starting point to the late 1970s, in this book I argue that the origins of these changes in fact can be traced to

the height of the Cultural Revolution in 1968-1969, when mass demobilization and restoration of party and state organizations were in full force.

The Cultural Revolution at the Margins participates in the ongoing debates on market reform, socialism, and revolution in China. The

This book is a history of the Cultural Revolution written from a bottom-up perspective, as much as an inquiry into its complex historical and political legacies. The analysis provided in the book makes possible a new understanding of the historical meaning and origins of Chinese postsocialism, in that it allows us to interpret China’s post-Mao “reform and opening up” as part of a continuous process of ideological and political maneuvers to contain, suppress, and neutralize the prevalent crises that resulted from the Cultural Revolution.

book attempts to remedy an intellectual and political lacuna, i.e., the virtual absence among many contemporary critics of “Chinese neoliberalism” or “capitalism with Chinese characteristics” (and in particular among many Chinese “New Leftist” intellectuals)¹ of a historically grounded and analytically rigorous understanding of the experience and legacy of Chinese socialism in the Mao era. In writing this book I was particularly concerned with what appeared to be the unfortunate correlate of this lacuna, that is, the underdevelopment of a vigorous critique of China’s revolutionary and socialist past. The romantic or sanitized images of the past have often been invoked by contemporary progressive critics in their

endeavor to contest the dominant neoliberal ideological formation. At issue here is not merely our scholarly curiosity about the Maoist past; rather, how we understand capitalist transformations in contemporary China (or “restoration of class power” as David Harvey (2005) has famously called it) depends crucially on the ways in which the ambiguities and contradictions of China’s socialist past are understood.

This book hopes to show that an alternative historical view is available. A fresh perspective focusing on the unruly “margins” of the socialist past (and of the Cultural Revolution in particular) is essential to the endeavor of tracing and excavating the wide range of illegitimate or subjugated ideas and practices constitutive of a long-neglected tradition of political criticism and oppositional imagination. This incipient tradition of popular dissent, as this book demonstrates, not only has the potential of producing a vigorous critique of the Leninist party-state that has dominated post-1949 Chinese society, but also is uniquely capable of inspiring an alternative standpoint of analysis and critique vis-à-vis China’s postsocialist present. As I argue towards the end of the book, our criticism of neoliberal-capitalist developments in contemporary China calls for a much more robust critique of actually existing socialism, a relentless immanent critique so to speak. It is the ultimate aim of this book to suggest that a coherent dual criticism—a critique of both capital and state, and of the logic of economic accumulation and bureaucratic power—is not only imperative but also possible.

The four critics at the SSHA session all raised important issues that urged me to think harder and more clearly about what I was trying to do, and how I could do better. Joel Andreas expresses concern that the title of the book and its framing idea—“the margins”—may be misleading, arguing that the episodes that the

book examines were not only centrally important to the development of the Cultural Revolution but also occurred in key urban centers. This is an important question that warrants clarification. Joel disputes my usage by pointing out the fact that such developments could involve tens of thousands of people and often even occurred in central locations. While I certainly acknowledge this fact, I would argue that this reading is based on an overly narrow understanding of the notion “margin” or “marginal,” which for Joel seems to mean trivial or having little political relevance. My usage of the term, whether in a rhetorical or conceptual sense, is closely related to cognate terms such as “from below,” “disadvantaged,” “illicit,” “subaltern,” and among others. Here “the marginal” pertains not only to the actors involved—those who were disadvantaged or marginalized in Chinese social and political life, but also to the issues and demands that galvanized contention—practices that went against the grain, and points of view outside the range of the politically permissible. Sometimes mobilized tens of thousands of people and occurred in major political centers as Joel has correctly pointed out, such developments at the margins entail a critical distance from—and consequently a unique vantage point of—the center of power. Margins, as anthropologist Victor Turner (1969, vii) argued, may be dangerous as they “have sufficient power and plausibility to replace eventually the force-backed political and jural models that control the center of a society’s ongoing life.” Such novel analyses and insights that emerged from the political and ideological margins of the Cultural Revolution, as I show in the book, offer a unique prism through which the social-class relationships constitutive of the state-socialist regime, as well as the historical origins of Chinese postsocialism, can be understood.

Both Michael Kennedy and Xiaohong Xu have kindly compared my book to the work of the

renowned historical sociologist William Sewell. That is an extraordinarily generous complement. Sewell's scholarship has centered on the development of a complex yet powerful theoretical vocabulary that simultaneously speaks to history and the social sciences, one that pays special attention to the dialectic relationship between process and structure and to the critical role of contingency and event in historical transformation (Sewell 2005). I deeply regret not having taken classes from Sewell while I studied at the University of Chicago. However, Sewell's critical-historical

Deliberately leaving the definition open, the idea of class that I deploy in the book owes far less to orthodox Marxist notions. Rather, class here refers both to the various ways in which marginalization, disempowerment, and domination are created and maintained, and to the discursive configurations that give meanings to fragmented social and economic experiences.

approach—through its dialogue with anthropologists such as Marshall Sahlins and Clifford Geertz—forms part of a general scholarly atmosphere at Chicago and has had an important impact on my intellectual development.

However, despite my book's visibly critical-Marxist, post-Marxist, and even poststructuralist accents (via Michel Foucault, Rosa Luxemburg, Antonio Gramsci, E. P. Thompson, William Sewell, Raymond Williams, Ernesto Laclau and Chantal Mouffe, among others), Michael Kennedy has also identified a certain "conventional Marxism

returning from time to time." By this I take Michael to mean by two things (and he is less explicit about the first than about the second). First, it refers to the book's focus on class as the main analytical category that grounds a new interpretation of the Chinese Cultural Revolution and its aftermaths and legacies. I argue in the book that of particular importance with respect to the historical understanding of the Cultural Revolution concerns the issue of class and class politics. Mao's "continuous revolution" was defined by those who initiated it as a class war against enemies of the revolution. But what did it really mean to talk about class? Over the past three decades, in correspondence with the global shift in political and ideological fashions, ideas such as class and class antagonisms have been viewed by many as obsolete. In contemporary China, stigmatized for being part of a historically aberrant episode in the nation's long march toward economic development and modernization, these ideas have been almost totally abandoned.

But such hollowing out of class occurs at the very moment when socioeconomic inequalities in China are drastically worsening. In this book I attempt to show the continued relevance of the concept of class—often a shadowy and illusive one—to understanding both China's recent past and its rapidly changing present. Deliberately leaving the definition open, the idea of class that I deploy in the book owes far less to orthodox Marxist notions. Rather, class here refers both to the various ways in which marginalization, disempowerment, and domination are created and maintained, and to the discursive configurations that give meanings to fragmented social and economic experiences. Rather than seeking a structural register of analysis that focuses on the "means of production," I examine social antagonisms and struggles not as derivative expressions of structural regularities, but as historically

mediated and discursively mobilized. This mobilization has an irreducibly political moment, as E. P. Thompson once argued, class in its heuristic usage should be treated both analytically and historically as inseparable from its “politics” or “struggle:” “Classes do not exist as separate entities, look around, find an enemy class, and then start to struggle. On the contrary, people ... experience exploitation (or the need to maintain power over those whom they exploit), they identify points of antagonistic interest, they commence to struggle around these issues and in the process of struggling they discover themselves as classes” (Thompson 1978, 149).

Second, Michael identifies a certain “socialist referent” in my book, which he hopes to “supplant.” Michael is unmistakably sympathetic to the values embodied in the “socialist referent” (as defined in the broadest possible way) characteristic of a wide spectrum of Marxist accounts—values such as equality, autonomy, freedom, democratic and civic participation, among others. However, he prefers that the alternative system or vision to remain “unnamed”: “The point may not be now to name the alternative system we seek, or even the policies that will redress. The point may be to name the injustices clearly so that we can hold accountable those who claim to be responsible. Indeed, we might just be able to find ‘new chains of symbolic equivalence... among diverse struggles’ if we might give up on defining our alternative in advance and rather embracing the struggle for justice in the now.”

Indeed, the historical experience of actually existing socialist societies has greatly damaged the credibility of the “socialist referent,” and the current Chinese regime’s obstinate self-identification as “socialist”—at the very time when it aggressively pursues the country’s integration into the global capitalist order—does not help boost the reputation of

the vocabulary, either. However, while I completely understand Michael’s concerns, I think it is also important that we remain vigilant against capitalism’s unique capacity to neutralize criticism and dissent and to contain or even absorb alternative forms of political practice that originally intended to challenge the existing system. In contrast to precapitalist (and in some cases, modern, non-capitalist) forms of society, in which appropriation of surplus was based on coercive means, in modern capitalist societies economic exploitation is not inextricably linked with extra-economic, juridical or political identities and inequalities. The extraction of surplus labor takes place in a relationship between formally free and equal individuals. In being structurally (and uniquely) indifferent to the sociopolitical identities of the people it exploits, capitalism widens and at the very same moment devalues the extra-economic domains of society in which various forms of human emancipatory politics occur, insofar as they do not radically challenge the private control of socioeconomic resources and production of class-based inequalities.² In examining popular protest and class politics in the Cultural Revolution, the aim of the book is not only to open up complex historiographical questions with regard to China’s turbulent 60s, but more importantly it is also to grasp the historical lessons they have bequeathed to the contemporary project of refashioning egalitarian politics. Therefore, whether we drop the “socialist referent” or not may be a separate rhetorical issue, it is important—I believe—that the anti-capitalist perspective and its associated alternative possibilities be robustly envisioned in order to ground a historically based analysis and critique of China’s state-socialist past and its postsocialist capitalist transformations.

While Michael Kennedy feels that my book tilts too much toward a Marxist angle, Marc Blecher expresses concerns that point to the

opposite direction. Marc clearly appreciates the book's focus on issues relating to class, but is skeptical of the manner in which they are approached. Marc compares my book with Andrew Walder's important work on Red Guard politics, in which he challenged the scholarly approach which highlighted the importance of entrenched sociological factors in shaping the trajectories of political conflicts. Walder (2002; 2009) rejects the widely held view that actors categorized in similar social arrangements share common identities and interests. Instead, he stresses the critical role of processual instability and contextual ambiguity, arguing that Cultural Revolution mass politics may be better explained in terms of the participants' contingent choices and shifting political alliances. Marc is concerned that my book has similarly deviated from the emphasis on the social determination of politics. This I must explain. I acknowledge that the origin of this book was in fact critically inspired by the earlier "social interpretation" paradigm, as I was interested in how social and political tensions generated by China's state-socialist system fractured Mao's Cultural Revolution and made possible the emergence of novel political ideas. In the process of research and writing, however, I became dissatisfied with the existing approach. This view, I have increasingly come to believe, is inadequate, as it assumes a direct causal relationship between the social and political realms. At the same time, however, I am not completely satisfied with Walder's solution. In rightfully severing the mechanical relationship between the social and the political as portrayed in the earlier scholarship, Walder seems to have thrown out the baby with the bath water, as in his account the role of the social realm in shaping political conflict has all but disappeared. Walder's work, therefore, represents a salutary advance from the previous scholarship and at the same time a partial retreat.

My book is driven by a different approach. While I remain sympathetic to Walder's criticism of the sociological interpretations, I believe that it is possible to reincorporate social factors back into a reconstructed interpretation of Cultural Revolution mass politics that not only emphasizes contingency and context, but also takes into account social interests, identities, and structural relationships as dynamically constituted. While social categories and positions were often critical in

...I believe that it is possible to reincorporate social factors back into a reconstructed interpretation of Cultural Revolution mass politics that not only emphasizes contingency and context, but also takes into account social interests, identities, and structural relationships as dynamically constituted.

shaping political conflicts, their impact was by no means direct. In the example of the "bloodline" debate in the Beijing Red Guard movement (examined in Chapter 3 of the book), social grievances against pre-Cultural Revolution political discrimination against officially designated social aliens catalyzed the emergence of new, powerful ideas critical of state-socialist institutions. The original grievances were reframed and transformed by these ideas, which turned them into the symbolic icon that inspired broader social and political struggles. In another example (Chapter 5), the rise of the Shengwulian coalition (a federation of intransigent rebels in south-central China who refused to demobilize) exemplified the significant divergence at the grassroots during the Cultural Revolution. The development of the Shengwulian coalition expressed a number of social grievances and discontents. The

combination of mass factional conflicts, locally based socioeconomic grievances, and the emergence of dangerous political ideas had an explosive impact on the course of the Cultural Revolution both local and nationwide. However, the social, political, and ideological aspects coalesced with one another in ways more complex than earlier sociologically oriented scholarship has portrayed. Rather than being preceded or directly caused by preexisting patterns of social antagonism, political conflicts and ideological disputes originated, as Theda Skocpol (1979, 320) once argued in her seminal book on the comparative-historical sociology of revolutions, from “conjunctural, unfolding interactions of originally separately determined processes.” Rebel factionalism in the region was rooted in contingent political processes not reducible to fixed social interests. The latter’s insertion into the developing factional politics only occurred at specific conjunctures of local political contentions, when the intransigence of certain rebel groups provided opportunities for the discontented elements. The factional realignment through the incorporation of the disaffected elements then took on new social characteristics, and the organizational entity that resulted from the process became associated with political agendas driven by preexisting social antagonisms.

The Shengwulian case also sheds light on the role of ideology in Cultural Revolution mass politics. In his review (2010) of *Fractured Rebellion*, Joel criticizes Andrew Walder for neglecting the importance of ideology in contentious politics: “The individuals in his account are largely free from ideological and political concerns ... Walder seems intent on stripping away the ideological and substantive political content of the movement to reveal the underlying instrumental calculations of the participants.” While I am sympathetic to Joel’s criticism, I suggest that to fully appreciate the

significance of ideology is not merely to bring ideological factors back into the historical equation and juxtapose them to politically motivated actions. It is equally important to develop a more robust conception of ideology in action that is capable of recognizing its significance in informing political and economic instrumentalities. In the Shengwulian case, for example, the development of novel political ideas gave new meanings to both the unfolding of factional conflicts and the expression of preexisting social grievances. Newly emerging heterodox ideas served the function of establishing a relation among diverse grievances such that their meanings became modified. A new ideological horizon was opened up when individual demands transcended their immediate circumstances and made references to broader social and political conditions. Through the activists’ radicalization of the official Maoist doctrine, local conflicts were emptied of their contextual specificities, and became bearers of new, wider antagonisms.

While Joel Andreas, Marc Blecher, and Michael Kennedy have asked questions concerning conceptual and political issues, Xiaohong Xu raised a question pertinent to both historical interpretation and methodological perspective: did the political differentiation and ideological multiplication from below occur only after the Cultural Revolution took its course, and become possible only after Mao launched the mass movement from above? This involves not only the problem of political agency but potentially also that of historical periodization. An affirmative answer to the question, in Xiaohong’s view, would under-appreciate the agency of the rebel activists who, in response to Mao’s call for rebellion, creatively appropriated and extended the Maoist ideology. Xiaohong points to the tensions and antagonisms simmering in Chinese society prior to the Cultural Revolution, suggesting that the “unruly margins” were in fact part of a

continuous and broader process of political and ideological activism, which had deeper roots and spanned across 1966, the year that Mao ignited the ferocious mass movement. This, I think, is an intriguing suggestion that deserves careful consideration. While I am certainly not arguing in the book that political and ideological differentiation was only enabled by Mao the Supreme Leader, the fact that my book has largely left out the pre-Cultural Revolution ideological antagonisms among the Chinese populace may indeed lead to such an impression. Xiaohong is absolutely correct that the Chinese social and political scene during the years leading up to the great turmoil was by no means static or homogeneous. In the mid-1960s, students at a number of elite middle schools in Beijing, where the Red Guard movement would soon erupt, were involved in political activism and debates over a diverse array of issues ranging from educational reform to current state affairs. These activities defied bureaucratic discipline and were unwelcomed by school and local government authorities, which deemed them undesirable or even illegal. We know very little about these currents, and better research in this area will in no doubt significantly contribute to a more historically grounded and complex understanding of the origins of the Cultural Revolution's mass political mobilization.

In closing, I would like to thank Xiaohong Xu for initiating the author-meet-critics session at the 2014 Social Science History Annual Conference in Toronto, at which these comments were first presented. I would also like to thank Michael Kennedy, Joel Andreas, Marc Blecher, and Xiaohong Xu for their insightful and engaging thoughts. Finally, I would like to thank Matthew Baltz, editor of *Trajectories*, for his invitation to publish this exchange.

Endnotes:

1. For a small sample of this body of critical literature, see Martin Hart-Landsberg and Paul Burkett, *China and Socialism: Market Reforms and Class Struggle* (New York: Monthly Review Press, 2005); David Harvey, *A Brief History of Neoliberalism* (Oxford: Oxford University Press, 2005); William Hinton, *The Great Reversal: The Privatization of China, 1978-1989* (New York: Monthly Review Press, 1990); Lin Chun, *The Transformation of Chinese Socialism* (Durham: Duke University Press, 2006); Wang Chaohua, ed., *One China, Many Paths* (London: Verso, 2003); Robert Weil, *Red Cat, White Cat: China and the Contradictions of "Market Socialism"* (New York: Monthly Review Press, 1996); Zhang Xudong, ed., *Whither China?: Intellectual Politics in Contemporary China* (Durham: Duke University Press, 2002). For the representative works of China's most prominent "New Leftist" intellectual, see Wang Hui, *China's New Order: Society, Politics, Economy in Transition* (Cambridge: Harvard University Press, 2003) and *The End of the Revolution: China and the Limits of Modernity* (London: Verso, 2010).

2. For an insightful discussion of capitalism's unique capacity to neutralize and contain dissent and criticism, see Wood (1995), especially chapters 8 and 9.

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Japan's Long Defeat: War Memory, Cultural Trauma, and East Asian Politics Today

Akiko Hashimoto

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*Editor's Note: The following essay draws on research appearing in the author's new book, **The Long Defeat: Cultural Trauma, Memory and Identity in Japan**, published this year by Oxford University Press. My thanks to Akiko Hashimoto for contributing her insights to the newsletter.*

Benedict Anderson (1991) reminds us that modernity has been characterized by the emergence of nation-states that can mobilize the passion of young men to “die for the country” on a mass scale. Once mobilized, nationalist passion allows a soldier in modern wars to believe “he is dying for something greater than himself, for something that will outlast his individual, perishable life in place of a greater, eternal vitality” (Rahimi 2006). But after demobilization, this passion withers, no longer fed and needed for everyday combat. For those on the losing side, this passion no longer even has any social and moral legitimacy. Justification for violent deaths on a scale of millions is especially hard to summon by the defeated.

The tension between recognizing the futility of the war and seeking something meaningful in the deaths has remained an unresolved dilemma after modern wars that called up millions of conscripts. The tension is especially acute in vanquished nations where, as Wolfgang Schivelbusch (2003) asserts, the desire to search for positive meaning in the defeat by seeking a progressive narrative of the

loss is a common and powerful need. So strong was the impulse for making meaning that it led to the myth of the Lost Cause among the American Confederacy after the Civil War, and also the myth of the Fallen Soldier among the German soldiers who died in World War I. Among the victors, too, mass deaths have called for moral justifications, the most famous of which was calling World War I “the war to end all wars” in Great Britain. Attempts to look for such justifications force the momentous question of the ultimate value in national sacrifice.

Reflecting on this year's 70th anniversary of the end of World War II, it is timely to seek a post-World War II update to these observations about the culture of defeat. As I show in my book *The Long Defeat: Cultural Trauma, Memory and Identity in Japan* (2015), coming to terms with the national trauma of World War II remains a protracted, painful process. The vanquished still respond with persistent attempts to overcome humiliation and disgrace, but they differ in approach. In Japan today, this difficulty of overcoming defeat lies directly at the root of the current Abe government's brazen push to elevate the military role of the Japanese Self Defense Force and its protest movements. As will be noted below, many current political problems – including its deteriorating geopolitical relations with China and the Koreans – are fueled directly by the contentious meanings of defeat that remain unresolved in Japan.

The influence of defeat on Japan's postwar culture has been immense, long-lasting, and complicated. Japan lost sovereignty after surrendering in 1945, and it was occupied for seven years by the winners, who imposed radical reforms in nearly all aspects of society from governance and law, to economy and education. Japan's perpetrator guilt in the war was defined explicitly at the Tokyo War Crimes Tribunal (1946-48) which indicted Japan's military leadership for committing crimes against peace and other violations of war conventions. At the same time, the Tribunal and numerous other war crimes trials in Asia overlooked the possible guilt of many others in the military, bureaucracy, government, business, and – controversially – the Emperor. Since then, long-standing fissures have emerged within Japanese society over who was guilty and responsible for the war. These fissures continue today. Underlying the fissures are two fundamental questions: Why did we fight an unwinnable war? Why did they kill and die for a lost cause? In answering these questions, people bring different narratives to bear, debate different rational positions, and opt for different solutions, but ultimately, the answers are formed by personal and political reactions to the memories of massive failure, injustice and suffering. At the heart of these debates are concerns not only over war responsibility, but also about national belonging, the relations between the individual and the state, and relations between the living and the dead.

Memories of wars, massacres, atrocities, invasions, and other instances of mass violence and death become significant referents for subsequent collective life when people choose to make them especially relevant to who they are and what it means to be a member of that society. Some events become more significant than others, because we manage to make them more consequential in later years for our understanding of ourselves and our own

society. Jeffrey Alexander has called this process cultural trauma, which occurs “when members of a collective feel they have been subjected to a horrendous event that leaves indelible marks upon their group consciousness, marking their memories forever and changing their future identity in fundamental and irrevocable ways” (2004, 1). The horrendous event emerges as a significant referent in the collective consciousness, not because it is in some way naturally ineffaceable but because it generates a structure of discourse that normalizes it in collective life over time (Eyerman 2004). In the process, the memory of the event is made culturally relevant, remembered as an overwhelmingly damaging and problematic collective experience and incorporated, along with all of its attendant negative emotions, as part of collective identity (Smelser 2004).

Over the decades, three categories of trauma narratives have emerged in Japan, diverse but deeply etched in the national sentiment. They are different in how they assess the moral import of military and political actions, and in how they characterize the negative legacy of failures and losses in the war. Each points Japan in a different direction for shaping its future.

The first category of narratives emphasizes the stories of fallen national heroes. These narratives embrace a “fortunate fall” argument, which justifies the war and national sacrifices in hindsight by claiming that the peace and prosperity of today are built on those sacrifices of the past. These heroic narratives tend to promote a discourse of indebtedness that is heard often in official speeches at commemorations. It is an ameliorative narrative intended to cultivate pride in national belonging; at the same time it diverts attention from the culpability of the state in starting and losing the war.

A second narrative promotes empathy and identification with the tragic victims of defeat. Here a vision of “catastrophe” prevails – a tragedy of epic proportions – accentuating the total carnage and destruction wrought by ferocious military violence. This discourse of suffering and anti-militarism is found often in family stories, popular culture stories, and the pacifist embrace of the victims in Hiroshima, Nagasaki and scores of other cities crushed by the atomic bombings and indiscriminate air raids. This narrative – embraced by many pacifists – also tends to divert attention, in this case from the suffering of distant others that the Japanese victimized in Asia.

The third type of narrative contrasts with the first two by emphasizing Japan's perpetrator acts of imperialism, invasion, and exploitation in China, Korea and Southeast Asia. This is a narrative of a “dark descent to hell,” stressing the violence and harm that Japan inflicted, with varied attribution of malicious intent. The most difficult and controversial of the three narratives, this vision, and its discourse of regret, is often found in investigative journalism and news media, documentaries, academic publications and intellectual discussions as well as some veterans’ memoirs and oral histories. Civic movements and friendship organizations dedicated to reconciliation in East Asia largely presuppose the acceptance of this perpetrator narrative.

This cacophony of memory narratives, far apart in moral sentiments and interests, accounts for the disarray in the nation's representation of its meta-history. This problem is evident even in the naming of the war. “The Pacific War” became a standard name for the war imposed by the U.S. occupation and is still often used in the fortunate fall narrative. A countervailing name used by Japanese progressive intellectuals and educators also gained ground, and it was used in their dark descent narrative; this name “the

fifteen-Year War” recognized the salience of Japanese Imperial aggression in East Asia for a decade preceding the war in the Pacific. Subsequent designations used to sidestep such naming politics have been “the Asia-Pacific War,” “the Shōwa War,” “World War II,” and, as people became weary of the political baggage that each name carried, the war ultimately came to be called “the last world war,” “that war,” and even “that unfortunate period of the past.” This problem of representing “that war” arises at every turn, from commemorative speeches and history textbooks to museum exhibits. The hundreds of regional, specialized “peace” museums scattered across the nation must address this “history problem” by not presenting a comprehensive national history of the War;

This cacophony of memory narratives, far apart in moral sentiments and interests, accounts for the disarray in the nation's representation of its meta-history.

rather, they present partial stories of cultural trauma, selectively emphasizing perpetrators, victims or heroes. Thus the common Western criticism that Japan leaves so much of war history unexamined points the finger in the wrong direction: it is not about national amnesia but a stalemate in a fierce, multivocal struggle over national legacy and the meaning of being Japanese

The current political contention over war memory across the East Asia region is grimly complicated by these long-standing divisions in Japan's project to recover its moral footing in the aftermath of imperial incursions in China and Korea. Several issues stand out today as particularly inflammable: the redress for wartime sexual forced labor (“comfort women”); the culpability for brutal massacres

(especially the Nanjing Massacre); the attempts to rehabilitate the perpetrators and war criminals as martyrs (Yasukuni Shrine); and the territorial disputes over the border islands (Senkaku/Diaoyu islands, Takeshima/ Dokdo island, and Hoppō ryōdo/ the Kuril islands). The issues are fraught with deep conflict among stakeholders not only internationally, but also nationally.

Moving beyond this 70th anniversary, former adversaries of the Asia-Pacific War face crucial choices for the future of the East Asia region. The mounting tension centered on war memory politics today among Japan, China, and the Koreas is not only about righting past wrongs, but also about jockeying for position in the shifting geopolitics owing largely to the rise of China, and the continuing belligerence of North Korea. In this context, Japan faces diverging choices for national policy and moral purpose in moving forward: nationalism, pacifism, or reconciliation.

Japan's widely reported struggle today over remilitarization is fought precisely by these nationalists, pacifists and reconciliationists whose divergent understandings of Japan's war and defeat exactly parallel the three war trauma narratives discussed in this essay. For the nationalists – who tend to espouse the heroic narrative of the tainted war – the most direct route to overcome the trauma of defeat is to reinterpret or revise Japan's peace constitution, strengthen Japan's military, and become a power to be reckoned with in the world. On the other hand, for the pacifists – who tend to emphasize the victim narrative of annihilation – the most effective pathway to overcome defeat is to uphold anti-militarism, protect Japan's peace constitution, and strengthen Japan's anti-nuclear influence worldwide. Finally, for the reconciliationists – who tend to embrace the perpetrator narrative of the war – the most appropriate approach to move beyond defeat is to promote diplomatic

resolutions and strengthen relationships with inherited enemies in East Asia, to foster trust and overcome the vicious cycles of resentment.

In considering the proponents of these three ways forward for Japan, we must recognize that they are preoccupied with different concerns and visions for the future, and Japan must ultimately find some compromises among them. And it behooves us to remember that in a globalizing culture of memory, national memories of war are no longer self-contained, and forgetting is no longer an option as it had been in the past. The new international world order demands imaginative concessions and innovative compromise to break the logjams of historical grievances.

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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 Colin Beck and Sarah Quinn for contributing essays, and to past newsletter editors for hatching the original idea.

We Were Never Properly Introduced

Colin J. Beck
Pomona College

My journey into comparative-historical sociology is a rather convoluted one, made more so by my only recent willingness to claim comparative-historical sociology as part of my professional identity. Much of this has to do with my entry into the discipline, and much has to do with my general reluctance to identify with anything. (The wonderful line from the film *Stranger Than Fiction* sums up my feelings about belonging: "The anarchists have a group!?") My partner, who made the study of identity her first specialization, once remarked that I was unusual in that I rarely made identity statements—the sort of thing that begins "I am the type of person..." This has led to a long running joke in our home that goes: "I'm not the type of person to make identity statements."

In any case, I first met comparative-historical sociology as an undergraduate, even though we were not properly introduced. I had begun college with a desire to major in international relations and pursue some sort of diplomatic career. A semester in India my sophomore year left me disillusioned with the detached grand paradigms of IR and its great man theories of history. So I chose to design my own major, an endeavor not feasible at most schools, and even when feasible not one I can recommend. It was not until my senior year, when I buttonholed the newly arrived Bruce Podobnik to tutor me in political sociology that I realized my self-designed major was in fact sociological. That semester I read the work of Antonio Gramsci, Immanuel Wallerstein, Edward Said, Benedict Anderson, and others. I quickly decided that I was a political sociologist at heart, not realizing the label of comparative-historical sociology even existed.

The advantage of my alma mater was that it gave me the freedom to discover this, but the disadvantage was to leave me somewhat adrift as I resolved to enroll in a Ph.D. program in sociology. With the barest knowledge of academia as a profession and sociology as a discipline, which is not an approach I recommend, I ended up matriculating at Stanford University. Stanford was an odd fit for my inclinations substantively and methodologically, but a boon intellectually. I quickly fell in with John Meyer after taking his political sociology seminar my first year. Anyone who knows John can attest he has an easy and humble charisma, which

complimented my late adolescent brashness well. And John, even though he may eschew the label, is very much a comparative-historical sociologist. (In fact, the ontology of world society theory shares much with the ontology

Comparative history was a revelation. I could assemble evidence, but I did not have to end my argument at a table of regression results. I could theorize grandly even while being sensitive to time and place. I quickly discovered that the study of revolution was the place to unify my various interests— movements, politics, and institutions and culture, religion, and ideology, all in global and historical context.

of comparative history, which is apparent to me, at least, and also I hope anyone else who reads both deeply.) At the beginning of graduate school, I was most interested in the study of social movements, and also began working with Doug McAdam, another mostly unremarked comparative-historical sociologist. But I grew disillusioned again; this time with the odd abstraction of social movement theory and the odd specificity of its cases.

Like my independent study in college before, a class once again changed my outlook. In my third year at Stanford I took a seminar in comparative-historical sociology with Gi-Wook Shin. Comparative history was a revelation. I could assemble evidence, but I did not have to end my argument at a table of regression results. I could theorize grandly even while being sensitive to time and place. I quickly discovered that the study of revolution was the place to unify my various interests—

movements, politics, and institutions and culture, religion, and ideology, all in global and historical context.

Yet I still felt an outsider to comparative-historical sociology, trained as I was in the Stanford model of quantification and variables oriented theory. John remarked after my dissertation defense, “Colin should have gone to Berkeley. So we made a little Berkeley here for him.” (Apologies in advance to those who actually did go to Berkeley.) When I was on the job market in 2008, not a year that I recommend to be seeking professorial appointment, one search committee asked me if I identified as a comparative-historical sociologist. I answered with hesitation, “Not really.” And this answer held true through the first couple years post-Ph.D. Even as I published in comparative-historical journals and began learning more about comparative-historical analysis for my ongoing project on knowledge accumulation in the study of revolution, I still felt like I did not fit well with the subfield. This was initially compounded by the institutionalization of global and transnational sociology within the ASA. (As Kiyoteru Tsutsui once remarked about G&TS: I feel like I have finally found my home.)

My lack of identity — remember, I am not the type of person who makes identity statements — only began to change as Julian Go roped me into service for the Comparative and Historical Sociology Section. I felt I owed Julian a favor, and always enjoyed his work, so when he asked me to organize the CHS roundtables in 2012, I agreed. This led to serving on the section’s dissertation award committee, and ultimately my appointment as Secretary-Treasurer. The rapprochement was made easier by the joint projects of CHS and G&TS. (Some of us may recall Julian and Julia Adams’s joke about their wedding at the first joint mentoring event at ASA in 2013. But it truly has been a marriage—a quarter of G&TS

members also belong to CHS.) Along the way, I also met a number of other comparative-historical sociologists who welcomed me to the fold. I shall not name them here in case they do not feel as warmly to me as I do to them; chains of affection are sometimes unidirectional.

In short, it took me a while to get here and there were a number of false starts, but I can say sixteen years after I first encountered the field: I am a comparative-historical sociologist.

Sarah Quinn

University of Washington

Looking back now, I can see that long before I started historical research a series of people helped guide me to that path, many in indirect and surprising ways.

Marc Steinberg's undergraduate theory class was my first introduction to the importance of history for sociological thinking. As a sophomore at Smith College, I was largely interested in how sociology informed current events. The only history class I had taken was a seminar on the Black Plague, which came highly recommended for its graphic readings about rats, pustules, and social disintegration. Starting his theory class with the work of Marx and Engels, Marc used his deep knowledge about labor history to make theory come alive in the classroom. At the time I thought the historical background was the price of admission for understanding the big ideas. In retrospect I see the class as my first lesson in how knowledge of historical transformations enriches our understanding of the social world, one learned years before I knew I would pursue a career in sociology.

Elizabeth Dilullo gave me a job right out of college and though she was not a sociologist, she was perhaps the individual most directly

responsible for my eventual research on the history of mortgage securitization. In 1998 Elizabeth made the decision to hire me to work in the credit department of a small derivatives firm. I was initially tasked with overseeing a database that tracked our counterparties' preparedness for the Y2K computer bug. Since the world did not end at the turn of the millennium I was soon given the chance to take on other work, like creating monthly reports for the credit department. This job was where I first learned about finance and where I became curious about the social conditions of possibility for its varied forms.

Elizabeth is smart and funny and has high standards. She gave me second chances when I made mistakes. She showed me how to take responsibility and take initiative. And she fought with me about politics. All the time. Because Elizabeth Dilullo was a proud Fox-news watching, free-market loving Republican. As an NPR-listening graduate of a Quaker high school and Smith College, I had never before debated with someone who rejected even my most basic assumptions. After five years of sparring with Elizabeth, I left for graduate school eager to understand not only how the new economy worked, but also to

...I left for graduate school eager to understand not only how the new economy worked, but also to understand why people had diverging opinions about how the market should work.

understand why people had diverging opinions about how the market should work. To some extent, when I am researching old political fights over financial policies, I am still trying to figure out why Elizabeth and I disagree so profoundly about so much.

As a sociology graduate student, my first systematic readings in economic sociology were in Marion Fourcade's "Economy and Society" graduate seminar. The same comparative approach that guides Marion's research was reflected in that syllabus. Her class taught me the power of comparisons, both across space and over time. It taught me to think systematically about the historical and social trajectories of categories like risk, finance, labor, and law. It gave me the chance to write my first paper on financial technologies. In her class was also where I first read the work of Donald MacKenzie. While my work had some historical aspects, my academic identity at that point was firmly that of an economic sociologist. Not long after that class, however, MacKenzie's *An Engine, Not a Camera: How Financial Models Shape Markets* inspired me to pursue my own research on the development of mortgage securitization, focusing on how the federal government contributed to its growth.

Making the decision to transition into a historical project meant that I needed more help than ever. Even after I had filed my prospectus and entered the archives, I still thought of myself as something of an interloper. I had a lot of ground to make up.

Archivist Allen Fisher at the Lyndon B. Johnson Presidential Library was instrumental in helping me move forward from that point. Every morning at the archive Allen would stop by my desk and ask after my progress with the files to see if I was on track. We also would discuss how this research was related to the economic crisis that mounted around us. By the time I left, Allen had helped me uncover some smoking-gun files that would become the cornerstone of my dissertation. He also taught me how to do serious archival research.

Of course, many more people helped me along this path than I have mentioned here: patient teachers, generous mentors, inspiring researchers, dear friends. As sociologists, we are trained to understand that many people shape our scholarly identities. This is nowhere more true than for comparative historical sociologists. Because we work at the intersection of societies, time-periods, and disciplines, we must continually reach outwards for data, for counsel and for expertise. For us experts in historical change, I suspect there is a special poignancy in seeing earlier influences linger and reemerge in surprising ways through our research. They are, after all, the happy unintended consequences of our own lives.

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Books and Edited Volumes

Chartering Capitalism: Organizing Markets, States, and Publics

Political Power and Social Theory, V29
Emerald Group Publishing, 2015

Emily Erikson (Editor)

This volume covers the evolution of the chartered company; contributions employ comparative methods, archival research, case studies, statistical analyses, computational models, network analyses, and new theoretical conceptualizations to map out the complex interactions that took place between state and commercial actors across the globe.

The Long Defeat: Cultural Trauma, Memory, and Identity in Japan

Oxford University Press, 2015

Akiko Hashimoto

The Long Defeat explores the stakes of war memory in Japan after its catastrophic defeat in World War II, showing how and why defeat has become an indelible part of national collective life, especially in recent decades. Divisive war memories lie at the root of the contentious politics surrounding Japan's pacifist constitution and remilitarization, and fuel the escalating frictions in East Asia known collectively as Japan's "history problem." Drawing on ethnography, interviews, and a

wealth of popular memory data, this book identifies three preoccupations - national belonging, healing, and justice - in Japan's discourses of defeat. Hashimoto uncovers the key war memory narratives that are shaping Japan's choices - nationalism, pacifism, or reconciliation - for addressing the rising international tensions and finally overcoming its dark history.

Race and the Origins of American Neoliberalism

Routledge, 2015

Randolph Hohle

Why did the United States forsake its support for public works projects, public schools, public spaces, and high corporate taxes for the neoliberal project that uses the state to benefit businesses at the expense of citizens? The short answer to this question is race. This book argues that the white response to the black civil rights movement in the 1950s, '60s, and early '70s inadvertently created the conditions for emergence of American neoliberalism. Neoliberalism is the result of an unlikely alliance of an elite liberal business class and local segregationists that sought to preserve white privilege in the civil rights era. The white response drew from a language of neoliberalism, as they turned inward to redefine what it meant to be a good white citizen. The language of neoliberalism depoliticized class tensions by getting whites

to identify as white first, and as part of a social class second. This book explores the four pillars of neoliberal policy, austerity, privatization, deregulation, and tax cuts, and explains how race created the pretext for the activation of neoliberal policy. Neoliberalism is not about free markets. It is about controlling the state to protect elite white economic privileges.

The China Boom: Why China Will Not Rule the World

Columbia University Press, 2015

Ho-fung Hung

Many thought China's rise would fundamentally remake the global order. Yet, much like other developing nations, the Chinese state now finds itself in a status quo characterized by free trade and American domination. Through a cutting-edge historical, sociological, and political analysis, Ho-fung Hung details the competing interests and economic realities that temper the dream of Chinese supremacy--forces that are stymieing growth throughout the global South.

Hung focuses on four common misconceptions: that China could undermine orthodoxy by offering an alternative model of growth; that China is radically altering power relations between the East and the West; that China is capable of diminishing the global power of the United States; and that the Chinese economy would restore the world's wealth after the 2008 financial crisis. His work reveals how much China depends on the existing order and how the interests of the Chinese elites maintain these ties. Through its perpetuation of the dollar standard and its addiction to U.S. Treasury bonds, China remains bound to the terms of its own

prosperity, and its economic practices of exploiting debt bubbles are destined to fail. Hung ultimately warns of a postmiracle China that will grow increasingly assertive in attitude while remaining constrained in capability.

Beneath the Surface of White Supremacy: Denaturalizing U.S. Racisms Past and Present

Stanford University Press, 2015

Moon-Kie Jung

Racism has never been simple. It wasn't more obvious in the past, and it isn't less potent now. From the birth of the United States to the contemporary police shooting death of an unarmed Black youth, *Beneath the Surface of White Supremacy* investigates ingrained practices of racism, as well as unquestioned assumptions in the study of racism, to upend and deepen our understanding.

In Moon-Kie Jung's unsettling book, *Dred Scott v. Sandford*, the notorious 1857 Supreme Court case, casts a shadow over current immigration debates and the "war on terror." The story of a 1924 massacre of Filipino sugar workers in Hawai'i pairs with statistical relentlessness of Black economic suffering to shed light on hidden dimensions of mass ignorance and indifference. The histories of Asians, Blacks, Latina/os, and Natives relate in knotty ways. State violence and colonialism come to the fore in taking measure of the United States, past and present, while the undue importance of assimilation and colorblindness recedes. Ultimately, Jung challenges the dominant racial common sense and develops new concepts and theory for radically rethinking and resisting racisms.

The Idea of Englishness: English Culture, National Identity and Social Thought

Ashgate, 2015

Krishan Kumar

Ideas of Englishness, and of the English nation, have become a matter of renewed interest in recent years as a result of threats to the integrity of the United Kingdom and the perceived rise of that unusual thing, English nationalism. Interrogating the idea of an English nation, and of how that might compare with other concepts of nationhood, this book enquires into the origins of English national identity, partly by questioning the assumption of its long-standing existence. It investigates the role of the British empire - the largest empire in world history - in the creation of English and British identities, and the results of its disappearance. Considering the 'myths of the English' - the ideas and images that the English and others have constructed about their history and their sense of themselves as a people - the distinctiveness of English social thought (in comparison with that of other nations), the relationship between English and British identity and the relationship of Englishness to Europe, this wide-ranging, comparative and historical approach to understanding the particular nature of Englishness and English national identity, will appeal to scholars of sociology, cultural studies and history with interests in English and British national identity and debates about England's future place in the United Kingdom.

The publication of the book is being accompanied by a one-day conference on November 27 at St. John's College, Cambridge University.

Representing Mass Violence: Conflicting Responses to Human Rights Violations in Darfur

University of California Press, 2015

Joachim J. Savelsberg

How do interventions by the UN Security Council and the International Criminal Court influence representations of mass violence? What images arise instead from the humanitarianism and diplomacy fields? How are these competing perspectives communicated to the public via mass media? Zooming in on the case of Darfur, Joachim J. Savelsberg analyzes more than three thousand news reports and opinion pieces and interviews leading newspaper correspondents, NGO experts, and foreign ministry officials from eight countries to show the dramatic differences in the framing of mass violence around the world and across social fields. *Representing Mass Violence* contributes to our understanding of how the world acknowledges and responds to violence in the Global South.

A free ebook version of this title is available through Luminos, University of California Press' new open access publishing program for monographs. Visit www.luminosoa.org to learn more.

News and Announcements

Call for Proposals

Can Comparative Historical Sociology Save the World?

Mini-Conference of the Comparative Historical Sociology Section

Friday, August 19, 2016
Seattle, Washington

The Comparative Historical Sociology section of the American Sociological Association and the Equality Development and Globalization Studies (EDGS) program at Northwestern University are pleased to announce a mini-conference entitled "Can Comparative Historical Sociology Save the World?" The conference will take place August 19th, 2016 at the University of Washington, in Seattle.

We live in a world where the most important policy concerns, from terrorism and climate change to the fight against poverty and infectious disease, transcend national borders. This conference explores how scholars might use the tools of comparative and historical sociology to engage issues of public concern. An opening plenary session moderated by Professor Monica Prasad will engage both advanced and early-stage scholars in conversation on this issue. Other sessions will be organized around the papers accepted through this call.

We encourage paper submissions from scholars at all career stages, from sociology and other disciplines. We are especially

interested in submissions that employ comparative and historical methods to examine important issues of our day, such as (but not limited to) global market regulation, questions of immigration and citizenship, poverty, environmental insecurity, and protracted race, gender and class inequality. We also invite submissions reflecting on the tradition of policy-relevant research in comparative historical sociology, as well as what the role of comparative and historical methods could or should be in public debate.

Please submit abstracts of no more than 500 words through the electronic abstract submission form:

<http://form.jotform.us/form/52724660569160>.

The deadline for paper submission is January 30th, 2016.

Conference participants and attendees will be asked to contribute a participation fee of \$25 for faculty and \$15 for students. Funding to defray costs of travel and lodging will be awarded on a lottery basis for interested graduate students and term faculty participants. Announcements about travel awards will be made after papers are accepted.

For questions, please contact the planning committee at chsminicon@gmail.com.

Organizing committee: Johnnie Lotesta, Aliza Luft, Josh McCabe, Andre Joshua Nickow, Sarah Quinn, Fiona Rose-Greenland, and Eric Schoon.

Chaos and Governance in the 21st Century: Prospects and Challenges to Peace and Justice in an Age of Uncertainty

February 18-19, 2016

Florida Atlantic University, Boca Raton, FL USA

Co-Organizers: the FAU Peace Studies Program and the Johns Hopkins University's Arrighi Center for Global Studies

At the close of the 20th century Giovanni Arrighi and Beverly Silver published their influential work, *Chaos and Governance in the Modern-World System* (1999). Arrighi and Silver (along with their collaborators Iftikhar Ahmad, Kenneth Barr, Shuji Hisaeda, Po-keung Hui, Krishnendu Ray, Thomas Ehrlich Reifer, Miin-wen Shih, and Eric Slater) employed world historical methodologies to take on crucial scholarly controversies about systemic and structural transformations characterizing global capitalism at the time, including the rise and spread of market fundamentalism, the financialization of capital, and processes of global economic integration and liberalization. Were these world systemic changes a sign of US hegemony or hegemonic decline? Did they signify a fundamental structural shift in the balance of power among states? Has "globalization" irremediably undermined state power? Has the world economy entered an unstoppable "race to the bottom" in conditions of work and life? Was the close of the 20th century the autumn of five centuries of Western dominance in the modern world system?

This conference is designed to provide a unique opportunity for scholars interested in revisiting these themes, questions, and propositions nearly two decades later, when the geopolitics, global economics, and the fabric of social life appear to be more uncertain and future directions more unstable.

Participation and Submissions:

Please submit proposals (1-2 page) by October 30th 2015 to phough2@fau.edu. Please include your institutional affiliation and contact information.

We welcome paper presentation proposals that critically engage with four core areas of inquiry laid out in *Chaos and Governance*:

- (1) Financialization, Geopolitics and Global Governance in the 21st Century
- (2) New Strategies of Capital Accumulation, New Modalities of Domination
- (3) Anti-Systemic Movements: Land, Labor and Environmental Struggles
- (4) Hierarchies of Wealth and Power: Global Inequality and the North-South Divide

We strongly encourage submissions from scholars at all career stages and from a range of theoretical, methodological and disciplinary traditions.

For further information:

Phillip A. Hough
FAU Sociology Department
phough2@fau.edu

Beverly Silver
JHU Sociology Department and Arrighi Center for Global Studies
silver@jhu.edu

Awards and Grants

Emily Erikson has been awarded the 2015 Ralph Gomory Prize from the Business History Conference and Alfred P. Sloan Foundation for her book, *Between Monopoly and Free Trade: The English East India Company*, Princeton University Press.

Aliza Luft's article, "Toward a Dynamic Theory of Action at the Micro-Level of Genocide: Killing, Desistance, and Saving in 1994 Rwanda" (appearing in *Sociological Theory*) has been awarded the 2015 Candace Rogers Best Student Paper Award by the Eastern Sociological Society and an Honorable Mention for the 2015 Outstanding Graduate Student Paper Award from the Collective Behavior and Social Movements Section, American Sociological Association.

Peter Stamatov has been awarded a five-year 1.1. million Euro "Consolidator Grant" by the European Research Council. Starting this fall, he will hold a dual appointment as Associate Professor at the Social Science Division of New York University, Abu Dhabi and as Distinguished Researcher at the Juan March Institute (Universidad Carlos III de Madrid). The title of the project is "The Transformation of Popular Politics in Europe's Long Nineteenth Century." The project explores the complex, multi-vocal and interdependent transformation of popular politics across Europe, seeking to understand the processes leading to the emergence and spread of typical forms of social movement activities outside of the North-West European core.

Work in Progress

Compon Project

An update from Jeffrey Broadbent:

"CHS folks will be interested to know that our Compon project - Comparing Climate Change Policy Networks - continues to plow ahead. We have research teams in about 25 countries plus Taiwan. If folks go to our website, www.compon.org, under publications, they will see over 30 publications based on our common cross-national comparative methods. Many more are coming, including ones that use the data to conduct explicit cross-national comparisons. The Compon project is open to people bringing in new cases--our climate change policy network survey form (collecting inter-organizational networks in the national climate change polity)) now is on-line and only takes about 15 minutes for the organizational respondent to complete, and a number of cases are deploying this survey so we are building up a comparative database on climate change politics, mobilization, information flow, coalition building, policy participation and so forth. It would make a good doctoral dissertation research project to conduct the survey on a new national case and join our stream of work. People can all help with some other cutting edge aspects of the Compon project, such as developing and applying automated in-depth framing content analysis of media coverage of climate change."

PhDs on the Market

Aliza Luft

University of Wisconsin-Madison

Dissertation:

Shifting Stances: How French Bishops Defected from Support for Vichy Anti-Semitism to Save Jews During the Holocaust

Shifting Stances: How French Bishops Defected from Vichy to Save Jews During the Holocaust, explains the mechanisms that motivated French clergy first to endorse Vichy anti-Semitism in 1940, and then to protest Vichy's policies toward Jews in 1942. I analyze newly available historical sources written in French and Hebrew and collected from fifteen archives in ten cities and three countries—France, USA, Israel—to explain how individuals and institutions trade the benefits of stability for the risky behavior associated with collective action in violent contexts. This project is distinguished from other research on high-risk mobilization in its recognition that the same organization may act in contradictory ways over time. I challenge research that assumes an organization's position can be predicted through its ethnic, racial, or religious affiliation, as well as work that treats behavioral stances as fixed throughout the course of a conflict. In its emphasis on the decision-making processes of clergy, *Shifting Stances* also explains what

triggers “first actors” in dangerous settings where mobilization places both self and community at grave risk. Drawing on a range of methodological tools, including comparative-historical, qualitative, and quantitative analysis, I find that critical events disrupt routines and provide openings for new lines of action in violent contexts. However, for these openings to be effective, the creation of clandestine networks that draw on pre-existing relationships is necessary to counter the constraints of existing formal structures.

Committee: Ivan Ermakoff (Chair), Chad Goldberg, Pam Oliver, Myra Marx Ferree, Bob Freeland, Laird Boswell (History)

Specializations: Political Sociology; War and Violence; Collective Action and Social Movements; Comparative-Historical Sociology ; Race, Ethnicity, Religion, Gender; Qualitative Methods; Sociological Theory

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Website: <http://www.alizaluft.com>

PhDs on the Market



Laura K. Nelson
University of California, Berkeley

Dissertation:

The Power of Place: Structure, Culture, and Continuities in U.S. Women's Movement

Much of the research on U.S. women's movements suggests that the first wave "woman suffrage" movement remained almost exclusively in the political realm, while the second wave feminist movement was unique in its emphasis on the social, cultural, and personal spheres. My dissertation challenges this account, showing that the so-called "between-wave" distinction existed, but it existed between cities within each wave. For instance, I show that what historians call second-wave feminism—the feminism that focused on cultural and personal issues—was actually New York City-style feminism that stretched all the way back to the first wave. To identify these patterns I use network and computational text analyses to measure the different structures and underlying cultural assumptions of the feminist fields in New York City and Chicago between 1865 and 1975. I detail three mechanisms producing this within-city persistence, each contributing to a city's unique "rolling inertia." This research points to the importance of the

institutionalization of city-based political cultures in shaping social movement fields. I suggest that computational methods, including automated text analysis, can open up new avenues for measuring (latent) cultural institutions and can lead to new ways of conducting formal comparative-historical social science.

Committee: Kim Voss (Chair), Raka Ray, and Robin Einhorn (History Department)

Current Affiliation: Postdoctoral Research Fellow in the Management and Organizations Department in the Kellogg School of Management at Northwestern University

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***Coming up in
the next issue of***

Trajectories

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

**Two new book symposia:
What Unions No Longer Do
by Jake Rosenfeld
Expulsions
by Saskia
Sassen**

**And
Much
More!**