Trajectories

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OP-ED CORNER

Comparative-Historical Perspectives on European Populism

Editor's Introduction

Victoria Reyes
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We lead this issue of *Trajectories* with four scholars who turn our eye to Europe to understand the causes, consequences, and possible futures of right-wing populism.

Mabel Berezin urges us to not prematurely celebrate centrist Emmanuel Macron's recent French victory over right-wing Marine Le Pen as an "end of populism" and reminds us how populism is a historically-rooted feature of modern France and that the problems that helped Le Pen rise to national prominence have not been solved.

Dorit Geva provides another look into France's National Front party and its leader Marine Le Pen, documenting how Le Pen

CONTENTS

Book Symposia

Page 10 Emigh, Riley, and Ahmed's two-volume *How Societies and States Count*
Page 43 Go's *Postcolonial Thought and Social Theory*
Page 63 Pacewicz's *Partisans and Partners*

Features and News

Page 78 Spotlight: Section Working Groups
Page 81 New Publications
Page 86 Section News

was able to transform the party to one that embraces a wide range of people, not just the petty bourgeoisie.

Seán Ó Riain situates Ireland's economic growth in European politics and cautions us in thinking that Ireland's success can be extrapolated elsewhere precisely because some of its success has resulted in conflicts with the EU and is reliant on UK and US, making it sensitive to modern populism movements, despite their lack of them within its borders.

Besnik Pula shifts our attention to Central and Eastern Europe and two emerging concerns in Hungary: the recent legislation that would directly and negatively affect the Central European University and the rise of right-wing extremism cloaked in racism and xenophobia across the region. He urges us to understand these events, not as tied to the region's postcommunist transitions, but rather, as linked to broader European politics.

Populism as Collateral Damage: Opportunities for Comparative Analysis

Mabel Berezin
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Populism Rising

I completed a book on Italian fascism (Berezin 1997) just as Jean Marie Le Pen and his right wing National Front party began to gain traction in France. Colleagues encouraged me to take a look at contemporary right politics. *Illiberal Politics in Neoliberal Times* (2009) was the result of that "look." Initially, I was skeptical that I would find much of interest in the contemporary right. The collective memory of World War II coupled with post-war affluence and an expanding European Union had relegated a serious right presence in Europe to the proverbial dust bins of history. In addition, an academic consensus had settled on the causal claim that immigration and xenophobia explained what might be described as a right wing revival.

After my first trip in 1998 to the National Front's annual *Fete des Bleu-Blanc-Rouge* held in a park on the outskirts of Paris, I changed my mind. Xenophobia may have been necessary but it was certainly not sufficient to understand the right wing impulse. I decided to take a more historical approach—what I describe as a comparative history of the present. I looked at temporality, rhetoric, institutions and mapped how events in France, Europe and beyond contributed to the emerging salience of the right. My methodological tilt allowed me to argue that the rise in support for the National Front was a form of collateral damage resulting from economic, political and cultural recalibrations. First, the collapse of traditional economic arrangements without replacements made Europe and globalization an object of right, and sometimes left, wing attack that resonated broadly. Second, the traditional left was in decline and/or no longer represented the interests of its classic constituencies who were experiencing unemployment and social displacement. Lastly, there was an attenuation of "thick security" --the social goods national states had provided such as welfare and labor market protection, as well as the prerogatives attached to citizenship.

The Power of Contingent Events

Unexpected events moved in directions that favored the right. If the sovereign debt crisis had not begun in spring 2009, right wing parties might have remained festering in the interstices of European society and economy. European Union austerity regimes encouraged right populist politicians to express their antipathy to EU in ever more strident terms. Between 2010 and 2015, right wing parties made electoral gains across Europe. Marine Le

Pen began her campaign for "economic patriotism" in 2011 when she took the reins of the National Front from her father. Even in Social Democratic Sweden, the nationalist Sweden Democrats achieved a vote share of 13% in the 2024 Parliamentary election.

The year 2015 represented a turning point. In Paris, the year began with the murders at the *Charlie Hebdo* offices and ended with the terrorist attack in the concert hall and cafes. The drama of the Greek debt referendum and the Syrian refugee crisis punctuated the time in between. Terror, austerity, refugees provided vivid images and public narratives that the European the right seized upon to make sure that citizens heard their voices.

The End of Populism?

Social scientists and media viewed the recent French Presidential election as the apogee of French and European populist politics. The "whole world was watching" on Sunday, May 7, when Marine Le Pen leader of French National Front lost the election. Neo-liberalism and globalization are well honed pejoratives across the French political spectrum. Yet, the French elected Emmanuel Macron a former Rothschild banker to stave off a neo-nationalist threat. Macron won with 66% of the vote. Twenty five percent of the French stayed home—the highest abstention rate since 1969. Another 8% cast "spoiled" ballots as a signal of protest. Macron has a fragile mandate.

Invoking the electoral setbacks of populists in Austria and the Netherlands, media narrated Le Pen's loss as signaling a halt to the surge of populism that had swept Europe and the United States in 2016. The "end of populism narrative" is misguided on two fronts. First, populism—I prefer the term extreme nationalism—did not just suddenly emerge. Populist parties have been a constitutive, if not always salient, feature of the European political landscape throughout the post-war period. Second, populism is not going away anytime soon—even if populist challengers lose elections—because the problems that austerity, migration and security have generated remain unsolved. Marine Le Pen's May Day address and her concession speech identified the struggle between "patriots and globalists" as the tension that will define French and European politics moving forward. If we substitute nationals or locals for "patriots," the Le Pen's predictions are not so far off the mark (Berezin 2015).

Going Forward

Populism in all its varieties raises a host of political and social questions that are amenable to the research concerns of comparative and historical sociologists. The populist moment in Europe and beyond requires a broad rather than a narrow analytic focus. As comparatists, we should focus on country by country variation as well as national susceptibilities to left versus right forms of populism.

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Opportunities for research abound. We need finely grained historical and contemporary case studies. With exceptions (Riley 2010; Mann 2004), comparative historical sociologists have tended to shun studies of interwar fascism. We also need studies that look to the 1920s and 1930s, as templates for the present (Eichengreen 2015). We need work that thinks about how to design policies that create new sources of thick security, social solidarity and resilience in a world that is radically altered technologically, demographically, and geopolitically not only from 1933 but also from 1968 and 1989.

On the contemporary scene, we need more detailed ethnographies and cultural accounts as exemplified in the research that Dorit Geva is conducting on the National Front; Virag Molnar's work in Hungary, and Cynthia Miller-Idriss's forthcoming book on Nazi clothing in contemporary Germany. We also need to explore non-European cases such as Robert Jansen's work on Latin America and Bart Bonikowski's work on the United States.

Sociologists tend to study movements and politics that they like and exclude movements and politics that they do not like such as fascism and populism. The current populist moment which is global in its reach demands that we turn our analytic attention to groups that are not our political soul mates.

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The F-Word and the French National Front

Dorit Geva
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The radical right-wing French National Front is changing French politics as we know it. It is not only a populist party, but is furthermore forcing a tectonic shift in the French political landscape. It will become a part of government, and is inserting into French democratic politics a reactionary force enabled by the decline of major party blocs and a sclerotic state managed by incestuous elites. While many have called the party "fascist" since its creation in 1972, it is only in recent years that socio-political formations in France have started to bear an uncomfortable resemblance to 1930s Europe.

Marine Le Pen's presidential campaign is a campaign for June's National Assembly elections. Current projections for parliamentary elections predict that the FN will shift from its current low number of two National Assembly members, to at least sixty members of parliament. Marine Le Pen herself could still stand for the National Assembly elections.

I started fieldwork on the French National Front (FN) in 2013, one year after Marine Le Pen had become party president. Four years ago I could observe at national FN events how working class and petty bourgeois groups from diverse geographic regions were getting accustomed to one another as members of the same political party. Whereas the FN used to be a petty bourgeois party centered in the south-east of France, by 2013 the FN was evidently pulling in new sectors of the French electorate.

One of the FN's strategies since Marine Le Pen became party leader has been to transform the party from a radical-right protest party, to a more genuinely populist party. This populism encapsulates some classic aspects of radical-right populist repertoires, including speaking for "the people," equating "the people" with an ethno-national body, criticizing elites, and forging strong personal relations between the leader and her followers. As a female populist, Marine Le Pen can especially position herself as a woman caring for the nation and embodying the people's will.

Marine Le Pen's strategy, however, has been not only to transform the party into a populist party. She has furthermore capitalized upon disintegration of the post-war centrist party blocs and has reoriented divisions within the French electorate. The FN now combines the disaffected working class, former communists, farmers, petty-bourgeois voters, and some segments of the bourgeoisie who self-identify as pro-market "sovereignists." These groups share a belief in a strong Jacobin state. Whereas they used to be political rivals, they are now members of the FN family.

At an FN gala dinner I attended in February 2017, I was seated at a table of young and educated assistants to FN Members of the European Parliament. They had Masters degrees in subjects like public law or economics, and spent part of the evening debating who was right about an obscure aspect of German central bank policy. The person who lost the argument was supposed to buy the table a bottle of champagne. Five years ago, most of them had voted for the centre-right presidential candidate, Nicolas Sarkozy. Towards the end of the dinner, one young man took out his iphone and photographed some of the tables around us. He grinned and said to me, "You see, we're one big family here."

Centre-right and centre-left elites, which have governed France from the start of the French Fifth Republic (founded in 1958), have been handmaiden to the persistence of the European Union as, at its core, a free-trade zone, and now an austerity zone, while citizens have little sense of democratic representation. It does not help that the French system of higher education produces a center-left and center-right elite who inhabit a tight social space. The desire to implement Blairite labor and finance reforms aimed at flexibility rather than democratic representation, and an inability to re-imagine social redistribution and new forms of social solidarity under conditions of late capitalism, have led to an inability to engage in serious reform and a feeling of deep electoral discontent.

As of June the FN will have a significant presence in government through perfectly legitimate democratic means. We will never see an exact repetition of the conditions identified by scholars like Barrington Moore Jr. (1966) which resulted in the emergence of fascist regimes during the twentieth century. But certain features identified by Moore and others, such as elite class alliances creating a wedge against state reforms, the emergence of a nationalist xenophobic party entering parliament through democratic choice, and a heterogeneous class alliance represented by a boldly strategic reactionary party seeking a strong leader and state (see Iordachi 2010, Mühlberger 2016), are worryingly similar to the present.

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Ireland in Europe: Best Child in the Class or Canary in the Coalmine?

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Things seem to be looking up in Europe. Macron's victory in the French Presidential election was the latest in a series of electoral setbacks for more 'populist' candidates. Meanwhile, the European periphery shows signs of recovery from the crisis, with Ireland leading the Eurozone in economic growth for a number of years.

However, supporters of the mainstream European project would do well to treat such positive signs with caution. Which French election speaks to the future of European centrism and populism? The first round, where the 'outsiders' of Left and Right took about half of all votes, or the second round where the almost prototypical modern centrist Emmanuel Macron took around two thirds of the total? What lessons should be drawn from the Irish recovery, its political moderation, and persistent strong support in opinion polls for European integration and immigration? Perhaps Ireland is a lesson in centrist management of the tensions in the European project, walking the tightrope of debt to re-establish steady economic and employment growth?

However, the lessons of the Irish experience are more complex – and both more reassuring and more challenging for the European Union and its future development. To understand this requires a brief detour into the European model itself. While this is normally associated with the welfare state and Keynesian macroeconomics, there was also a very strong supply side and productivist element to Europe's export-oriented economies. Strong public services, unifying social protection and social investment, combined with strong unions to operate high productivity workplaces – all supported by high rates of business investment. An egalitarian productivity coalition was at the heart of the European model – especially in the Nordics but also in the Continental Christian democracies. This was crucial to the politics of equality – the welfare state was only left with a certain amount of redistributive work to do, and in any case was redistributing to people in relatively similar economic situations.

What does this mean for the Irish experience? Economically, Ireland has seen genuine economic and employment growth. A significant element in this has been the re-animation of the flows of foreign investment, particularly from the US. Important as it has been, this is hardly a model that can be emulated across the Eurozone – and indeed it has generated significant tensions between the EU and Ireland, for example around Apple's minimal tax payments. But there are other aspects that don't fit the prevailing narrative – while domestic demand was hit hard by the crisis and austerity, other parts of the domestic economy did better. Irish owned exporting firms, boosted by state enterprise supports, added more employment than foreign firms – typically trading with the UK and the rest of the EU. Even the construction sector added many new jobs, in a process of commercial real estate development that was heavily managed by the state. While fiscal stability and cost competitiveness mattered, access to international investment and an activist state policy have been central to Irish growth – Ireland has linked together connections to the US and UK with elements of the classic developmental European model to generate its recovery.

Furthermore, politically, Ireland has seen comparatively little anti-immigrant feeling and has no serious right wing populist party or movement. This is partly due to the historical dominance of centre right parties in mainstream politics. However, it is also due to the particular configuration of politics in recent decades where the "populist niche" has been largely occupied by left nationalist Sinn Féin, a variety of smaller left parties and independents. These parties have significant local organisations and have attracted the support of the working class voters whose support has been crucial to the growth of right wing populism across Europe, in the process helping to obstruct the emergence of anti-immigration platforms.

The Irish polity has pragmatically navigated its unique international position – what Joe Ruane calls the "multiple interface periphery" – helped along by the absence of a populist right. But this political conjuncture may not be enough to protect Ireland. Domestically, Ireland is characterised by polarisation in the economy, with high levels of market income inequality and deep divides between managers and professionals and the rest of the workforce.

For both domestic and external reasons, Ireland may need the European project to revitalise itself, just as much as the countries racked by political turmoil in the core.

Investments, public and private, remain anaemic and unequal. Ireland's adoption of the classic European model is dangerously partial.

Furthermore, external developments are making this an increasingly urgent issue. As Trump pressurises US firms around tax and trade issues, Brexit threatens the primary market for Irish owned firms and the EU retreats from a regional investment strategy just when it is needed most, Ireland's "multiple interface" strategy could unravel very quickly. Ireland may ironically end up suffering more than other countries from the rise of populism. For both domestic and external reasons, Ireland may need the European project to revitalise itself, just as much as the countries racked by political turmoil in the core.

Is De-democratization the Future of Central and Eastern Europe?

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On April 4th, the Hungarian parliament approved legislation that, if implemented, would directly impede the operation of Central European University (CEU), one of Central and Eastern Europe's top academic institutions. The move drew an expected uproar from academic communities in Europe and the United States and sparked a movement inside Hungary to defend the university. Many critics have argued that what is transpiring in Hungary is an effort by the government of Viktor Orbán to undermine an independent academic institution in ways that are consistent with the pattern of attacks against democratic institutions and civil liberties he and his Fidesz party have been carrying out since their coming to power in 2010.

Are Hungarian trends indicative of a broader movement towards de-democratization in Central and Eastern Europe? I use de-democratization here in Charles Tilly's (2007) general understanding as a process by which governmental policy and decision-making is increasingly less bound by binding consultation with citizenries, and governmental subjects are increasingly less protected from arbitrary action from governmental agents. Indeed, the question is important not only from a normative concern for civil liberties and democracy. In the reigning theories of postcommunist democratization, Hungary was often championed as one of the region's great success stories. These theories are at a loss for explaining, yet alone having foreseen or predicted, recent trends in the region.

Freedom House, which tracks political developments in each country, shows that since 2007, democracy scores in the region have remained relatively steady, though in addition to Hungary, Poland, Bulgaria, and Slovakia have also seen declining scores (see chart). Others, like Czech Republic and Slovenia, have experienced slight declines. Hungary, however, has been the only country in the region to have its regime status downgraded by Freedom House, from a “consolidated democracy” to a “semi-consolidated democracy.” More recently, troubling developments have taken place in Poland, another one-time leader in postcommunist democratization. Under the current ruling Law and Justice party (PiS), Poland has also seen policies undercutting independent institutions and efforts to undermine and discredit opposition and civil society.

Another concerning development is the apparent rise of right-wing extremism in the region, with racist, xenophobic, and homophobic positions sometimes espoused by mainstream politicians. Freedom House notes the rise of violent extremism in the Czech Republic, Slovakia, and Bulgaria, in particular the emergence of vigilante groups attacking Middle Eastern and North African refugees who transit through these countries to seek refuge in Germany and other affluent economies. Both the Czech president Miloš Zeman and the Slovak prime minister Robert Fico have made statements deriding Muslims and have vituperated against the European Union (EU) for its efforts to admit more refugees. In sending such messages of xenophobia and Islamophobia, they have joined in unison with the choir of the populist-nationalist right in Europe. Some countries in the region have seen the rise of a new generation of xenophobic and ethnonationalist parties, including Jobbik in Hungary, Kotleba in Slovakia, and Ataka (Attack) and the National Front for the Salvation in Bulgaria, all of which have garnered seats in their respective national parliaments.

Given the region’s history, it is tempting to describe these troubling trends as the symptom of incomplete postcommunist democratization and the return of nationalist ghosts from the past. The trends, however, cannot be interpreted outside of the context of the political and economic crisis that has afflicted the entire EU, marked economically by punishing austerity policies and politically by the rise of xenophobia and right-wing populism across the continent. In Central and Eastern Europe, neoliberal globalization has taken place under the guise of the postcommunist “transition to the market,” but in reality performed through the region’s rapid incorporation into FDI-driven transnational production and financial networks. The benefits of this transformation have been highly uneven both cross-regionally and domestically, leading to a sense of disempowerment and anger among those who find themselves on the losing end of the long postcommunist political and economic transformation. Given the newness of democracy itself, such publics are more likely to associate perceived social and economic ills with malfunctions of democracy itself. As early as 2012, the European Social Survey found 13 percent of respondents in northwest Europe dissatisfied with the way democracy works in their country, while in Central and Eastern Europe this number stood at 32 percent.

> ***Finding scapegoats to blame for the new insecurities of neoliberal globalization is the specialty of ethnonationalist parties who fill the void left by a sclerotic EU and the political ineptitude and unimaginativeness of mainstream parties who for a long time saw their chief task to be doing the local bidding for Brussels.***

Finding scapegoats to blame for the new insecurities of neoliberal globalization is the specialty of ethnonationalist parties who fill the void left by a sclerotic EU and the political ineptitude and unimaginativeness of mainstream parties who for a long time saw their chief task to be doing the local bidding for Brussels. In Central and Eastern Europe, such scapegoats abound: from imaginary existential threats from Islam and/or refugees, to local corrupt politicians, ex-Communist apparatchiks, former spies and informants of the secret police, the liberal and cosmopolitan intelligentsia (such as those housed by institutions like Budapest's CEU), and civil society, particularly organizations supporting women's reproductive rights and advocating for the LGBTQ community. These alleged malefactors are all out to steal the nation's wealth, surrender its sovereignty, and destroy its traditional values and morality. Curiously enough, such symbolic attacks typically spare large transnational corporations, who do indeed own and control much of the region's productive capital. Even Hungary's Orbán has taken great strides to ensure that his rhetoric and policies do not offend the German, Dutch, American, South Korean, and other foreign companies that make up the most important sectors of Hungary's economy. Orbán may be ahead of the curve in forging a new nationalist-authoritarian accommodation with neoliberal globalization that may offer a model for others to follow, but the elements of this new political-economic structure are yet to fully coalesce, and are still not adequately understood.

This is not to paint an overly grim and pessimistic image of political trends in Central and Eastern Europe. The threats of a deepening trend of de-democratization are real, but the outcome is not inevitable nor the trend irreversible. As the protest wave sparked by the attack against CEU demonstrated, responses to, and resistance against, both de-democratization and illiberal politics have emerged. Last October, a massive, women-led protest wave in Poland forced the PiS government to rescind a proposed measure to ban abortion. In the Czech Republic organizations supporting refugees have fended against popular stigma and even violence from right-wing extremists to support and aid refugees. In Slovenia, a new radical left movement, the United Left, successfully competed in parliamentary elections, garnering as many votes as the mainstream Social Democrats. In recent months Hungary has seen a proliferation of new liberal and left parties seeking to challenge Fidesz and shake up Hungary's party system in the 2018 elections. Many other such examples can be found across the region.

The potential for solidarity and progressive change is there. Yet in the wider view, the region's fate, both economic and political, is deeply tied to the fate of Europe as a whole. As much as a shadow of its variegated authoritarian pasts, Central and Eastern Europe is both subject and party to efforts aiming to remake Europe's political economy and place the EU project on a new, more inclusionary footing. However, should these efforts ultimately fail, and Europe remain in the thralls of austerity, low growth, and in the continuing grip of political sclerosis, the echoes of de-democratization and illiberal politics will likely be felt beyond the hallowed halls of Budapest's CEU to universities, parliaments, cabinets, and courthouses throughout the region.

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Book Symposium

The Antecedents of Censuses from Medieval to Nation States

Changes in Censuses from Imperialist to Welfare States

How Societies and States Count, Vols. I & II

Palgrave Macmillan

Rebecca Jean Emigh, Dylan Riley & Patricia Ahmed

Editor's Note: The following text is based on author-meets-critics sessions held at the annual meeetings of the Social Science History Association, the Pacific Sociological Association, and the California Sociological Association, as well as an event organized at the University of California, Los Angeles. My thanks go out to Daniel Hirschman, Mara Loveman, Cristina Mora, Jacob Foster, Tong Lam, Corey Tazzara, Jean-Guy Prévost, Emily Merchant, Rebecca Emigh, Dylan Riley, and Patricia Ahmed for contributing their comments to the newsletter. -MJB

Comments on *Antecedents of Censuses* and *Changes in Censuses*

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Thank you for inviting me to comment on this two-volume work by Emigh, Riley and Ahmed (henceforth ERA) on *Antecedents of Censuses* and *Changes in Censuses*. I am, by training, a historical sociologist and science studies scholar. Most of my work has been on the history of national income statistics, a very different sort of official data production effort, and it was delightful to take the amorphous set of concerns I carry with me through that work and try to think along with ERA as they travel through the history of censuses in the US, UK, and Italy across nearly a millennium.

ERA state their main argument and contribution clearly and forcefully. They begin with an old problem: the relationship between the state and society. For ERA, censuses are a useful site to explore the dynamic relationship between states and societies. They produce a kind of "circular flow" model of state-society interactions, which turns our attention to the relationships between different levels of society and state (micro, meso, macro), and their direct and indirect influences on one another.

ERA use this model to dispute what they see as a heavy state-centric bias in the literature on official statistics in general, and censuses in particular, and offer a counterpoint society-centric approach. This approach is meant to be used in parallel with the state-centric approach, rather than a pure rejection of it. They summarize the state-centric approach this way: "information gathering starts with states' administrative structures, its bureaucrats develop techniques to collect information, individuals report information according to their specifications, and as a result, the states' categories become widespread throughout social institutions and structures as the information is used and disseminated." (*Antecedents*, 30)

For their alternative, they state: "Our society-centered perspective is analogous, but the directionality is reversed: information gathering originates in society and social institutions, social actors press for information activities to be conducted, state actors implement these requests, and the information collected changes the state and its institutions. In short, societies influence states through information gathering." (Antecedents, 30)

This society-centric approach, they argue, allows them to correct five mistakes of the state-centric approach. First, state-centric approaches "exaggerate the correlation between state power and information gathering" (*Antecedents*, 11). That is, stronger states don't always collect the most information. ERA argue that the US state was relatively weak in the 19th century, but produced a stronger census than the UK.

Second, state-centric approaches overstate the capacity of the state to "impose novel categories" and thus to extract information based on those categories. In contrast, ERA argue that states are heavily reliant on existing folk or lay categories. Following from Gramsci, they argue that information gathering "must be based on common sense" because the information must be gathered from the populace at large (*Antecedents*, 25). I found this to be perhaps the most compelling and broadly useful contribution of the book—at least for my peculiar concerns around state economic data collection, and I will be thinking about how this dynamic works in macroeconomics for some time.

Third, state-centric approaches overemphasize the power of state bureaucrats to set the agenda for developing and implementing censuses. Intellectuals and experts, for example, influence the kinds of questions asked and the purposes for which censuses are seemingly produced.

Fourth, state-centric approaches downplay the power of social movements and other powerful societal actors to influence information gathering. In particular, social movements may facilitate or block the implementation and thus success of a census.

Fifth and finally, state-centric approaches assume that censuses reflect the current intentions of the state at the moment of implementation. That is, these approaches ignore both the intentions of intellectuals and social movements (which may shape censuses) but also historical path-dependencies. Censuses are constrained by past censuses; information which has been gathered before is easier to gather again.

Looking through their long historical lens, ERA identify three modes or styles of census: extractive, descriptive, and interventionist. The first censuses (or census-like efforts) were tied directly to taxes, and these censuses existed to help the state extract resources from society. Descriptive censuses came next, as states learned to see their populations as a source of strength and prestige. Finally, interventionist censuses participated in various forms of *government* in the Foucauldian sense, the management of populations to promote health, growth, and so on. These categories all foreground state intentionality, but ERA demonstrate throughout that in all three (loosely defined) eras, states were heavily constrained by society and census practices routinely reacted to society rather than leading it.

So, that's a sketch of the theoretical framework and contribution. The details are…well, what you'd expect from a thoroughly researched 500-page two-volume work on the history of censuses. I will not focus much on them here, but whatever you make of ERA's theoretical contributions, these chapters will be incredibly useful to scholars who want to understand the broad outlines of the history of censuses. Instead, seeing as this is an author meets *critics* feature, I'll turn now to some of my concerns with the book—which also focus largely, though not exclusively on the theoretical setup.

My biggest concern is the binary around which the entire argument is framed: state vs. society. This binary tends to overly dichotomize and reify two very blurry entities. As we have learned from a line of research by state theorists in sociology and political science like Timothy Mitchell (1991), Elizabeth Clemens (2006), and most recently Damon Mayrl and Sarah Quinn (2016), the state is a heterogeneous mishmash, and the boundaries of the state are actually not clearly determined. The state is an "effect" in some important sense, a product of various kinds of work done to demarcate what is and what is not the state.

For example, Rebecca Emigh teaches at UCLA. Is she a part of the state? If you look around online you can find a wonderful map showing the highest paid government employee in each state in the US.[1] In almost every state, the highest paid employee is a football or basketball coach at a public university. And yet, when we think of "the state" we do not think of Rebecca, or of the University of Michigan's football coach Jim Harbaugh, but of the central government and perhaps state governors and state legislators. And yet, this is in some sense a cultural artifact, a particular settlement of a kind of ideological dance about what counts as in and outside the state. But if we acknowledge that the boundaries of the state are porous and contested, we must acknowledge that concepts like "state strength" are similarly fuzzy. I'll come back to this point in a moment, in regard to the historiography of the US state in particular.

Just as "the state" is an empirically underdefined object, so too is society. A line of conceptual history by scholars like Howard Brick (1996) and Mitchell Dean (2010), among many others, traces the lineages of "society" as a muddy concept, one which can variously oppose "nature", "politics", "culture", "the state", and more. The modern concept of society in fact post-dates the earliest censuses discussed by ERA. Science studies scholars might even go so far as to say that sociotechnical devices like censuses help to construct ("perform") "society" as an object of knowledge and intervention. That is, our conception of "society" is in part constituted by practices like censuses (in the 18th-19th centuries) and more recently practices like polling and survey data. So there is some danger of anachronism and all that brings. And that's also worrying for an ontology that presumes "society" to be a relatively stable object that can be contrasted with "state" to ask, which wins in a fight? Who leads and who follows?

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For traditional sociological debates about the state on the other hand, "society" seems to include everything that people do that isn't the state – I think? But as we can see, this is not the only possible starting point, nor the only possible ontology to underlie an analysis of the history of censuses.

To summarize here, given the centrality of the state-society binary to the text, I would have liked to see—and would like to hear—an articulation of what exactly those two terms mean, what exactly those two objects *are* for ERA, and how they grapple with these recent debates about the contested nature of the state and the historicization of state and society as social kinds.

A related concern focuses on the history of the US state in particular. As scholars like Bill Novak (2008) and Brian Balogh (2009) have argued, the American state was not actually as weak as previous historiography would suggest. Instead, they argue, the US state was uniquely good at "governing out of sight", that is, at exercising power without manifesting that power as "the state." This kind of insight has been picked up extensively by political scientists like Suzanne Mettler (2011) who argue that the US has a kind "submerged state" all the way up to present, which masks the extent of state influence in economic and social life. I raise this point because *if* we reject the idea that the US state was weak, it might undermine ERA's claim that state strength is uncorrelated with state information gathering. I'm not sure I would go that far, as I found the claim largely compelling, but I would at least like to hear their response on the issue, and in particular, how they would (having defined "the state" already) now define "state strength" in a way that grapples with these difficult issues of the blurry boundaries of state action.

Finally, a few smaller or more particular concerns.

First, I'm not totally convinced that the state-centric approach to understanding official information gathering is quite as hegemonic as ERA claim. As an STS scholar, my training tends to lead me to the opposite sort of bias: to foreground the work of experts, and the internal debates within fields like sociology, demography, or economics, as the main drivers of statistical constructs. This is not a critique of the actual findings so much as a question about whether it should have been quite so surprising given what we know about the power of experts to produce and promote sociotechnical tools.

Second, a minor point of contention about Foucault. ERA lump Foucault in with the state centric approach. I think this might be a misreading. If there's one thing we're supposed to learn from Foucault's approach to politics, it's "to cut off the head of the king." That is, to *decenter* the state from our analysis of the workings of power. So I'm not convinced about the reduction here of Foucault to a state-centric theorist. This connects to my next quibble.

Third, I think Foucault fits awkwardly because the books are much less concerned with the *consequences* and *uses* of censuses than they are with censuses as an outcome or index by which we can measure and view the interplay of state and society. So, we get very little on how, say, racial data are *used* following the 1980, 1990, and 2000 US censuses (just a sense that they are used for distributing resources and are thus worth fighting for). But in the work of scholars like Michael Rodríguez-Muñiz (2015) we can see the *effects* of that census data as it swirls out into academic and political discourse and mingles with other tools, like population projections (one subject of Emily Merchant's [2015] work). I think the Foucauldian approach would be to trace those swirling circulations and to thus decenter the state in that way—which is not ERA's project! And on some level, that's fine. But I do worry that we might miss some of the important interactions and feedback loops by focusing so much on the contention around *who* gets to ask questions and *what* they ask and not enough on how the data actually collected then circulates.

Finally, and related to this last point, the books focus very single-mindedly on censuses. And yet, I think this focus may weaken their ability to make some of their strongest theoretical claims that relate back to the broader question of states' capacity to *gather information*. Especially in the 20th century, censuses ceased to be the only information game in town. With the rise of macroeconomic indicators, social surveys, and myriad other forms of data collection, the census – while still tremendously important – no longer plays the role it might have in 1850. Because the state is not a monolith, and the census is not its only form of information gathering, it's possible that the census itself may not be a great index of "information gathering" power, especially in the 20th century, in which case the refutation of the simple correlation between state strength and information gathering may not hold up quite as strongly as the book indicates.

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Following from recent state theory, we might ask if some state actors strategically choose to emphasize forms of information gathering that are viewed as most acceptable and potentially least "state-like" while achieving the necessary information gathering. Think here of the NSA and its PRISM program—what better example of government out of sight! At least, until it was made visible. But even then, because it did not involve asking individuals to reveal anything about themselves, but rather gathered such data passively, it was difficult to explain to a public trained on thinking about "data collection" as something that happens in survey forms and phone calls from nosy pollsters. To what extent would putting the history of censuses in dialog with the history of other forms of state information gathering reshape our understanding of the dynamic relationships between state and society in the process of official information gathering?

Endnotes

1. http://deadspin.com/infographic-is-your-states-highest-paid-employee-a-co-489635228

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Comments on *How Societies and States Count*

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This major two-volume work represents a significant contribution to the history and sociology of official statistics. It also makes a provocative theoretical intervention that challenges us to think differently, and harder, about how we analyze states, societies, and their interactions in historical and comparative perspective. In contrast to what they term "state-centric" accounts of census development, and on the basis of exhaustive primary and secondary research on three regional cases across centuries (regions that became Italy, the United States, and the United Kingdom), the authors argue that early states were dependent on non-state actors to collect and categorize information about populations. Their research and analysis demonstrates that lay intellectuals outside the state played a decisive role in the historical development of censuses as instruments of state power.

In the first volume, *Antecedents of Censuses: From Medieval to Nation States*, the authors adopt a Gramscian perspective to challenge the prevailing view of census-taking as a top-down administrative, political, and cultural imposition on subject populations. The authors develop a general theoretical model and a series of historical arguments that privilege a focus on the relationship between non-state and what we might call incipient, or proto-state, actors as pivotal for the development of censuses. More specifically, they argue that the nature of interactions between actors they call "lay intellectuals" of various stripes and agents of nascent state bureaucracies explain important things about censuses –like when and how they started, what categories they used to collect and sort information, and how socially useful was the knowledge produced.

A central argument that runs through *Antecedents of Censuses* is that power configurations at the local level between elite and non-elite social actors shaped census content and practice, with lasting consequences for official knowledge production. In *Changes in Censuses from Imperialist to Welfare States*, the authors document some of those lasting consequences by tracing the histories of censuses in the modernizing and then contemporary United States, Italy, and England. The comparative case studies from the mid-19th century to the present show the path-dependent effects of early state-society interactions on later censuses. But the careful historical case studies also show that state-society interactions were (and are) themselves at least partially politically and socially contingent. Census development did not always proceed in a linear fashion toward better, more thorough, more useful knowledge production. In *Changes in Censuses*, the authors elaborate and refine the general theoretical model proposed in *Antecedents of Censuses* in relation to their case studies to present a "fully interactive" state-society explanation of modern censuses. Taken together, this extraordinarily ambitious, two-volume work is a significant contribution to the literature on historical state formation that forces a reassessment of existing "state-centric" accounts of census development.

There is no question that one of the great strengths of these volumes is that they challenge us to reassess the tendency to privilege the state as the main historical agent in the development of modern census-taking. Yet this great strength is also a potential source of great frustration. I say this because it is somehow quite difficult, after reading the two volumes, to pin down *exactly* what this reassessment will or should entail.

Many of the arguments made in these two volumes are clearly correct; the general thrust and reasoning behind other arguments are intuitively compelling; the historical case studies are undeniably well-researched and interesting; and the general explanatory model is general enough and flexible enough to be somewhat hard to argue against. And yet, I find that I am still left with a long list of questions that, to my mind, need to be clarified or elaborated in order to fully appreciate the challenge posed by these works to existing scholarship on census history and historical state formation.

In the present context, I will mention six questions, without too much elaboration, and I will leave it to the authors to decide which of these we might want to discuss more deeply in the discussion.

(1) The authors suggest that if the state-centric perspective on census history is correct, then generally speaking, strong states should be able to conduct censuses while weak states cannot (p.15). The cases examined in these works do not support this prediction and this fact, it is suggested, provides evidence against the state-centric view. This is of course a simplified version of the argument that comes from the introductory pages. In other places in the texts we find more nuanced variants of this main argument. Whether in its simple or nuanced form, however, a key question that needs to be answered before we can assess this argument is: how is "state strength" conceptualized and operationalized in the historical analyses in these two volumes?

It seems in several places that the authors' argument is invoking a definition of state strength akin to Max Weber's concept of coercive power or to Michael Mann's concept of "despotic power"—power of an autonomous state *over* society. But as Mann and others have shown, states can also be strong through the development of "infrastructural power"—power *through* society. This latter notion seems more in keeping with the general state-society perspective advocated by the authors. Arguably, it also seems more in keeping with the concept of state strength used in many of the "state-centric" accounts against which the authors position their argument. So my first question is: What is the definition of "state strength" in these works? And why did the authors opt to work with this conception rather than others? Clarification on these points would help to underscore the distinctive theoretical contribution of these works to the existing historical scholarship on the development of modern census-taking and the formation of modern states.

(2) In the state-centric perspective, the authors argue, a common way of interpreting the rise of censuses is to treat them as *outcomes* of state formation. In contrast to this, the authors suggest a more processual and interactive perspective: "We suggest that censuses arose out of an interaction between bureaucracies and social interests. Censuses constituted public, official knowledge not where they were insulated from social pressures but rather where there was intense social and political interaction around them" (p.5). In juxtaposing their perspective to one that treats censuses as

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an outcome of state formation, the authors problematize the linear, before-and-after storyline implied in some state-centric accounts. Curiously, however, their correction seems to neglect the fact that state formation, too, was—constitutively—both a process and an outcome generated by interaction between emergent bureaucracies and social interests.

Rather than refuting the view that censuses were an outcome of state formation, it seems the historical analyses in these volumes demonstrate that state formation itself – just like census-taking—was driven by state-society interaction and is best analyzed as both process and outcome. And if this is the case, then how does the argument differ, exactly, from existing work on state formation, especially work on state-formation from the bottom up? Is the problem with "state-centric" accounts of census development that they have not done an adequate job analyzing *censuses* as interactional historical processes and accomplishments, or is it that they have not done an adequate job analyzing *state formation* as an interactional (state-society) historical processes and accomplishment? Or do the authors believe that accounts of census development that the authors label "state-centric" suffer from both of these analytic deficits?

(3) A related question is whether there is tension between the historical argument, which approaches census development through tracing the evolution of interactive state-society relationships, and the comparative analysis, which seeks to identify and explain variation in "census outcomes" across cases and over time. The authors argue that censuses developed historically through the interaction between emergent state bureaucracies and the interests and activities of "non-state" intellectuals and other lay actors embedded in society. The historical narratives provide ample examples to illustrate this central point. At the same time, however, the comparative analysis seeks to leverage variation over time and place in "census outcomes" to advance more general theoretical claims. Perhaps the seeming tension between the historical argument and the comparative logic could be dissolved through a clearer exposition of what the authors mean when they refer to "census outcomes." Does a "census outcome" refer to the successful administration of a national survey of the population? To the categories that are used in the census? To the social influence of census categories on the population that is enumerated? Or to the general societal importance of the knowledge a census may produce? Does it encompass all of these things (and possible others as well)? And if so, does the general explanatory model work the same across these varied outcomes or is it specific to one or some of these but not others?

(4) A fourth question concerns the argument about the historical trajectories and transformations of the motivations for taking censuses over time. The organization of the chapters and the historical narratives within them suggest that across all the cases, at some point in the nineteenth or early twentieth century, there is a move *away* from primarily descriptive motivations for censuses and *toward* interventionist motivations for censuses (see, for example, p.52 on the UK case between 1841-1931). It is not clear why the authors conceive of the changes in censuses in this period as a move away from descriptive and toward interventionist aims. The historical record could be read instead as evidencing the *addition* of interventionist motives to descriptive ones, or, in some cases, the *fusion* of descriptive and prescriptive aims (as Alain Desrosieres describes in *The Politics of Large Numbers* (2002). Do the authors see these alternative readings of the historical trajectories of motivations for censuses as plausible in the cases they examined? If the historical narratives are revised to incorporate these alternative readings, what are the implications

for the broader arguments about the ways that shifts in state-society relations fuel changes in censuses over time?

(5) The authors argue that “censuses cannot produce knowledge that populations don’t know,” and that “censuses cannot produce knowledge if they do not elicit willing or unwilling collaboration of millions of respondents.” Whether these statements are true or false depends on what exactly the authors mean by “knowledge.” Does knowledge have to be true to count as knowledge? Or does it suffice for those who consume the information to believe that it is true? Historically, censuses have produced mountains of pseudo-knowledge alongside and inextricable from empirically true information about demographic trends. And there are of course plenty of examples of stories told with official statistics that became socially or politically influential, regardless of whether or not they were true. How does the model developed in these books deal with the fact that many early censuses produced knowledge as if it were generated through a comprehensive survey of the population even when the survey actually fell far, far short of the coverage claimed? Or censuses where data were drawn from sources other than the populations they supposedly enumerated? What about the publication of census results where the census was more performance of nation-stateness than actual data gathering enterprise at all?

(6) Finally, I have to ask about the concluding sentence of the book. Volume II ends by advising us all to revisit Marx on The Jewish Question, “to remind ourselves that the origins of our categories of understanding, cruel or benign, lie in the dark recess of civil society, not in the elegant and illuminated halls of the modern state.”

I have to admit this ending left me scratching my head—not because I am perplexed by the idea that our basic categories of understanding originate in social relations (and I would add in social cognition) in everyday life, but rather, because . . . well, what and where is this “elegant” modern state of which they speak, with its brightly illuminated halls? The image certainly does not resonate with the nineteenth-century statistics agencies that I have studied in the Latin American context. And it seems to invoke a much sharper distinction between state and society than appeared in their own historical accounts of census history in Italy, the UK and the United States as well. (As an aside, the reference to “illuminated halls of the modern state” reminded me of a contrasting image that emerged from the archives I worked in while researching the history of census taking in nineteenth-century Brazil. Surviving reports from functionaries who worked in Brazil’s first central statistics agency in the 1870s include bitter complaints about the literal lack of sufficient light to do their work!) This reminded me, as well, that all but a few of those hired to work in Brazil’s small central statistics office in the 1870s were neither non-state census intellectuals, nor dedicated state bureaucrats; they were members of the literate class of urban Brazilian society, recruited *as temporary help* to undertake the tedious work of manually compiling, aggregating, and transcribing the partial and incomplete returns from Brazil’s first attempt to take a national census.

With these 1870s-era Brazilian statistics agency temp workers in mind, I will end by asking whether, by challenging us so effectively to reassess the history of the census and the history of modern state formation through a state-society interactionist perspective, these two volumes end up forcing us to seriously reassess the sharp conceptual and analytic distinction between state and society itself—with the attendant risk of undermining the central analytic distinction upon which the state-society interactionist model at the core of these volumes relies.

Comments on *How Societies and States Count*

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How Societies and States Count is an ambitious feat. There exist individual texts about the census in the US, the UK, and Italy, but never have authors considered them simultaneously. The scale and scope of comparisons across time and place is courageous and the payoff is great. In a nutshell, the authors show that censuses are products of state and society interaction. While others have certainly argued that censuses are political constructs, rather than simply a reflection of pre-existing lay categories, *How Societies and States Count* uses comparison to offer a more complete "interactionist model." Specifically, their model shows how the interplay between state, science, and society happens at the macro, institutional level, at the meso level of organizations, and at the micro level of individuals.

The authors contend that society and state interactions are, however, structured according to different principles. In the US the principle of race, especially the protection of "whiteness," shapes how state-society interactions lead to census classifications. As such, classification efforts in the US have focused on counting whites and distinguishing them from the myriad of other non-whites that have populated in the nation. While non-white categories such as black, octaroon, Mexican, Hindoo, and other minority classifications have been changed over time the white classification, the authors show, has remained ever present (the classification has remained, yet the issue of who is white has certainly changed). In the UK, class has been the underlying principle. Class categories were instituted in part because of a strong labor movement and interests within the industrialist class. For these stakeholders, information about workers was paramount. Last, in Italy the organizing principle has been about place. Italian intellectuals and state leaders viewed the census as an opportunity to demote identities tied to principalities and instead elevate regional and national identification. Hence the state used census data to make claims about the nation as a whole, albeit a whole divided by a northern and southern social rift.

So it has been race, place and class. And the authors develop a society-state interactionist model to contend that states pick up on lay, popular categories through reflection and refraction. In other words, states classify and label society not by imposing categories from above but, rather, by massaging pre-existing, lay definitions and forms of categorization.

Given this, there are three main issues that I considered while reading the texts—they all concern the interactionist model. While I found it to be useful for understanding the broad evolution of censuses, there were several moments when I wondered whether it could be further refined. For example, the authors describe society broadly as everything that is not the state—from political parties, to intellectuals, to elite lobbyists. Yet they generally did not include the role of the market within their comparison. So I was left wondering—what role might the market have played in the construction of censuses? The states in question had more or less different market configurations at different points, but the book said little about whether these market actors played a role in shaping the census—if, not, why? How might making the role of the market explicit complicate the author's state-society interactionist model?

The issue of the market is pertinent because much work has shown that marketers have historically played an important lobby role within the US Census Bureau. Members of professional marketing societies have sat on advisory boards or have held meetings with Bureau leaders at least since the 1930s. Among other things, these meetings helped lead to more census questions about assets and income. In the 1970s, industry members were also present in several Bureau negotiations about the development of the "Hispanic" category. Given this, and given the market's important role within society, I wondered whether and how it might influence censuses across time and place.

Additionally, I thought about how the state-society model could be refined by the type of interaction. There are at least two kinds to consider: conflict and cooperation. Thus, some categories emerge as community stakeholders lobby and force the state to recognize their group classification. Yet categories also emerge with cooperation, as the state elicits support from elite and community members. I wondered whether refining the model to consider the relationship between conflict and cooperation could tell us more about which classifications become institutionalized and which do not.

Last, I thought of the extent to which the availability of other data sources influence the kinds of information censuses will collect. Could it be that religious organizations, market firms, or even local-level municipalities collect data that, in turn, influences the type of information that national census agencies will seek out? The US Census Bureau did not historically collect detailed occupational information, but this type of data could at times be gleaned from other local-level agencies as well as from other federal agencies such as the Department of Labor. Thus I was curious about how the field of data collection influenced census bureaucrats' decision making.

To conclude, I should note that the issues I present are opportunities for future research and further assessment of the contemporary history of censuses – they are not critiques of the larger, broader argument. Overall, I am quite convinced that the interactionist model can best help us to understand the longer sweep of census-taking history. All in all, the *How Societies and States Count* is an ambitious work that brims with clear insight about how states classify, label, and sort populations. Its comparative framework presents a refreshing new step in the study of censuses by revealing how underlying principles shape who the state sees. This is a must read for all interested in data collection regimes and classification struggles.

How Societies and States Count: Five Variations

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In harmony with my colleague Rebecca Jean Emigh's passion for music, I decided to organize my comments on *How Societies and States Count* as a series of "variations" on their theme. To preview the five variations: I will reflect on the authors' use of models (diagrammatic, not statistical) in developing their argument; discuss their salutary treatment of categories as socially-constructed but balky and resistant; highlight their "phylogenetic" thinking, which combines comparative and genealogical methods to give a novel analytic treatment of path-dependence; develop their argument that vibrant censuses are precisely those that become "matters of concern" (in Latour's coinage); and outline how their tools can be applied to contemporary institutions that gather information about and classify the population (e.g., social media platforms), as well as contemporary contests between state, lay, and individual categorization (e.g., around gender).

Living Models

By serendipity, I happened to read *How Societies and States Count (HSSC)* while I was teaching UCLA's required graduate course on sociological theorizing. *HSSC* wonderfully illustrates the heuristic value of models as analytic simplifications. If you are wondering where a computational sociologist like me finds a "model" in *HSSC*, I would direct your attention to the diagrams in Chapter 2 of Volume 1 (*Antecedents of Censuses from Medieval to Nation States*). In fact, I would argue that this basic schematic, with micro-, meso-, and macro-levels cutting across two domains (state and society), provides the theoretical engine that drives the empirical program of *HSSC*. Following the diagram clockwise provides the standard "state-centered perspective on information gathering": it highlights the extractive, administrative, or biopolitical agenda of the state, with society-driven processes providing literal resistance to flows of "state power and classification." Following the diagram counter-clockwise provides the countervailing society-centered perspective, highlighting "mechanisms of social power and categorization."

On the page, the diagrams suggest a strong binary, splitting state and society and treating the former almost as a separate entity (which *seems* dissonant with the authors' stated Weberian orientation; see Poggi 2006). But as the authors (and readers) animate the diagram, turning it one way and then the other, it dissolves the fiction of state as entity; as played by the authors, the living diagram becomes an instrument for emphasizing the co-constitutive entanglements between state and society at all levels, micro, meso, and macro. *HSSC* provides a master class in the subtle art of theoretical counterpoint, playing its two themes of state and society against each other to structure empirical elaborations of dizzying depth and complexity.

Challenging Categories

Following from their co-constitutive understanding of society and the state, the authors of *HSSC* emphasize the foundational importance of lay categories in censuses and similar forms of state information gathering. This stands in stark contrast with the state-centric perspective, of which Bourdieu provides a classic example when he writes: "Through classification systems, the state molds mental structures and imposes common principles of vision and division, forms of thinking" (Bourdieu 1994).

Why do we sociologists believe so strongly in the power of state classification? A certain fetishism of the state is part of our disciplinary tradition, but I think it also reflects a latent disciplinary naïveté about categories: what John Levi Martin calls the "nominalist" tendency of the social sciences (Martin 2015). We usually call this tendency social constructivism: a belief that categories are human artifacts, rather than maps of things in the world. But the constructed nature of categories does not imply that all categories are equally easy to construct, or equally easy to extend to novel situations. Just because categories are constructed does not mean they can be made and remade with ease. Buildings, too, are constructed; but not all buildings are equally easy to construct or to tear down. It all depends on the raw materials at hand.

And now to the "power" of state classification. As *HSSC* brilliantly shows, state classifications that build on or resonate with *existing* social categories will be easier to learn; indeed, this follows from a basic result in computational learning theory, which I am exploring in my own work (see Valiant 2013 and Foster in prep). If we consider the resources required to more-or-less uniformly distribute the categories and practices involved in literacy, numeracy, etc. through state provisioning, it is clear that insofar as the state succeeds in establishing mental structures, this is either because the state brings significant resources to bear for *training* (as in public provision of education) or because these distinctions are embedded and *encountered* continuously in the environment. Note that this is true not just for categories but for information practices (as *HSSC* amply illustrates). Early Italian information gathering (e.g., cadastral registers) relied on established habits of record keeping, notions of property, notions of contract (and things that are subject to contract), etc. Early English information gathering, by contrast, built on practices of oral testimony and notions of rights (to land or labor) rather than ownership.

Moving forward in time, we see censuses in Italy, the UK, and the US reflecting the dominant lay categories of their respective states: place, class, and race. Novel categories are taken up insofar as they become socially prominent and relevant, as with race in the recent UK census. On *HSSC*'s account, novel census classifications (e.g., a panethnic "Hispanic/Latino" category) are embedded in society not through the brute power of state information gathering to impose state classifications; instead, social embedding occurs through the *use of* census data as a resource in various social practices (e.g., the use of demographic projections in current political debates).

Phylogeny as Comparative-Historical Method

HSSC emphasizes the importance of socially-available "raw materials" in state classification. In doing so, the authors provide a concrete mechanism underlying the more commonly invoked (and oft underspecified) idea of *path-dependence*. They are able to do so through an impressive (and time-consuming) union of comparative and genealogical methods. Indeed, comparison gives them a powerful tool against the state-centered approach, as they can exploit convergence or variations in state power to show the constraining role of history. Their mode of analysis goes beyond the usual logic of path-dependence, i.e., the logic of the counterfactual: what might we expect to see, if agents had free play to build the institution that best addresses the needs of the situation. I think the notion of path-dependence is much deeper than that—for there is no true counterfactual. There is almost never a "bare" institution constructed from scratch, and attempts to do so are rarely successful. The problem with the simple idea of the past "constraining" the present parallels John Levi Martin's brilliant takedown of the idea that "culture enables and constrains" in *Thinking Through Theory*: there is no "bare" individual to be enabled and constrained by culture, just as there is no "bare" institution to be constrained by the past (Martin 2015).

HSSC's fusion of comparative and genealogical methods points toward a new mode of comparative-historical analysis, one that can address this more nuanced (and empirically adequate) view of path-dependence. Call this a "phylogenetic" method, to borrow a term from evolutionary biology.

HSSC's fusion of comparative and genealogical methods points toward a new mode of comparative-historical analysis, one that can address this more nuanced (and empirically adequate) view of path-dependence. Call this a "phylogenetic" method, to borrow a term from evolutionary biology. To show what I mean, permit a brief digression. As many readers may know, native Australian mammals are marsupials; they spring from a different branch of the mammalian family tree than the more familiar placental mammals. Yet there are striking correspondences in form and function: there are marsupial "wolves" and "moles" and "anteaters" (Springer, Kirsch, and Case, 1997). What drives this convergence? The interaction of situational, ecological demands and generative resources (in this case, genetic and developmental resources). If you ask the mammalian genome to evolve something that is, functionally, a mole, it turns out there is one "natural" way to solve the problem in terms of gross morphology. The mammalian generative resources "enable" certain solutions to ecological problems, but they also constrain: when the mammalian and the dinosaurian lineages occupy the aerial niche, they use their distinct generative resources to produce distinct solutions to the aerodynamic problem (bats and birds respectively). In other words, based on the resources available to "solve the problem," some forms become inaccessible, a point *HSSC* illustrate with the institutionalization of the census bureau in Italy.

So *HSSC* suggest the possibility of usefully constructing institutional phylogenies. By exploiting variation in high capacity states, in an "ecology" where an effective census might be expected "in principle" (or where, now, it might be "in principle" obsolete), the authors can do more than point to the blunt fact of different histories. They can point to the specific sequence of material, symbolic, and categorical resources available as a function of time; the histories of interaction between social and state actors; the presence or absence (and independence) of census intellectuals; histories of linking the census to elite and popular social interests; and sequential configurations of social power—that together provide the raw materials for constructing and reconstructing these institutions over time.

Matters of Concern

One of the key findings of *HSSC* is that the most "vibrant" censuses—those that are lively, in a certain sense, engaged and politically engaging—are those around which state and society interact intensively. Is this just a tautology? I believe not, once we unpack the meaning of "vibrant" and get to the analytic point being made. *HSSC* makes a persuasive case that successful information gathering (in which the state gathers the information it wants, and society gets the information it needs) is highly interactive, with *both* the state and society taking strong roles. This stands in stark contrast to the standard argument that the only useful information is "high quality" information, and that high-quality information is obtained by a disinterested, depoliticized institution.

When the authors speak of a vibrant census, they mean an organ of the state that *seems* to be doing whatever it is supposed to do, and *seems* at no risk of withering away, precisely because its outputs are widely used and appreciated; as they put it, as a function of "how much the information is used socially." This is a census that is highly *interested* and politicized: a census that is entangled with society. This parallels Fligstein and McAdam's argument that the stability of a strategic action field is related to how "dependent it is," by which they really mean its interdependence with other fields (Fligstein and McAdam 2012). At first blush, this seems paradoxical: wouldn't more interdependence mean more opportunities for outside disturbances or perturbations? And wouldn't a more politicized, interested census generate low quality information?

As a useful contrast, consider first the highly institutionalized Italian census, with its limited interdependence (tied chiefly to industry). This might seem like a good candidate for a "vibrant" census on the typical account: it is relatively "autonomous from other state agencies," "distinctively depoliticized and insulated from non-elite pressure." Yet the Italian census is rather moribund, failing to "transform lay categories." In fact, its institutionalization "weakened the vibrant tradition of interaction around information that characterized the country historically." By contrast, the vibrant US census is highly *entangled* in the field of power, but these entanglements serve to recognize and validate the knowledge produced by the census. Another way of expressing the social use of information: the information capital generated by the US census is more (relatively) valued in the field of power, and can be more valuably interconverted into capital in other fields, like the field of electoral politics, welfare provision, or even financial investment (see, for example, Mora 2014).

In other words, the vibrant census has become a "matter of concern," to use Bruno Latour's phrase (Latour 2004). After all, the census as a social fact can be disbanded, as in the German case. There have been attempts to eliminate it in the UK and Canada. The sturdiness of a census as a matter of concern is a function of how many participants are gathered to make it exist and maintain its existence. Because the US census did not limit its *potential* audience—because it was vouchsafed by the Constitution and the founding ideology of no taxation without representation—it constituted itself as an increasingly stable social fact to which many concerns could be lashed; in other words, as a matter of perpetual concern.

Contemporary Classification

Consider, first, some of the most promiscuous contemporary projects of information gathering: social media platforms like Facebook. Can we analyze these using the tools developed in *HSSC*? I belief we can. First, note that Facebook has constituted itself as a "matter of concern:" society interacts, often intensively, with its projects of classification. For example, Facebook famously expanded its gender categories, but did so interactively, responding to changing lay categories and social demands for more nuanced self-categorization and identification. Just as census engagement with lay categories and concerns makes the project of information gathering more successful, so Facebook's engagement invites further user engagement, and produces more valuable data.

The Facebook case suggests that gender, as a lay category, is becoming increasingly socially relevant. Yet the US census currently gathers information on "the sex composition of the population."[1] Will this change? *HSSC* makes a clear prediction: as categories become socially relevant, as they become sites of political engagement and contestation, they are taken up by a vibrant census (often *because* of engagement by society of this state organ). As North Carolina's infamous "bathroom bill" illustrates, gender is a site of intense contestation around state classification, lay categorization, and self-identification. Precisely because I am persuaded by *HSSC*'s argument, I expect gender to be the next "matter of concern" taken up by the highly engaged, highly engaging US census.

I hope these variations give the reader a sense of the breadth of Emigh, Riley, and Ahmed's contribution. Not only have they provided incisive tools for thinking about key conceptual and methodological problems in comparative-historical sociology (from categories to path dependence); they have made the analysis of historical information gathering speak to subfields as diverse as cognitive sociology and the sociology of science. Perhaps most consequentially, they have provided a new way of looking at a contemporary world rich in projects of information gathering, classification, and categorization.

Endnotes

1. https://factfinder.census.gov/help/en/gender.htm

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The Census and its Antecedents

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I have long admired Rebecca's work on Tuscan agrarian contracts and what she called the "undevelopment of capitalism", and so I was delighted to serve as a discussant for her new books on the census.

Since I am an historian of early modern Europe, I shall focus my comments on the first volume, which deals with the medieval and early modern efforts to count heads—the "antecedents" of the modern census.

Let me begin by confessing that I am deeply sympathetic to Rebecca and her co-authors' project of "socializing" state knowledge, of rooting it in the basic structures of society—and the interests and conflicts of interest that society generates—rather than in any top-down project of epistemological violence perpetrated by disembodied state actors. As much as I would have loved seizing upon specific disagreements to fulfill my role as critic, I found myself agreeing with almost everything in the book. So my comments are really an invitation to enlarge upon some of the issues raised by their book, rather than an inquisitorial attack on their choices.

For many early modernists, the patron saint of state studies remains Michel Foucault and his work on governmentality. Foucault was obsessed with the state's capacity to control—both through knowledge and physical coercion—its populations. His sources were highly prescriptive texts, written mainly by cultural elites for the consumption of state officials, often for the purpose of exacting patronage: they were fantasies of power, in short, and often it seems that scholars have taken them more seriously than contemporaries did.

Instead, what the authors demonstrate is that early fiscal surveys and population censuses were the product of social collaboration in a number of senses. The major categories that oriented census-like inquests were lay rather than state categories: for instance, in medieval England the notion of "rights and privileges" were mostly kept through oral record while in Renaissance Italy exclusive property rights were mostly recorded in paper documents. Later, for the long-nineteenth century, the authors find that "class" mattered extensively for English censuses, versus "race" in the United States.

But it wasn't simply that the categories were relevant to lay society: it is clear from their accounts how much social structure shaped the actual implementation and outcome of census processes, not least because early modern states lacked the autonomous bureaucratic apparatus necessary for gathering fiscal or demographic information. Thus, local gentry mattered tremendously in the English context, as did parish priests and autonomous notaries or lawyers in the Italian context.

Even the decision to take a census in the first place, and the content or goals of the census, were strongly affected by social forces beyond the bounds of the state. I found this clearest in the case of England, where censuses in the early nineteenth century were strongly affected by the public health and morality lobby; and it would not be too difficult to show how such a lobby was shaped not only by Malthusian considerations, but also by those of an activist Protestant Christian conscience. This relatively benign social influence gave way to a more sinister Social Darwinian or Eugenicist turn in the late nineteenth century—although that influence was soon counterbalanced by principles associated with the incipient welfare state. In any case, the larger point is clear: programs for social improvement that emerged outside the bounds of the state nonetheless exerted an important influence on state census projects.

This issue of social influence leads to my first question. Census taking was a Europe-wide phenomenon, deeply rooted in the fiscal and demographic ambitions of state actors, as well as transformations in the Republic of Letters at large. The cases presented in these volumes get increasingly less independent over time, so that by 1775 at the latest, state actors could draw on a knowledge-base that was international and debated among cultural elites across the West. To what extent did the development of political economy as a discipline affect the overall trajectory of census taking?

My second question concerns the generalizability of the authors' findings. Many students and scholars probably still encounter the census through Benedict Anderson's discussion in *Imagined Communities*, which emphasizes not only the state imposition of categories and the state-led reorganization of colonial society, but also the arbitrariness of those categories—it is thus an object lesson in the brutal absurdity of colonialism. To what extent do the ideas and the models developed in these two volumes—which focus on the Western world—apply to the colonized world in the nineteenth and twentieth centuries? Would more detailed research show that forms of social collaboration underlay even imperial censuses, and presumably other techniques of the imperial state? Or is there something to be gained by treating the western and the colonial contexts separately?

Finally, I am interested in the authors' more general views on what is left of the state-centered perspective, especially for the premodern world. The major thrust of their argument is to socialize both the knowledge and the process of gathering demographic information. As I said, that seems persuasive, but it does make me wonder how they would situate the role of states in this process. For example, I found myself thinking about Marx's

The major thrust of their argument is to socialize both the knowledge and the process of gathering demographic information...that seems persuasive, but it does make me wonder how they would situate the role of states in this process.

view of the state as a kind of executive committee for the bourgeoisie. Their model diagrams continue to emphasize the ongoing importance of the state, but I did not see that reflected in the text, at least not until the development of the interventionist welfare-state censuses of the mid twentieth century.

Put another way, the authors want to shift the discussion about the social constructedness of knowledge from the state to society, even when, as with the census, that knowledge is exploited by the state. This seems like a very sensible move to me and is not likely to be controversial among early modern historians, although some modernists might feel differently. What, then, is at stake for scholars insisting on the state-centered perspective? This may not be so much a theoretical as a cultural question: why is it that scholars today still want to hold the state responsible?

Comments on *How Societies and States Count*

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In light of the rise of nativism and xenophobia in Europe and the US, it becomes more important than ever for us to examine the politics of enumeration, and to learn about the possibilities of resisting and disrupting the discriminating policies of the bureaucratic state. In this two-volume study, *How Societies and States Count*, Rebecca Jean Emigh, Dylan Riley and Patricia Ahmed have provided a timely comparative study of the historical census cases in Italy, the United Kingdom, and the United States from an extended period of time. A major accomplishment of this work is its ability to play attention to historical details without losing sight of the larger picture, namely, to develop a model that claims to have predictive power on making sense of the diverse ranges of census practices across time and space. What are the key factors and mechanisms that enable the success of a census? How does a weak state end up collecting more information than a strong state? These and other important and intriguing questions from the book are excellent examples of how historical sociology could help us to make sense of local stories in the wider context.

Just as the authors are interested in striking a balance between social theories and historical data, they also are keen on examining the meso-level of historical processes rather than merely the micro- or macro-level of events. And it is at this meso-level that they show convincingly how the top-down processes encountered those of the bottom-up. This approach, needless to say, effectively reinforces their central argument that both the state and society have historically played crucial roles in developing and implementing censuses. Overall, in spite of its ambitions, the book's arguments are lucid, and the census theory it puts forward is an excellent starting point for scholars who are interested in this topic.

Yet, all strengths notwithstanding, the work is not without limitations. Without doubt, the idea of information gathering often invokes a certain conception of the state in the public imagination, potentially causing hesitation, resentment, and fear. As such, the authors' insistence on critiquing what they call the state-centered approach is well-taken. But many previous scholarly works on censuses have already argued that people are never being counted passively, and they have demonstrated how census categories can also be enabling. In many ways, thanks at least in part to Foucault's analysis of power, scholarship on the census have long moved beyond the state-centered analysis.

In their collaborative work, however, the authors contend that the Foucauldian framework is yet another example of the state-centered perspective. Granted that there may be seemingly unlimited ways of reading Foucault, I suspect that most scholars would at least agree on the point that the Foucauldian approach to power is anything but state-centered. For Foucault, in order for power to exist, it has to be exercised, reactivated, and reanimated through everyday practices. This involves, in other words, the circulation of power outside of state actors and state institutions. In this way, power is always diffused rather than concentrated, and it is always embodied and enacted in social processes by census intellectuals, public health workers, ordinary citizens, and so forth rather than possessed by state authorities. This understanding also explains why Foucault regarded power not as negative and coercive but discursive and productive. People who are being counted, for instance, may embrace the technology of the census for their own purposes. And this certainly does not only happen in liberal democracies. In contemporary China, too, vulnerable minorities often want to be counted as such for the census results could also bring them benefits and advantages.

Indeed, there seems to be a periodic swing between the state- and society-centered approaches.[1] But instead of debating whether we should relegate or elevate the category of the state (and society), it seems to be more productive to think about how the boundaries of the state and society are always mutually constituted, as Timothy Mitchell (1991) has reminded us. Significantly enough, censuses are precisely one of those practices that create the effect of the "state" in relation to "society." This approach would not undermine the authors' argument about the importance of states as well as societies in the making of censuses; it would, in fact, enrich and strengthen their observations.

The authors have done an admirable job of bringing multiple sets of historical data together. Their aspirations to present a somewhat universal model for making sense of census practices across time and space is also commendable. However, for those of us who study the "rest of the world," we rarely would have the confidence to make such a generalization.

Another shortcoming of the book is the curious absence of empire in its analysis. The authors have done an admirable job of bringing multiple sets of historical data together. Their aspirations to present a somewhat universal model for making sense of census practices across time and space is also commendable. However, for those of us who study the "rest of the world," we rarely would have the confidence to make such a generalization. And I suspect this is more than just a disciplinary difference. Instead, we often ask how our "narrow" areas could help to rethink the dominant theories that are mostly generated based on historical cases that are no less narrow and provincial.[2]

More importantly, to insist on the significance of empire in the making of Europe is not to advocate a politics of liberal inclusion. Put differently, it is not about coverage or identity politics. Instead, as Dipest Chakrabarty and others have put it, the project of "provincializing Europe" is to explore new ways of thinking about the past with an acknowledgement that the world has been an integrated whole (Chakrabarty, 2000; Conrad, 2016). In other words, in this intertwined world, national history and even comparative national histories are no longer sufficient. A truly global history, meanwhile, does not only provide a better understanding of our connected past and present, but it can also help to cultivate tolerant and cosmopolitan global citizens.

In the case of censuses, much have been done in the history of enumeration and governmentality in South Asia, East Asia, the Middle East, and beyond. This includes works that show how colonial and semi-colonial spaces functioned as the laboratories of modernity.[3] The empire, in short, always played a vital role in shaping the bureaucratic practices of the metropoles as well as the colonies. Ironically, had the authors paid closer attention to the wider scholarship on the history of enumeration—including the related postcolonial scholarship which critiques and is also inspired by Foucault's work—they would probably come to a very different reading of Foucault.

Still, none of my quibbles should undermine the significance of this ambitious work. *How Societies and States Count* is a noteworthy and timely intervention in the study of census processes. It reminds us of the value of collaborative work. It urges us not to be afraid to compare and generalize. In this era of fear and resentment when certain segments of our society are increasingly being singled out and demonized, a proper understanding the social and political processes of censuses would help us to resist such emerging dark politics. In this respect, this work has offered a much needed starting point for our conversations, reflections, and actions.

Endnotes

1. Decades ago, as the authors also note, some scholars including sociologists (Evans, Rueschemeyer, and Skocpol, 1985) were arguing for the need of "bringing the state back in," and that debate is still ongoing.

2. For example, we were grabbing precisely with this question in a recent collaborative project on the history of science in China and India, see (Phalkey and Lam, 2016).

3. Some of the examples include works by Bernard Cohn (1996), Arjun Appadurai (1996, especially chapter 6), Tong Lam (2011), Omnia El Shakry (2011), Helen Tilley (2011), Omnia El Shakry (2007), and Ann Laura Stoler (2009).

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The Past and Future of Censuses: Reflections on *How Societies and States Count*

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This two-part book is undoubtedly impressive as to the time span it covers—from medieval times to the present—but it is also remarkable for how it puts into play a number of distinctions and concepts: "state-centered" vs. "society-centered" perspectives, "census intellectuals" in contrast to mere "bureaucrats", "interventionist" vs. "descriptive" censuses. Now, one may discuss abundantly the bluntness of categories like 'state' and 'society', the contours of who should be counted or not as 'census intellectuals' and their position vis-à-vis the two latter categories, and so on. In this short comment, however, I will limit myself to examining the heuristic value of Emigh, Riley and Ahmed's effort, beyond the limited number of countries (Britain, the United States and Italy) on which they have built their case. More precisely, I will try to answer two questions: First, can their analysis be extended to other national experiences? In other words, is it susceptible to generalization? Second, can we use it to interpret present tendencies in census taking? To this end, I will largely rely on the authors' concept of the 'interventionist' census.

To begin with, the authors draw a distinction between the descriptive and the interventionist census. While the former "collects information to describe populations", the latter does this in order "to alter these populations" (*Antecedents*, 48). This distinction becomes relevant during the 19th century, which saw population constituted as a category of knowledge, and studies that have highlighted the role of the census in the creation of the modern nation have, even though they may not have used the word interventionist, underlined this dimension. But our authors move to a further distinction, that between "interventionist in intention" and "interventionist in practice" (*Changes*, 21): interventionism is now defined here by "the strong interaction between the society and the state" (*Antecedents*, 51) and by the role the census plays with regard to policy rather than on a symbolic plane (*Changes*, 21). This leads in turn to a scale of interventionism, with post-World War II censuses ranging from 'weakly' (Italy) to 'highly' (the U.S.) interventionist character, with Britain somewhat in between. A highly interventionist census will thus be largely "society-driven" rather than merely "state-driven"; it will be one in which census intellectuals are more influential than census bureaucrats; it will give rise to public debates about its categories and their uses; and it will be one whose results have a consequence in policy terms.

Now, does all this provide some light beyond the three cases selected by our authors? To answer this question, we need at least to find other cases of highly interventionist censuses but also some that exhibit a weakly interventionist character. Canada may offer us a case in point of the former category. In Canada as in the U.S., the census has been set up for the purpose of apportionment (Canada's constitutional act of 1867 consciously replicated the principle that was put down in the US constitution in this regard). This has given rise to struggles around the methods of census taking in the 19th century, but, in the more recent past, it is the apportionment formula (rather than the census itself) that has been the object of debate. But other issues that have had direct political implications in the recent past exhibit more obviously census interventionism: namely language and ethnicity.

In the first case, we have seen minority linguistic groups intensively lobbying for more census information on language and even appealing to the courts on this issue. The Canadian census now includes no less than 7 questions dealing with language (over a total of 65). We have even seen an academic sub-discipline, demolinguistics, developing around the analysis of census results to these questions. The work and arguments of these demolinguists display widely different viewpoints and part of their esoteric vocabulary has found public echo in the media. Since language rights are defined in the constitution as well as in federal and provincial legislation, language statistics, from census results to projections, are a major locus of political debate.

With regard to employment and discrimination, the census includes questions about "visible minorities" and about "disabilities" that have been brought partly by the activism of minorities and intellectuals associated to them, and partly by the existence of the Employment Equity Act, which was adopted following pressures from the same groups. The notion of 'visible minorities' (which is the Canadian euphemism for "race") is a good example of what Emigh, Riley and Ahmed define as a "lay category" (*Changes*, 15) and it has been included in the census following serious resistance from Statistics Canada itself. Another example of lay category that was introduced against the wishes of census bureaucrats is that of 'Canadian' as a valid choice for the ethnic identity question, following a successful nationalistic (and chauvinistic) campaign known as 'Call me Canadian' in the 1980s and 1990s. Other groups that have been active introducing questions in the census are aboriginal peoples. This is clearly an example of an interventionist census and it should come as no surprise that when the Conservative government decided in 2010 to dispense with the compulsory character of the long form census, the outcry it met came not only from census intellectuals but also from a significant part of the population. (The Opposition had promised a return to the *status quo ante* if elected and it did deliver on this issue.) By contrast, calls for boycott of the census have been heard regularly over the years from both the left and right of the ideological spectrum, but with no tangible success.

France provides us instead with an almost contrary example. There, the census is of course used as a tool for determining resource allocation at the local level, but it shies away from any role with regard to two of the most crucial political issues: immigration and the integration of minorities. As is well known, French law forbids—save in certain strictly defined cases—the collection of information about racial or ethnic origins. Moreover, census intellectuals and the various stakeholders involved are radically divided on this issue. No question about language either can be found in the French census. This makes France a clear case of weakly interventionist census and here also, it should come as no surprise that, when it was decided to move from a traditional census model (characterized by exhaustiveness and simultaneity) to rolling samples (each year, 20% of cities under 10 000 inhabitants are completely enumerated, while samples of 8% of households is taken in larger cities), it was seen as a purely technical decision and raised no political discussion whatsoever.

This brings us to the future of the census: in the present world of registers and "Big Data," is the census a technology of the past? Among the three cases studied by the authors, Italy has replaced the traditional census by an annual computer download of data from communal registers, while the United Kingdom has also seriously examined the possibility of doing away with the census in 2021 (though this project has for now been downsized to making better use of existing registers). Overall, there are now 11 European countries that have altogether renounced the traditional census in favour of so-called register-based censuses (a few others around the world have either done it or contemplated it and, for its part, France, as we have seen, has moved to a mix of enumeration and sampling). There is clearly a tendency in that direction, with redundancy in data and cost reduction as the main arguments put forward in support of such a move.

Coming back to our authors' concepts, we may predict that weakly interventionist censuses are more susceptible to such a fate, as indeed was the case of the Italian census, considered as the less "socially relevant" (*Changes*, 19) and most "insulated" (*Changes*, 197) of those under study. We may also predict that in the environment of registers, there will be no place for lay categories; that registers being less flexible than census categories, there won't be much ground for census intellectuals to interact here; and that relevance will have to be found elsewhere than in the census. Pioneers in this direction were the Nordic countries, where ethnic homogeneousness, willingness to comply with data requests on the part of government, and a developed welfare state based on institutional compromise have surely facilitated this move. Conversely, abandoning the census may prove much more difficult in countries where the census is mentioned in the constitution, where mobilization around its content is high, and where there may be political or legal obstacles—for instance, federalism or stricter norms about privacy—to the setting up of the kind of registers needed for that purpose. Here again, *How Societies and States Count* provides us with tools and insights to further such speculations.

Comments on *How Societies and States Count*

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These books begin with what all of us here already know about official statistics in general and censuses in particular, which is that they don't transparently reflect what is out there in the world. Rather, they reduce complicated and messy realities to classification schemes that can be represented in neat tables. This reduction occurs both in the representational realm and in lived experience. To mix metaphors from James C. Scott (1998) and Ian Hacking (1986), censuses render populations legible and initiate a looping effect that makes up people who are more easily read the next time around. Censuses and other official statistics are, by definition, produced by states. Nonetheless, the authors of these books contend that neither official data collection nor their consequences are entirely driven by states. At least, not always. This is their intervention into a sociology and science studies literature that they describe as being dominated by theories of states shaping societies by collecting data on them. As a corrective, the authors demonstrate that societies not only participate in this process, but often take the lead. They conclude that effective and useful efforts to collect official information about populations must involve interactions between the state that is collecting the data and the society that the data aim to describe.

The first book begins with a review of the literature and the development of three analytic models. The first is the state-driven model that the authors associate with the majority of existing literature on official data collection. In this model, information-gathering activities begin and end with the state. States decide what data to collect and develop classification systems. Individuals are required to fit themselves into those systems, and in the process take on their taxonomies, which then become the organizing categories of civil society. In each round of data collection, societies more closely resemble the official classificatory schema, reinforcing its applicability. Subjects of data collection can resist official classification schemes, or can fashion novel uses for them, but in this model, they are always responding to the state's lead. The authors operationalize this model as a wheel, with the state domain on the right and the societal domain on the left. Each is divided into three levels, with micro on the bottom, meso in the middle, and macro on top. This state-driven model moves clockwise, with influence running from macro state down through the levels of the state domain and back up through the levels of the societal domain. In the second model, which the authors describe as society-driven, these flows are reversed. Data collection begins with social actors understanding themselves through a set of lay categories and demanding that the state count them as such. Results of data collection then inform policies that further reinforce social categories. The final model is fully interactive. Data collection results from dialectical interactions between every pair of neighboring nodes in the wheel, with power and influence moving in different directions at different times and places.

The authors argue that neither the state-led model nor the society-led model is complete on its own, and that only a fully interactive model can account for historical and geographical differences in official statistical activities. Although the model is interactive, the empirical analysis presented in the books emphasizes social agency as a corrective to the emphasis on state agency the authors see in the literature. This empirical analysis covers 1000 years of information gathering in what is now the United States, the United Kingdom, and Italy. The authors aim to show simply that the state-driven model is incomplete and that a society-driven model can also explain their empirical findings. To demonstrate that the state-led model is incomplete, the authors show that state strength did not always correspond to the strength or level of detail of the censuses they produced. To show how information-gathering is also at times society-driven, they demonstrate that censuses can only collect information that social actors already know, that this information is translated from lay categories to state classification schemes by social actors the authors dub “census intellectuals,” and that this translation process depends on social power. The authors show that each round of information gathering is constrained by the previous one, which means that the historical narrative of successive rounds of data collection can be read through the model in either direction. The authors argue that official data collection activities are most socially relevant when they produce or result from intense interactions between state and social actors.

The empirical case studies in these books provide excellent information about the history of censuses and their precursors in three countries with long traditions of information gathering. I learned a lot from reading them, and the authors tease out of them some very important arguments. I won’t list them all, just the two that I found most compelling. The first is that historical narratives can be read in multiple directions with multiple starting points, and that these alternative readings can produce very different stories about agency, power, and causality. The second is that knowledge is produced interactively. If we take as our starting point that censuses don’t simply record facts that are out there waiting to be recorded, we must also recognize that census bureaus don’t produce knowledge without participation from the subjects of that knowledge. The authors demonstrate that states and their census bureaus can’t simply impose classification schemes on publics that don’t recognize them. These classification schemes only work if they can be translated into the lay categories through which people already understand themselves. We learn through comparison between Italy and the United States that census data are only socially useful to the extent that the public participates in their production. A further implication of this argument that the authors present as their final word is that social categories, whether exclusive or inclusive, oppressive or liberating, come from society itself and are neither created nor imposed by the state. These are all important arguments and, together with the empirical detail, make these books valuable additions to the literature.

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But as I read them, I found that the scientific categories used by the authors, who are sociologists, did not quite line up either with the lay categories at my disposal or with the scientific categories I use as a historian. The categories I found most foreign were those most fundamental to the book: state and society. I even consulted the glossary on the ASA website[1] to see if these terms had some technical meaning to which I was not privy. In particular, I was puzzled by the fact that the authors present state and society as independent and parallel. Each domain occupies one side of the model. They meet at the bottom in interactions between state and social actors and they meet at the top in affinities between state and social structures. In their empirical examples, the authors classify each actor and institution as belonging to either state or society, as if these categories were mutually exclusive. The model identifies state, society, and the interactions between them as the independent variables, and censuses as the dependent variables. This framework limits what we can know about states, societies, and censuses in three ways. It presents societies and especially states as monoliths, it elides the imbrication of states and societies, and it provides little opportunity to see the work that censuses do to establish and maintain both states and societies. I'll go through these briefly one at a time.

In these books, the state appears as a single entity with unified agency and purpose. In the few places where we see conflict within the state, one side gets coded as society so that the conflict can be narrated as a state/society interaction. The clearest example of this is in the discussion of the 1753 and 1801 Census Bills in the United Kingdom. The authors describe the defeat of the 1753 Census Bill as an instance in which a social group, landlords, prevented the state from collecting population information. They describe the passage of the 1801 Census Bill as an instance in which another social group, industrialists, pushed the state to begin collecting population information. The authors describe both cases as state-society interactions, but they may be more accurately classified as instances of social conflict occurring within and through the state. Landlords were able to block the passage of the 1753 Census Bill because of the seats they held in Parliament, and the passage of the 1801 Census Bill was facilitated by the fact that some of those seats had by then turned over to industrialists. Landlords and industrialists had competing social projects, one of which included a census while the other did not; the census occurred when the group favoring it had a stronger grasp on state power. For this reason, I never quite bought the contention that a state-driven model would imply that stronger states collect more information; it would simply imply that stronger states have a greater ability to collect the information that those at their helm want to collect.

The authors do acknowledge more diversity in the social domain, which they describe as being occupied by elite and nonelite actors. Once we recognize that state and society are not monoliths, we can see the ways in which the two domains are imbricated. The social conflict between landlords and industrialists in the late-eighteenth-century United Kingdom was centrally about how state power would be allocated between those groups. On the society side, the state maintains the distinction between elites and nonelites by enforcing the claims elites have on the resources of nonelites. These books designate all actors as belonging either to the state or to society, but state actors don't cease being social actors when they enter government service, and the state participates in the allocation of power and resources among social actors. As an example of why this matters, the authors describe census intellectuals as social actors who translate lay categories into official classifications. But if state actors are also social actors, then official classifications are already someone's lay categories, and the work of census intellectuals may be less that of translating between lay categories and official classifications and more that of negotiating whose lay categories become official classifications. Thinking about their work as negotiation rather than translation fits well with the authors' argument that information is produced through interaction, but it makes that interaction one between social actors who have differential access to state power, rather than one between actors identified only with the state and actors identified only with society.

These books examine how interactions between states and societies produce official information, but when we allow for the imbrication of state and society, we can ask how the census itself produces and maintains the dialectical relationship between these two domains. The long time span of the books emphasizes that information gathering did not originate with states in the historical sense; rather, censuses drew from and built on earlier modes of data collection that originated in the social domain. But this long time span also obscures very real ruptures that accompanied the first censuses in the United States, the United Kingdom, and Italy. The authors argue for the incompleteness of the state-led model by demonstrating that early censuses in the U.S. and Italy were strong, despite the weakness of the states that carried them out. But saying that the U.S. and Italy were weak states at the time of their first censuses is an understatement. They were at best emergent states, and their censuses helped bring them into being, co-producing these states with the societies in whose names they ruled. These were new kinds of government at that time, one based on democratic representation and the other based on nationality. The American and Italian states claimed legitimacy on the grounds that they represented society, and censuses embodied that representation. Censuses were therefore foundational documents that constituted citizens as the source of state power and simultaneously constituted states as institutions on which citizens could make claims to rights and resources.

The work that these authors have done to examine the state-society dialectic is very important to our understanding of how official statistics are produced, and I think that taking a more nuanced view of state and society can help us understand the work that official statistics do. One way that we might transcend the state-society binary is to consider an international dimension. The authors mention that International Statistical Congresses attempted to exert some influence on how censuses were done, but that they didn't have much effect on any of the three cases described in the books. This I believe, but it's not the only possible international influence. Censuses facilitated the mutual constitution or mutual legitimation of states and societies in an international context. To go back to the U.K. example, the authors demonstrate that the social identity of members of the Parliament

The long time span of the books emphasizes that information gathering did not originate with states in the historical sense; rather, censuses drew from and built on earlier modes of data collection that originated in the social domain. But this long time span also obscures very real ruptures that accompanied the first censuses in the United States, the United Kingdom, and Italy.

changed between the 1753 Census Bill, which was defeated, and the 1801 Census Bill, which passed. But the passage of the 1801 bill may have had something to do with the fact that the international community had also changed. The bill was introduced in the wake of the American and French Revolutions and amid calls for electoral reform in the U.K. The resulting census could be seen as a harbinger of impending democratization or as an effort to stave it off by introducing the trappings of democracy in order to show that this mercantilist state was still able to produce population growth, which was its chief measure of success (Glass 1973). Looking at Italy 60 years later, the architects of the first national census certainly relied on the model of information gathering set by regional and local states, as the authors demonstrate, but they may also have used census-taking to signal the legitimacy of their state-building project to an international community in which censuses were becoming a hallmark of state power. In the mid-twentieth century, the United Nations began to coordinate the censuses of member states, encouraging them to follow a common schedule and use categories that would be internationally comparable (United Nations 1947). These guidelines likely had little effect on any of the countries discussed in these books, but they may have been formative for countries that were, at that moment, throwing off colonial rule and introducing censuses that established new states and societies and legitimized them as members of the international community. These censuses likely involved negotiations not just between social categories and official classifications, but between social categories and international classifications.

These books provide a wealth of empirical detail and a compelling argument about the interactive nature of knowledge production, particularly when states are producing knowledge about societies. Yet the bifurcation they impose between state and society obscures the imbrication of these two domains and the role of the census in producing each one and defining the relationship between them.

Endnotes

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Reply to Critics

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Dylan Riley
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We were delighted to be the subject of multiple author-meets-critics sessions,[1] and we greatly appreciate that a number of these authors were able to respond in writing. Instead of responding point by point to each, we hope we arrived at a fruitful approach by answering substantive groups of issues together.

We start with the points that our critics found appealing about our books. There seemed to be widespread agreement that our interactive model is good and that it fruitfully shows how state and social (elite and nonelite) actors interact to produce knowledge at different levels of social analysis (micro, meso, macro) for three different kinds of information gathering (extractive, descriptive, interventionist) (Foster, Hirschman, Lam, Merchant, Mora, Tazzara). Lam suggested that this model seems to be widely applicable to a variety of cases in time and space. We were very pleased that Lam noticed the emancipatory thrust of the book as it highlights the power of ordinary actors' actions. Loveman and Foster added that the method of tracing historical patterns comparatively is useful. This methodology allows Mora and Foster to agree with our primary conclusion that national censuses had strong imprints of race, place, and class because state actors had to draw on these social categories. Tazzara applauded this move to socialize the state.

We were also delighted with some of the explicit or implicit suggestions—and we heartily agree with all of them—to extend our model to other cases. Prevost argued that our interactional model explains census outcomes in Canada and France. Foster suggested that the model may explain future cases of shifting social categorization and classification like gender. Mora and Hirschman suggested that we extend our model to include other forms of information gathering. Whether the model extends to cases like NSA, as Hirschman suggested, is of course an empirical question. However, we suggest that NSA may simply be a case where the state-centered forces shown in the state-centered model in Figure 2.6 (Vol. 1, p. 39) are stronger than the society-centered ones. Foster suggested how the model could be extended to social media platforms like Facebook. The model could also be easily expanded to include different domains (Vol. 1, p. 33); for example, Mora suggested that we more explicitly consider the market in the theoretical model (though of course we do consider how the growth of capitalism facilitated census reform). Mora's suggestion to include conflict and cooperation could also be explicitly included as mechanisms in the model (Vol. 1, p. 39) or as ways that social actors are incorporated into the state (Vol. 1, p. 26, which we drew from Loveman). Finally, Loveman's suggestion to consider when and where extraction, description, and intervention are additive strikes us as a useful avenue for further empirical research, although we think that our argument that the focus of official information gathering shifted works well as a historical narrative for our cases. Our favorite extension, however, is Foster's example of institutional phylogenies as analogous to Australian marsupials—this of course we never considered, but we hope to see a treatise on it in print very soon!

Hirschman, Merchant, and Loveman questioned whether we are considering the correct outcomes; we view these comments as friendly suggestions for extensions, as our model could be easily adapted to different outcomes because it is not unidirectional and can be used for multiple empirical implications (Vol. 1, p. 41). As Merchant suggested, one advantage of our work is that the historical narratives can be read in different directions with different starting points. We specify independent and dependent variables only provisionally for specific empirical contexts, and as the model shows, the direction of the effect can be easily reversed to consider multiple outcomes. We believe then that we can, as Hirschman, Merchant, and Lam suggested that we should, look at how the state and society are constituted and demarcated. Similarly, in response to Loveman, we note we can specify different kinds of census outcomes. Indeed, in the volumes we have sometimes considered several of her suggestions (i.e., the successful administration of a national survey, the social importance of the knowledge a census produces). We would like to think that our model applies to all of the options she proposes, in addition to ones that others might propose in the future. One of our main points is that we hope that outcomes are empirically examined, not assumed (like the increase in state power). Along these lines, Loveman noted that we criticized a common interpretation of censuses, that they are outcomes of state formation. She noted that both censuses and the state are products of an interactional historical process. We do not disagree, but our point here is that censuses are not an inevitable product of state formation nor is state formation an automatic product of censuses. Instead, it is an empirical question as to whether state formation produces census formation (or vice versa). We may have censuses without much state formation (consider the Italian peninsula) or considerable state formation without many censuses (consider Britain between the Domesday Book and 1801 or Napoleonic France). Furthermore, they may both appear together but have little to do with each other (for example, a developed census may actually have little to do with a strong state).

Well, as Hirschman noted, since one of the functions of this forum is to criticize the books, of course, our commentators also did their jobs! A number of our critics liked our approach, but suggested that it is not as novel as we are suggested (Hirschman, Lam). Of course, we drew extensively from other scholars, but there is no other comprehensive theorization of the interaction of state and social forces on information gathering. Hirschman and Lam suggested that Foucault, his various interpreters, and STS scholars in fact have a similar view. Like all of these scholars, we also view the state as a set of multiple agencies with multiple network ties to society (Vol. 1, p. 33), although our dialectic approach is more explicitly focused on the internal connections between state bureaucracies as public powers and nonstate private actors. However, despite their undoubted theoretical sophistication, we argued that when it comes to looking at information gathering, the Foucauldians and STS scholars in practice tend to emphasize the power of the state over social interests. Thus, censuses are typically seen as a method of governmentality, a mode of state power. This form of power is not overtly coercive, and it works through social actors, but for the Foucauldians, the impetus comes from the state. STS scholars, when considering information gathering, also emphasize the state as a center of power. This state-centered emphasis is apparent even in Lam's own example of how the "census categories are enabling": individuals embrace the census, and minorities want to be counted in the census. It illustrates the typical ordering that most Foucauldians (and Bourdieuians, for that matter) deploy: once censuses exist, social actors resist or rework the categories to their favor. In sharp contrast, however, we argued for a much more fundamental role of social actors at every step, including the creation and shaping of the categories. Thus, we disputed

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Foucault's (and Bourdieu's) overly general claim that censuses are instruments of state power, however that power might be defined, and instead argued that it must be determined empirically whether or not censuses uphold state power depending on the interactional pattern of state and social actors. Of course, Foucault and Bourdieu may be correct that a census can uphold state power; we simply claimed that this must be established empirically, not assumed theoretically. We even judged Foucault in particular more charitably, as intending for state and social power to be dialectically constructed (although he was allergic to this sort of terminology), but his approach is usually empirically implemented in a way that emphasizes state power, as Lam's example shows. Hirschman also noted that we should not have been surprised that experts, such as elite social actors, had a large influence on official information gathering. However, we claimed that if anything is surprising, it is that nonelite actors had so much influence on official information gathering. This influence is generally underresearched, in part because of difficulties in finding adequate source material (though not unanticipated by the phenomenological and ethnomethodological literature we draw on) (Vol. 1, pp. 19–23).

Merchant and Hirschman's assertion that we have misconceptualized the state is somewhat of the opposite nature: we think we all agree in principle but that they have misunderstood our model (Vol. 1, p. 39). As Foster elegantly noted in his comments, the binary diagram is animated exactly for the purpose of understanding the "co-constitutive entanglements" of states and societies. We did not characterize states and societies as monoliths either theoretically or empirically, we did not assert that they are independent or parallel, and we did not overly reify them. A heuristic always must be bracketed for the purposes of understanding a piece of it (Vol. 1, p. 40); thus, the model draws out certain connections between the state and social domain, but not others. However, as we pointed out, both domains contain a multiplicity of actors, and the connections between them could be drawn in any way. The model, therefore, provides a flexible way of looking at the interconnections between state and society. In fact, we did point out exactly the point that Merchant and Hirschman made when we stated that neither the state nor society "is a single entity or actor, nor are they historically invariant." (Vol. 1, p. 33). As we noted (Vol. 1, p. 36), actors and mechanisms can be added to the models to capture a huge variety of links between state and society. These can be easily adapted to a wide range of circumstances to look at the interaction between multiple actors at different levels. In fact, contrary to Hirschman's suggestion, because the model explicitly specifies feedback loops and bidirectionality (Vol. 1, p. 36–37, 39), the effects of censuses are always considered for all of the empirical cases in all the time periods (see the conclusions for each empirical chapter).

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We also disagree with Merchant's contention that we classified all actors dichotomously as state or social ones. In fact, we considered extensively the nature of the relation of actors in these domains by borrowing the mechanisms of co-option, usurpation, imitation, and innovation from Loveman (Vol. 1, p. 26). Merchant's suggestion to replace the word translation with negotiation confuses how much power the actors have (that is, whether there will be any sort of "negotiation") with how the work of shifting between categorization and classification occurs. We explicitly argued that the possibility of translation depends on the relationships of power between the actors, so we did not assume an equal relationship required for negotiation that is not always present. Thus, we also disagree with her conclusion that any of her criticisms undermines our contention that the state model suggests that strong states collect more information. She reframed our argument as suggesting that strong actors at the helm of strong states can collect more of the information that they want, but this is just semantics, as we were already explicitly looking at the interests of actors within state and society. In fact, we examined historically a huge variety of different possible relations between state and social actors, ranging from social actors who essentially collect the information and hand it over to state actors, to state actors who conduct censuses with virtually no social influences. Of course, most of the cases that Merchant and Hirschman pointed to are in between these extremes, so we analyzed these empirically in detail, showing the vast array of complicated arrangements that Merchant and Hirschman desire to see. We of course did not explicitly set up our analyses to consider universities like UCLA as Hirschman suggested, but we do not understand why these cases particularly puzzling. The UCs are social organizations over which the State of California has formal oversight and some financial input but which are largely independent on a daily basis. Our model could be easily adapted to show these relationships. Of course in other locations, "public" universities have quite different relationships to their states, and in some cases, these universities may be state institutions. The advantage of our model is that it can deal with exactly these sorts of complications, and not in fact treat them as unitary, as Hirschman and Merchant seem to suggest.

Both Hirschman and Loveman question our characterization of state strength. We defined state strength as territorial authority and control (Vol. 1, p. 51). A state has territorial authority if it controls a relatively contiguous geographical space without the intervention of competing powers. We could not look at the interplay between state strength and the census if we defined state strength as infrastructure. We used this definition so that we could see if state strength corresponded to one sort of infrastructural development, the census, as Loveman suggested. Similarly, by this definition, the early US state is weak. It simply did not control its territory, either vis-à-vis other states or local entities.

Merchant also disagreed with our interpretation of several empirical cases. Merchant claimed that the US and Italy were emergent states that censuses brought into being. This is in fact an assumption of the state-centered approach to examining official information that we disputed precisely because this outcome varied. It is possible to argue that in some ways, the US census helped constitute the nation by apportioning representation, but then it would be just as easy to argue that the census led to the civil war by apportioning power between slave and free states. Empirical evidence, not state-centered theory, is needed for adjudication. However, the Italian census did not, in fact, create a nation, even though its creators intended for this effect, because it reinforced pre-unification divisions. Merchant also disagreed with our characterization of the effect of state-social interactions on the first British censuses. We argued that the pattern of state-society interaction led to the failure of the first census bill in 1753 and the passage of the second census bill in 1801 (Vol. 1, Chapter 5). Merchant argued that these outcomes are explained by social conflict occurring within the state. We disagree. If we focused on the conflict occurring just in Parliament, as Merchant suggested and as state theorists would do, we would miss many important dynamics. In 1753, social actors pressed for the census because they thought it would enhance the power of the state; in 1801, social actors pressed for the census because they thought it would enhance their own power. Left on their own, it is unlikely that members of Parliament would have passed a census bill at all (at least not until international pressure in the mid 1800s might have induced them to do so). It took social pressure to force them to do it, as Tazzara noted in his comments. Of course, we argued that the shifting composition of Parliament intersected with these social forces, and this is the point of our model. In 1753 as well as 1801, most members of Parliament did not want the power of the central state increased, but the shifting composition towards commercial interests meant that more of them could see how a census was in their commercial, social interests in 1801.

Lam and Tazzara noted our omissions. Our original plan proposed a single volume with a chapter on colonial censuses that included Italian, British, and American examples and that addressed the references in Lam's footnote 3. However, we found, contrary to Lam's suggestion, that the examination of this work did not change our models. In fact, most of this work, like the work on the census in general, emphasized state power. We found, however, that where colonial censuses were successfully conducted, there were pre-colonial information-gathering apparatuses with strong social influences that facilitated colonial census taking. Thus, our empirical work quickly expanded, creating insurmountable space constraints. More importantly, however, this realization forced us to rethink whether colonial censuses could be usefully classified or periodized by the colonizing power. Thus, we have been working on publishing this work separately. In examinations of information gathering in India, East Africa, and Puerto Rico, we show how censuses were not merely instruments of colonial power but depended on the colonized as Tazzara suggests. When published, we hope that these treatments will reinforce our interactive model and continue to show the incompleteness of the state-centered model. Thus, although we disagree with Lam that we would change our interactive model in light of examining colonial censuses, we do agree with Lam's final conclusion that our work can show the connectedness of the various parts of the world through information gathering.

Tazzara and Merchant also asked if we missed international influences, such as the ideas of the various international statistical conferences, the United Nations, or political economy. Of course, we agree that international influences from the statistical congresses, or the guidelines from the United Nations (Merchant), or general European ideas about political economy (Tazzara) could have been crucially important in cases that we do not examine (as Loveman showed for Latin America). However, these international influences, as we argued, were relatively small in the three cases we examine. However, more importantly, if international influences had been larger, our main theoretical point would be even stronger: censuses bear strong marks of the local lay categories of social actors despite strong international influences.

To several of Loveman's comments, we can only speculate. During the trial of Jesus, Pontius Pilate out of sheer frustration in getting Jesus to answer any of his questions, asks mostly rhetorically, "what is truth?" We doubt we have any complete answers to Loveman's questions that revolve similarly around truth. In particular, she asks about our central claim that "censuses cannot produce knowledge that populations don't know." Our claim is that a survey instrument can produce knowledge from that instrument only to the extent to which populations can answer the questions that the survey is asking. Thus, both state and social actors, must in some sense, "know" the same

Our claim is that a survey instrument can produce knowledge from that instrument only to the extent to which populations can answer the questions that the survey is asking. Thus, both state and social actors, must in some sense, "know" the same information for it to be compiled and widely accepted as useful social knowledge.

information for it to be compiled and widely accepted as useful social knowledge. We claim that this can be examined empirically, and thus answer a number of Loveman's questions about what is true or known. Of course, in hindsight, much of what is now known was unknown in the past, or perhaps, as Loveman suggested, is now called untrue. So truth, but perhaps better called knowledge in our context (though perhaps Pontius Pilate meant something deeper) does change over time. And of course, controversy is inherent in the production of information. Of course, as Loveman suggested, official statistics can be manufactured from virtually nothing and said to be "scientific" or taken from samples that are clearly not representative of their populations. But we were not really addressing these sorts of issues. Furthermore, censuses are full of examples of categories introduced by social or state elites that make little sense from the point of view of respondents, and therefore, produce little knowledge (e.g., US census categories, Vol.1, p. 166–167; Vol. 2, p. 79–80). While Loveman cited examples of censuses that claim results based on more comprehensive coverage than existed, we noted that in many early modern censuses, the state's apparatus, not the population's knowledge, was inadequate (e.g., see Vol. 1, Chapter 3, England; Vol. 1, Chapter 4, the Italian peninsula).

Finally, we close by assessing Tazzara's question about whether there is anything left of the state model. As Foster noted, the state-centered view is deeply entrenched in sociology. We see this tendency in our critics as well. Merchant and Hirschman sometimes reacted to our work by moving the theoretical and empirical focus back to the state. But we argue that tactic of reorienting everything back to state power reveals little about the interaction between state and social forces and even less about the empirical conditions that produce the relative balance of state and social influences on state information gathering. Thus for example, instead of focusing on a relationship between state and society, Hirschman suggested that a useful tactic would be to analyze a "submerged state" that governs out of sight. This line of research is of course very useful, but a subtle analysis of state-social interaction cannot be produced by moving everything to the side of the state. Loveman's example of Brazilian urban literates, thus essentially social actors, recruited temporarily in the central statistical office seems to confirm our main point. To use ourselves as an example, our contemporary census practices do not in fact, begin in the offices of the US Census Bureau, but come to us historically through the interaction of state and social actors. To read the current sociology of official information literature, however, we could easily think otherwise, with its focus on censuses as instruments of state power. Though this is certainly plausible, much more evidence is needed to show if, and if so, where and when, this is true. Furthermore, we will have to understand much better the historical interplay between state and social actors to understand what is actually an effect of state power. We would like to reiterate the main points of the book, that we do not reject state influences on information gathering, we simply would like to adjust for their overuse, by developing explicitly society-centered views that can be considered interactively with state-centered ones. We believe it is a matter of empirical investigation as to where and when state versus social influences predominate.

Endnotes

1. We would like to thank the California Sociological Association and our commentators there, Corey Tazzara and Charles Thorpe; the Pacific Sociological Association and our commentators there, Thomas James Dandelet, Mara Loveman, Massimo Mazzotti, and G. Cristina Mora; the Social Science History Association, our organizer, Lyn Spillman, and our commenters there, Daniel Hirschman, Tong Lam, Emily Klancher Merchant, and Jean-Guy Prévost; and our commentators Jacob G. Foster and Theodore M. Porter at our UCLA talk.

Book Symposium

Postcolonial Thought and Social Theory

Oxford University Press

Julian Go

Editor's Note: The following text is based on an author-meets-critics session held at the annual meeeting of the Social Science History Association in November, 2016. My thanks go out to Aldon Morris, Zine Magubane, Marco Garrido, and Julian Go for contributing their comments to* Trajectories. *-VR

Julian Go's World: Postcolonial thought, Social Theory, and Human Liberation.

Aldon Morris
Northwestern University

Let me begin by congratulating Professor Julian Go for writing *Postcolonial Thought and Social Theory*. This intellectual gift is a provocative work challenging much of what we thought we knew about the world ascertained through the lens of mainstream social science and the humanities.

The central thesis of *Postcolonial Thought and Social Theory* is that mainstream social theory, from its very beginning, has misled us because of the foundational premises rooted in the context from which it emerged and took shape. That context is western imperialism which has been an instrumental feature of Europe and the United States beginning in the seventeenth century and enduring throughout much of the twentieth century. Because social theory formed in this context, it has been corrupted by empire and often in collusion with it. To put it simply, modern social theory and social thought have been crafted by privileged white thinkers in the west who themselves were shaped and molded by imperialist interests and conceived the world through the lens of empire. They were children of empire because through their routine social and scholarly socialization, imperialist thinking became lodged within their intellectual DNA.

Empire has basic characteristics. First and foremost empire is a set of power relations determining who rule the earth and which populations are the subjects of that rule. Empire is about privilege where those embedded at the top enjoy superior life chances. Empire is also an engine of exploitation. Empire generates feeling of superiority to those it defines as others. From the standpoint of empire, the world is made in its image. Empire insists that "others" exist to meet imperialist needs because they are lesser beings not requiring the finer things making life pleasurable and worth living. And empire utilizes physical and symbolic violence and nasty brutality to conquer and subdue others. In the modern period, the architects of empire have been whites of Europe and America.

Thus, European countries including England, France, Germany, Spain, and Italy along with America became empires as they conquered dark skin peoples in the Global South and within its own borders.

Go argues that scholars are not aloof, objective, thinkers and they never can be. Scholars, like the rest of humanity, are shaped by their environments and so is their scholarship and worldviews. It is impossible for thinkers to float free in an intellectual heaven granting them abilities to produce unadulterated truths. Indeed, intellectual thought is socially determined.

Upon this epistemological foundation, Go builds his argument. First, all knowledge is partial. Second, knowledge by its very nature is local and particularistic. Third, scholars cannot know the complete truth because they are socially bound creatures incapable of comprehending the complex social world because their social positions limit analytic sight and intellectual reach.

Giving these limiting factors, Go argues that there is no social theory that can be universally applied to all human societies through time and across space. Because all knowledge is partial, social theory can only provide slices of truth because theories are constructed by socially determined theorists. No theorists, argues Go, are capable of pulling off the God trick by sitting on high and accurately surveying realities on the ground.

Go's main thesis is that western intellectuals proceeds as if they have actually pulled off the God trick. They believe they have developed social theory that can explain all societies irrespective of time and space. As a result, they embrace the notion they have produced intellectual systems of thought that can be applied universally to all societies clearly explicating the mysteries of antiquity and modernity.

Go does not embrace the intellectual God trick, claimed by the West, one bit. He investigates theoretical claims from the Enlightenment, Hegel, Marx, Durkheim, Weber, Foucault, Bourdieu and many others, reaching a challenging conclusion: None provide explanatory frames that can be universally applied to all societies because their insights are particularistic and fragmentary. That is, these theories cannot be universal because they are socially determined and represent a limited point of view anchored in particular local circumstances.

Go reaches the heart of his argument. These systems of Western thought were crafted by theoreticians situated in European and American empires. In a fundamental existential sense, these theorists embody the interests of empires and their worldviews and theories are inextricably tied to the well-being of empires. For them, far flung societies are to be explained through their theories based on the universal experiences and social processes characteristic of the west; characteristic of empire; characteristic of those who rule; characteristic of those where the sun never sets on their empire; and characteristic of white Westerners.

Go's argument is not that western theories are useless. Indeed, he argues they contain important social truths. But those truths are partial and represent skewed angles of visions. They are applicable to certain social circumstances and not others. Such theories dress themselves in garbs of universality but reflect particular social dynamics triggered by local rulers. These theories proclaiming theoretical universality do not travel well to societies of the dominated, the colonized, the enslaved, dark-skinned peoples, the wretched of the earth, and peoples dominated by European and American empires.

Yet, argues Go, populations outside empires are just as important in the making of the world as those atop empires. In fact, the world has been, and continues to be made, by the joint actions of the dominated and rulers. If these comingled efforts were severed down the middle, the global social edifice would collapse. There would be no modernity. As western social thought obsesses with rulers, it obscures the historic and contemporary agency of the subjugated majority who, with rulers, build a world of co-dependency relationships.

Go innovates by probing this interconnected world. He argues that the dominated have real feelings and experiences just as the inhabitants of empire. Even though crushed to the bottom of human hierarchies, the dominated struggles to make a living in colonies and oppressed wastelands, where they absorb the sting of the lash and the fire of the gun. They struggle with empire to breathe free. They fashion their own sets of interactions, identities, and social structures and trigger social processes which shape the world. They aspire to mold the world in their own images. They fight to overthrow empires and sometimes they successfully lead liberation struggles against empires. And sometimes, they win. Together with those of the empire, they have fashioned a modernity that would have been impossible without their toil and strife.

Go is most interested in the worlds of the dominated and how those worlds have made a difference in shaping humanity. This is where Go turns to Postcolonial thought. It turns out that there is a body of thought developed by scholars in the periphery that sheds theoretical and analytic light on the dominated and enables a new, more comprehensive, understanding of empire. The tragedy is that western scholars have neglected and marginalized this body of thought thus impoverishing their own social theory.

Theorists of postcolonial thought include W. E. B. Du Bois, Aime Cesaire, Frantz Fanon, Amilcar Cabral, C. L. R. James, Leopold Senghor, Edward Said, Dipesh Chakrabarty, Homi Bhabha, Gurminder Bhambra and R Connell to name just a few. Go points out that although postcolonial thought is not an integrated theory with one uncontested theoretical line, it does share common theoretical insights which include:

- An analysis of the feelings, experiences, social interactions and identities of the dominated that reveal how these factors help shape the social world.

- An analysis of the agency of the oppressed. Postcolonial thought reveals that western thought has marginalized and even erased the agency of the oppressed. This is a major error of social theory because the historical agency of the oppressed has helped shape the world, including developments such as the French Revolution, industrialization leading to capitalism, and the American civil rights movement.

- Societies, their social structures, and social processes are to be analyzed in a relational manner. This means that master theories such as those of the class struggle, mechanical and organic solidarities, or bureaucratic rationality cannot be universally applied to all societies because of inherent blind spots.

- Systems of social thought should proceed from standpoint theory. That is, social analysis should work from the bottom up with the experiences and subjectivities of the dominated.

- Finally, postcolonial and western social theory should be in mutual dialogue, thus creating analytical grounds for fruitful theoretical syntheses.

Now I wish to quibble with the idea concerning postcolonial thought and standpoint theory. Go writes that in terms of postcolonial thought, "We always begin with the experiences and practices of dominated groups." "Instead of starting from atop or from afar, instead of starting with theories and concepts cultivated from the standpoint of power, we start on the ground. We start from the standpoint of the subjugated."

The idea of starting on the ground is problematic. Where is the ground? It is true that the subjugated stands on its unique grounds.

The idea of starting on the ground is problematic. Where is the ground? It is true that the subjugated stands on its unique grounds. Likewise, the powerful stands on its grounds. Yet, the powerful have experiences and practices as do the dominated. Therefore, rather than always starting on the ground of the dominated, would it not be theoretically fruitful to investigate the grounds of the subjugated and powerful simultaneously, constantly comparing and reworking each perspective in pursuit of theoretical synthesis?

Likewise, the powerful stands on its grounds. Yet, the powerful have experiences and practices as do the dominated. Therefore, rather than always starting on the ground of the dominated, would it not be theoretically fruitful to investigate the grounds of the subjugated and powerful simultaneously, constantly comparing and reworking each perspective in pursuit of theoretical synthesis? The analytic tension created by juxtaposing these standpoints may provide fertile grounds for new theorizing, making possible a strategic theoretical universalism cautiously applied across societies.

An example from Du Bois' analysis of white folks and westerners embedded in empires can illuminate this approach. It is true that Du Bois began with the experiences of the dominated in America and across the globe. He looked deeply in the souls of African Americans and people of color globally. He posed a trenchant question to these populations: how does it feel to be a problem? From this standpoint, Du Bois produced reams of social theory demonstrating how the historic agency of the oppressed shaped the modern world.

But Du Bois did not stop there. He shifted his angle of vision to the souls of white folk, asking how they came to dominate the modern world and the human feelings clustered with this project of domination. In the "Souls of White Folk" (Du Bois, 1920) Du Bois proceeds as if the question is: how does it feels to be atop empire and rule over others? From this standpoint, he reveals the structural and emotional realities of those who dominate. Du Bois is clear about the standpoint from which he dissects the souls of white folk:

> High in the tower, where I sit above the loud complaining of the human sea, I know many souls that toss and whirl and pass, but none there are that intrigue me more than the Souls of White Folk.

For Du Bois, the "tower" is his standpoint which yields truths concerning the dominators:

> Of them I am singularly clairvoyant. I see in and through them. I view them from unusual points of vantage. Not as a foreigner do I come, for I am native, not foreign, bone of their thought and flesh of their language. Mine is not the knowledge of the traveler or the colonial composite of dear memories, words and wonder. Nor yet is my

> knowledge that which servants have of masters, or mass of class, or capitalist of artisan. Rather I see these souls undressed and from the back and side. I see the working of their entrails. I know their thoughts and they know that I know. This knowledge makes them now embarrassed, now furious. They deny my right to live and be and call me misbirth! My word is to them mere bitterness and my soul, pessimism. And yet as they preach and strut and shout and threaten, crouching as they clutch at rags of facts and fancies to hide their nakedness, they go twisting, flying by my tired eyes and I see them ever stripped,—ugly, human.

Du Bois analyzed the feelings and viewpoints about black people necessary for whites if they were to fulfill the interests of empire:

> The European world is using black and brown men for all the uses which men know. Slowly but surely white culture is evolving the theory that "darkies" are born beasts of burden for white folk. It were silly to think otherwise, cries the cultured world, with stronger and shriller accord. The supporting arguments grow and twist themselves in the mouths of merchant, scientist, soldier, traveler, writer, and missionary: Darker peoples are dark in mind as well as in body; of dark, uncertain, and imperfect descent; of frailer, cheaper stuff; they are cowards in the face of mausers and maxims; they have no feelings, aspirations, and loves; they are fools, illogical idiots,—"half-devil and half-child."

However, Du Bois was clear that these white feelings and worldview, including the invention of whiteness, were socially constructed to advance empire:

> There must come the necessary despisings and hatreds of these savage half-men, this unclean *canaille* of the world—these dogs of men. All through the world this gospel is preaching. It has its literature, it has its secret propaganda and above all—it pays! …There's the rub,—it pays. Rubber, ivory, and palm-oil; tea, coffee, and cocoa; bananas, oranges, and other fruit; cotton, gold, and copper—they, and a hundred other things which dark and sweating bodies hand up to the white world from pits of slime, pay and pay well, but of all that the world gets the black world gets only the pittance that the white world throws it disdainfully.

Du Bois' standpoint analysis enables him to empathize with the rulers of empire as structural prisoners:

> And yet, somehow, above the suffering, above the shackled anger that beats the bars, above the hurt that crazes there surges in me a vast pity,—pity for a people imprisoned and enthralled, hampered and made miserable for such a cause, for such a phantasy!

Here we witness Du Bois' ability to move interactively between the standpoint of the dominated and the dominant, extracting understanding of this complex ideological and structural relationship. Even as Du Bois interrogates the world of the ruler, he unswervingly acknowledges the agency of the oppressed to topple empire:

> What, then, is this dark world thinking? It is thinking that as wild and awful as this shameful war (World War 1) was, *it is nothing to compare with that fight for freedom which black and brown and yellow men must and will make unless their oppression and humiliation and insult at the hands of the White World*

> *cease. The Dark World is going to submit to its present treatment just as long as it must and not one moment longer.*

Thus, by grasping the agency of the oppressed, Du Bois anticipates the anticolonial struggles, independent movements in Africa, Asia, and South America and the United States that rocked the globe in the second half of the twentieth century to undercut empire. It is, therefore, critical that insurgent sociology navigate the standpoints of both rulers and their challengers revealing the structural and ideological factors making humanity prisoners although in vastly different unequal worlds.

I conclude by emphasizing that Professor Go has initiated an important debate by pointing out the limitation of reigning social theory and arguing that these limitations may be transcended by offerings from postcolonial thought. I, for one, welcome this timely debate because God knows we live in a time where we desperately need to understand the world of the dominated and the powerful interactively. Perhaps the future can be swayed by such understandings. Thanks to Professor Go for lifting our theoretical sights and the pressing need to transform humanity.

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Towards an Ungovernable Social Theory: Postcolonial Thought, Social Theory and the Coloniality of the Present

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Julian Go's *Postcolonial Thought and Social Theory* is an even-handed appraisal of what postcolonial thought can and cannot add to our existing social theory. How, he asks, might "social theory be enlightened by postcolonial thought" (2)? It is immediately apparent that Go, although fully cognizant of sociology's complicity with colonialism's destructive impulses, and the ways in which postcolonial thought arose to challenge them, nevertheless does not seek to destroy sociology. Rather, his aim is to demonstrate that social theory can be redeployed in ways that meet the postcolonial challenge. The text treads carefully between the radical and impassioned impulses of anti-colonial political imperatives and the "objective" requirements of disinterested social scientific argument. In answer to the question, "what does postcolonial thought mean for social theory" he concludes: "Postcolonial critique cannot be read as an indictment of an entire discipline or field. …Postcolonial thought alerts us to *certain* strands, elements, and *tendencies* within the social sciences even if they are not definitive of the social sciences (102). This level-headed response is a far cry from Aimé Césaire's declaration that "Europe is morally, spiritually indefensible" (1972: 10). Or Frantz Fanon's pronouncement that "reluctance to qualify opposition" to colonial ways of knowing stemmed from the fact that "every qualification is perceived by the occupier as an invitation to perpetuate the oppression" (1965: 123).

Of course, Go's purpose in writing *Postcolonial Thought and Social Theory* was very different from that of a Fanon or Césaire. "Robin D.G. Kelley once described Aimé Césaire's *Discourse on Colonialism* as a "declaration of war" that was "full of flares, full of anger, full of humor." As such it was "not a solution or a strategy or a manual." Rather it was "a dancing flame in a bonfire" (2000: 7). Although he draws great inspiration from Césaire, in important respects Go has set his sights on something else. His aim *is* to be something much closer to a solution or strategy. Although he opens the text with the caveat, “the ultimate goal of this book is not to offer the concluding statement on how social science can be transformed by postcolonial thought. It is only to suggest that it should be” it is clear that one of the text’s key aims is to vigorously critique sociology so as to make its revitalization possible (xi). As he puts it in the introductory chapter: “If social theory can be challenged for its persistent imperial gaze and its embeddedness in the episteme of empire, how can we reconstruct it, making it more attuned to the global challenges of our ostensibly postcolonial present” (10)?

Whereas the postcolonial theorist from whom he drew inspiration wrote polemically, Go’s text is a model of social scientific restraint. I point this out, not in a spirit of criticism, but rather to highlight the ways in which the impact of political context on Fanon’s *Studies in a Dying Colonialism*, Césairé’s *Discourse on Colonialism*, and Edward Said’s *Orientalism* was both similar to, and yet quite different from, the context that surrounded the production of *Postcolonial Thought and Social Theory*. All three texts, it is true, strongly reflect the emotional temperature of their times. Yet Go’s text is different in that the emotional temperature of his times underwent a violent upward spike between the completion and publication of the text.

Césaire wrote *Discourse on Colonialism* in 1955 when the United States, the newly emergent global superpower, was in the grip of Cold Warriors grappling with the “dilemma of differentiating their own imperium from the personae non gratae of the European empires” (Wu 2014: 5). It was thus that Césaire (1972: 9) opened the text with the wry observation that:

> Europe is indefensible.
>
> Apparently that is what the American strategists are whispering to each other.

Césaire went on to argue that “the barbarism of Western Europe has reached an incredibly high level, being only surpassed—far surpassed, it is true—by the barbarism of the United States” (1972: 26). He concluded that American domination was “the only domination from which one never recovers” (1972: 60). Fanon, on the other hand, published *Studies in a Dying Colonialism* in 1959. The preface written by Adolfo Gilly for the first American edition opens with the simple declaration that: “Revolution is mankind’s way of life today. This is the age of revolution; the ‘age of indifference’ is gone forever” (1965: 1). The preface ends by noting that “the Algerian is also united with the American. …The pressure of the world revolution is weighing on the United States” (1965: 14). Edward Said similarly noted that two key aspects of his “contemporary reality” were at play when he wrote *Orientalism*. He had to confront the academy’s insistence on making a “distinction between pure and political knowledge” and his positioning within that binary as “humanist.” This title served to index how unlikely it would be that “there might be anything political about what [he did] in that field” (1979: 9). His a-political positioning as an academic sat in uneasy tension with his everyday experience of life as an “Arab Palestinian in America” which he described as “disheartening” due to the “almost unanimous consensus that politically he does not exist, and when it is allowed that he does, it is either as a nuisance or an Oriental” (1979: 27). Hence, *Orientalism* was written as a way to demonstrate how “the general liberal consensus that ‘true’ knowledge is fundamentally non-political (and conversely,

Whereas the postcolonial theorist from whom he drew inspiration wrote polemically, Go’s text is a model of social scientific restraint.

that overly political knowledge is not 'true' knowledge) obscures the highly if obscurely organized political circumstances obtaining when knowledge is produced" (1979: 10). *Orientalism* not only provided a refutation of that claim, but also modelled a method for rendering the "obscurely organized political circumstances" that structured the production of knowledge *visible*.

The timing of Go's book is unique in that it was written entirely in the pre-Trump era, but will be read, responded to, and subjected to further development in a world where politics is more uncertain and volatile and the possibilities for a "third wave of postcolonial thought, emerging within and for the social sciences" moving from the margins to the center is more than an exciting possibility (188). It is now an imperative. The care that Go takes in the text not to "overreach" ("the ultimate goal of this book," he writes, "is not to offer the concluding statement of how social science can be transformed by postcolonial thought. It is only to suggest that it should be") reflects the fact that as recently as three years ago, no one in the mainstream media seriously entertained the suggestion that America ever was or could be a fascist state. Whether or not it was even *racist* was also held, by many thought leaders, as seriously in doubt. From 2004, onwards all the talk was about America as a 'post-racial' state.

Conversely, even in the sociology of race, the field where colonialism has, typically, been given the most analytical space, the idea that postcolonial thought would be much more than a "boutique" or "fringe" interest seemed unlikely. Just as the media spoke endlessly about "post-racialism" the reigning paradigms within the sociology of race "color-blind racism" (Bonilla-Silva 2013), "laissez-faire racism" (Bobo, Kluegel and Smith 1997), "competitive racism" (Essed 1996) and "symbolic racism" (Sears and Kinder 1981) all suggested that a "new racism" reigned supreme. To be sure, none of these authors suggested that the United States was not racist. However, the seeming invisibility of racism sat at the center of their conceptual architecture. The "new racial order," we were told, had a "slippery" and "beyond race" character (Bonilla-Silva 2015: 1358) or was "kinder and gentler" (Bobo, Kluegel and Ryan 1997). Its discourses and practices were "covert" and its mechanisms "subtle." It avoided "direct racial terminology" and its political agenda "eschewed direct racial references" (Bonilla-Silva 2015: 1362).

In the color-blind, competitive, or laissez-faire racist theoretical world, postcolonial perspectives didn't have much theoretical purchase. Internal colonialism theory, the only theory within the postcolonial orbit that Eduardo Bonilla-Silva, who developed the theory of Color-Blind Racism considers, was said to lead unequivocally to "nationalist solutions" and thus was incapable of providing a "rigorous conceptual framework" for understanding racially stratified societies (Bonilla-Silva 1997: 466). Nor did postcolonial perspectives merit any consideration from the proponents of the "racial formations" school. In answer to the question, "how effective are perspectives that frame U.S. racism as a form of colonialism," Omi and Winant (1994: 46) answered thus: "The analogy between U.S. conditions and colonial systems of discrimination composed of colonizers and colonized—systems which made use of racial distinctions—does not automatically carry over into postcolonial society."

We are in a new world now. It is now clearer than ever that "the US may be a 'new' nation, but its newness does not reside in its distance from colonialism" (Bhambra 2014: 473). The White House extends a gracious welcome to anti-Semites, racists, neo-Nazis, homophobes, xenophobes, and misogynists. Centrist outlets like the *New York Times* and *Washington Post* are awash with articles that seek to understand

and explain the rise of American fascism.[1] It turns out that Barbara Fields was right. In 2012, when America was still woozy with the thought that the era of post-racialism had unequivocally arrived, she had the temerity (and the foresight) to suggest that: "Whatever the 'post' may mean in 'post-racial,' it cannot mean that racism belongs in the past. Post-racial turns out to be—simply—racial; which is to say, racist" (Fields and Fields 2012: 10).

Whereas before sociologists had to marshal a veritable theoretical arsenal just to rebut the claim that racism was "declining in significance" now the field is wide open for new approaches. These approaches must not only dare to proclaim that not only is the United States racist and fascist, but that those tendencies are the inevitable result of American imperialism and colonialism "coming home." As James Q. Whitman (2017) argues in *Hitler's American Model: The United States and the Making of Nazi Race Law*, American anti-miscegenation laws, immigrant restriction laws designed to preserve the dominance of 'Nordic' blood in the United States, and voting restrictions based on ancestry provided inspiration for Adolph Hitler's murderous regime. Indeed, Whitman points out that "the ugly irony is that when the Nazis rejected American law, it was often because they found it too harsh" (Whitman 2017). In Trump's America, we are experiencing "postcolonial relationalism" in real time. Postcolonial relationalism, as Go explains, recognizes "the mutual constitution of the powerful and the powerless, the metropole and the colony, the core and the postcolony, the Global North and Global South" (142). The ubiquity of these imperial interactions has been largely covered up and made invisible by standard social science. Hence, the inadequacy of the sociology of race to anticipate much about the world we find ourselves living in now. The "failure of perspective" that has dogged mainstream approaches in the sociology of race almost since its inception (Steinberg 2007; McKee 1993) are, in no small part, a consequence of the how its analytical constructs purposefully elided imperialism and colonialism. The "mainstreaming" of fascism as a political and analytical construct thus presents an opening, but also a danger.

Peggy Von Eschen reminds us that during the Cold War, some civil rights activists equated racism with Nazism in order to legitimize their struggle. This was a double-edged sword. Whereas on the one hand there were activists and scholars who were deeply invested in anti-colonial struggles that "portrayed Nazism as one consequence of imperialism and one manifestation of racism, seeing antifascism as a critical component of democratic politics *but not to the exclusion of anti-colonialism*"(1997: 153, emphasis mine). There was an alternative take, however, that ultimately gained hegemonic status wherein "Hitlerism was evil and un-American; Hitlerism equal racism; therefore, racism is evil and un-American. …It was a powerful argument but it took the case against racism…out of the context of colonialism" (1997: 153).

I would like to suggest that there is a particular urgency to having race theory undergo the kind of radical upheaval that Go's work provides an opening for. How much of it will survive the disruption is an open question.

Go helpfully leaves the notion of what constitutes "social theory" quite open. He doesn't name any specific sociological sub-disciplines, fields, or theoretical schools that should seek specifically to have their theories revolutionized by postcolonial perspectives or whose perspectives are particularly in need of revamping. However, I would like to suggest that there is a particular urgency to having race theory undergo the kind of radical upheaval that Go's work provides an opening for. How much of it will survive the disruption is an open question still. For so much of the field is still unconsciously structured by a colonial logic. Indeed, the very nomenclature by which most courses in race theory are identified "Race Relations" trace directly to the purposeful removal of analyses of colonialism in the study of US racism (Fields and Fields 2014: 149; Steinberg 2007: loc 104; Von Eschen 1997: 153). It is by dint of colonial oppression that US sociology has been "historically segregated" such that there are "two distinct institutionally organized traditions of sociological thought—one black and one white" (Bhambra 2014: 472). It is by dint of analytical bifurcation that race theory has been segregated as a "topic" within sociological theory and the study of racism has been isolated from issues of "general sociological concern" (Bhambra 2014: 475). It is only by insisting upon seeing America's endogenous forms of stratification as sharing important similarities with and enduring connections to America's own brand of 'coloniality' that it will be possible to accurately comprehend, analyze, and respond to our present political dilemma.

Go should be commended for having the temerity to insist upon the relevance of postcolonial thought to sociology for two decades despite being repeatedly told he was on a "fool's errand." It turns out that he was right and his many detractors were wrong. He has had the curious fortune of having written a book that is, quite possibly, far more revolutionary than he had imagined it could be. The book serves, in some sense, to domesticate postcolonial theory by making it work within the strictures of social science. The challenge remains for the rest of us, particularly those of us who are sociologists of race, to pick up the gauntlet and run with it. Now that Go has domesticated postcolonial theory, we must use it to make theory un-governable.

Endnotes

1. See, for example: Robert Kagan, "This is How Fascism Comes to America." *The Washington Post* (May 18, 2016); Christopher Ingraham "Interest in Fascism Surges in the Trump Era," *The Washington Post* (February 9, 2017); Why Americans are Thinking About Fascism" *The Washington Post* (February 9, 2017); Ross Douthat, "Is Donald Trump a Fascist?" *The New York Times* (December 3, 2015); Peter Baker, "Rise of Donald Trump Tracks Growing Debate Over Global Fascism," *The New York Times* (May 28, 2016).

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Comments on Julian Go's *Postcolonial Thought and Social Theory*

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Postcolonial Thought and Social Theory is a book that deserves recognition. I strongly agree with its main assertion: Social theory will be enriched by taking postcolonial thought into account. Postcolonial thought will expand its horizons and help correct its parochial tendencies.

I especially appreciate Go's indictment of metrocentrism, by which he means "the transposition of narratives, concepts, categories, or theories derived from the standpoint of one location onto the rest of the world, under the assumption that those narratives, concepts, and categories are universal" (p. 94). Metrocentrism promotes a false universalism. It leads us to read the history of the third world in terms of gaps and deficiencies. It blinds us, in short, from a world of difference.

To correct for metrocentrism, Go argues for highlighting the subaltern standpoint, which he defines as "a social position of knowing…rooted primarily in geopolitics and global social hierarchy," "the activities, experiences, concerns, and perspectives of peripheral populations" (p. 159). The subaltern standpoint is not an essential but a relational identity. It is not reducible to race, culture, or geographical bloc, but speaks from a position of relative powerlessness. It represents a "subjugated knowledge." The subaltern standpoint is not privileged. It is just another perspective but one that is normally occluded. It is imperative therefore, for the sake of objectivity or, if you like, truth, to bring it to light. Moreover, excavating the subaltern standpoint, Go writes, will lead us to new knowledge—new categories, concepts, and theories—and even new ways of knowing.

The conceit here is that a distinction posited at one level between center and periphery, pointing out a difference, really, in political, economic, and cultural power, is felt down the line as a difference in experiences, perceptions, discourses, and even rationalities. This conceit of difference is central to postcolonial thought but is, I think, more complicated than it first appears. Threading through these complications is worthwhile and doing so can help us better understand the place of postcolonial thought vis-à-vis social theory. I would like to use this space to do just this. Let us pursue the following set of questions: Is there an identifiable subaltern standpoint? Is it coherent? Is it self-aware? Does it represent a new way of seeing?

Based on my own research on the urban poor in Manila, I can say that the term "third world," despite being politically incorrect, gets to a real difference in empirical context. Whether this difference comes down to colonization, late industrialization, or peripheral position in a global system of capitalist production—probably some combination of all three —I do know that being in Manila represents a different urban experience than being in Chicago. This difference does not just boil down to them being different cities. It is not the same thing as saying Chicago is different from San Francisco or Manila from São Paulo. I am pointing out a difference that cuts across the development divide, a difference in historical experience and social structural terrain.

Cities in the Global South began to urbanize rapidly, explosively, around the 1950s and 60s. Unlike Northern cities, which grew as they industrialized, these cities urbanized without extensive industrialization. Their economies were unable to absorb the tremendous growth in population and, as a result, large informal economies emerged. The lack of adequate housing led to widespread squatting. These cities came to be distinguished by a structural division between formal and informal sectors in both work and housing. Indeed, this division has become emblematic of the third world city as we know it.

Given this difference, the analyst has no choice but to revise the standard categories of urban sociology. To take an example close at hand: Segregation in Manila does not look like segregation in Chicago. It cuts across class not race. It is not characterized by ghettoes concentrated in one or two parts of the city but by slums and enclaves spread across the metropolis. Unequal spaces are not far apart but close together. Their residents are not isolated from one another but interact unequally such that, paradoxically, interaction enforces, rather than diminishes, social distance.

Fine, but does this difference suggest a subaltern standpoint—one that is identifiable, coherent, and self-aware? On one hand, people in the third world use the same categories as people in the first world to articulate society. They speak of states, social classes, civil society, and citizenship; they speak, that is, in terms of the basic categories of political sociology. On the other hand, they are aware that these categories ring differently in their contexts. Sometimes they evaluate this difference by deploring it as an index of their shortcomings as a society, of how far they have yet to go in order to be truly "modern," lamenting, for instance, the lack of discipline on the road, the fact that traffic lights are taken as advisory rather than imperative. In this respect, they reinscribe a metrocentric narrative of development. Sometimes, however, they evaluate their difference from the West by celebrating it as an index of the humanity they have retained and not lost in the drive to modernize, pointing out, in this case, the flexibility of road rules and the receptiveness of traffic enforcers to a particular type of "reason."

To the extent that there is a subaltern standpoint, it does not consist in the repudiation of "Western" categories. It does not consist in the invocation of new, "indigenous" categories. These are the gestures an analyst makes in the name of the subaltern. Difference becomes knowledge not in the form of new categories but in the re-inflection of familiar ones.

In any case, my point is this: To the extent that there is a subaltern standpoint, it does not consist in the repudiation of "Western" categories. It does not consist in the invocation of new, "indigenous" categories. These are the gestures an analyst makes in the name of the subaltern. Difference becomes knowledge not in the form of new categories but in the re-inflection of familiar ones. People negotiate with the categories at hand. There is elbowing to make room for their empirical reality. But they do not discount these categories because of their Western provenance. Their "corruption" is hardly an issue. Here I think of Anthony Appiah's comments (1991) on a Yoruba sculpture of a man on a bicycle. James Baldwin selected the sculpture for inclusion in an exhibit of African art in New York City. For Appiah, the piece stood out for its "less-anxious creativity." The other pieces, he writes, were self-consciously African, made "in the mold of the Africa of 'Primitivism,'" but with a postmodern wink. In this respect, they seemed more oriented towards the Western art market. In contrast, *Man with a Bicycle* was produced "by someone who does not care that the bicycle is the white man's invention: it is not there to be Other to the Yoruba self; it is there because someone cared for its solidity; it is there because it will take us further that our feet will take us; it is there because machines are now as African as novelists…and as fabricated as the kingdom of Nakem" (p. 357). In sum, I think that for most people in the third world the categories of modern social life, like the bicycle, are not seen as foreign or imposed but simply accepted, with some retooling, as the terms of reality.

But this is not all. We can also speak of a hard difference, an ontological difference. Best to illustrate. The Katipunan, the Philippine anticolonial movement against Spain and later the United States, is usually understood in the terms laid out by its elite leaders. In fact, however, the movement was largely plebeian in composition. As Reynaldo Ileto (1979) points out, both the ilustrado and indio components of the Katipunan were oriented towards independence, but they understood "independence" (kalayaan) very differently. For the ilustrados, the educated and largely mestizo elite, independence meant that they would take over from Spanish and American administrators; they would be the ones driving the modernizing project. For the indios, Ileto suggests, kalayaan meant the overthrow of this project. The difference ran even deeper. It was not just that the ilustrado and indio segments had different goals but, in at least some cases, different conceptions of what—or who—independence was.

One local Katipunan leader, Ruperto Rios, claimed to actually possess independence. He and his men went around with a wooden chest marked "independence." The chest was inscribed with various protective hieroglyphs and guarded by three virgins. When his followers deserved it, Rios said that he would open the chest and, Ileto quotes the American colonial governor, "'Independence would jump out, they would catch her, and be ever afterwards happy'" (p. 189). The Americans found this notion utterly "fantastic." They also found it subversive. It is not hard to see why. According to one of Rios' followers, "'When independencia flies from the box, there will be no labor, Señor, and no jails and no taxes.'"

I call this a hard difference. Postcolonial thought can recognize it and even celebrate it, although I suspect it is easier to celebrate from a historical distance. The Spanish described the Katipuneros as fanatics and "hallucinated unto death," running heedlessly and headlong into their bullets in the belief that their faith, embodied in the form of various totems, amulets, potions, and holy cards, would render them impervious to harm. It is probably more evident today than ever of late that this is a world that remains with us. Postcolonial thought can recognize this difference, try to understand it, and take it into account—indeed it should; I see this as its mandate—but it cannot, ultimately, integrate it. This hard difference represents the limit of postcolonial theory and, as such, serves to put it in perspective. Go has portrayed social theory and postcolonial thought as being in opposition, one born in empire and the other in reaction to it, one speaking from and for the center and the other from and for the margins. I would suggest that the two are in opposition only locally; more broadly they play on the same team.

It is clear to me that postcolonial thought is a continuation of the Enlightenment project. To be sure, it takes aim at the parochial assumptions of social theory, and by doing so enriches it, expands its horizon, globalizes it, but it does so out of a shared sense of mission and on the basis of the same essential grounds. Explanation looks to the domain of the social rather than to the realm of gods and spirits, it proceeds by reason, and it holds notions of equality and freedom as sacrosanct. Are these not precisely the values that Fanon and DuBois upheld against the unreason of racial discrimination? The long argument postcolonial thought has been having with "Western" thinkers is, as I think Go shows, a sympathetic one. It is not aimed at proving them wrong but at showing that they did not go far enough in pursuing the spirit and promise of the Enlightenment beyond their shores, and even beyond their stations. As with the bicycle, provenance does not corrupt. What matters is that ideas take us somewhere, and postcolonial thought, as not apart from but a part of, social theory has taken us far indeed but largely along the tracks laid down by the Enlightenment project, which we must justly regard as a global one.

The long argument postcolonial thought has been having with "Western" thinkers is, as I think Go shows, a sympathetic one. It is not aimed at proving them wrong but at showing that they did not go far enough in pursuing the spirit and promise of the Enlightenment beyond their shores, and even beyond their stations.

The hard difference makes this clear by posing dilemmas that we cannot ignore or finesse, try as we might, but must confront head on. In *Habitations of Modernity*, Dipesh Chakarbarty (2002) points to one such dilemma as faced by the anthropologist Nita Kumar. One of Kumar's informants dies of "mysterious ailments." She becomes frustrated that his family is content to leave its cause vague. It is clear to her that he is killed by the filthy conditions of the Banaras neighborhood in which he lives, the very conditions that, Kumar writes, "are extolled by indigenous Banarasis as beyond any considerations of stench and garbage." She continues:

I do not care for my informants' lifestyle in the way they do. I want them to live longer, enjoy better health, earn more, beget fewer children, and out of place as it sounds, learn of modern science. I do not know how best their culture can be encouraged to coexist with such development, but, however it does happen, a precondition will be a knowledge of this culture in itself (p. 79).

In my view, these sentiments are as good a sketch as any of both postcolonial thought's mandate and limits. A commitment to understand the "subaltern" on their own terms cannot easily be disassociated from normative judgment, nor should it, even though making such judgments can trouble our notions of center and periphery and which side we speak for and from.

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Postcolonial Historical Sociology? A Reply to Garrido, Magubane, and Morris

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Over the past decade, historical sociologists have adopted a new interest in empire and colonialism, grappling with such issues as the economic legacies of colonialism, the class bases of imperial decline, or the determinants of colonial policy. But the epistemic legacies of empire upon our own sociological theories, concepts, and research remain unexplored.1 Inspired by postcolonial thought, my book considers some of these legacies and attempts to map ways of transcending them. The question is whether my attempt has been successful, and I am grateful to Marco Garrido, Zine Magubane and Aldon Morris for critically assessing it. Ultimately I concur with most if not all of their points, and I welcome this opportunity to clarify some of my own. I hope that this response, however brief, does justice to their insightful and gracious readings of my work.

The Task of the Book

Let me begin with a brief overview of what I had hoped the book might accomplish. The larger task of the book is to address a question that has perplexed me ever since graduate school, when I read about postcolonial theory with Dipesh Chakrabary. Can social theory and postcolonial thought be reconciled? The question perplexed me because the histories of the two bodies of thought suggest that they are diametrically opposed. On the one hand, social theory and its practical arm of disciplinary social science, has been developed in, of and for the Anglo-European empires. The very notion of the "social" – as a space between nature and the spiritual realm – was developed in the 19th century by European elites to make sense, and to try to manage, resistance to social order from workers, women, and natives. Social theory thus embedded the concerns, categories and interests of white males in imperial metropoles.

On the other hand, postcolonial thought first emerged in *opposition to empire*. It emerged from the margins if not the underbelly of empire, flourishing amidst anti-imperial protest and resistance from subjugated peoples around the world. Today, when academics utter "postcolonial theory," many most likely think of the academic trend of postcolonial studies that flourished in Departments of English and Literature beginning in the 1980s. They think of scholars such as Edward Said, Gayatri Spivak, and Homi Bhabha or historians like Dipesh Chakrabarty. But this was merely a second wave of postcolonial thought. The earlier first wave of postcolonial thought included writers and activists such as Frantz Fanon, Aimé Césaire, Amilcar Cabral, W.E.B. Du Bois, and C.L.R. James (among many others). These are the thinkers in whom the second-wave found inspiration. And their ideas originate as responses to the racialized violence and exploitation of that very imperialism to which sociology was tied. Indeed, this is why this body of thought is called "postcolonial", not because the "post" signals a historical movement after colonialism, but rather because it signals ways of knowing and seeing the world that escape the confines of the imperial episteme. Postcolonial thought seeks to transcend the modalities of thought associated with the colonial and imperial projects of the past centuries.

In short, this is the problem: social theory embeds the culture of imperialism; postcolonial thought manifests critiques of empire. Social theory comes from the center of modern empire and was part of the imperial episteme; postcolonial thought rose from its margins and offers sustained critiques of imperial formations while envisioning post-imperial futures. This basic tension has more current manifestations. For example, it is manifest in the resistance from some social theorists and social scientists to postcolonial studies on the grounds that postcolonial studies is too namby-pamby, postmodern, or that it is only about identity politics, culturally reductionist and overlooks "real" materialist questions (Chibber 2013). On the other hand, the tension between postcolonial thought and social science is also manifest in the resistance to social science from postcolonial scholars in the humanities. To many of these scholars, social science is far too positivist and woefully scientistic to be useful as a critical tool against Northern hegemony. As a tool of empire, it could never be a tool of anti-imperialism.

> ***In short, this is the problem: social theory embeds the culture of imperialism; postcolonial thought manifests critiques of empire. Social theory comes from the center of modern empire and was part of the imperial episteme; postcolonial thought rose from its margins and offers sustained critiques of imperial formations while envisioning post-imperial futures. This basic tension has more current manifestations.***

My argument in *Postcolonial Thought and Social Theory* is that the two bodies of thought can and must be reconciled. First, I argue against the humanities critique of social science. This critique fails to recognize the multiple ways in which postcolonial thought itself depends upon a certain form of sociological thinking and claims about the social. Postcolonial thought cannot in fact jettison social theory without cratering upon itself. This does not mean postcolonial thought is useless though. To the contrary, and second, I argue that postcolonial thought helps illuminate two analytic tendencies of social science which are inherited from its embeddedness within the imperial episteme: "analytic bifurcation" and "metrocentrism." Finally, I argue that overcoming these problematic tendencies does not require rejecting social thought but rather rearticulating it, partly by drawing upon certain other tendencies already immanent to social science. I thereby argue that we can meet the postcolonial challenge through "postcolonial relationalism" and the "subaltern standpoint," both of which might serve for a new "third wave" of postcolonial thought (this time, rooted in the social sciences).

But here arises the critiques.

The Limits of the Standpoint

Morris and Garrido both raise concerns about the "subaltern standpoint" approach. This approach is an injunction to begin our investigations not through the metrocentric use of existing dominant theory but through excavating first the experiences, concerns and categories of the "subaltern"; that is, of those groups located at the bottom of geopolitical hierarchies, and whose experiences have thereby been occluded from our social theories. While this approach has some relevance to "histories from below", going back to E.P. Thompson's work, I argue that it is distinct, and that its lineage also lies in the methodological and epistemological insights of first wave postcolonial thinkers like Fanon and Dubois. It also draws upon feminist standpoint theory while hitching it to recent calls for "Southern theory," though I argue that, in order to escape the essentialism, subjectivism and epistemic relativism typically associated with these existing approaches, a subaltern standpoint approach needs to be grounded in perspectival realism: the ontological and epistemological position, developed also in scientific perspectivism in the philosophy of science, that all knowledge is socially-situated, partial and yet objective.

Morris, however, wonders: Why restrict our analyses to "the subaltern"? Why not also examine the standpoint of the "powerful"? My articulation of the subaltern standpoint would countenance this wholeheartedly, for it rejects the conventional claims of epistemic privilege made by earlier feminist standpoint theory. My proposal rests upon the assumption that all standpoints have the capacity to produce knowledge, just that they each offer only partial knowledge They are like different maps of a city: a map of the subway system tells us something about the city but not everything, while a map of the streets tells us something different (but also not everything). In this sense, the subaltern standpoint is theoretically equivalent with the imperial standpoint: both are socially-situated, both are partial and yet (potentially) objective. And they each tell us something we may need to know, though never everything we need to know. It follows that the imperial standpoint and the sociologies that emerge from it must not be rejected entirely. Instead, the knowledge generated from the imperial standpoint must be seen for what it is: partial rather than universal.

The warrant for a focus upon the subaltern standpoint is two-fold. First, too often social science does not recognize the situatedness and partiality of knowledge. It instead assumes that the knowledge produced from the imperial standpoint is universal (hence it falls prey to metrocentrism). Second, because of this metrocentrism, other standpoints have been neglected if not repressed entirely. My call for a postcolonial sociology that begins from the subaltern standpoint is in this sense strategic: because the imperial standpoint is hegemonic, we need to pay attention to the other standpoints conventionally repressed by it. We have been for too long living with only one map. We need new ones, and we can get them by starting from the subaltern standpoint.

Garrido's insightful comments hit on this issue exactly, and calls attention to the many difficulties that arise in trying to develop and employ this subaltern standpoint approach. One issue is whether the subaltern standpoint is

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"identifiable, coherent, and self-aware." I would suggest that this is an empirical question. Some subaltern standpoints might be identifiable, coherent and self-aware; some might not (a class may or may not be of itself and for itself). But either way, the subaltern standpoint approach does not require a coherent and self-aware subject. The approach rather indexes an empirical starting point. Do we begin our analysis of colonialism by investigating only what the colonialists say, think, or experience? Or do we begin as Fanon or Dubois did: by investigating how colonized peoples experienced it? The subaltern standpoint approach suggests the latter. Furthermore, the approach does not suggest that the experience of colonized peoples is itself social knowledge. It merely suggests that social knowledge is better produced by starting with the categories and concerns of those groups. So whether it is coherent and self-aware is not a requirement.

But this gets at a larger issue raised by Garrido. He brilliantly distinguishes between two types of difference that the category "subaltern" unwittingly summons: "soft" difference and "hard" difference. This reminds of Spivak's (1988) seminal essay on whether the subaltern can speak. What Garrido refers to as "hard" difference, from what I can tell, is akin to the space of untranslatability that Spivak refers to in her category "the subaltern." This is a distinction that I do not discuss in my book, but it is illuminating. The distinction allows me to here adumbrate some of the different purposes of the subaltern standpoint approach. In the book, I suggest that one benefit of a subaltern standpoint approach is not just to find new categories and concerns upon which to mount our postcolonial sociologies but also to push at the limits of seemingly universal knowledge – to draw the boundaries of the imperial standpoint from the standpoint of the particular. This is exactly the strategy offered by Dipesh Chakrabarty (2000: 20), whose project of "provincializing Europe" partly involves critiquing dominant European narratives from the standpoint of "non-European life worlds." The goal is not to fully translate or understand the experiences or subjectivity of the subaltern. (as if full "understanding" were possible). Rather, the goal is to reveal the limits of our analytic categories by showing how they cannot fully enclose the experiences or categories of the subaltern – which we can access, however partially. Accordingly, if we start from the space of "hard" difference (in Garrido's helpful term) without purporting to subsume that difference into sameness, we can unsettle our metrocentric theories. Rather than writing our histories of power from above, we can write postcolonial historical ethnographies that provincialize our assumed universalisms (Comaroff and Comaroff 1992; Go 2013). This to me seems something worth trying.

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The Possibilities of Postcolonial Relationalism, pre-Trump and Today

Magubane's essay approaches the book from another angle: she historicizes it. Magubane astutely observes that the measured and highly academic tone of the book differs markedly from the passionate, fiery, and activist-oriented writings of, say, Frantz Fanon. She also hypothesizes that the historical context of its enunciation (Obama-era United States) helps explain its tone. "The care that Go takes in the text not to 'overreach'" Magubane observes, "reflects the fact that as recently as three years ago, no one in the mainstream media seriously entertained the suggestion that America ever was or could be a fascist state." Furthermore, she suggests that our present Trump era gives the book a different valence. Today, "where politics is more uncertain and volatile and the possibilities for a 'third wave of postcolonial thought, emerging within and for the social sciences' … is more than an exciting possibility," she announces. "It is now an imperative."

Magubane's essay is provocative. It compels me to think harder about what a postcolonial sociology might contribute to our understanding of more recent events in the United States. How, for instance, might we think about race relations today from a postcolonial perspective?

Let me take an example from my own new ongoing research. It is by now known that over the past decades police detectives in Chicago have repeatedly detained and brutally interrogated African-American criminal suspects. In a secret warehouse at Homan Square in North Chicago, certain officers were found to have shackled African-American suspects to walls for hours on end, threatening to harm family members and vowing to pursue the death penalty in order to compel confessions. In some cases, officers were found to suffocate suspects with plastic bags or apply electrical shocks to their genitals in order to force confessions. Now, we could easily connect this story to the pressing problem of police brutality today against African-Americans across the United States. But what interests me is that this is *not* just a domestic or national story. Consider that at least one of the detectives who had participated in the brutal regime in Homan Square did not spend his entire career in Chicago. Instead, this officer, by the name of Richard Zuley, later took a job with the American military at Guantanamo prison. There, many of the same interrogation techniques that were used at Homan Square were used against America's terrorist suspects – including innocent men from the Middle East who had been abducted by the United States military, just as African-American men in Chicago had been abducted by the Chicago police.

Zuley's story is also part of an even longer history: another officer in Chicago who participated in the interrogation regime at Homan Square in the 1970s had previously served as a military police investigator in Vietnam during the Vietnam War. His name is Jon Burge, and he had tortured suspects in 1968 using portable electric generators; he then went to Chicago as a police detective and purportedly tortured close to one hundred African American men from the early 1970s to the early 1990s.

All of this amounts not just to a "national" or "domestic" story but an imperial and hence a global story: a story of transnational imperial power targeting marginalized populations here, "at home" and overseas, "over there"; a story of power that connects the inside and the outside, the domestic and the foreign, the national and the imperial, and puts them into co-constitutive relations. And we could multiply such examples of connectedness. Recall, for example, that at the Black Lives Matter protests in Dallas where five police officers were shot, the shooter was Micah Johnson, a veteran of the Afghan wars. Recall, too, that the robotic bomb apparatus that the police used to eventually kill Johnson was the ANDROS F-5; an apparatus initially designed for the United States military to use in its wars overseas. We could also reach back further in time and find similar connected histories. The first Special Weapons and Tactics Team (SWAT) in the United States, for example – teams that have been so often deployed to repress urban "insurgencies" and Black Lives Matter protests – did not emerge from thin air. It was founded in Los Angeles by John Nelson, a Vietnam veteran and former United States Marine.2

These are kinds of assemblages of imperial power that postcolonial relationalism would alert us to, thereby allowing us to see the connected experiences of subjugated populations in the US and abroad. Yet, most of these connections and transnational relations of power are occluded in sociological theory and research – including even, as Magubane suggests, in critical race theory.

There are exceptions. Magubane's own work is an exception (2004; 2005; see also Jung 2015). Another exception is Whitman's *Hitler's American Model* (2017) to which Magubane appropriately refers in her essay. While Whitman does not draw from postcolonial theory, it is the sort of transnational relational analysis of power that evinces the principles of postcolonial relationalism. Rather than only an endogenous development, Germany's internal colonialism was embedded in a wider field of imperial power wherein rested American models of racial policy towards immigrants, African-Americans, Puerto Ricans, and Filipinos. And Hitler's regime praised and emulated those models. This is a revelation.

What is it about the subfield of comparative-historical sociology that has rendered it, like critical race theory and like other sectors of social thought, immune to postcolonial thought? And might that change in the near future?

Manifesting Edward Said's notion of "overlapping territories" and "intertwined histories" upon which postcolonial relationalism is mounted, Whitman's analysis helps us better sketch the connectedness of subjugation.

Yet, notably, Whitman's work is not from a historical sociologist. Whitman is a legal historian. This should make us wonder: what is it about the subfield of comparative-historical sociology that has rendered it, like critical race theory and like other sectors of social thought, immune to postcolonial thought? And might that change in the near future?

Endnotes

1. Many comparative-historical sociologists that I have spoken to seem to think of "postcolonial theory" as either meaningless word-play from literary critic, analyses of post-colonial societies, or a body of theory that makes colonialism a new variable in our otherwise conventional analyses. A key dimension of postcolonial thought, however, lies in its epistemic critique, as I discuss in the book.

2. The foregoing is extracted from Go (In Press).

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Book Symposium

Partisans and Partners

The Politics of the Post-Keynesian Society

University of Chicago Press

Josh Pacewicz

Editor's Note: The following text is based on an author-meets-critics session held at the annual meeeting of the Social Science History Association in November, 2016. My thanks go out to Elizabeth Popp Berman, Michael McQuarrie, and Josh Pacewicz for contributing their comments to the newsletter. -YS

Partisans and Partners: Perceptive, Prescient, and Pessimistic

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Partisans and Partners is an impressive book. The project is huge, the integration of ethnographic and comparative-historical methods is outstanding, and the book integrates several very different stories—about urban politics, national polarization, and federal neoliberalism—in new ways. The methodological appendix will make an excellent assignment for research methods classes.

The book itself makes a set of overlapping arguments, not a single argument. First, it makes the case that federal policy change led to a redistribution of city-level resources in a way that favored one type of urban political actor (partners) over another (partisans), and transformed city politics by depoliticizing it. Cities once received federal pots of money that the dominant political party could control. Over time, funding patterns changed, requiring cities to compete with each other in an effort to garner smaller amounts of state support. This advanced the role of public-private partnerships: those who were willing to put aside differences and form temporary coalitions in order to sell their towns to external audiences were successful. In the new reality, such coalitions became not only a necessity but an ideology—partisans saw partners as sellouts, while partners saw partisans as uselessly political.

This in turn reshaped local party politics, as partners intentionally abstained from party politics. Instead, party politics was left to activists who represented more extreme positions, a tendency that reinforced itself and intensified in successive waves. By showing that dynamics were likely to have been similar in other cities, and that national-level explanations like top-down party polarization and campaign finance changes can't explain this new party activism, *Partisans and Partners* makes a strong case that changes in federal policy—encouraging corporate mergers and buyouts, and fracturing funding mechanisms and introducing more competition—was what transformed this environment. Federal changes facilitated the rise of the partners and, indirectly, the polarization of the partisans.

The book goes on to show how the new partisan/partner divide is reflected in the political understandings of ordinary voters. While some older voters ("traditional voters") still conceptualize business and labor as the core groups that Republicans and Democrats represent, most voters see contemporary urban politics as reflecting a tension between partners (people working together for the community) and partisans (people representing particular interest groups). Voters see themselves, too, as either partner or partisan.

Partisans are alienated from contemporary life. They think the world is going to hell in a handbasket. They're looking for change, and find outsider candidates appealing—candidates who promise to tear things down. Typically, partisans have a strong preference for one of the two political parties, but unlike traditional voters, that preference is rooted in disaffection with the alternative rather than longstanding positive commitment to a party. Partisans tend to like more extreme candidates or third parties, if they are not entirely disaffected.

Partners, on the other hand, seem to think that politics should not exist—that we should just all get along. They too are frustrated by the polarization of politics, but think conflict is avoidable and everyone is really the same underneath. Partners distrust politics, their party affiliation tends to be provisional, and they often respond only to negative ads around hot-button issues. They are overrepresented among younger voters.

The Obama campaign illustrates the political commitments of these two types. In the primaries, Obama appealed most to partners, who liked his post-partisan image. Traditional Democratic voters and partisans found Obama less appealing. In the presidential election, however, traditional voters and partisans tended to get on board with Obama as representative of the Democrats, while partners waffled as Obama came to appear more partisan. Partners' votes, or whether they'll vote at all, were less predictable.

The uncommitted partisans were the most erratic group. They were disaffected and angry and wanted politics to solve their problems, but weren't strongly committed to a party. These citizens wanted somebody—anybody—to shake things up, to burn down the system. And they wanted someone to represent *them*—the regular guy, the outsider. These general patterns of thinking held past the 2008 election and through the 2012 one, though voters' perceptions of Obama sometimes changed during that period.

In general, the argument of *Partisans and Partners* is very well made and strongly defended. But I would push back on two points. One is whether the partisans-to-partners shift is really caused specifically by neoliberal policies, versus broader political-economic changes. There is a case to be made that the loss of the local business community was the most important cause of partnership. But while that loss was facilitated by financial deregulation, it was also driven by global competition and the rise of finance. The latter shifts are not entirely unrelated to neoliberalism (and the book does talk about shareholder value and the corporate takeovers of the 1980s), but reflect a slightly different configuration of changes than "neoliberalism."

I agree that the shift in the urban funding mechanism is the proximate cause for promoting partnership, and making cities compete for funding is a neoliberal approach. But other things were changing here as well.

The power of labor and the power of traditional business were both in decline even before the funding mechanism changed—a power vacuum was already opening up. And power brokers would have had to work hard to lure new businesses in, even without the change in federal financing. So I wonder whether the emphasis on neoliberal policies specifically—versus broader political-economic transformations—is misplaced.

The second point I'd question is the third part of the argument—the look at individual voters and how they conceptualize politics. This part of the analysis can get a bit fuzzy. While traditional voters seem like a clear enough type, I didn't get a sharp sense of the difference between partners, partisans, and the mixed type that combines the two—especially since each of those groups has subtypes of its own. And the argument relies on partner voters' position being homologous with partner leaders', and partisan voters' with partisan leaders', a claim of which I'm not convinced.

Partners voters and partners leaders do seem to have similar political views: compromise can always be found and interests don't have to fundamentally diverge. But partisan voters are different from the business Republican/labor Democrat groups the book earlier characterizes as "partisan." Partisan voters aren't partisan in the "believing in interest-group politics" way of the old-school labor and business leaders. And they aren't "partisan" in the sense of the party activists of the present. In general, they don't seem so "partisan" at all—just angry and disaffected. Like the partners, they hate politics. And if you can have "partisans" who are not committed to either political party, who basically want to burn down the system, what does "partisan" even mean?

A related question is whether this "partisanship" that looks more like disaffected anger is in fact connected to the partisan-partners story told in the first two-thirds of the book. It makes sense that the partner perspective among voters mirrors the shift in city politics. But it's less clear that the anger of the partisans is driven by the increasingly activist character of the parties. If it is, the pathway seems likely to be less direct—activism leads to Congressional deadlock which, in the context of economic stagnation, turns into political anger. So I am skeptical not that these are real types of voters, but that the partisan/partner story told in the final third of the book follows directly from the story about city politics told in its first two thirds.

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With those critiques made, though, I want to touch briefly on the 2016 election, because it's impossible to read this book on the polarization of Rust Belt politics at this moment in time and not think about it. How do we read *Partisans and Partners* in light of the Trump victory, which was produced by a few tens of thousands of voters in places like these two Iowa cities? And what can the book tell us about the Trump voter?

Partisans and Partners clearly foreshadows what we saw in November. It captures the contempt Democratic activists have for many residents of Prairieville, who are "too busy watching TV" or "blinded by religion." Pacewicz quotes one: "'You try to talk to them, break it down into simple steps, but then they just look at you like this,' the activist concluded, making a dull-eyed face with his mouth agape." This is a far cry from the union leaders who saw themselves as part of the working class, and saw their political role as bringing economic benefits to it.

If the Obama of the 2008 primary was the perfect partner candidate—building bridges, eschewing politics as usual, not yet tainted by the political system—Trump was the ultimate partisan candidate. And this is where it's important to note that the defining characteristic of the "partisan" group is not its strong allegiance to a party, it's disgust with the system. Partisans want to blow things up. Tear things down. They're angry—with economic decline, with cultural change, with political corruption—and they want something different.

Key to understanding the 2016 election is a subset of the partisans. The partisans who are angry, but nevertheless strongly identify with one of the two parties, aren't the ones who decided the election. The critical group is the "small subset of partisans who repudiated existing politics, but were ever on the lookout for a political candidate who might shake things up." As voter Linda says, "I'm always looking for the outsider, because they won't know what to expect and will just go with their gut feelings." She also said, "Personally, I think that the country may need another civil war." In an election like the one we just had, Pacewicz's book implies that partners will become disaffected and not vote. Traditionalists and party-affiliated partisans will vote the same way they always do. But the nonaffiliated partisans? Those are the Trump voters who won the election.

While hindsight is, of course, 20-20, it's very easy to read the 2016 election through the lens of partisans and partners. The partisans—and again, this points to the imprecision of that term—go for the populist candidate, the outsider. So the rise of the partisan can explain the success of Bernie as well as Donald. On the Democratic side, though, Bernie lost, and the Clinton-Trump fight was unusually ugly. After such a campaign, one would expect the traditionalists, the party-affiliated partisans, and the mixed voters to rally behind their party's candidates, which is more or less what happened. The partners, not seeing a post-partisan candidate like Obama in 2008, stayed home. The unaffiliated partisans, though, had the bomb-thrower they were looking for, and turned out for Trump. It's at least as plausible as any of the other stories out there—explaining Trump's narrow win, but with fewer people voting than in 2008 or 2012.

Now that we're in this brave new world, though, and partisan polarization continues to intensify, what should we expect in the future? Pacewicz appears to pessimistic—at one point he writes, "The trends that I discussed…would appear to spell trouble for American democracy." City-level partnership leads to party-level polarization which leads to Washington gridlock which leads to anger, cynicism and either detachment or a desire to blow up the system. But the cycle doesn't work in reverse. Even if one could magically reregulate finance and restore block grants, it wouldn't be enough.

Moreover, the extent to which this is truly a story of unintended consequences—a federal change reorganizes city politics in a way that transforms parties, feeding back to have federal-level effects—makes any reform strategy seem likely to play out in ways that are difficult or impossible to predict in advance. The one modest lever that seems visible is redistricting. More competitive districts would put some reins on the cycle of ever-increasing partisan extremism. It might not be enough, but it would be something.

In the end, though, we have to live with the limits of what sociology can offer—insight into how things turned out the way they did, and new ways to think about the world around us. Ultimately, the social world is enormously complex and confidence in the consequences of our actions is almost always misplaced. That's not much of a comfort in the current context, but it is probably the best we can do.

Comments on *Partisans and Partners*

Michael McQuarrie
London School of Economics

For someone like me who studies urban and national politics, and is often interested in the links between the two, this book is an exciting pleasure. It makes use of numerous sociological traditions including community studies, institutionalist political sociology, and field theory in order to make its case. Josh is himself a scholarly pragmatist who manages to stitch together the insights of a variety of theoretical traditions and interpretations of American political history into a novel new narrative of America's political transition between the Keynesian and Neoliberal eras. The tremendous nuance and complexity underlying much of the book is nonetheless distilled into a series of tightly connected arguments about the changing political landscape in the two cities he studies, River City and Prairieville. The empirical and theoretical underpinnings to his arguments means that Josh's characterizations of political culture in these two cities are useful for more general hypotheses about a national shift in American politics.

The core of the argument revolves around the relationship between cities and state and national governments. Regulation School institutional analysts and urban scholars like John Mollenkopf have long noted that Keynesianism had a spatial dimension that entailed using tax revenue to even out uneven capitalist development. Government spending has a geography and can either reinforce or mitigate the spatial dynamics of private investment. Importantly, it is often assumed that this spending was useful to legitimate "regimes of accumulation" even in territories that were poorly served by the historically-specific geography of capital accumulation in the post-war era. But the assumption behind this was a politics of material interest whereby consent was secured through the provision of material well-being. This sort of argument makes heavy use of the idea that politics is fundamentally about the distribution of material resources to secure consent and that citizens are, first and foremost, interested in material gains.

Josh accepts the institutional argument that relations between cities and other scales of government are politically important, but he doesn't buy the idea that it is because citizens are effectively bribed into consent. Instead, he argues these relationships matter because they are fundamental to the organization of local public spheres, that is, arenas of political deliberation, negotiation and contestation. Josh effectively weds a historical institutionalist argument to a much richer argument about politics that is characteristic of public sphere scholars, or scholars of political speech.

In terms of historical narrative, Josh hits upon a very novel and convincing shift between the Keynesian era and the neoliberal era of urban public spheres, namely the role of stakes. For Josh, post-war Iowa towns were characterized by a partisan, but nonetheless pragmatic, form of politics that was heavily embedded in local social structures and institutions. He cites a number of characteristics of these cities and the Keynesian era more broadly that contributed to this style of politics, but a key factor that he emphasizes is that federal transfers to localities meant that there was something to pragmatically argue about: namely, how should we spend this money?

The neoliberal era does not lead to an absence or shortage of resources according to Josh, but rather a fundamental shift in how resources are made available and restrictions on their use. Urban coalitions now have to compete for grants and mobile capital and to do so successfully they must enroll diverse urban constituencies into support for the proposal. This emphasizes "partnership" and subsumes the distinctiveness of the social positions that make up coalitions of partners. The changed relationship between the city and other scales of governance shift, not so much because resources are fewer, but because the type of politics they require or enable is fundamentally different.

At the same time, people who aren't partners no longer have any local resources which might provide locally-controlled stakes for politics. With nothing else to argue about partisans have a more ideological and less pragmatic orientation to partisan issues, which importantly have no hope of resolution through local political or civic action. The result is a form of minority extremism.

Josh is eloquent about a number of the consequences of this shift for American politics, the most important of which is probably the idea of "structural deceit". That is, neoliberal institutional arrangements result in fundamental, structurally-determined, misunderstandings of the real and the possible.

Ok, with that groundwork I want to pull on two threads. The first is the relationship between the particular and the general.

Josh makes a general argument about neoliberal urban statecraft in which there is a tension between partners and partisans and between the two ideological wings of partners. But the configuration of actors and institutions is often radically different from what he describes. For example, a fuller understanding of the type of statecraft he describes would require an understanding of how the northern black revolt was incorporated and, later, the backyard revolution described by Harry Boyte. How does the partisans/partners division map onto the opposition between indoor and outdoor politics, or partisan action vs. nonprofits or civic action? To sustain itself as a general narrative, which I think is plausible for a number of cities, there probably needs to be a stronger explanation of how this works in cities that have different political cultures from those in the cities he studies.

How does the partisans/partners division map onto the opposition between indoor and outdoor politics, or partisan action vs. nonprofits or civic action? To sustain itself as a general narrative, which I think is plausible for a number of cities, there probably needs to be a stronger explanation of how this works in cities that have different political cultures from those in the cities he studies.

A central and obvious issue to think about in this regard is the role of city power. After all, Josh is centrally focused on the relationship between spending, policy, and political culture. One of the first sources of variation in this regard, and one that has effectively—if infrequently—been taken up in studies of urban governance is the relative ability of different cities to act autonomously in the use of taxation, spending, and policy to create distinctive political cultures in cities. If city power matters, and I think it does, then the relationship of the specificity of these cities needs a more in-depth treatment to be linked to a more general account of American politics. But this also points to a more general problem with the specific and general that Josh has. I think he has a powerful account here, but Prairieville and River City can only underpin a loose analogical form of reasoning to get to the general.

The second point is a theoretical point and a question of historical narrative. In situating himself relative to theories of the public sphere, Josh engages with the Habermas of *A Theory of Communicative Action*, and an interpretation of Tocqueville which emphasizes a style of civic deliberation that subsumes strong interests and emphasizes authenticity. These are appropriations of Habermas' and Tocqueville's work that are common in sociology but that also de-historicize their arguments. I would argue that Josh suffers for choosing to engage on this relatively typical-for-sociology turf. This turf does have the advantage of making Josh's arguments appear more distinctive relative to Habermas and Tocqueville, respectively, but the disadvantage of masking real theoretical elaboration while passing on an opportunity to fundamentally modify our narrative of the public sphere.

Both Habermas and Tocqueville were much more interested in the social and institutional conditions that enable different types of publics to emerge than Josh lets on. Both are interested in the emergence of publics that are not ideological and polarized. For Habermas, this emerges in the Britain with the rise of the state and with it, the rise of a literary culture that was focused on the state and was sustained in salons and coffeehouses. This public breaks down with the emergence of mass culture.

Tocqueville's argument about American exceptionalism is that the weak state forces pragmatic self-reliance and self-interest rightly understood. It yields pragmatism and moderation that is focused on getting things done and, one gets the sense from Tocqueville, is intolerably mediocre and banal. For both Habermas and Tocqueville, intersubjective norms and institutional arrangements are central to the functioning of democratic public spheres. Josh's "relational public sphere framework" doesn't actually seem that different from the Habermas of *Structural Transformation of the Bourgeois Public Sphere* or Tocqueville.

What is different about Josh's account is an imagination of what these public spheres look like in the modern world. Both Tocqueville and Habermas argued that their public spheres were incompatible with modernity. For Tocqueville a large state and bureaucracy would replace self-reliance which would, in turn, leave people with ideological divisions to argue about and, when that happens, American democracy loses its distinctiveness and becomes vulnerable to the sort of ideological revolution that occurred in France. For Habermas, the historically-specific public sphere of *Structural Transformation of the Bourgeois Public Sphere* gets subsumed in mass society and a political discourse of the lowest common denominator.

In showing that authority over meaningful stakes produces moderation and civic virtue Josh's argument is basically the same as both Tocqueville's and Habermas'. It is the stakes that organize the norms for all three, or at least some version of all three. The radical difference with Josh, and he does note this by the way, is that it is not *separation* from the state that produces this, as is assumed by many scholars and theorists of democracy and civil society, but integration with it. What matters isn't autonomy from the state, but the form the relationship takes.

The second and more profound difference in theoretical terms is that Josh, rejects both

Habermas' and Tocqueville's assumption that modernity is incompatible with civic moderation. In showing that the form of the state-citizen relation matters as much as the fact of it, Josh opens the door to thinking about various forms of statecraft and the sort of politics they enable and underpin, which is a radical improvement over constant invocation of civic decline or the emergence of ever more transactional and cynical forms of politics. Of course, there are others who are generally on this beat, but Josh's work resonated with me because it crystallized a number of these issues in a generative way. At some point, Josh could productively build on his work to provide a more theoretically-elaborated account of the disconnect between our narratives of the public sphere and the practices of statecraft in different eras and different polities. But I should also add that this would merely be the icing on an already tasty intellectual cake. Josh's analysis is novel and thought-provoking. The fact that it is also very timely merely magnifies its already considerable appeal.

Reply to Critics

Josh Pacewicz
Brown University

It is a pleasure to read and respond to this kind, thoughtful commentary on *Partisans and Partners*, not least because Beth Berman and Michael McQuarrie have faithfully spelled out the book's central arguments and noted limitations and critiques that I mostly agree with. That only leaves me fun tasks: reiterating certain points, conceding others, stubbornly re-planting the flag as necessary, and speculating semi-responsibly about the future of American politics.

Can analysis of River City and Prairieville sustain a general narrative about American politics?

This is a place where I need to reiterate: the book's method is not primarily inductive when it comes to arguments about the relationship between federal policy, urban governance, and grassroots party politics. The ethnographic material notwithstanding, the analysis is historical-comparative in that I make the most plausible explanation of today's crazy politics by juxtaposing facts and trends noted by other scholars, but usually examined in isolation. This engagement with existing scholarship should mostly stand on its own; I employed ethnography to point me towards relevant literatures, to identify where existing account were implausible, and to illustrate my arguments (with the exception of arguments about what voters think—qualitative studies of voting are few, so there I relied more on induction). To ground my response, let me start by pointing to what I see as these key trends and facts about American politics.

First, mid-20th century American politics was anomalous. Before the mid-20th Century, partisan polarization in Congress was comparable to today, declining only in the 1930s (this was only partially because of the Dixiecrats), and rising again in the 1980s. Trump's xenophobic rantings are also arguably straight from the 19th Century.

The 1980s-era polarization of American politicians preceded polarization among voters. Baldassari and Gelman (2008) show this convincingly, and one can see the same thing in Pew Research Polls (but read past the headlines)—peoples' policy positions do not grow consistently more Republican or Democratic until the 2010s, from a baseline in which a majority of Americans held an almost even mix of liberal and conservative views (most still hold a mix of views). What's more, polarization of the political system moves from the bottom-up. Early accounts of hyper-partisan grassroots activist appear in the late 1970s, state legislatures start changing in the 1980s, but Congress does not measurably polarize until the 1990s. Because political money goes largely to federal candidates, it is probably not the main culprit.

Given these facts, many political scientists argue that the big changes in American politics began at the level of grassroots organization: local parties, once in the hands of community elites, are now in the hands of polarized, ideologically-motivated activists (this matters because activists shape candidate's perceptions of the world and help them win primaries). This grassroots trend is well-documented and not particular to Iowa or the Rust Belt—for instance, see Masket's (2008) fascinating account of the rise of "ideological political machines" in California's cities. So I see this as the right starting point for the book: what happened in local governance that led local elites to cede control of grassroots politics to weekend activists?

Of course, much happens to cities in the 1980s. Financial deregulation in the 1970s creates a credit-rich economy, which produced the largest corporate merger movement of the 20th Century—this merger wave eliminated once socially and politically dominant local owners, splintering local business communities. Labor Unions also went into free fall. The 1980s also coincided which a transformation of American federalism—urban policy is gutted (federal transfers fall from 20 to 3% of municipal budgets during the decade). Social services also see cuts. There is also a shift in how federal funding is apportioned: increasingly via competitive grants over formula based transfers. The book provides ethnographic descriptions of how these processes play out on the ground, but the claim is not that similar things happened elsewhere because I observed them in River City and Prairieville—we already know that similar things happened elsewhere.

The book's original arguments are about the causal processes that connect these trends—in effect, I trace the causal links between changing political economy and the polarization of party politics in the US (the latter being a prerequisite for the rise of someone like Trump). The argument is that changes in American federalism—especially the corporate merger waves that followed 1970s-era financial deregulation and changes in the prevailing logics of inter-governmental finance—reshuffled each city's leadership class, ultimately leading them to withdrawal from politics. This happens first because corporate mergers and other policy shifts thin the ranks of

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traditional union and business leaders, who were most engaged in partisan conflicts. Second, remaining leaders were left constantly marketing the city to woo corporate employers or win competitive grants. The most effective way to market the city is by building broad-based, ecumenical partnerships that present the city as the right thing to the right set of funders. This mode of politics falls apart if the city is riven with factions, so community leaders gradually ostracize those who they associate with the conflicts of the past.

My argument about this causal process is inductive in that I observed all of these processes on the ground, but it is also the most plausible account given what all the relevant literatures say (so following Tavory and Timmermans (2014), the logic is abductive, not inductive). For instance, I engage with Skocpol (2003), who notes a similar disconnect between grassroots affairs and partisan politics, but chalks it up to local leaders' growing parochialism. This account simply does not square with scholarship on community governance, which unanimous describes local leaders as increasingly entrepreneurial and outwards-focused.

To address Michael's point directly, then, I agree that River City and Prairieville can only support a "loose analogical reasoning" about cities elsewhere, but that is not exactly the book's aim—rather, my aim was to detail how structural conditions produce an ideal-typical process, with the full understanding that this process might play out differently in different places (or, in some extreme cases, maybe not at all). The book is meant to sustain an analogical reasoning, but analogical reasoning about a process, not about a case or cases.

Along these lines, River City, Prairieville, and the Rust Belt in general, are good to think with because they embody the historical extremes of American political economy. As Cybelle Fox (2012) tells us, much of New Deal policy was designed with the needs of the region's white-ethnics in mind but—as we've all heard ad nausea since the election—the last three decades have not been kind to them. Given the sharpness of the transition, the consequences in the Rust Belt are stark, obvious, even operatic (e.g., the once-proud traditional leadership class collapsed within a few years). But ultimately, community leaders in other places experienced the same structural pressures to shift towards an ecumenical, broad-based, post-partisan civic style.

In sum, then, I see the book's goal as provoking exactly the kind of question posed by Michael: how would these pressures play out in a different context? There is no priori way to answer this question—it calls for further research or engagement with similar analyses elsewhere.

Along the latter lines, I find Michael's analysis of Cleveland instructive, because the context is different but many of the trends are similar (see e.g., McQuarrie 2013a, 2013b, 2014). Cleveland experienced a black revolt, which did not happen in the two cities I studied (like the rest of Iowa, they are overwhelmingly blue-collar white). This occurred via a "backyard revolution," which legitimized neighborhoods as site of identity and mobilization in the 1960s and 70s. Thereby, African Americans gained clout via a form of factional bargaining that ultimately culminated in populist/progressive coalitions exemplified by figures like Carl Stokes and Dennis Kucinich. So, unlike in my cases, the dominant local factions were a business elite based in the downtown and an opposition of neighborhood-based, working-class minority and white leaders—but, both in Cleveland and in my cases, pre-1980s politics was essentially factional (I'm simplifying somewhat; see also Mollenkopf 1983, who places greater emphasis on conflicts between black and white members of the New Deal Coalition). After the 1980s, Cleveland became a "partnership city" populated by economic development groups that define proper allocation of resources with return on investment, not social need. The prevailing mode of governance become a hyper-relational style predicated on achieving an economic development consensus—one that privileges already competitive (that is, privileged) areas of the city and undercut the kind of antagonistic politics that traditionally benefited African Americans.[1] One might even argue that this quiescence of traditional politics opens the door to more intransigent movements like Black Lives Matter. This story would sound awfully familiar to Rust Belt Iowans. So I agree that thinking about how structural forces produce similar outcomes in different contexts is good. Doing so sharpens our understanding of those structural forces.

Does the book make too much of Keynesianism and Neoliberalism?

This is where I stubbornly plant the flag, maybe more vehemently than I should. As I see it, the book is about political economy, not politics or the economy (so I agree that political-economic transformations are what should be central to the story). The book is also not about federal policy transformation per se, but about how policy transformation changes community governance and ultimately people's political intuitions.

If *Partisans and Partners* was focused on late 20th-century policy change, then I might agree that periodization into Keynesian and Neoliberal policy is counterproductive. The task then would be to identify how policy agendas evolve over time, are transformed as they hop across domains, and are repurposed by strategic and entrepreneurial actors—a gradualist perspective. But from the ground-up, things look more like a turning point. Cities like the ones I studied lose their local business class, unemployment hits the high teens, once proud union leaders lose their position and are back on the shop floor, downtown stores get boarded up—all that happens in less than a decade. Indeed, people still talk spontaneously of the 1980s as a turning point. Periodization allowed me to work backwards to piece together what happened.

In terms of what happened, I fully agree that economic forces larger than the United States are important to the story. As I say in the conclusion, the claim is not that, were we to get New Deal-era policies back, River City and Prairieville would start to look the way they did in the 1970s. The claim is rather the obverse: absent New Deal-era policy, the world as experienced by people on the ground would have been unrecognizably different.

For example, much of River City and Prairieville's history is intertwined with the post-war manufacturing boom, which was probably inevitable given America's place in the world. But the fact that this boom created robust communities of local business owners is inseparable from policies that discouraged mergers within the same sectors, stringent financial regulations, and so on. The era's business communities were anchored by local banking families, which is unimaginable absent restrictions on branch banking. Similarly, the gradual decline of American industry may have been inevitable, but the cataclysmic crash of the 1980s was avoidable—much of it was due to corporate acquisitions and layoffs and corporate employers restructuring to achieve short term profits. Today too, the Rust Belt's sad state is inseparable from an absence of urban policy, which allows employers to play cities against one another and cities against their suburbs (as in Detroit). The decline of the auto industry may have spelled trouble for Flint, but there is no economic law that compels us to treat its poisoned water as, essentially, a local failure to stimulate economic development.

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This said, I take the critique that *Partisans and Partners* emphasizes political causes over broader economic trends. This is because my goal was to analyze voting behavior in a way that incorporates the insight that policies create politics. Most historically-minded sociologists would accept the proposition that, for instance, the post-war Democratic coalition was forged through the New Deal and Great Society. But such insights rarely make it into folk theories of voting, which are awfully ahistorical. We've all heard people say things like, "Trump's win was not about economics and social policy, because his supporters did not list those things as a high priority."

In contrast to this, the book formulates a theory of voters that highlights how policies shaped the intuitions of generations of voters (and empirically illustrates how this happen). For instance, the blue-collar voters who reliably turned out to vote Democratic well into the 21st Century were a product of the New Deal, but not—as conventional folk theories of voting would have it—because they had a picture of FDR hanging on their wall or even thought much about policy. As the book shows, many of them went to the polls thinking, essentially, "I'm a working person and therefore vote Democratic," but the fact that they thought this was the product of multiple local factors: an indigenous class of business and union leaders, who competed with one another, tried to mobilize support in the economic, civic, and political realm, and so on. Those local factors were a consequence of multiple historical trends, but they were also unimaginable absent New Deal-era policy. Traditional Democrats were a product of the New Deal whether they realized it or not. To be more provocative, Trump's voters are a product of neoliberalism (for lack of a better word), whether they realize it or not.

Are Partisans and Partners the best labels? (No!!! I probably should have titled the book Partners and Populists.*)*

I agree with Beth that the terms partisans and partners are occasionally confusing (particularly partisans). This may be a case of hindsight being 20/20. I did the research before the 2008 and 2012 elections, wrote the book mostly in 2013-4, and submitted a final draft in 2015. During that period, the type of anti-establishment-ism ultimately channeled by Trump (and arguably Sanders) was most visibly directed into hyper-partisan, but relatively traditional party organs like the Tea Party (hence the term partisans). Were I to write the book today, I would probably use the term populist (an adjective I do use in the book).

I use this labeling of voters to distinguish between generational patterns in peoples' political intuitions. Traditional voters (who are mostly older), identify positively with politics: they see public life as contested by a blue- and white-collar side, and see this distinction as interwoven with various aspects of daily life. Nontraditional voters (mostly younger voters) tend to establish political identification negatively, if at all—they perceive daily life and politics as in tension. There are two types of non-traditional voters. Partners implicitly identify with the world around them over partisan politics: they see politics as hopelessly combative and wish it would be more consensus-based and ecumenical. Those I term partisans are angry about the state of the world and look to politics as a way to bring down the status quo. Unlike traditional voters, they see no sides in politics—just an overbearing, heartless elite that preys upon defenseless regular people.

One of the book's arguments is that there is no natural affinity between such predispositions and the parties or candidates that people support—although, importantly, predispositions have consequences for peoples' partisan loyalty and their extremism. Traditional voters tended to be moderate in policy positions (or disinterested in policy altogether) and very reliable. These were the voters who, when asked for an interview, would just say "I'm a union Democrat" or "I tend to see things from more the business perspective" (and hence vote Republican), then express surprise that there might be much more to say. Many partners had political preferences, but established them negatively—they'd say, "politics is ridiculous and I hate it, but I blame that on the Republicans/Democrats for being so uncompromising." Because their underlying orientation was a rejection of politics, partners were not reliable and often checked out during the negative campaigning that proceeded the general election.

Partisans (or populists?) were likewise all over the political map—some thought the GOP was most likely to shake up the system, some supported Obama, and some were apolitical on the grounds that no candidate seemed sufficiently against the system and for "the regular Joe." It is in this respect that the term partisan is confusing (as Beth notes) as some of these voters were uncommitted to a party. Their overarching commonality was a reactionary rejection of the status quo—what we now widely recognize as populism.

To respond to Beth's other question, I do think that voters' intuitions/orientations are the product of community level changes, but not due to a single, direct process. My claim is not that most people say "I like the way my city manager talks—I wish my congressional representative would be more like that!" Rather, peoples' political imaginations are shaped by community-level dynamics in various, indirect ways over a long period of time. The most important mechanism is simply the disappearance of meaningful public conflict. If you live somewhere where labor and business leaders are constantly fighting, it is not too surprising that—one way or another—you come to see politics as a contest between blue- and white-collar sides. After public conflict mostly disappears in community affairs, it is not surprising that people eventually start to see conflicts between politicians as staged and bizarre (or, if life is going badly, says "Hey, nobody is fighting for me! Lets tear this system down!"). But there are other ways in which community affairs shape peoples' political intuitions—Chapter 8 of the book lists a half dozen processes.

Ultimately, this argument rests on the assumption that people have deep, multiplex community ties. As such, the argument may seem particularly implausible to academics, a mobile population. But this is not the modal experience of people in Rust Belt Iowa, who mostly live in the same city (and often same neighborhood) where they were born. To make this more intuitive to myself, I often thought back to high school, with its thick, multiplex ties that subsumed multiple social distinctions (for instance, it has only recently occurred to me that all of the theater kids that I hung out with while growing up in Austin also had parents who worked as mobile professionals, were not originally from Texas, probably had similar politics to my mom, and so on). Now imagine that you still live surrounded by those you went to high school with, only now high school's social distinctions have evolved into labor market outcomes. To quote Lazarsfeld: "a person thinks, politically, as he is socially."

Habermas, De Tocqueville, and the theoretical added value of Partisans and Partners

I fully take Michael's point about the conclusion of the book engaging with the sociology-lite version of De Tocqueville and Habermas (to be fair, though, that is exactly what I claim to do in the conclusion). Michael is also right about the conclusion's intent, key argument, and what I see as its' added value vis-à-vis theories of the public sphere. My aim was to think about moral aspects of the public sphere in relation to a large, complex state. The conclusion is that meaningful democratic contestation occurs in complex integration with the state. The added value is the intuition that much can be gained by thinking about the public sphere in relation to theories of political legibility and action at different scales—a marriage between the classics, Alexander (2006), and Scott's (1998) *Seeing like a State.*

The important shift is between phenomena that I label as the Keynesian and Neoliberal public spheres. The Keynesian public sphere works like this: federal policies shelter local institutions and transfer discretionary dollars to local political bodies and commissions, which makes political contestation appear as a zero-sum distributional conflict. This system is legible from the bottom-up in that local affairs give people an intuitive grasp of politics at different scales—that is, city leaders fight over how to apportion finite resources just like state or congressional representatives. With the neoliberal public sphere, funding is apportioned competitively and, in addition, local leaders are competing over mobile capital (admittedly, a consequence of economic shifts other than policy). This makes the local public experience an unreliable guide to politics at other scales. Contemporary federalism cloaks the reality of scarcity and tradeoffs—there is a finite amount of corporate investment (Molotch 1976) and public dollars, but because they are apportioned through competition, some cities might gain a lot, others go the way of Flint or Detroit. Because winning this contest requires subsuming conflict to an economic development consensus, observation of public life at local scales communicates the false impression that consensus produces a windfall of resources in general (e.g., federally). I continue to think that there is much to be gained by combining theories of the public sphere with a richer institutional account of the state—but admittedly should think more about it.

Is Partisans and Partners *a pessimistic book?*

Of course, I will be grateful if people read the book at all, but—if they're going to read it—I'd prefer that they read it as sobering rather than pessimistic. The take-home implication is that we should expect electoral politics to be volatile for the foreseeable future (particularly in light of the 2016 election). The swing from Obama to Trump has understandably given a lot of people pause, and many observers' reaction is to assume that something big must have happened since 2012. This book implies that nothing big happened—swings like those between 2012 and 2016 are just 21st Century American politics as usual. That is, many peoples' political intuitions do not run the gamut from left to right, so much as from partner to partisan/populist—so Obama and Trump are both normal and expected electoral outcomes (and, as traditional voters continue to die off, will become more so).

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As Beth notes, *Partisans and Partners* implies that the process of partisan polarization and mounting anti-establishment-ism is self-reinforcing at the grassroots. This might sound pessimistic to those hoping that Trump will fail spectacularly, people will wise up, and American politics will revert to something more like the mid-20th Century (I am not suggesting that Beth implied this). Prediction is a tricky thing, but that is probably not going to happen, both because of the institutional party dynamics that I discuss in the book and because voters' preferences usually endure over the life course. If Trump is disgraced and impeached, Rust Belt populists will not revert to lunch bucket Democrats any more than lunch bucket Democrats turned populist overnight as their world collapsed around them in the 1980s. Since Rust Belt populism is here to stay, one could just as easily read the book as optimistic since it shows that there is no necessary affinity between it and Trump's crazed xenophobia nor even the GOP. Indeed, the book makes pessimistic statements about "trouble for American democracy" precisely because it identifies a mismatch between voters' intuitions and actually existing political divides, which almost certainly means continued, mounting polarization and possibly something crazy like Trump—so, in some ways, the thing that the book is pessimistic about has already happened.

Partisans and Partners also does not point towards simple, short-term solutions, but I see that as a selling point. My sense is that Democratic campaign professionals have all the short-term electoral strategies they need. It is their medium- and long-term game that has been less impressive (maybe because they theorize voters synchronically). Depressingly, they have also cannibalized past political sacrifices by relying on Rust Belt voters to win elections without giving them much in return (and here I mean communities of color too—consider that the most significant piece of urban legislation since the 1980s was Bill Clinton's tough on crime funding). *Partisans and Partners* provides a contrasting focus on the medium- and long-term electoral consequences of policy—good reading material for a come to Jesus moment among historically-minded observers of American politics. I do not think that there is a prescription for how policies produce electoral coalitions outside of historical context, but my hope is that the book provides useful intuitions along these lines and therefore better insight into how policies make politics.

Endnotes

1. Levine (2016) tells a somewhat similar story about African American political incorporation in Boston.

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Section News

Spotlight: Comparative Historical Sociology Section Working Groups

Editor's Introduction

Marilyn Grell-Brisk
Université de Neuchâtel

As part of the "Can Historical Sociology Save the World" initiative, members of the section formed problem-solving working groups. The rules for creating a working group were simple – the efforts were to be directed at actually solving a problem, and they were to help the group's members publish peer-reviewed scholarship based on those efforts.

Trajectories has created a recurring spotlight feature that allows the working groups to share their efforts with the rest of the section. In this issue, we are highlighting the Carbon Tax and the Tax Reform problem-solving working groups. They both have very directed agendas but with different approaches.

For those interested in joining an established working group, or would like to create their own, please visit:

https://docs.google.com/document/d/1myGlNGAFSaxu4XtMfcZPCh1xUQrN7FN0n98l68cPLfI/edit

Carbon Tax Problem-Solving Working Group

Of all the threats from which the world needs saving, changes to the Earth's climate may be the most serious. This is the problem we are trying to help solve in a working group on the comparative sociology of carbon pricing. We are doing so by studying the experiences of countries and regions that have implemented, or tried and failed to implement, some form of carbon tax or emission trading system (ETS), including as applied to greenhouse gases other than CO2. Our aim is to identify ways of expanding such efforts, in effective ways, to other contexts.

It's an active group, with monthly video meetings on Skype; last year some members also met face-to-face at ASA. We are currently considering the possibility of an edited volume, with chapters presenting case studies of efforts to put a price on greenhouse gas emissions. Another possibility, maybe for the nearer future, is a multi-authored article reviewing such efforts in a briefer way. In the longer term, we have talked about organizing an international workshop. So far, we have mostly concentrated on reviewing and discussing the existing literature on carbon pricing in comparative perspective, and exchanging notes from background reading about specific cases.

We have had some fascinating conversations--with some debate!--about a range of important issues and questions:

- What are the limits of what environmental taxes and trading systems can accomplish? What are more versus less effective forms of such systems? Do carbon taxes actually even reduce carbon emissions? (Maybe firms just choose to pay the tax, rather than cut their emissions.)

- What factors have made carbon pricing initiatives politically feasible in some places and not others?

- Setting aside the policy pros and cons, what are the political pros and cons of an ETS versus a tax? What do voters prefer? Are there properties of carbon pricing initiatives that make them more appealing (or at least less unappealing) to the public?

- Does business have a strong preference? And does their preference depend on whether firms can get free permits under an ETS?

- What are the possible (progressive/regressive) distributional consequences of carbon pricing systems?

- How should we deal with the carbon embodied in imported goods and services? Who should pay for greenhouse gas emissions generated in the production of goods for export? How much are apparent reductions in countries' emissions due simply to the outsourcing of polluting activities to other countries?

We have also discussed the very different experiences of a number of key cases:

- Australia, which holds the dubious distinction of being the only country that had a seemingly effective carbon tax, and then decided to repeal it;

- Sweden, an early pioneer, with a carbon tax applied at a very high level but reduced in practice through the granting of many exemptions;

- British Columbia, which took the bold step of introducing an ambitious, revenue-neutral carbon tax in 2008;

- Washington State, whose voters just last year rejected a proposed carbon tax very similar to BC's;

- California, whose ambitious cap-and-trade system might not exist but for the leadership of a Republican politician (Governor Arnold Schwarzenegger);

- the Regional Greenhouse Gas Initiative (RGGI), which encompasses nine northeastern states whose carbon emissions declined 40% in the ten years after 2005, while their economies grew 8%;

- Public Benefit Funds, a set of under-the-radar taxes on electricity consumption, applied in many U.S. states under a variety of remarkably innocuous labels.

The group's membership has been stable for a while. But if other comparative sociologists are interested in getting involved, they are warmly invited to contact the group's coordinator, Malcolm Fairbrother (ggmhf@bristol.ac.uk).

We have found video meetings by Skype surprisingly useful. And for us a problem-solving approach to comparative-historical sociology has felt quite natural. Putting a price on carbon emissions is an effort to solve a problem, so learning how to do it more and better is no great stretch. What we have found more challenging has been reconciling our various prior premises and beliefs about the politics of carbon pricing, and defining a corresponding common agenda.

Current active members:

Malcolm Fairbrother (Bristol) -coordinator

Josh Basseches (Northwestern)

Jean Boucher (George Mason)

Jeff Broadbent (Minnesota)

Bill Holt (Birmingham-Southern College)

Steven Karceski (Washington)

Monica Prasad (Northwestern)

Ethan Schoolman (Rutgers)

The Tax Reform Problem-Solving Working Group

The tax reform group is focusing on capping or killing the home mortgage interest deduction (HMID)—the second-largest tax break in the U.S., the benefits of which go predominantly to the wealthy.

Here are some things we've done that worked well:

Individual papers on one topic

Whereas some of the other working groups are collaborating on a single piece of research, we decided to focus on this one issue (the HMID) but write individual papers on it. This has the advantage that we can all take different positions on the issue, and hopefully get a scholarly debate going with our eventual publications. Another advantage is that it forced us to have a big discussion on which issue to focus on, which was fun and educational, as it required us to put together our understanding of the current tax policy landscape and what reforms might be feasible in the next few years with our assessments of where sociological analysis could be most useful.

In-person meetings

We've been lucky to have met twice in person now, once at ASA, and once in February. For the February meeting one of our members was able to secure funds to fly the rest of us in. We're planning on meeting again in July, and again at SSHA in the fall. Even with the ease of video conferencing these days, there is still nothing like meeting in person: people make time in their schedules for it, they come prepared, and having made the effort to travel they are ready to spend the entire day discussing the issue.

A good historical question

Unlike some of the other problem-solving groups, we have not had the problem of trying to make sociological theory speak to our issue. There is already a thriving research group on "fiscal sociology," and we slot well into that. Of course, the real test of this will come when we try to publish our papers.

And one thing that perhaps hasn't worked so well:

Focusing at the national level

In retrospect, focusing on national-level politics in the U.S. may not have been a wise idea, as this is the policy domain perhaps most difficult to influence. It seems clear that we'll be able to publish peer-reviewed research from the discussions of this group, but much less clear what happens after that. Some of the other working groups have been having more success working with non-governmental organizations, and research at the state or local level may also be more likely to find traction in the political field. On the other hand, the advantage of a national focus is that the HMID is a big, juicy target, that can justify a lot of coordinated effort over several years.

New Publications

Books and Edited Volumes

Neoliberal Apartheid: Palestine/Israel and South Africa after 1994

University of Chicago Press, 2017

Andy Clarno

In the early 1990s, both South Africa and Israel began negotiations with their colonized populations. South Africans saw results: the state was democratized and black South Africans gained formal legal equality. Palestinians, on the other hand, won neither freedom nor equality, and today Israel remains a settler-colonial state. Despite these different outcomes, the transitions of the last twenty years have produced surprisingly similar socioeconomic changes in both regions: growing inequality, racialized poverty, and advanced strategies for securing the powerful and policing the racialized poor. *Neoliberal Apartheid* explores this paradox through an analysis of (de)colonization and neoliberal racial capitalism.

After a decade of research in the Johannesburg and Jerusalem regions, Andy Clarno presents here a detailed ethnographic study of the precariousness of the poor in Alexandra township, the dynamics of colonization and enclosure in Bethlehem, the growth of fortress suburbs and private security in Johannesburg, and the regime of security coordination between the Israeli military and the Palestinian Authority in the West Bank. The book addresses the limitations of liberation in South Africa, highlights the impact of neoliberal restructuring in Palestine, and argues that a new form of neoliberal apartheid has emerged in both contexts.

Modernity and the Jews in Western Social Thought

University of Chicago Press, 2017

Chad Alan Goldberg

In the late nineteenth and early twentieth centuries, prominent social thinkers in France, Germany, and the United States sought to understand the modern world taking shape around them. Although they worked in different national traditions and emphasized different features of modern society, they repeatedly invoked Jews as a touchstone for defining modernity and national identity in a context of rapid social change.

In *Modernity and the Jews in Western Social Thought*, Chad Alan Goldberg brings us a major new study of Western social thought through the lens of Jews and Judaism. In France, where antisemites decried the French Revolution as the “Jewish Revolution,” Émile Durkheim challenged depictions of Jews as agents of revolutionary subversion or counterrevolutionary reaction. When German thinkers such as Karl Marx, Georg Simmel, Werner Sombart, and Max Weber debated the relationship of the Jews to modern industrial capitalism, they reproduced, in secularized form, cultural assumptions derived from Christian theology. In the United States, William Thomas, Robert Park, and their students conceived the modern city and its new modes of social organization in part by reference to the Jewish immigrants concentrating there. In all three countries, social thinkers invoked real or purported differences between Jews and gentiles to elucidate key dualisms of modern social thought. The Jews thus became an intermediary through which social thinkers discerned in a roundabout fashion the nature, problems, and trajectory of their own wider societies. Goldberg rounds out his fascinating study by proposing a novel explanation for why Jews were such an important cultural reference point. He suggests a rethinking of previous scholarship on Orientalism, Occidentalism, and European perceptions of America, arguing that history extends into the present, with the Jews—and now the Jewish state—continuing to serve as an intermediary for self-reflection in the twenty-first century.

A Social Revolution: Politics and the Welfare State in Iran

University of California Press, 2017

Kevan Harris

For decades, political observers and pundits have characterized the Islamic Republic of Iran as an ideologically rigid state on the verge of collapse, exclusively connected to a narrow social base. In *A Social Revolution*, Kevan Harris convincingly demonstrates how they are wrong. Previous studies ignore the forceful consequences of three decades of social change following the 1979 revolution. Today, more people in the country are connected to welfare and social policy institutions than to any other form of state organization. In fact, much of Iran's current political turbulence is the result of the success of these social welfare programs, which have created newly educated and mobilized social classes advocating for change. Based on extensive fieldwork conducted in Iran between 2006 and 2011, Harris shows how the revolutionary regime endured though the expansion of health, education, and aid programs that have both embedded the state in everyday life and empowered its challengers. This first serious book on the social policies of the Islamic Republic of Iran opens a new line of inquiry into the study of welfare states in countries where they are often overlooked or ignored

Innovation in Science and Organizational Renewal: Historical and Sociological Perspectives

Palgrave Macmillan, 2016

Thomas Heinze and Richard Munch (Eds.)

This book looks at the types of new research organizations that drive scientific innovation and how ground-breaking science transforms research fields and their organization. Based on historical case studies and comparative empirical data, the book presents new and thought-provoking evidence that improves our knowledge and understanding about how new research fields are formed and how research organizations adapt to breakthroughs in science. While the book is firmly based in science history, it discusses more general sociological and policy propositions regarding scientific innovations and organizational change. The volume brings together leading scholars both from the United States and Europe.

Breaking the WTO: How Emerging Powers Disrupted the Neoliberal Project

Stanford University Press, 2016

Kristen Hopewell

The global economy is being dramatically transformed by the rise of new powers, such as China, India and Brazil, and the corresponding decline in the political and economic dominance of the US and other Western states. This book provides the first analysis of the impact of contemporary power shifts on the American-led project of neoliberal globalization, by examining a core institution of global economic governance, the World Trade Organization (WTO).

Its central argument is that the emergence of new powers has disrupted the neoliberal project at the WTO. Paradoxically, however, this is not because the rising powers rejected the rules and norms of the multilateral trading system, but just the opposite, because they embraced the system and sought to lay claim to its benefits. Rising powers usurped the dominant norms, discourses and institutional tools of the WTO and used them to challenge US hegemony. Yet, when the weapons of the powerful became appropriated by formerly subordinate states, the system itself broke down. A situation of more equitable power relations among states caused the Doha Round of trade negotiations to collapse and, in the process, cut short the neoliberal project at the WTO. This breakdown represents a crisis in one of the core governing institutions of global neoliberalism.

Intimate Interventions in Global Health: Family Planning and HIV Prevention in Sub-Saharan Africa

Cambridge University Press, 2017

Rachel Sullivan Robinson

When addressing the factors shaping HIV prevention programs in sub-Saharan Africa, it is important to consider the role of family planning programs that preceded the epidemic. In this book, Rachel Sullivan Robinson argues that both globally and locally, those working to prevent HIV borrowed and adapted resources, discourses, and strategies used for family planning. By combining statistical analysis of all sub-Saharan African countries with comparative case studies of Malawi, Nigeria, and Senegal, Robinson also shows that the nature of countries' interactions with the international community, the strength and composition of civil society, and the existence of technocratic leaders influenced variation in responses to HIV. Specifically, historical and existing relationships with outside actors, the nature of nongovernmental organizations, and perceptions of previous interventions strongly structured later health interventions through processes of path dependence and policy feedback. This book will be of great use to scholars and practitioners interested in global health, international development, African studies and political science.

Doing Violence, Making Race: Lynching and White Racial Group Formation in the U.S. South, 1882-1930

Routledge, 2017

Mattias Smångs

The subject of lynching has spawned a vast body of important research, but this research suffers from important blind spots and disjunctures.

By broadening the scope of research problem formulation, staking out new theoretical-analytical tracks, and drawing upon recent innovations in statistical methodology to analzye newer and more detailed data, *Doing Violence, Making Race* offers an innovative contribution to our understanding of this grim subject matter and its place within the broader history and sociology of US race relations. Indeed, this volume demonstrates how different forms of lynching fed off and into the formation of the racial group boundaries and identities at the foundation of the Jim Crow system. The book also demonstrates that as dominant white racial ideologies and conceptions took an extremist turn, lethal mob violence against African Americans increasingly assumed the form of public lynchings, serving to transform symbolic representations of blacks into social stigma and exclusion. Finally, Smångs also explores how public lynchings were expressive as well as generative of the collective white racial identity mobilized through the southern branch of the Democratic Party, whilst private lynchings were related to whites' interracial status and social identity concerns on the interpersonal level.

What is an Event?

University of Chicago Press, 2017

Robin Wagner-Pacifici

We live in a world of breaking news, where at almost any moment our everyday routine can be interrupted by a faraway event. Events are central to the way that individuals and societies experience life. Even life's inevitable moments—birth, death, love, and war—are almost always a surprise. Inspired by the cataclysmic events of September 11, Robin Wagner-Pacifici presents here a tour de force, an analysis of how events erupt and take off from the ground of ongoing, everyday life, and how they then move across time and landscape.

What Is an Event? ranges across several disciplines, systematically analyzing the ways that events emerge, take shape, gain momentum, flow, and even get bogged down. As an exploration of how events are constructed out of ruptures, it provides a mechanism for understanding eventful forms and flows, from the micro-level of individual life events to the macro-level of historical revolutions, contemporary terrorist attacks, and financial crises. Wagner-Pacifici takes a close look at a number of cases, both real and imagined, through the reports, personal narratives, paintings, iconic images, political posters, sculptures, and novels they generate and through which they live on. What is ultimately at stake for individuals and societies in events, Wagner-Pacifici argues, are identities, loyalties, social relationships, and our very experiences of time and space. *What Is an Event?* provides a way for us all—as social and political beings living through events, and as analysts reflecting upon them—to better understand what is at stake in the formations and flows of the events that mark and shape our lives.

New Publications

Articles & Book Chapters

Altınordu, Ateş. 2016. "The Political Incorporation of Anti-System Religious Parties: the Case of Turkish Political Islam (1994–2011)." *Qualitative Sociology* 39(2): 147-171.

FitzGerald, David Scott, David Cook-Martín, Angela S. García, and Rawan Arar. Forthcoming. "Can you become one of us? A historical comparison of legal selection of 'assimilable' immigrants in Europe and the Americas. *Journal of Ethnic and Migration Studies.*

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Section News

News and Section Announcements

SECTION MEMBERSHIP RECRUITMENT DRIVE

Our section's annual membership recruitment drive is under way and we have a lot of work to do. At the moment, our membership stands at 637, far short of the 800 we need to maintain our robust complement of sessions at the 2018 annual meetings. Please encourage your colleagues to join the section today! And then go further by giving the gift of membership to all the budding comparative historical sociologists you know.

ASA policy has been changed so the section gift membership system will close this year on July 31st rather than in the fall. This means we have to ramp our recruitment now rather than waiting for September as we have done in past years.

Here's how to give gifts memberships: visit http://asa.enoah.com/Home/My-ASA/Gift-Section (log in using your ASA user name and password). Select the section for the gift, then search for your recipient's name in the ASA database. Section membership for 2017 requires current ASA membership, but you can purchase several gifts at the same time and then pay online. Each recipient will receive an e-mail immediately after your payment notifying them of the section gift.

If you have good ideas for recruiting new members or want to help, please contact our ace recruitment committee:

Carly Knight (crknight@fas.harvard.edu)
Diana Rodríguez Franco
(dianarodriguezfranco2014@u.northwestern.edu).

DEMOCRACY CONVENTION III

August 2-6, 2017

University of Minnesota, Twin Cities, Minneapolis Campus

The Democracy Conventions bring together policymakers, community leaders, movement intellectuals, and researchers working to strengthen democracy where it matters most: in the institutions and the daily life that constitute U.S. society. As the progressive reformer Robert M. La Follette wrote, "democracy is a life [that] involves constant struggle" in all sectors of society. The Democracy Convention recognizes the importance of each separate democracy struggle, as well as the need to unite them all in a common movement for democracy in the United States. More than a single event, therefore, the Democracy Convention houses nine conferences under one roof. This year, these will include the Community & Economic Democracy, Democratizing the Constitution, Earth Democracy, Education for Democracy, Global Democracy, Media Democracy, Peace & Democracy, Race & Democracy, and Representative Democracy conferences.

To register or to find more information, see:

http://www.DemocracyConvention.org.