Trajectories

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Letter from the Chair

Social Science in the Twenty-First Century

Bruce G. Carruthers

"Preaching to the choir" is not a phrase for praise. The activity is something to be avoided, a type of communicative redundancy in which one person tells other people what they already know. A quick application of Google's Ngram reveals that use of the phrase "preach to the choir" briefly spiked around 1900, and then it virtually disappeared until the late 1970s when frequency of use began to climb steadily. Perhaps admonishing people not "to preach to the choir" has itself become a form of preaching to the choir. Nevertheless, I'm going to preach to the choir. And I begin by casting doubt on the received

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wisdom about this very phrase by noting that every Sunday many, many people preach to many, many choirs. Does the widespread recurrence of this activity not suggest that "preaching to the choir" can serve a useful purpose? Someone thinks it does, and I am inclined to agree.

So now let me preach to you, the choir. And this fall's sermon is about the value of comparative-historical research (feel free to raise your hands at any time, or shout out an "Amen"). The spirit moves me through a couple of prompts. The first involves the uncommon attention given to Thomas Piketty's recently translated and justly acclaimed book, *Capital in the Twenty-First Century* (Harvard University Press, 2014). This was a publication event impossible to miss, and for a time the book put economic inequality squarely on the U.S. public agenda (until it was displaced by the Kardashian sisters, or baseball, I forget which). By several orders of magnitude, the public impact of the book far outstripped that of the academic articles previously co-authored by Piketty and Emmanuel Saez (2003, 2006), even though those articles in many respects foreshadowed the book's major results. To be sure, a number of factors contributed to the book's success, but one crucial element was the comparative and historical scale of the analysis: it is both a big book (685 pages), and a "big picture" book, based on an extraordinary data collection process that covered both time and space. Early on, figure 1.1 gives the reader a century of U.S. income inequality in one panoptic glance, and no-one can fail to see that inequality reached an extreme in the late 1920s, declined dramatically until the mid-1940s, remained at a low level until 1980, and then began a long increase that returned inequality to its "roaring 20's" peak in the years just before the 2008 financial crisis. Two pages later, figure 1.2 presents the reader with a chart tracking the capital/income ratio from 1870 to 2010 in Germany, France and Britain. As a multiple of national income, private wealth declined dramatically in these three countries in response to the shocks of World War I, the Great Depression, and World War II. Then, starting around 1950, private wealth accumulations began to recover and did so steadily through 2010. And Piketty is just getting warmed up. Such a grand scale characteristically casts a light on taken-for-granted features of society, poses real counterfactuals, and invites big questions about longue durée processes and fundamental social conflicts. One might wonder, for example, why income inequality in the U.S. peaked just before the last two major economic crises: the Great Depression and then the Great Recession. Inquiring minds want to know. In other words, Piketty's ambitious breadth and scale, and his willingness to engage arguments made by classical thinkers like Ricardo and Marx, mirrors just the kind of analysis that is the staple of comparative-historical sociology. He is an economist, to be sure, but also a kindred spirit.

The second prompt concerns scandals that have underscored the political power of finance, one of the collateral effects of financialization. For starters, a number of official investigations (resulting in large fines imposed on banks by various regulatory agencies) revealed corruption in the process whereby one of the world's key interest rates (so-called LIBOR, the London Interbank Offer Rate) is set. This benchmark interest rate is used in millions of financial contracts, and so the integrity of its creation is of paramount importance. More recently, a wrongful termination lawsuit filed by Carmen Segarra against the Federal Reserve Bank of New York revealed how deferential bank regulators had become with respect to the big Wall Street banks they ostensibly regulated: as near-perfect a picture of regulatory capture as one could want. And shortly after the 2008 financial crisis, an SEC investigation (Securities and Exchange Commission 2008) revealed how much conflicts-of-interest had undermined the independence of the credit rating process applied to the structured financial products of major Wall Street banks. Rating analysts worked hard to appease their clients and grant as high a rating as possible to tranches and tranches of the CDOs, ABSs, and RMBSs that banks originated.

While it is tempting simply to wring one's hands and denounce personal moral failures and the temptations of filthy lucre, a comparative and historical perspective on finance provides a more temperate and insightful diagnosis. Work by scholars like Fred Block (1977) and Greta Krippner (2011) sets the broader historical context for financialization, and accounts for its rise. Some, like Giovanni Arrighi (2010), even argue that the rise of finance is a recurrent event. Comparative analysis shows that Anglo-Saxon finance is not the world's only model for a financial system, not now and not in the past. At the very least, many have noted the contrast between bank-based (e.g., Germany, Japan, France, South Korea) and capital-market-based (e.g., U.S. and U.K.) financial systems, and recognize the significant implications for public policy and financial intermediation (Deeg 1999, Zysman 1983). Furthermore, even if it is true that credit rating agencies like Moody's and Standard and Poor's now pass judgment on sovereign nations around the world by rating their debt (Sinclair 2005), the diffusion of other iconic features of modern finance, like credit cards, has proven to be a much more complex and uneven process (Rona-Tas and Guseva 2014).

I could offer more examples, but a good preacher knows when to stop. Time to bring this sermon to a close, release the choir, and let them go about their daily business with renewed conviction that comparative and historical analysis is simply a very good way to do social science in the twenty-first century. Feel free to leave some money in the offering tray before you head out.

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Book Symposium

The Fracturing of the American Corporate Elite

Harvard University Press

Mark S. Mizruchi

Editor's Note: The following text is based on an author-meets-critics session organized by Bruce Carruthers that took place during the American Sociological Association Annual Meeting in San Francisco in August, 2014. My thanks to Mark Mizruchi, Bill Roy, Judy Stepan-Norris, Tony Chen, and Bruce Carruthers for agreeing to write up their comments for the newsletter.

The Irony of Twenty-First Century Pluralism

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If there was any theme that unified twentieth century political sociology, it was the fate of democracy in the modern world. Classical political theory implicitly assumed a simple society, making distinctions only among political persuasions and between citizens and non-citizens. Fundamental social differences such as race, gender, or class were not meaningfully theorized. But the complexity of modern society posed a challenge for democratic theory evoking responses that underlay major debates in political sociology. Early in the twentieth century, political scientists developed a new theory of democracy—pluralist democracy. Instead of framing the accountability of government to its citizens in terms of individual representation, the complexity of society was refracted through a system of organizational representation based on interest groups. Occupational groups, business groups, regional groups, and identity groups would mediate between the individual and government, with the complexity of society and the shifting lines of coalition preventing the domination of any one group in a system of pluralist democracy (Bentley 1908). Mid-century sociologists challenged pluralism, asserting that democracy was impossible in the modern world because major institutions had become so hierarchical and centralized that ordinary citizens were marginalized from meaningful political influence. Prefigured by Marx's characterization of the state as the executive committee of the bourgeoisie, the self-described "plain Marxist" C. Wright Mills shattered the pluralists' benevolent image of modern democracy with his 1956 book, *The Power Elite*. In contrast to rule by a plurality of interest groups, Mills wrote, "By the power elite, we refer to those political, economic, and military circles which as an intricate set of overlapping cliques share decisions having at least national consequences. In so far as national events are decided, the power elite are those who decide them" (Mills 1956: 18). A decade later G. William Domhoff systematically documented the degree of elite cohesion and the important role of coordinating organizations such as the Committee for Economic Development (Domhoff 1967; Domhoff 1979).

This debate fueled a golden age of political sociology, spear-headed by veterans and supporters of the New Left who grew up believing in American democracy but were disillusioned by poverty, racism, sexism, and militarism. By the mid-70s, power elite theory seemed to have won the debate as pluralism faded from the agenda and the debate moved onto debates among Marxist approaches and then various institutionalist approaches. In the 90s and 00s, political sociology remained lively but focused on various sociologies of—sociology of race, immigration, gay rights, development, taxation, cities, gender, family, inequality, etc., much of it viewed through a cultural lens.

In the last decade, political events have again posed a challenge for political sociology. Like most Americans, political sociologists are flummoxed by the political polarization and paralysis of government. How can political sociology explain how our present system became so—to use a technical term—fucked up? Up steps Mark Mizruchi with a formidable, ambitious, and bold analysis, rooted in political sociology theory, thoroughly documented with rich data from systematic sociological studies, archival sources, and solid journalism. Anyone with any interest in a sociological explanation of our present quagmire should read this book.

The book is written for a broad audience, with its sophisticated theoretical underpinning more subtext than explicit. It is my hope that contextualizing this terrific book within political sociology's debate over the fate of democracy can foster greater appreciation for its achievement and some insight about the current situation. The book's singular theoretical achievement is to show that both pluralists and power elitists erred in assuming that the decay of corporate unity would enhance democracy. In fact, the opposite has come to pass. The fragmentation of the formerly cohesive elite instead has induced paralysis. Thus has Mizruchi (225) challenged the relationship of the corporate elite to democracy held by both pluralist and power elitists: "American democracy actually thrived during a period in which the corporate elite–those at the very top–experienced a broad level of unity. It is perhaps ironic, therefore, that as this unity frayed in the 1980s and then disappeared in the 1990s, American democracy found itself imperiled. The fragmentation of the corporate elite created a vacuum of leadership that led to political stagnation, a system 'stuck in neutral.'"

The Fracturing of the American Corporate Elite thus sparks a new stage of what now seems like an archaic debate, but in doing so, demonstrates that the changed historical circumstances can be better understood with analytic tools inherited from that debate. Neither the pluralist and power elite descriptions of American politics may fit reality as well now as the post-war period, but we need to understand both theories to explain what is wrong with the current system. Mizruchi is transcending the debate with a synthesis of the pluralist/power elite dialectic. The narrative arc of the book and the major source of variation is a before and after story in which the post-war "before" of moderate and pragmatic corporate leadership is contrasted with the post-70s "after" when the fragmented corporate elite has abandoned moderate leadership on behalf of aggressively pursued short term interest. The major independent

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variables that distinguish the before and after are drawn from pluralist and power elite theory.

But it is significant and ironic that a major contributor to the sociology of corporate power has reinterpreted the "before" in more pluralist than power elite terms and characterized the "after" by the abdication of leadership. Not only does he conclude that the most fundamental conditions for elite power rule has eroded, he also reinterprets the American post-war political system to fit the pluralist description better than the power elite description. His description of the post-war political system is that a coherent, cohesive corporate elite coordinated by commercial banks and peak organizations (as portrayed by power elitists) was constrained by powerful unions and government regulation (as portrayed by pluralists), resulting in pragmatic and moderate leadership. That is, pluralist constraint trumped power elitist cohesion. To be sure, true to power elite theory, corporate power was unrivaled, but it is corporate moderation and pragmatism that is persistently documented. What the power elitists at the time saw as corporate domination, fomenting the military-industrial complex, American imperialism under the guise of anti-communism, the evisceration of American cities and spread of poverty, is now seen wistfully as the rule of moderate and pragmatic elites taxing themselves when needed, tolerating unions, expanding the welfare state, and reining reactionary business interests. The point here is not the accuracy of either portrayal; Mizruchi is certainly correct that in relative terms, the post-war system was preferable. The issue at hand is the theoretical source of the analysis and how the book draws on both pluralism and power elite theory. Especially important is how Mizruchi draws on their common assumptions.

Both theories assumed that power was at least in part a function of how cohesive a group is, the extent to which they hold a monopoly of authoritative positions (in contrast to the presence of countervailing groups), and the extent to which cleavages among groups aligned into polarized clusters or cross-cut into a plurality. The crux of the debate took the form of methodological and empirical claims. Pluralists examined specific decisions, typically legislative decisions, to show that no group dominated, that different issue areas were influenced by different constituencies, and that coalitions shifted (Bachrach 1962; Dahl 1967). Power elitists sought to demonstrate that the power elite was cohesive, that major corporations dominated the higher levels of all institutions, and that the power to set the agenda, which took place out of public view, was more consequential than particular decisions. But both sides would agree that if the corporate elite became more fragmented, if they lost their monopoly over authoritative positions, and if cleavages fissured along new lines, democracy would be enhanced. Mizruchi shows that both were wrong. Corporate cohesion has eroded but democracy has not been enhanced.

Why? Mizruchi's analysis shows that some of the assumptions in both theories were wrong. The pluralists underestimated the extent to which a system of pluralist democracy would necessarily serve the public as a whole. As early as 1960, political scientist E. E. Schattschneider wrote: "The flaw in the pluralist heaven is that the heavenly chorus sings with a strong upper-class accent" (Schattschneider 1960: 34). By now, it is not only a strong accent, but for all practical purposes, the entire language. The mechanisms once seen as the core of pluralist democracy are now widely viewed as grossly distorted and fully captured by corporate interests—elections, lobbying, interest groups, even public interest groups. Not only have corporations mobilized more resources with greater skill, but the state itself has stacked the deck against ordinary citizens through neoliberal reform and judicial activism. When journalists and sociologists document excessive corporate power and the assaults on democracy, they typically focus on the practices and institutions once seen as the heart of pluralist democracy.

Just as pluralist mechanisms were shown to be less democratic than claimed, Mizruchi documents that corporate elite rule was more moderate, and at times, public-spirited than its critics would have admitted. The power elitists assumed that if the corporate elite was cohesive and if they controlled policy making agencies, the state would become an accommodating and uncompromising instrument of corporate interests. One of Mizruchi's most provocative and fully developed arguments is how the corporate elite at its most cohesive and most organizationally active was also at its most moderate and pragmatic.

Thus Mizruchi has offered a formidable challenge to the prevailing liberal interpretation of the role of large corporations in American politics. Most left-leaning commentators have assumed that the rightward drift in American politics since the 80s is due to an increase in corporate power. Mizruchi writes that while there has certainly been a decline of the left, it is the decline of the political middle that concerns him most. The decline of the middle, he asserts, is a result of elite fragmentation. The rise of the Tea Party and anti-state right may be assisted by some corporate elites, but overall, their rise is also a result of elite fragmentation and corporate abdication of responsibility.

As important as it is for political sociology to bring the pluralist and power theories up to date, Mizruchi's approach has one limitation: *The Fragmentation of the Corporate Elite* is more about "fragmentation" and "elite" than about "corporate." In terms of the agenda addressed in the golden age of political sociology, if the book had addressed some of the issues raised by marxian political sociology (with a small m), it would have had a stronger explanation of corporate power in both the postwar period and the early twenty-first century. What difference does it make that he is talking about the corporate elite? Focusing on the explanation of moderation and pragmatism, he neglects what corporate interests specifically were and how they were served. To be sure, in discussing specific issues such as taxation, health care, unions and regulation, there is plenty of content, but always to illustrate how the corporate elite was moderate and pragmatic back then, but not now. It is understandable that one would want to avoid painting corporate politics in a naive way. Thus the need for an explicit explanation for why they were moderate and pragmatic rather than purely self interested. But one should also consider the possibility that what today appears to be pragmatic and moderate may have in fact operated for the long term interests of the corporate sector. Could the change in corporate politics have resulted from a change in corporate interests, not just the lifting of constraints that had masked what the corporations wanted to do all along? Asking that question poses the risk of backward reasoning of what Arthur Stinchcombe called Marxist functionalism (Stinchcombe 1968). That is the reasoning that assumes if something happened it must be in the interest of the powerful because the powerful always get their way. So one looks at what happened and then tries to find how that actually served the interests of the powerful. We do want to avoid that sort of reasoning but perhaps we at least need to ask the question.

Another way to say that Mizruchi has more to say about fragmentation than corporations is that his story is primarily one of political logic, with only intermittent consideration of the systemic operation of the political economy. For Mizruchi, political actors relate to each other in networks and attempts to influence each other. They constrain each other's actions by imposition of will. Their unity and network coherence thus underlies their power. These are all crucial to the political system, but are never autonomous from economic relations. For Mizruchi, economic events do set the context for some changes such as the transformative collapse of the Keynesian consensus in the midst of stagflation. Variables such as unity, coherence, and political mobilization are relevant for all political actors from NIMBY neighborhood associations to the corporate-led Business Roundtable. Corporations are treated as interest-seeking organizations, but the content of those interests is seen more as growth and decline than profits. One major insight of marxian theories of political economy is that the political power of groups is influenced by more than purely political factors, that dominant economic actors get more bang for the political buck. Corporate unity, cohesion, and mobilization have greater payoffs than non-corporate actors. The book says a great deal about how politics works but little of how capitalism works. The political logic without economic factors is clearest in the discussion of banking, especially commercial banking. Bruce Carruthers fully elaborates the role of banks in his comments, so I won't comment further except to say that for Mizruchi, commercial banks played a coordinating and moderating role in the post-war system, but their decline was an important factor in the fragmentation of the corporate elite. I applaud that he treats bank power and bank role as historically specific, not something built into the system. But you miss something if you treat banks only as just another organization that had the advantage of playing an important role because of their centrality in the system of interlocking directorates.

There is one final issue debated by the pluralist and power elitists relevant for this book: the locus of power. Here again, Mizruchi tilts toward the pluralists. As Alford and Friedland put it, each theory had a home domain. For the pluralists, it was the electoral and legislative system. Alford and Friedland (1985) argued that pluralists not only had conducted more empirical research at that level, but had a stronger conceptualization of how that system worked. So it does make sense that if one wants to understand government gridlock, one would look where the gridlock is centered. Mizruchi's final chapter describing the effects of corporate fragmentation uses examples from the legislative process: Taxation and health care policy. Both are distributive issues, which better fit pluralist mechanisms. Mills called the electoral and legislative system a middle level of power and more or less irrelevant to real power. Power elite theory placed the locus of power in the executive and policy-making process. Focusing on the executive branch would lead to different conclusions about the extent of corporate power. For power elitists like Domhoff (2006), the policy-making process is still intact, but with new organizations like the Heritage Foundation. And while the Republican Party has sabotaged the legislative process, the last two democratic presidents have been highly pro-business, moderate and pragmatic. And when corporate interests are really on the line, as in the economic crisis of 2008, the federal government has stepped up to save the day.

These quibbles aside, the book remains our discipline's most formidable intervention into the country's political crisis. Its original interpretation, extensive documentation and theoretical sophistication make it a must read. It is not only at the state of the art of political sociology; as the Political Sociology Section recognized by awarding it this year's book prize, it is defining the state of the art. The pluralist and power elite debate is usually cited as a generative debate that fueled political sociology in the 50s and 60s that was superseded by debates within Marxism, then by the cultural and organizational turns. Some graduate students probably learn about it for field exams. Mizruchi was an important contributor to the debate, especially about the historical roots of corporate cohesion and contemporary analyses of corporate politics and giving. Though situated on the power elite debate, his method was to let empirical findings adjudicate theory. That he can now as a leading senior scholar of corporate political action tap the theoretical wells of both sides to analyze how America has historically entered a new stage of corporate politics reflects on his open-mindedness, his theoretical acuity, and his skill as a researcher. That America has changed in ways that neither pluralists nor power elitists of the 60s or 70s could have imagined reminds us all of the immaturity of our discipline. That a leading proponent of power elitist sociology roots his explanation of that epochal transformation without losing the critical edge of his analysis reminds us that what seems at first blush ironic shows us the wisdom of taking us where the truth leads us.

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Labor and *The Fracturing of the American Corporate Elite*

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I was asked to join this panel mainly to comment on the chapter on labor. This will be my focus here.

Here is the structure of the argument in Chapter 4: Just before and after WWII, there were various factions of capital with different views on labor unions. The National Association of Manufacturers (NAM) was hardline conservative; the Chamber of Commerce (CofC) was generally conservative, but beginning in 1942, had a liberal President (Johnston). Mizruchi draws on two conceptualizations of the divisions. One comes from Harris (1982), who calls one category "belligerents" (mostly NAM supporters).[1] Harris calls his second category of managers "sophisticates" (those who were willing to selectively use violence, but were more subtle in their antiunion activities). And his final category he calls "realists" (this was the most common group consisting of core manufacturing firms; they grudgingly accepted unions). Johnston of the Chamber of Commerce co-sponsored a "Labor-Management Charter" with AFL and CIO Presidents Green and Murray at war's end, where labor ceded managerial prerogatives and agreed to support capitalism in general and managers supported the right of labor to organize and collectively bargain, and promised employment at wages assuring a steadily increasing standard of living. Harris' conceptualization is in contrast to that of

Lichtenstein (1989), who highlights the role of "practical conservatives" (the common view of management in core manufacturing). They recognized the role of unions in stabilizing the work force.

Mizruchi's synthesis: both views are correct. Managers were prepared to fight unions at every step, but they also were able to see the silver lining in the cloud—that unions could have positive aspects as well. Management had accepted its end of the bargain: acknowledging the legitimacy of collective bargaining and independent unions. Unions agreed to focus demands on wages and benefits rather than control of firm decision-making. This is "realism." And "These earlier views would probably not have developed had workers, backed by a sympathetic state, not compelled management to adopt them."

That describes the class factions on the capitalist's side. Except for the statement that workers, backed by the state, compelled management to adopt their more accommodating views, Mizruchi pays little attention to the balance of class forces and importantly, he neglects to consider how class factions and debates within the labor movement mattered. Especially important is the role of the left in the organized labor movement of the 1930s and 40s.

During the 1930s, the Congress of Industrial Organizations (CIO) challenged the dominant U.S. labor federation, the American Federation of Labor (AFL), by organizing rival unions along industrial lines. This challenged a different set of capitalists than those affected by AFL organizing. Many of these were in mass production industries and constituted some of the largest employers in the country. Within the CIO were three internal factions: left, right, and center. The left was led by and/or aligned with Communist Party members and its sympathizers. The center was either middle of the road between right and left or had both factions vying for control. The right was basically aligned with the Democratic Party, and was to the left of most (but not all) AFL unions. As Maurice Zeitlin and I show in *Left Out* (2003), unions with leaders in these various camps negotiated different types of collective bargaining agreements, had different levels of democracy, and had different levels of focus on, and attention to, integrating women and racial minorities into their unions and leaderships. Most important for Mizruchi's argument is the difference in their approaches to managerial prerogatives. The left-wing unions were significantly more likely to negotiate contracts that refused to cede managerial prerogatives than the other two camps. They were also significantly less likely to cede the right to strike during the term of the contract, and this is crucial for Mizruchi's argument on capital's desire for predictability and stability in production.

Management prerogatives play a large role in his story. Management was intent on maintaining shop floor control. He tells of how managerial prerogatives was an important component of the postwar "Labor-Management Charter." But this charter later became irrelevant and NAM developed a set of principles that became the basis of the Taft-Hartley Act. Mizruchi emphasizes how Taft-Hartley entails "broad acceptance by large corporations of the legitimacy of independent, organized labor unions as a central institution in American life." Nevertheless, Taft-Hartley led to important changes that eventually emasculated the labor movement, not the least of which was the requirement that union leaders sign non-Communist affidavits.

Why did NAM move from its staunch anti-union stance, to the more moderate stance embodied in the Taft-Hartley Act? Piven and Cloward (1977) would argue that state managers (and capitalists) were forced to recognize unions due to their disruptive capacities. Therefore, labor's power to disrupt production and to interfere with the internal decision-making within firms, not capitalist's goodwill or their desire to be appropriate leaders of their class, explains the change in NAM's stance.

The two most important aims of NAM's labor relations policy was stabilizing production and regaining control of the shop floor. Eliminating Communist unionists would do both. The Taft-Hartley non-Communist affidavit in effect led to the demise of the left wing CIO faction that was winning shop floor control for workers. Once the CIO was rid of the Communists and their sympathizers, organized labor was less prone to fight for eliminating management prerogatives and to maintain their right to strike. This made it much safer for firms to acknowledge their existence and for unions to fulfill the role of "stabilizers."

Strikes increased throughout the 1960s. Mizruchi argues, like many others who are not cited, that tight labor markets are associated with more strikes. There are obviously other strike determinants that are not mentioned (Ashenfelter and Johnson 1969; Rosenfeld 2014).

Mizruchi's argument is that the context where the new management offensive and the dissolution of the postwar accord were possible, occurred in the late 1960s because of 1) the late 1960s strike wave; 2) productivity and profit decline (perhaps due to unions' inability to control wildcat strikes); and 3) public opposition.

But this context was very similar to that of the postwar period, which 1) had an even bigger strike wave; 2) I don't have data for this comparison; and 3) had very similar public opposition to unions. Rosenfeld (2014) finds public opposition to unions to be about 18% in 1966 and a little above 20% in 1946.

I would argue that there were three crucial differences between the labor movement of the late 1940s and that of the late 1960s: 1) union density had declined (and the trajectory was upwards in the late 40s and downwards in the late 60s); 2) labor had shed its left wing faction and therefore its fight for shop floor control and the right to strike; and 3) labor was united (no major federation rivalries). Southworth and I (2010) demonstrate that progressive labor federation competition leads to a positive increase in the rate of change in union density. By the 1960s, the vast majority of collective bargaining agreements ceded managerial prerogatives and included no-strike clauses. In sum, labor's threat level was considerably reduced by the 1960s. In addition, by the end of the 1960s, there was one big difference facing capitalists which figured into their profit levels: foreign competition.

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Capital's Class and Intra-class Struggles

In the early part of the 20th century, NAM successfully led capital in the Open Shop Campaign and the "American Plan." Griffin et. al. (1986) document negative effects on organized labor, which suffered a decline. With the Great Depression, increased industrial unrest, the ascendancy of Communist and other radical activity (Unemployed Councils, left-wing organizers in CIO unions), and the New Deal, NAM's conservative position appeared to be out of sync and self-interested.

During the 1930s and 1940s, the Chamber of Commerce (with a broader and bigger base than NAM) became the main voice of capital (it takes a much more liberal direction once Johnston became its president in 1942) and FDR welcomed it into tripartite deliberations. But Johnston never won the confidence of the majority of his organization.

NAM was intent on winning class leadership back from the Chamber. Its staunch anti-union stance had failed (to see why, we need to refer to the strength of the unions and the source of that strength), and in order to appear to be reasonable, it had to make changes. From the late 1940s on, the National Manufacturers Association in fact regained class leadership. How did it do this?

According to Andrew Workman (1998), NAM's ascendancy is due to internal changes initiated by its absolute failure at the 1941 Labor-Management Conference (where it was effectively shut out). Here's how it restructured: First, it changed its power structure to allow top leaders more authority and autonomy. Then it worked with the Chamber officials who were to the right of Johnston to get its endorsement on its 5-point program and agreement to proceed jointly at the upcoming labor-management conference. But then Johnston developed a Charter with AFL and CIO agreement, which NAM refused to endorse (due to the charter's support of the Wagner Act). NAM was again isolated. But the Charter subsequently lost crucial support due to an internal AFL/CIO conflict. Meanwhile, Truman was concerned about strikes affecting the public interest. NAM saw its opening. It drew up the blueprint of a new approach to labor policy that utilized a two-pronged strategy.

NAM produced sophisticated policy proposals backed by empirical analysis (with the help of specialists in law and labor economics). It then worked to mold public opinion (by conducting polls and engaging public relations specialists). Here, NAM turned from its position of ending the New Deal and towards an attempt to convince the public that NAM and the free enterprise system operated in the public interest.

Meanwhile, it developed a strategy to control the conference. It did this by 1) forcing its position on press releases; 2) making an agreement on the selection of representatives (it made an agreement with the Chamber to jointly approve of representatives, thus insuring conservative appointments); 3) dividing the conference into multiple committees, each with NAM research at their fingertips; and 4) requiring supermajority endorsement. The Truman administration's participation was in disarray, and labor was divided. NAM was able to capitalize on its pre-conceived and coherent position. The program called for: revision of the Wagner Act to make it more "fair," the protection of managerial prerogatives, limitations on the right to strike against the "public interest," federal regulation of unions' internal affairs, and opposition to the organization of foremen. The big General Motors strike caused public anger and Truman gave up on the conference. NAM had brought the business community together under its leadership while it provided a program that led to the Taft-Hartley Act.

Internal class struggle among leaders of the capitalist class (in response to changes in the power of the working class) led to a turnover of business leadership from NAM to the Chamber of commerce then back to NAM. Capital leadership became important when industrial disruption was intense (high strike rate) and control of production facilities was in question (lack of managerial prerogative clauses in collective bargaining contracts). The larger class struggle between labor and capital set the stage for intra-class struggles within both classes. In particular, the situations with regard to predictability of production (strikes) and especially with managerial prerogatives were the result of struggles within the labor movement.

Labor's Class and Intra-class Struggles

The 1930s was a time of upheaval. As mentioned above, the more left-oriented CIO (with left, right and center factions) challenged the AFL's dominance. The CIO challenge (intra-class rivalry) led to gains for both labor federations, and therefore to the overall strengthening of working class organization. Besides a more organized working class, capitalists faced a more threatening organized labor movement. This was because within the CIO, Communist-led unions fought against ceding managerial prerogatives and the right to strike, and were very successful in their efforts. These were the two features of labor unions to which capital was most opposed.

In response to the success of Communist-led unions on these fronts, capital's offensive focused on their elimination from the labor movement. The Taft-Hartley Act was one important source of pressure on the labor movement to rid itself of Communist leaders and sympathizers; the state's move towards McCarthyism was another. To save itself, the CIO expelled Communists from its midst and then proceeded to attempt to eliminate them from the organized labor movement. The Communists responded meekly, and although they remained effective in a few areas, they were, for the most part, destroyed as a major player in the organized labor movement. The AFL and CIO merged in 1955, drawing to a close the period of labor insurgency, militancy, and radicalism.

While subsequent periods of militancy occurred (the strike wave of the 1970s), they occurred in the absence of major rivalry within the labor movement, substantial radical union leadership, and while union density was on the decline. Capital could count on mainstream unions to help suppress wildcat strikes. This made periods of militancy much less threatening than earlier periods.

In sum, capital's position on labor unions has always been overwhelmingly negative. Very few firms welcome sharing their control over workers and production with unions. Yet when unions are strong, they must be responsive in order to continue producing. In particular, capital will not tolerate a radical labor movement that threatens its ability to ensure predictable production and to make profit. Where these develop, capital launches concerted campaigns against them. As we saw, this was true of the 1940s campaign against the CIO Communists, and there was an equally repressive campaign against the Industrial Workers of the World (IWW) in the earlier part of the last century. If it must (due to workers' power), capital accommodates a tame labor movement, for as long as it is necessary (but without radicals, tame labor movements have not been proactive and are susceptible to attack and decline). The anti-unionism of the more recent period reflects capital's ability to make gains when the labor movement is weakened. This serves to further drive up profit levels. When capital has a position of strength, its efforts are not contradicted by its countermovement, and therefore it tends to be more successful.

Endnotes

1. The NAM is not a big part of Mizruchi's story, yet Griffin et. al. (1986) demonstrate how the resources they amassed mattered for the decline of unionism in the 1920s and Workman (1998) demonstrates how NAM reorganized and raised money in the 1940s in order to take the steps necessary to re-gain its role as business leader, culminating in the early drafting of the Taft-Hartley Act.

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Comments on *The Fracturing of the American Corporate Elite*

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Mark Mizruchi's *The Fracturing of the American Corporate Elite* is a major contribution to political sociology. It is a perceptive guide to a series of important theoretical conversations that have perhaps unfairly languished in sociology over the last few years. It is also a highly instructive introduction to the latest empirical research on business and politics in sociology, political science, and history. Above all, it is a deeply sociological contribution to this broader interdisciplinary conversation, nowhere more obviously than in the nature of the argument that it makes.

Mizruchi's book repays close reading, but let me resist the temptation to comprehensively recount it in fine detail. Instead, what I would like to do is lay out what I see as the main question posed by the book, sketch out the basic thrust of how the question is answered, consider how strongly Mizruchi's answer is supported by the evidence, and suggest a somewhat different interpretation that strikes me as also consistent with what we observe historically.

The overarching question that Mizruchi explores is one that I am sure all of us have pondered in one form or another at one time or another: What in the world is happening with the United States? The country faces no shortage of major problems, ranging from financial crisis to public education, from rising health care costs to crumbling infrastructure, from massive deficits to political gridlock, from high levels of inequality to ongoing racial conflict. But serious and sustained efforts to address these problems are obviously in short supply. Perhaps the most notable absence of all—for a country that is home base to the biggest and richest companies in the world—has been the absence of any meaningful corporate leadership. This vacuum, Mizruchi argues, is "one of the primary causes of the economic, political, and social disarray that American society has experienced in the twenty-first century" (Mizruchi 2013: 4).

A corporate elite did once play an "important role in addressing, if not resolving, the needs of the larger society" (Mizruchi 2013: 4). This occurred during the postwar period, when a "relatively active and highly legitimate state" along with a "well-organized and relatively powerful labor movement" and a fairly integrated "financial community" encouraged a non-trivial segment of the business community to adopt a moderate, pragmatic stance on major questions of public policy, such as industrial relations and economic regulation (Mizruchi 2013: 6).

The happy equilibrium of the postwar period was disrupted by the unprecedented economic and political shocks of the 1970s. New foreign competition, rising energy costs, stagflation, Vietnam, Watergate, and the advent of the new social regulation combined to make business more conservative, touching off a "counteroffensive" against government regulation and labor unions. Bit by bit, American business gained the upper hand. By the 1980s, it had succeeded in large measure—and it began to fragment as a result. Ironically, American business grew less unified. It "had been 'killed' by its own success," Mizruchi concludes (Mizruchi 2013: 8).

> ***...it is worth asking whether his evidence shows that corporate leaders during the postwar period exercised "leadership and vision" in a way that contributed independently to stability, security, equality, and affluence in the United States. Did they really ever act, or were they mostly acted upon? Did they lead, or did they follow?***

Today's corporate elite is a shadow of its former self, much to the detriment of the country in Mizruchi's reckoning. It is narrowly self-interested, disorganized, and largely ineffectual. America's shambolic descent into mediocrity is a prime consequence. If aspects of our collective life seem closer to the post-apocalyptic dystopia of Mad Max than the City on a Hill imagined by John Winthrop, then the abdication of leadership on the part of corporate America is one of the main reasons why. According to Mizruchi, corporate leaders once provided a "degree of leadership and vision" that underpinned many of the most desirable features of postwar life, including the "expanding economy," "declining inequality," "a relatively high level of security, a well-functioning political system, and a widespread belief that problems were solvable" (Mizruchi 2013: 5). That they have gone missing of late is what ails the country. "The gridlock in Washington, the prominent role of extremist elements…the inability to address serious problems…are all due in part to the absence of a committed moderate elite capable of providing political leadership and keeping the destructive sectors of the American polity in check" (Mizruchi 2013: 8-9).

Many aspects of Mizruchi's argument ring true. Some segment of the business community did seem to exhibit genuine moderation in its ideas about fiscal policy, industrial relations, and civil rights. This moderation does seem to have stemmed in large measure from the newfound strength of organized labor and the federal government.

But it is worth asking whether his evidence shows that corporate leaders during the postwar period exercised "leadership and vision" in a way that contributed independently to stability, security, equality, and affluence in the United States. Did they really ever act, or were they mostly acted upon? Did they lead, or did they follow?

This, in turn, requires reflecting on what "leadership and vision" means and how it should be measured. Does it mean that corporate moderates pulled the political economy in a direction that it would not have gone if they had not led? Or does it mean that corporate moderates got the political economy more quickly to a destination where it was basically already heading? Does it mean that corporate moderates came up with ideas that they succeeded in getting realized? Does it mean that corporate moderates understood which way the political economy was heading and had the good sense not to get in the way when they could have? Does it mean—does it have to mean—that leaders of particular companies or industries sacrificed their narrow interests for the greater good? If it does not mean any of these things, then what does it mean to argue that business played an important or critical role?

So what I mainly want to do in my comment is raise questions about evidence and interpretation: What kind of evidence does Mizruchi have that a particular segment of the corporate elite led, and how should it be interpreted?

These are not easy questions to answer. But one way to get traction on them is looking at specific and important outcomes across different policy areas to see whether we can observe things that justify attributing these outcomes to anything like the "leadership and vision" of a corporate elite.

What is it that we observe business doing in various areas of policy then?

Employment and macroeconomic policy

Whether and how government should promote employment was one of the most important questions that was getting resolved as the country was pulling itself out of more than a decade of depression and war, and here it is simply not clear that corporate moderates showed anything like leadership or vision. Most of the business community seemed mainly interested in obstructing policy proposals like the Full Employment Bill of 1945, which to them smacked of what Margaret Weir has evocatively called "social Keynesianism" or deficit-spending of a discretionary nature that sought to simultaneously achieve economic and social objectives (e.g., reducing unemployment and therefore poverty) (Weir 1992: 50). Weir points out that the U.S. Chamber of Commerce (USCC) and the National Association of Manufacturers (NAM)—along with the American Farm Bureau Federation (AFBF)—lobbied aggressively against the Full Employment Bill, motivated by a desire to avoid the experience of the National Recovery Administration, which they considered a disaster for their members (Weir 1992: 46). They mostly succeeded, and what ultimately emerged instead was the Employment Act of 1946, which set up a far less ambitious and powerful program. Mizruchi points out rightly that it contained elements that were "inspired" by proposals originally worked out by the Committee on Economic Development (CED), most notably the Council of Economic Advisors (Mizruchi 2013: 56). But the legislation as passed did not necessarily reflect a robust, coherent, long-sought vision of how government should relate to the economy, so much as it was designed to minimize economic offense to the membership of the USCC and AFBF (Weir 1992: 50-3). If there was leadership and vision anywhere in the picture, it belonged to the conservative elements of the business community, which pulled dozens of policy details in a direction that largely favored economic elites, including business. The primary role of the CED, if it had one, was perhaps to simply put a happy face on whatever emerged at the end of the legislative process.

Industrial relations and labor law

Mizruchi is surely correct to insist that the "modal response" of postwar business to organized labor was acceptance, sometimes grudging (Mizruchi 2013: 110). And if the benchmark against which to gauge the moderation and pragmatism of business is whether it sought to utterly annihilate organized labor—perhaps by dismantling the Wagner Act—then much of postwar business certainly seems moderate and pragmatic. But just because the most reactionary impulses of business were curbed does not necessarily mean that business was moderate or that moderates exercised "vision and leadership." The prospect of total rollback was never really in the cards. Organized labor had simply gotten too strong. The real stakes of the conflict were the specific terms of accommodation between capital and labor on key issues of political economy, and the main question is whether the terms tended to favor one side or the other. By how much? In what ways? With what long-term consequences? More specifically, how would corporate governance, collective bargaining, and employment itself be structured, and how much influence would organized labor wield within the resultant framework? Here it seems that business wound up with a better deal than labor, even in instances when business did not get exactly what it wanted. For instance, as documented by Howell John Harris, who is cited extensively by Mizruchi, business groups like NAM hoped to sharply limit the growth of unions and weaken their position in collective bargaining by influencing the design and content of labor laws (Harris 1982: 120-1). They got much of what they wanted in the Taft-Hartley Act of 1947, which among other things banned the closed shop, authorized states to pass "right-to-work laws" that outlawed the union shop, and essentially ratified the emergence of a decentralized, firm-based process of collective bargaining over a sharply circumscribed set of issues. The law can be seen as snuffing out the modest corporatist possibilities that remained in play after the Second World War (Lichtenstein 1989: 134). Business leaders like Eric Johnston of the Chamber of Commerce, Paul Hoffman of Studebaker, and shipbuilder Henry J. Kaiser certainly voiced less conservative views about labor law reform and other issues than many of their corporate counterparts, but they were a "relatively uninfluential minority" (Lichtenstein 1989: 130), if not the "least influential section" of the business community (Harris 1982: 110). In this respect, it is telling and perhaps not coincidental that the CED was self-avowedly not a lobbying group (Mizruchi 2013: 55) and served mainly as a source of contrapuntal ideas. The real leaders in the business community who did not shy away from mustering and applying political influence were "practical conservatives" like U.S. Steel's John A. Stevens, NAM's Ira Mosher, and General Motors' Charles E. Wilson, and what they sought was a broad-based "restoration of managerial prerogatives" over numerous aspects of their business operations (Lichtenstein 1989: 130; Harris 1982: 117). Practical conservatives may not have "gone for broke" (Harris 1982: 119) by pushing for a repeal of the Wagner Act, but they certainly "aimed for decentralized collective bargaining in which the law assisted management in keeping the upper hand" (Harris 1982: 121). Although they did not get to write every provision in the law to their ultimate satisfaction, Taft-Hartley went a long way toward realizing their broad goals.

These are only two examples, and more are certainly needed to build a stronger case. But what we observe in these examples does suggest a different interpretation than the one advanced in *The Fracturing of the American Corporate Elite*: Certain segments of the business community did exhibit moderate views, but they did not participate in a serious leadership role. Nor did they really provide much in the way of a comprehensive, alternative vision for the organization of the political economy. They followed more than they led, and they often found themselves reacting to the flow of events rather than proactively shaping them. The real action was being driven by more conservative business leaders—often associated with NAM and later the Business Roundtable—who organized themselves in increasingly sophisticated ways to exert political influence over the design of specific pieces of legislation. These men had a clear sense of what they wanted, even after they gave up on trying to restore the status quo ante of the laissez faire years. They were the ones with a vision—it was a vision in which business interests stood at the center of the political economy, first among equals—and their vision was the one that was eventually realized. The country turned away from "social Keynesianism" and toward "commercial Keynesianism." Organized labor was confined and domesticated—politically, legally, institutionally, demographically, and geographically—and it became just another interest group. To be sure, the important gains of the New Deal continued to flow toward many Americans, and so it was that the country enjoyed a period of relative income equality and economic stability. But there could be little doubt that American business at the behest of "practical conservatives" had regained the upper hand over government and labor—something they would manage to keep even after the "shock of the global" in the 1970s ended the "golden age of capitalism."

This interpretation may not hold up to closer scrutiny. It certainly requires further refinement. To the extent that it is valid, however, it raises a clear question about Mizruchi's call for a moderate corporate elite to reassert itself. If corporate moderates never really exercised "leadership and vision" in the past, what good is it to ask them to show "leadership and vision" in the present?

If there is to be real change today, perhaps the sources of it must be sought elsewhere.

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Are Bankers the Philosopher-Kings of Capitalism? Reactions to *The Fracturing of the American Corporate Elite*

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Capitalists compete. Not all of the time, but a lot of the time. However, they can do much better when they stop competing with each other. They might try to reduce competition unilaterally, through strategies like product differentiation or product innovation. They can also act in bilateral or multilateral ways to restrain trade, perhaps to fix prices, control supply, restrict market entry, and so earn higher profits. In short, collective action among capitalists can produce a cartel. These strategies sometimes involve political action: lobbying, campaign contributions, bribery, regulatory capture, etc.

Within an industry, firms can charge higher prices if they form a cartel. But individual firms also have an incentive to defect from the cartel, because they can capture more market share by lowering their prices. Through cooperative standard-setting, firms can take advantage of network externalities. But they may also have ex ante conflicting interests in the adoption of different standards (e.g., VHS vs. Betamax, Mac vs. PC, or open vs. proprietary standards), even though ex post no single firm has an incentive to defect from the standard.

In sum, a real market economy is for business a complex and unstable mixture of competition and cooperation, both operating at a variety of different levels that include both markets and politics. And the mixture of cooperation and competition waxes and wanes over time. In reality, there are no durable competitive or cooperative equilibria.

Capitalist cooperation is particularly interesting because it is multifunctional. That is, cooperative arrangements among capitalists can be used to serve their individual self-interests, their collective interests, and can even be repurposed to serve broader social interests. Because market economies involve private ownership of the means of production, to get capitalists to agree to do something is to obtain cooperation from a very powerful and consequential group. The corporatist arrangements studied by political scientists demonstrate the efficacy of peak group representation: business, labor and government can sit down and negotiate a social pact. In his analysis of the US, Mizruchi takes this corporatist trio and adds a fourth: the banks.

In general, cooperation is more likely to occur among smaller groups. It is easier to detect and prevent defection from cooperative arrangements when the numbers involved are small. Since elites are by definition few in number, elite capitalists are a group that potentially could undertake and sustain cooperative arrangements. Cooperation is also easier in the face of external adversity, and so having a credible enemy helps with organization. Organized labor in the US was strong mid-century and gave corporations something serious to worry about.

The stylized fact of Mizruchi's study is that US corporate elites were able to organize and cooperate for many decades during the 20th-century, but that after the mid-1970s they became increasingly disorganized and so today are unable to play a meaningful or pragmatic role in the articulation and pursuit of general social interests. Global climate change? Health care reform? Tax policy? Where are the corporate leaders? It is interesting to note the overlap between this periodization and the more general post WWII political economy (1945-1973: sustained economic growth, rising wages, diminishing income inequality, etc). Evidently enlightened corporate elites were associated with good times, and it would be interesting to combine Mizruchi with recent arguments offered by Thomas Piketty.

If organized pursuit of broader social interests by corporate elites benefited American society in the middle of the 20th-century, then two things were necessary: organization, and the realization of broader interests. The first I've already mentioned: small-n groups facing external threats tend to become organized. The historical argument works. But consider the second: how do corporate elites recognize and pursue broader social interests? In Mizruchi's analysis, the banks play a key role in this respect. Why might they do so, and is the argument convincing?

Banks can have leverage over corporations because of their control over a key resource: capital. To the extent that firms had to borrow from banks, they had to listen to their bankers.

When firms could use retained earnings or some other source for capital, then banks lost their leverage. And it turns out that for much of the post-WWII period, corporations were not heavily dependent on banks for funding (although this changed during the 1960s). Banks often provided their top people for corporate boards of directors, and so board interlock networks reveal the centrality of banks in these networks of social contacts. But what makes banks special? What enables them to rise above particular interests and see the big picture? According to Mizruchi, banks pay attention to overall trends, to the macro-economy, and do not focus on particular industries, sectors or regions. To the extent that banks are highly diversified lenders and operate nationally, this indeed makes sense. Furthermore, banks are giant repositories of information operating in credit markets characterized by asymmetries of information, so knowledge and analytical capacity are among their key assets. But banks are also subject to the same problems of bounded rationality as the rest of the world, and indeed as the recent London Whale episode for JPMorgan Chase makes clear, bank CEOs may not even understand what their own personnel are doing, let alone where the rest of the world is headed. Banks that are publicly-traded are subject to the same shareholder-value-short-term-goal-seeking that afflict corporations, so the far-seeing "patient capital" role may be hard to fill. Furthermore, key financial institutions whose job it is to manage the global economy were nevertheless unprepared for the global financial crisis of 2008. The Independent Evaluation Office of the IMF issued several reports in early 2010, criticizing the IMF's own surveillance of the US and global economy for failing to see the big picture [one of the key problems: too many economists on staff and too much "group think"]. Only a few credible folks foresaw problems, and they were ignored (e.g., Raghuram Rajan in 2005). Banks were as good at failing to connect the dots as everyone else.

And even assuming banks are able to see the big picture, do they have an incentive to impart their wisdom to their corporate clients and counterparts, and to guide them along the path of enlightened pursuit of the common good? Not necessarily. Banks make money off their clients, not only by lending them money but increasingly by providing lots of transactional services. Often it is to their advantage to keep their clients and counterparties ignorant rather than informed. Consider the durable resistance from big banks to regulation of the OTC derivatives markets. These banks are mostly active on the "sell side" of the swaps market, and they make more money when the market remains non-transparent. Furthermore, banks have been good at making their own particular institutional interests look like the general interest, and so what passes for the common interest may not in fact be so: consider the issue of "too big to fail." The largest banks were able to convince politicians that their survival was in the general interest, and that the ordinary rules of the marketplace (i.e., that insolvency leads to failure and closure) should be suspended. Furthermore, consider who actually benefited from the public bailouts: the Federal Reserve System provided $85 billion in support to AIG in 2008, and this directly benefitted AIG's swaps and derivatives counterparties. Who were they? Some were big US banks like Goldman Sachs, Bank of America, Merrill Lynch and so on. But beneficiaries also included foreign institutions like Societe General, Deutsche Bank, Barclays, Credit Suisse, and so on. And the Fed worked hard to keep that latter fact confidential.

As US bank activities become globally diversified, and as they separate physical location from legal domicile, banks become detached from the US and so banks may have no particular interest in doing what is "right" for America. I'm sure that many US investment banks profitably advise their corporate clients on how to engineer "inversions" that allow firms to reincorporate abroad, reduce corporate taxes, and worsen the fiscal situation of the US federal government. Tax revenues may increase abroad, which will be good for Ireland or wherever the firm goes, but it won't help the US.

In sum, I think that Mizruchi's analysis of how capitalists come to cooperate, or not, is very reasonable. On the second point, I am as intrigued by the idea of banks as the "philosopher kings" of capitalism as I am skeptical of it. This means I have my doubts, but am not ready to discard the idea: it warrants further research. But my overall reaction is simply testament to the fact that Mark Mizruchi has offered us a strong, plausible, well-documented and stimulating analysis, and that is hard to beat.

Response to Critics

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Back in August 2013, I received a message from Bruce Carruthers inviting me to participate in an Author Meets Critics session at the 2014 ASA meetings. I was thrilled. Who, after all, would not want the attention to one's book that such a session virtually guarantees? I happily accepted the invitation, and basked inwardly at the glory that I was certain would follow; until a few months later when I learned who the critics were going to be: four people, all of whom I greatly respect, but all of whom I knew had the ability to poke serious holes in my argument. Images of a firing squad danced before my eyes. This could get ugly, I reasoned.

Fortunately, the critics decided to go easy on me. After listening to their comments in San Francisco and then reading their written versions this fall, I was relieved to see that I had been spared—at least partly. Still, all four of them raised issues that have required me to think long and hard about what I was trying to do, and whether I succeeded. The four comments touch on a wide range of issues, so broad in fact that I think it makes sense for me to provide a brief recap of my argument, as I did at the session. I will do that first, and then respond individually to each critic.

The Fracturing of the American Corporate Elite deals with a paradox. Today's corporate elite, the leaders of the largest U.S. firms, seems to have more power than at any time since at least the 1920s, able to gain political favors virtually at will. Yet the group seems strangely ineffective in addressing a series of issues with which it is highly concerned, yet which require collective action to accomplish: tax policy, health care, immigration, and the Export-Import Bank, to mention just a few. Historically, the vast majority of American businesspeople have taken political positions that by contemporary criteria would be considered conservative, including support for free markets, low taxes, and limited government regulation as well as opposition to organized labor. In the period between the end of World War II and the early 1970s, however, there was a relatively small group of corporate leaders, primarily those at the head of the largest corporations, who exhibited a more moderate approach to politics. These officials were willing to accept a degree of government regulation of the economy, including Keynesian economic policies, and they were willing to accept, even if grudgingly, the existence of independent labor unions. These people were not liberals, but they were pragmatic with regard to strategy. They operated according to a philosophy that the Committee for Economic Development, the prototypical organizational representative of this approach, referred to as "enlightened self-interest."

These corporate leaders did not operate in a vacuum, however. Their moderation and pragmatism were imposed on them by three forces: a relatively active and highly legitimate state, a relatively powerful labor movement (the focus of Judy Stepan-Norris's comments), and a financial community that played a role in generating normative consensus and a broad, long-term orientation among the leaders of major firms (the focus of Bruce Carruthers's comments). Although American society was far from a utopia during this period, the economy was strong—the median standard of living in the population nearly doubled between 1946 and 1970 and the poverty level was reduced by half during the 1960s—and the political system, although fraught with problems, worked reasonably well, with members of both major parties able to point to legislative and executive accomplishments.

The situation began to unravel during the 1970s, however, in the face of a number of exogenous and endogenous forces: increasing inflation accompanied by high unemployment, the emergence of foreign competition, two energy crises, the decline of legitimacy among major societal institutions (including both government and business), and the emergence of economy-wide regulatory agencies staffed with officials whom businesses believed were overly aggressive. In response to these perceived threats, the corporate elite mounted a counteroffensive. Aligning themselves with the traditional conservatives whom they had previously shunned, they attacked organized labor, as well as what they viewed as excessive government regulation.

This counteroffensive proved to be extremely successful. By the time Ronald Reagan became president, both the government and organized labor were significantly weakened. This had an unintended consequence, however. No longer constrained by the forces of government and labor, it was no longer necessary for large corporations to be politically organized, and the group became increasingly fragmented during the 1980s. Two developments led to further fracturing. First, the commercial banks began to lose their influence, and thus abdicated their role as the meeting place for the leaders of major nonfinancial corporations. Second, a massive acquisition wave placed sitting chief executives under siege. By the time the dust cleared at the end of the 1980s, the corporate elite had lost its ability to act collectively to further its interests, and corporate CEOs found themselves operating in a very different environment, relieved of the constraints of government and labor but facing far more day-to-day pressure from Wall Street investors and financial analysts. The corporate elite that emerged from this development was both disorganized and ineffectual, increasingly able to gain favors for their specific firms, but increasingly unable to address a series of pressing issues that required collective solutions, such as health care and tax policy. Moreover, big business was no longer able to rein in the right-wing forces that it had managed to keep at bay during the postwar period. In those years, the corporate elite occupied the near right segment of the political center. In the 1970s, the group allied itself with traditional conservatives. As the elite fractured by the early 1990s, however, it was no longer able to control these conservatives, who had become increasingly extreme in their positions. The result has been the political gridlock that we observe today.

There are multiple theoretical underpinnings to this argument, and one of the key ones is eloquently captured in Bill Roy's comments. In a way that only he can do, Bill does a great job of identifying the central issues of the twentieth century debates on the nature of democracy. Pluralist political theorists and their critics argued vehemently about the extent to which elites in developed capitalist societies were unified. Both sides agreed, however, that a unified elite was detrimental to democracy; for the system to function effectively, divisions within the elite were necessary. The debate was therefore largely an empirical rather than a theoretical one. My story suggests, however, that American democracy actually functioned relatively well when the elite was unified, and that the subsequent fragmentation of the elite has had a negative effect on our politics.

Bill notices that I have inverted the traditional view of elite unity and democracy, but he adds a twist: He suggests that I have reinterpreted, contrary to my earlier writings, the postwar period in a manner consistent with pluralism rather than elite theory. Well, yes and no. He is correct that I emphasize the role of what John Kenneth Galbraith called countervailing power, in this case of government and organized labor, which served to constrain the actions of big business. On the other hand, I continue to share the elite theory view that the corporate elite of the postwar years was relatively unified and able to act collectively to address the group's common concerns. I also indicate, as Bill notes, that the elite largely "got its way" during that period. My nod to pluralism, to paraphrase Marx, is that if the elite made its own history, it did so not under conditions of its own choosing.

Bill also suggests that what from today's lens appears to have been moderate and pragmatic behavior might have simply reflected the "long term interests of the corporate sector." In other words, big business may have been just as self-interested in the 1950s and 1960s as it is today. It's just that its interests perhaps were different back then.

The corporate elite of the postwar period faced constraints that today's elites do not—in particular a highly legitimate government and a relatively strong labor movement.

Yet not only do I not disagree with Bill on this, but this is precisely what I was trying to argue. The corporate elite of the postwar period faced constraints that today's elites do not—in particular a highly legitimate government and a relatively strong labor movement. This created limits on what the group saw as politically possible. One reason for the elite's long-term approach might have been that it often could not win in the short term. But there were at least two others. First, corporate chief executives at the time had a high degree of job security. Management was powerful, and CEOs were not constantly looking over their shoulder at Wall Street, fearful of any action that might jeopardize the firm's short-term stock price. Second—and this is a point with which Tony will probably disagree (more on that later)—the elites of the postwar period really were different from those of today in their concern for the well-being of the larger system. They were not altruists. They certainly pursued their class interests (as well as their individual ones) as they saw them. But they also believed that in order to maintain their privileges, it was necessary for the society to rest on a strong foundation, and that meant concerning themselves with some of the key problems of the age, including poverty, racial discrimination, and wages in general, the latter an important concern because of the perceived need for the population to have sufficient purchasing power, a central tenet of the Keynesian wisdom of the time.

Bill also suggests that in focusing on the legislative process, I have played into the pluralist emphasis on what Mills called the "middle levels of power," and thus neglected the "real" center of power, which was lodged in the executive branch. Elite theorists and Marxists of the postwar period had argued that as capitalism moved from its competitive to its monopoly stage, the locus of power in the state shifted from Congress to the executive branch, a process that solidified during the 1930s. By paying so much attention to Congress, have I perhaps missed the forest for the trees? This is a legitimate question, and it raises an interesting possibility: As the elite has become increasingly fragmented, the center of power in the national government may have returned to the legislative arena. We can see this in the Obama administration, in which virtually every action proposed by the president has been thwarted by Congress, and even big business was unable to prevent Congressional Republicans from shutting down the government in 2013. But we can also see this as far back as the early 1990s, when big business went to Congress to get the tax increase it had called for, bypassing President Bush, who had campaigned on the slogan of "read my lips, no new taxes." Perhaps the power of the executive branch was tied to the centralization and cohesion of the corporate elite. Perhaps the decline of that cohesion has restored Congress as the center of the action.

Turning to Judy's comments, one of the central issues in debates over the nature of developed capitalist societies was the relation between managers and workers. I argue that organized labor was sufficiently strong in the postwar period to act as a significant constraint on the actions of firms. In responding to this constraint, I suggested (consistent with the arguments of several leading labor historians) that the leaders of many large corporations reached an accommodation with their workers. This did not mean that business executives "loved" unions, or that in an ideal world they would not have happily been rid of them. As Tony Chen notes in his comments, destruction of unions was "not in the cards," however, and management thus came to accept them as a necessary evil. Managers were even able to see some possible benefits, especially as unions agreed to expel their radicals, allow management to maintain full shop floor control, and sign contracts that included no-strike clauses, all in exchange for higher wages and benefits.

Judy, an eminent figure in the sociology of labor, does not take issue with my overall story, but argues that things were more complicated than I indicate. There was considerable variation not only among corporations—which I show—but also within the labor movement itself. Only by understanding this variation can we fully understand management's reaction to its unions, she suggests. We are treated to extensive and fascinating discussions of divisions within the labor movement dating back to the 1930s (and earlier), and how these divisions led to variation in union-management negotiations across firms. We also learn about the role of the National Association of Manufacturers in promoting the vehemently anti-union Taft-Hartley Act in the 1940s, and how the weakened state of labor in the late 1960s affected collective bargaining during that period. Judy uses this detailed analysis to conclude that "capital's position on labor unions has always been overwhelmingly negative," and that "few firms welcome sharing their control over workers and production with unions."

As much as I learned from Judy's discussion, I must concede that I'm a bit puzzled by it. Although Judy argues that my story suffers from not having taken the within group labor and management variation into account, there is virtually nothing in her analysis with which I disagree, nor is there anything that runs counter to what I argue in the book. My focus was on over-time variation in management's response to organized labor. Although there was within-group variation among both management and unions, cataloguing that variation in detail was not my focus. Everything that Judy describes — management's detestation of unions, the anti-union nature of Taft-Hartley (and the NAM's role in formulating it), the fact that management demanded complete shop floor control in exchange for its willingness to work with unions, the fact that the unions purged the radicals from their ranks as part of this agreement, the fact that management became more aggressively anti-union (and was more successful in this pursuit) as unions weakened in later years—all of these play a central role in my story (see, for example, my discussion of the role of the NAM in the Taft-Hartley Act, on pp. 90-94). If I did not convey this material with the level of detail that Judy had hoped, I can only say that it was not my purpose, in a single 30-page chapter, to do more than demonstrate that unions represented a constraint on the actions of management in the postwar period and that the corporate elite reached an accommodation with them, one based on a pragmatic acceptance of the political realities it faced.

Just as Judy focused on a single chapter, on organized labor, Bruce, perhaps the leading sociological authority on the topics of money and finance, focuses on my chapter on the banks. Although I treated the banks in the postwar period as the third of the primary constraints faced by the corporate elite of the day, my goal was not to claim that they did this by dominating nonfinancial firms. On the contrary, as Bruce notes, the banks' power with respect to nonfinancial corporations was relatively low through the 1950s and well into the 1960s, until a capital shortage began to turn the tide. What the banks did provide was a central meeting place for the leaders from a broad range of industries. In 1972, the board of Chemical Bank, a leading New York institution now part of JP Morgan Chase, included officers from seventeen major nonfinancial corporations. As late as 1982, the board of what was then Chase Manhattan Bank included the CEOs of fourteen Fortune 500 companies. The fact that these nonfinancial executives sat as outsiders on the boards of the major banks suggests that the banks were not in fact exercising control over those companies. It also indicates, however, that the bank boards provided an arena in which representatives from a broad range of industries came together, making the boards a setting for the diffusion of information as well as a source of normative consensus.

Bruce asks whether the banks, with their pan-industry orientation, could have served as the "philosopher-kings" of capitalism. I would love to have been able to demonstrate this, but the evidence was simply not available. Although the banks occasionally intervened in the internal affairs of the firms that were indebted to them, there was little indication (beyond a few anecdotes) that they spoke politically for the business community as a whole. I completely concur with Bruce that even had they been able to do this, the banks would have been subject to the same bounded rationality that limited the decision making of nonfinancial firms. I also agree that the financial community in the present not only operates with bounded rationality (as evidenced by the financial crisis), but also that it has little concern for the kind of enlightened self-interest that characterized the corporate elite in the postwar era. How we got from that period of relative enlightenment to what we observe today is the primary story I attempt to tell in the book. Had it been my goal to include a more detailed analysis of the contemporary financial community, Bruce's discussion would have provided an excellent framework for it.

I also think Bruce is right to note that the farsighted corporate elite of the postwar period was associated with good economic times, and I agree that it was not a coincidence. Which caused which is hard to say, but a case could be made that although it was easier for business leaders to be generous when they were swimming in profits (and the profit squeeze they experienced in the 1970s goes a long way toward explaining their aggressive response toward government and labor), it is equally possible that the accommodating view that the corporate elite took toward the general public during the postwar period, including the acceptance of relatively high (and increasing) wages, contributed to the prosperity of the time.

Finally, Tony, with his characteristic incisiveness, raises a number of questions. While not taking issue with the facts of my case, he does suggest an alternative interpretation. He asks whether the corporate elite actually led, as I seem to suggest, or whether their actions were driven by simple expediency. In other words, did the presumed moderate activity of the elite during the postwar period reflect a genuine ethos of responsibility, or was it just a reaction to forces that it was unable to fully control? After all, he notes, business did quite well even at the time, able to weaken employment legislation, labor law, and welfare provisions.

So, did postwar corporate elites really lead, or were they led? The answer is both. Even the most moderate among the elite were far from liberal in a philosophical sense. They were strong supporters of the free enterprise system with minimal government intervention (except when necessary to help generate profits). They were at best suspicious of organized labor. They were critical of the tax structure, arguing for a reduction of the highest marginal rates even during their most accommodating years. And they were strong supporters of the foreign policy that underlay the Cold War. The fact that the corporate elite was nevertheless willing to accept the principles of Keynesian economics, the legitimacy of organized labor, and occasional tax increases even on themselves represented not altruism, but pragmatism. In that sense, they were indeed dragged along by events, led by them rather than leading.

But that is only part of the story. If corporate elites were constrained by the forces of government and labor, they also made active efforts to accommodate those forces, in ways that did suggest a genuine exercise of the kind of leadership that we do not see today. In fact, it is by contrasting the elite's actions in the postwar period with those of today that we can see just how responsible, at least in relative terms, the business leaders of the postwar period were.

It is important to clarify that I am not talking about the majority of corporations and corporate leaders, but rather a small subset. The vast majority of American businesses, organized into the National Association of Manufacturers and the Chamber of Commerce, exhibited the same kinds of highly conservative attitudes that we observe among businesses today (although even the NAM and the Chamber occasionally took more moderate positions during the postwar period). Yet the small subset of corporate leaders at the top, represented by the Committee for Economic Development, was disproportionately influential during that era. Tony is correct that the CED was not a lobbying group, but this does not mean, as he suggests, that the group was "never centrally involved in shaping policy." On the contrary, the CED developed the Marshall Plan. It developed the Employment Act of 1946, however watered-down the final version ultimately became. The group later devised what became, virtually verbatim, Richard Nixon's health care plan, a plan that in political terms was far to the left of what both Bill Clinton and Barack Obama eventually proposed. The CED, with its willingness to embrace the political realities of the age, its acceptance of Keynesian economics, and its insistence that most businesspeople, with their laissez-faire attitudes, were hopelessly removed from those realities, was able to exercise enormous influence.

> ***...the corporate elite of the postwar period was concerned with its self-interest. There is no disputing that. But it was self-interest with a set of limits regarding not only what was possible, but also what was reasonable.***

It is true, as Tony notes, that intense lobbying by business was behind the passage of the Taft-Hartley Act and the weakening of the Employment Act. Yet things could have been different. As I explain in the book (again, see pp. 90-94), even as the NAM played a major role in the development of Taft-Hartley, its leaders faced down the more conservative members who wanted an all-out effort to repeal the Wagner Act. Tony indicates that total repeal of Wagner was "never in the cards," but business leaders could have pushed much harder for repeal and did not. Similarly, although the Employment Act ultimately stopped short of a true commitment to full employment, the elite could have opposed the bill altogether, yet its members offered support instead. Most importantly, the elites did not have to support tax increases, on themselves, to pay for the Korean or Vietnam Wars, but they did. Where are the corporations advocating tax increases on big business (or wealthy individuals) today?

Let me again emphasize that the corporate elite of the postwar period was concerned with its self-interest. There is no disputing that. But it was self-interest with a set of limits regarding not only what was possible, but also what was reasonable. Big business fought organized labor tooth and nail, but it did not question labor's right to exist. Its leaders argued that tax rates on the wealthy were too high, but they were still willing to tax themselves to pay for wars. Its leaders strongly supported the free enterprise system, but they also believed that this system worked most effectively when the needs of the broader population were taken into account. Perhaps most important, the elite believed that the government had an obligation to address the societal problems of the age, and they were willing to provide the resources necessary to do this. All of these are aspects of the elite that stand in stark contrast to the corporate leaders of today. Even those who seek to do good, such as Bill Gates, Howard Schultz, and Warren Buffett (to take three examples), operate independently, and privately. There are virtually no voices calling for a coordinated effort, backed by the state, to deal systematically with the myriad problems with which the United States is confronted. And today's corporate elites are so fragmented and ineffectual that even when they do acknowledge the need for such an effort, they are unable to generate the kind of concerted, organized action that would enable them to influence current policies. So yes, Tony is right that even in the golden age, the American corporate elite was far from altruistic. Compared to the present, however, the group did exhibit a level of responsibility, and leadership, that is in short supply today.

I would like to thank Bruce Carruthers for organizing the session at which earlier versions of these comments were first presented. I would also like to thank Bruce, Bill Roy, Judy Stepan-Norris, and Tony Chen for their extremely insightful and engaging criticisms. Finally, I would like to thank Matthew Baltz for his invitation to publish this exchange in the CHS section newsletter and for his help with editing the comments.

Book Symposium

The Land of Too Much:
American Abundance and the Paradox of Poverty

Harvard University Press

By: Monica Prasad

Editor's Note: The following text is based on an author-meets-critics session organized by Michael Hout for the American Sociological Association Annual Meeting in San Francisco in August, 2014. My thanks to Monica Prasad, Greta Krippner and Lis Clemens for agreeing to submit their comments to the newsletter.

Comments on *The Land of Too Much*

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It's a pleasure to be asked to comment on Monica Prasad's justly celebrated new book, *The Land of Too Much*. The book is original and bold in its conception, taking much of the conventional wisdom about the nature of American capitalism and up-ending it. There are many surprises here, and yet Prasad's argument doesn't feel reckless, but judiciously weighs the evidence and considers counter-arguments. One gets the sense that what drives this enterprise is not Prasad's inclination to be deliberately provocative (although provocative she often is) but rather an insatiable curiosity and desire to understand how capitalism works in all its gritty detail. To this end, she's not afraid to take things apart, tinker with the pieces, and re-assemble, bringing the reader along for what is invariably an illuminating experience. Indeed, what I loved most about this book is how much I learned from reading it. Prasad is second to none in her mastery of the vast comparative literature on the historical evolution of capitalism, and her display of this knowledge is quite often dazzling. But like the best scholars, Prasad wears her erudition lightly, and the book is written in a clear and engaging style that is a true rarity among books in this genre. For all of these reasons, *The Land of Too Much* is an impressive accomplishment, and a very worthy sequel to Prasad's well-received first book.

Prasad starts from a puzzle – perhaps *the* puzzle – that has long vexed students of comparative political economy: How can we understand the apparent paradox that as the society that has generated unprecedented prosperity over the course of the previous century, the United States is also the society that is afflicted by the highest rates of poverty in the developed world? The standard way of thinking about this puzzle – that relatively high rates of poverty in the United States reflect the liberal orientation of American political culture and the attendant disinclination to interfere with market outcomes – is firmly rejected by Prasad. Instead, Prasad's approach to the question is shaped by a recent literature, produced largely by historians, that reveals the conventional view of the United States as a liberal, laissez-faire society to be a myth. Prasad builds this case by mobilizing an array of evidence that suggests that not only is the U.S. state interventionist; it is actually more interventionist than European states (at least across the regulatory domains surveyed by Prasad; the large, looming exception to this characterization, as Prasad herself notes, is the welfare state).

If we can't explain the paradox of American poverty by pointing to the liberal, laissez-faire state, then where should we look? Here Prasad is influenced by political scientist Elizabeth Sanders's (1999) examination of the agrarian foundations of American political economy. Prasad argues that the unprecedented productivity of the American economy at the turn of the previous century produced a shock to the international economy, resulting in worldwide price declines. Europeans responded to this threat by erecting protective tariffs, but this remedy was not available to Americans, whose excess production was itself responsible for destabilizing prices. As a consequence, in the United States, the resulting economic volatility launched the agrarian discontent that came to be known as the Populist movement. Anticipating Keynes, agrarian populists developed a unique diagnosis of their economic difficulties that suggested that the concentration of wealth was preventing ordinary Americans from enjoying the fruits of their abundant economy and threatening a crisis of overproduction. As such, agrarians advocated the progressive income tax and wider availability of credit, both policies that would spread consumption more evenly and alleviate downward pressure on prices. Prasad's provocative argument is that this "agrarian solution" to disequilibrium in the world economy launched the United States on a distinctive path of development based on private consumption (financed through easy access to credit) that has continued to shape American society to the present day.

More specifically, Prasad sketches two distinct pathways from agrarian influence on American state formation to the paradox of poverty. The first path examines the role of agrarians in institutionalizing the progressive income tax as the most important component of the American tax system. In contrast, European states relied more heavily on the national sales tax to raise revenues – a tax that American agrarians rejected because of its regressive nature. As Prasad explains, for reasons that are well documented in the literature, the income tax is politically more vulnerable than the sales tax. As a result, one achieves a better result from a social welfare perspective raising revenues through a regressive sales tax and then redistributing these revenues in a progressive fashion, rather than raising a smaller amount of revenue through the progressive income tax. In short, Prasad suggests that differences in the revenue-generating capacities of tax systems organized around the income tax and the sales tax account for the less generous nature of the public welfare state in the United States relative to Europe – and relatedly, the higher rates of poverty in this country relative to other developed economies.

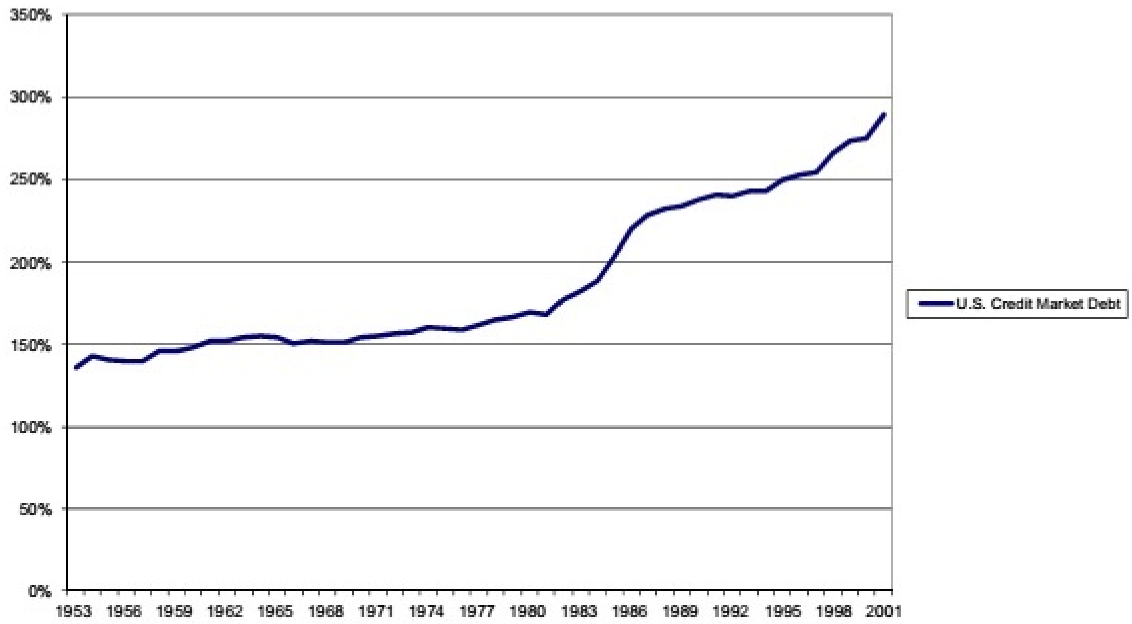
The second pathway traces what Prasad calls "the agrarian regulation of finance." Prasad again starts from the considerable power of agrarians at a critical moment in the development of the American state, arguing that agrarian influence was reflected in regulations put in place early in the twentieth century that prevented banks from operating branches across state lines. This "unit banking" structure, Prasad argues, made the United States much more vulnerable in the banking crisis of the 1930s, amplifying the impact of the Great Depression. It also directed the response to the crisis in a particular direction, as Roosevelt sought to aid recovery by building a financial infrastructure that facilitated wider access to credit, particularly through reforms to the mortgage market and the creation of the Federal Housing Administration. The resulting democratization of credit was realized even more fully, Prasad argues, when in the 1970s social movements pushed for an end to gender and racial discrimination in credit markets. Easy access to credit, Prasad argues, substituted for a weak public welfare state in the United States, culminating in the uniquely American pattern of consumer-led, credit financed growth, and also making the U.S. economy especially prone to boom and bust cycles in financial markets.

There is much to like about this argument. In broad strokes, it is quite compelling, and offers a new angle of vision on some old questions regarding the distinctive features of American capitalism. The book probably makes its most important contribution in specifying the credit/welfare tradeoff more systematically than others have, and in tracing the historical origins of this relationship in a sophisticated and nuanced way. I am, however, more fully convinced by the first pathway (from agrarian influence to progressive taxation to less generously funded welfare state) than I am by the second pathway (through restrictions on branch banking to democratization of credit to financial crisis). Here, the connections Prasad wants to draw are somewhat less direct, and her central argument about the foundational influence of agrarian politics on subsequent developments feels a bit strained. Prasad is undoubtedly correct in observing the significance of restrictions on branch banking in making U.S. banks prone to failure in the 1930s (although obviously much more was involved in creating the Great Depression than branch banking, as Prasad herself acknowledges). But it's not entirely clear to me how Roosevelt's response to the financial crisis of the 1930s – and in particular, the growing importance of mortgage finance as "the wheel within the wheel"[1] turning the economy – reflected agrarian influence. (Prasad points to the prior experience of the Federal Farm Loan Act, which offered a template for the creation of the amortized mortgage loan, but this seems like a rather tenuous connection on which to hang her broader claim.) Once the New Deal credit infrastructure was in place, of course, it developed a life of its own, and the expectation that access to credit was fundamental to economic citizenship underpinned the development of a mass consumer society, as historians such as Lizabeth Cohen (2003) have emphasized.

> ***I think Prasad overstates the continuities between the immediate post-war decades and the post-1970s period...the magnitude of the growth of credit in the U.S. economy in the period after the 1970s swamps changes in the use of credit prior to this period.***

But in this regard, I think Prasad overstates the continuities between the immediate post-war decades and the post-1970s period. Here Prasad takes aim at my work (see Krippner 2011), and without being (I hope) too defensive, I'll just say that I disagree with her characterization of financialization as a process that was set in motion before the 1970s. Yes, it's true that a credit infrastructure was created in the 1930s and 1940s, allowing the expanded use of credit in the economy relative to both the prior history of the United States and to the contemporaneous experience of many European economies. And it's equally true that financialization could not have occurred without this credit infrastructure: it was a necessary precondition for the turn to finance in subsequent decades. But it's also true that the magnitude of the growth of credit in the U.S. economy in the period after the 1970s swamps changes in the use of credit prior to

Figure 1: U.S. Credit Market Debt as a Percentage of GDP, 1953-2001



Source: Data on outstanding credit in the U.S. economy from Flow of Funds, L.1; data on GDP from National Income and Product Accounts, table 1.1.5, line 1.

this period.

Consider, as a rough and ready illustration of this point, Figure 1 depicting U.S. credit market debt as a percentage of GDP over the postwar period, a standard measure of the extent to which the economy relies on credit. The trend-line is practically flat in the 1950s and 1960s and then starts to drift gently upward in the 1970s before surging strongly upward in the 1980s. To put a few numbers on these trends: In the year 1970, total credit market debt stands at 154 percent of GDP, a modest increase over typical levels of debt in the prior two decades; by 1980, this number climbs to 169 percent; and by 2001, total credit market debt is 289 percent of GDP. In short, credit market debt relative to the size of the overall economy nearly doubles over the period from 1970 to 2001 (and if I were to extend the data into the 2000s, the upward trend visible here becomes even more dramatic). These data indicate a dramatic change in the role of credit in the U.S. economy, with the inflection point occurring in the early 1980s. This is – not coincidentally, I believe – a period in which domestic financial markets are deregulated through the removal of interest rate ceilings, the Reagan deficits begin to attract enormous sums of capital from abroad, and the Federal Reserve adopts new methods of policy implementation – all changes that I have identified in my own work as being of key importance in creating an environment of free-flowing credit that gave rise to financialization, and the ensuing tendency to financial instability in our economy.

This is not to deny, of course, that there is a uniquely American relationship to credit which starts much earlier and has its roots, in part, in agrarian politics – precisely Prasad's argument. But it is to suggest that there is a fundamental transformation in the role of credit in our economy in the post-1970s period that Prasad passes over too quickly. Let's remember that while Americans may have relied on credit to a much greater degree in the 1950s and 1960s than did Europeans, this was nevertheless a regulated credit economy, in which flows of credit were subject to precise controls. For related reasons, the financial crises that are a regular feature of our own financialized economy were rare occurrences in the immediate postwar decades. Indeed, even the emergence of the social movements that pushed to democratize access to credit were a product of an environment in which credit was strictly rationed. It is noteworthy that these movements did not so much "win" as their demands simply became irrelevant in a deregulated environment in which credit was freely available even to those individuals considered by lenders to be high risk, in particular women and minorities. And of course these changes in the regulatory environment were driven not by lingering agrarian influences on social policy, but by the manner in which inflation wreaked havoc on New Deal financial regulations. It is curious given the centrality of the deflations of the late-nineteenth and early-twentieth centuries to Prasad's argument that the inflation crisis of the late twentieth century receives very little attention in her analysis.

None of this should take away from what the book accomplishes. Prasad's overarching argument that a consumption-based growth strategy in the United States has been less successful in combating poverty than European efforts to suppress private consumption in favor of social investment is persuasive and important. And, of course, I'm very enthusiastic about the book's fundamental insight that credit represents distributional politics by other means, and therefore that the political economy of credit deserves the same close attention that scholars have directed at the welfare state for several decades now. I also find Prasad's argument that financial deregulation reflected a broad societal consensus and not merely the interests of financial sector actors quite compelling. But this last point also makes me wonder about Prasad's forceful rejection of the "myth" of liberal, laissez faire capitalism. It is one thing, I think, to suggest that even liberal market economies rest on extensive state intervention and quite another to argue that this means they are not really "liberal" after all. Liberalism is, of course, a powerful ideology as well as a set of practices, and the fact that the ideology mis-describes those practices does not make it any less real – or any less constraining. In this regard, one wonders if the fact there is no natural constituency for regulating financial markets – and more generally, that our society has chosen to rely on credit rather than the welfare state to provide its citizens some degree of economic security – is not in part a reflection of deeply entrenched features of American political culture that privilege "market" over state solutions. If this is indeed the case, then the possibilities for achieving the sorts of policy reforms that Prasad describes in the concluding chapter of her book become all the more daunting. Let's hope that I am wrong!

Endnotes

1. This evocative phrase belongs to Marriner Eccles, the Chairman of the Federal Reserve under President Roosevelt, and was turned up by Sarah Quinn (2010) in her dissertation research.

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Reflections on *The Land of Too Much*

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For European scholars of revolution and collective violence, bread matters. Riots over the availability of bread, debates over the moral frameworks for determining the just price of bread, waves of unrest that follow seasons of drought and failed harvests – all of these are familiar, but unremarkable, catalysts for destabilizing social conflict. Bread represented a point of intersection that anchored relations between city and countryside, among peasants and urban workers and landowners, between everyday life and the legitimacy of regimes. In her powerful new book, *The Land of Too Much: American Abundance and the Paradox of Poverty* (2012), Monica Prasad makes a related claim on a sweeping scale. In the Atlantic world – stretching broadly from the breadbasket of the North American Midwest through the center of Europe to the grain-producing areas of the Baltics, agricultural abundance and scarcity have shaped the trajectories of state development, including the dimensions of state power, the configuration of welfare states, and the robustness of efforts to address poverty.

Because Greta Krippner has already recounted Prasad's central arguments, I will focus on two other aspects of this challenging work: the structure of the "in comparative perspective analysis" and the theorized relationship between destabilizing episodes and the establishment of durable trajectories of political development. Three questions organize what is necessarily a highly stylized presentation of a rich and complex argument:

- What is to be explained?
- Why are explanations dominant in comparative political economy inadequate?
- What is the causal path that leads from agrarian issues to the shape of the American state and its distinctive portfolio of policies with respect to taxation, the regulation of finance, and the alleviation of poverty?

LOTM opens by making a case that scholars have asked the wrong comparative questions about American political development and the emergence of European welfare states. In these discussions, the distinctive weakness of the American state has been the central issue. Whether this concern is traced to the experiences of Progressive-era reformers as "policy tourists" in Europe or to European scholars unimpressed by the social safety net in the United States, these approaches all pose the question about American political development as a puzzle of absence. Why was there no development of a strong, centralized bureaucratic state in the Weberian mode? Why was there so little progress toward the consolidation of a fully-realized welfare state?

To reframe the question, Prasad invokes an interdisciplinary literature on American political development – one advanced by historians, political scientists, and a very few sociologists. Countering the image of the weakness of the American state that derives from a particular reading of Tocqueville, from Louis Hartz' influential post-war portrait of a fundamentally liberal American political culture, and Stephen Skowronek's description of the nineteenth-century American state as a "state of courts and parties," new developments in APD have focused on dismissing the "myth of the weak American State." In effect, this involves making a case for alternative ways of understanding state "strength" and "capacity."

As Prasad's discussion of these literatures makes clear, there have been many ways of characterizing the distinctive strength of the American state. Legal historian William Novak identifies it in the structure of American law, political scientist Daniel Carpenter as well as a host of historians have focused on the "invention" of new forms of independent regulatory agency such as the Food and Drug Administration or the Interstate Commerce Commission. Still others (myself included) have drawn on Michael Mann's concept of infrastructural power, focusing on the web of relationships between state actors and private individuals, firms and associations to produce a capacity for state-centered societal mobilization. And, along with her collaborators Isaac Martin and legal historian Ajay Mehrotra, Monica Prasad has been in the lead of those making a compelling case for the importance of taxation and the emergence of a powerful fiscal state.

Having argued forcefully for the importance of taxation to the distinctive capacities of the American national state, this part of the argument ends – in a way that is simultaneously unsatisfying and potentially generative. After introducing a cast of scholars working as her allies in confronting a characterization of the American state as weak, Prasad does not follow with a sustained argument about why taxation matters more or differently than all these other ways of thinking about the strength of the American state. In this respect, *LOTM*'s lack of much discussion of federalism is particularly puzzling. But, overall, Prasad delivers an enormously plausible argument for the need to understand the origins of the distinctive configuration of taxing strategies and capacities that marks American political development.

After making her case for the right way to pose the question, Prasad's brief for what matters about the character of modern states is then inserted into a different set of arguments about "whether capitalism can benefit everyone in a society" and "which model of capitalism can best produce sustained economic growth" (pp. 25-26). The argument proceeds piecewise, eliminating lines of argument as inadequate for explaining the United States as an important contrast to development across other advanced industrial democracies, both in Europe and in the settler societies that emerged from the British Empire: Canada and Australia above all. Seriatim, she disposes of:

- **Class-based arguments** (pp. 27-30) that attribute variations in social provision and the redistributive capacity of the state to the strength of organized labor and the expression of that strength through party politics.

- The influential **"varieties of capitalism" argument** advanced by Peter Hall and David Soskice (pp. 30-35) which explains variations in social provision as the result of employers' efforts to manage the recruitment of a skilled labor force and the establishment of reliable networks of finance and supply.

- **National culture arguments** (pp. 35-39) have long been a particular target of Prasad's critical efforts. In the context of *LOTM*, she is particularly concerned to establish that appeals to an American taste for free markets and individualism cannot explain the more powerful development of antitrust legislation and "adversarial regulation" in the United States as compared to Britain, Germany or France.

- **Racial Fragmentation** (pp. 39-42). Here, Prasad argues that "one can imagine an alternative political history in which the United States developed a public welfare state along European lines but restricted it to whites, developing cross-class solidarity on the basis of racial solidarity, and one can imagine this welfare state gradually being extended to incorporate all races in the later part of the twentieth century, just as the 1935 pension provisions of the Social Security Act gradually developed in both the percent of the old-age population they covered and the generosity of the benefits they delivered." It should be noted, that in some places the history of American social provision does take exactly this form, most notably the political agreements struck by Progressives in the border states who linked the institutionalization of Jim Crow to the expansion of public education for whites.

- **State Structures and Historical Institutionalism** (pp. 42-44). For this large literature, Prasad singles out Ellen Immergut's argument about political fragmentation and the multiplication of veto points as adequate for an explanation of what the US did not do but not as an explanation for those respects in which the US developed particularly effective forms of state intervention.

This is a masterful survey of a number of rich and complex literatures. But, in Prasad's telling, it is also very schematic. The definition of the outcome to be explained and the criteria for the evaluation of arguments shift from section to section over the course of the important chapter on "comparing capitalisms." This piece of the discussion will merit repeated readings to carefully assess just how definitively each potential competing argument has been dismissed.

This section ends with Prasad setting out the criteria by which she wants the remainder of the argument to be judged: "a good theory should explain the heavier state intervention in some domains in the United States as well as the less developed public welfare state in the United States that results in greater poverty and inequality. Second, a good theory should explain why European states were able to combine economic growth with redistribution for several decades. And a good theory of comparative political economy should be able to explain the origins of the divergences in the American and European trajectories" (p. 44). On this point, a problematic aspect of the structure of the analysis should be noted. Competing theories are eliminated on the basis of cross-national comparison rather than repeatedly re-adjudicated in the context of the American case. For example, once the case is made that "strength of organized labor" cannot account for the overall pattern of cross-national differences, "strength of organized labor" is then not systematically brought in and assessed in the explanation of the steps in the causal chain that follows.

On this score, we can look forward to an intriguing clash of titans in the near future: Monica Prasad v. Ira Katznelson, whose Bancroft prize-winning *Fear Itself: The New Deal and the Origins of Our Time* (2013) argues insistently that the master key to American politics of the 20th century is not agrarian populism but rather the configuration of race, Congress, and southern influence within Congress. The politics of the land west of the

...a problematic aspect of the structure of the analysis should be noted. Competing theories are eliminated on the basis of cross-national comparison rather than repeatedly re-adjudicated in the context of the American case.

Mississippi play only a minor role in his analysis. This argument meets the first standard for a good theory, that it should explain both the underdevelopment of the public welfare state and heavier state intervention in other respects – this lies in Katznelson's claim for the preferences of Southerners in Congress to insulate their own states from public spending programs that might destabilize racial hierarchies but their willingness to support northeastern nationalists when it came to a strong military. "South" certainly overlaps with "agrarian" but the terms are not identical. Notably, support for the 16th amendment to the Constitution allowing for a federal income tax came from both regions. And, arguably, the legacy of slavery is always a powerful contender to explain divergences between the development of the United States and the industrial nations of Europe. Both books were in press at roughly the same time, so *LOTM* can't be expected to have anticipated Katznelson's argument. How might Prasad adjudicate these overlapping but distinctive claims for the central factor in twentieth century American political development?

Finally, let me turn to the structure of the historical argument. This piece of the analysis begins with an absolutely critical point for comparative historical sociology:

> the United States was experiencing and responding to very different problems than any of the European countries. This has been obscured by the attempt to treat the countries as comparative units that can be indexed by variables such as the degree of labor or employer power they possess. While these types of comparisons are certainly useful for arriving at partial answers in some domains . . . , they must always be combined with an overarching awareness of the very different roles the countries played in world history. As an agricultural exporter, the United States witnessed a strikingly different pattern of agrarian politics than did the importing countries of Europe (p. 45).

The significance of this point is underscored by another wonderful book, William Cronon's *Nature's Metropolis: Chicago and the Great West* (1991). Cronon's account illuminates the eventful qualities of Prasad's argument which involve a series of dramatic changes in relative magnitudes of supply and demand rather than a single crisis. With the development of new transit capacities by canal and rail, waves of grain flowed east, across the Atlantic destabilizing both politics and markets. In the process they catalyzed new social technologies – grain elevators and commodity exchanges and futures markets – and parallel industrial changes in meat-packing that would generate the new massive packing houses that would, in time, become subject to the regulation of new federal agencies.

Prasad follows out the reverberations of this wave of grain along the specific path of agrarian populism. Given the perversity of agricultural abundance, years of great harvests are years of depressed prices. The resulting volatility of incomes led to greater reliance on credit. Because many farmers carried heavy mortgages on their land and because all farmers were exposed to a seasonality of credit – loans in spring in order to plant followed by repayment contingent on the returns to their harvest – they were deeply entwined in financial markets. This helped to generate both the political culture of populism (a hostility toward those who controlled financial systems and grain markets) and a distinctive political and economic inventiveness which fueled the search for new monetary policies as well as the "subtreasury system" that would insulate farmers from volatility and exploitation by bankers and middlemen. These circumstances generated a propensity to call on government for the regulation of markets and money, an "agricultural statism" that Prasad adopts from political scientist Elizabeth Sanders.

These add up to a form of class-based politics that is not structured by position within relations of industrial production. This politics was amplified by the institutional structure of federalism. With westward expansion in the context of a federal system, the agrarian influence on politics was augmented with the seating of each additional pair of senators and each additional vote in the electoral college. Through this interaction with institutional structures, agrarian populism gained a degree of leverage out of proportion with the population of the Midwest.

Why does this matter for the structure of taxation and, specifically, the adoption of the income tax? The language of the Constitution (scattered throughout Article I) set firm limits on the taxing power of the General Government:

> Representatives and direct taxes shall be apportioned among the several States which may be included within this Union, according to their respective Numbers. . . . The Congress shall have Power to lay and collect Taxes, Duties, Imposts and Excises, to pay the Debts and provide for the common Defence and general Welfare of the United States; but all Duties, Imposts and Excises shall be uniform throughout the United States. . . . No Capitation or other direct, Tax shall be laid, unless in proportion to the Census or Enumeration herein before directed to be taken.

So here at the birth of the Republic is a fundamental point of divergence with European state-building projects. Whereas rulers in Britain or France could develop elaborate systems for assessing wealth and extracting taxes (Brewer 1990), the taxing powers of the "general government" in the United States with respect to property were limited by the requirement of proportionality with respect to population whether in Massachusetts or Kentucky, New York or Nebraska. So at the beginning, there is a powerful constitutional decoupling of economic development from the expansion of state power.

This constitutional feature accounts for many of the oddities of the American tax regime such as the dependence on the tariff or import taxes which were assessed at the borders of the nation rather than state-by-state. Excise taxes – which were classed as "indirect" taxes on products rather than persons and their property – were also an important and controversial source of federal revenue as quickly became evident in the Whiskey Rebellion of 1791. I want to flag this, however, inasmuch as excise taxes are a form of particularly focused sales tax that produced a substantial fraction of federal revenues. Thus, adoption of the income tax made prohibition possible and the end of prohibition, in 1933, helped Roosevelt's administration support the public spending that would attempt to pull the nation out of the Great Depression. So we may not have a national sales tax, but we certainly do rely on a nationwide system of taxing one particular form of consumption.

The dilemma created by the constitutional decoupling of uneven economic development from the revenue-gathering powers of the national government precedes the great waves of grain crossing the Atlantic. The dilemma was evident in the repeated efforts to find a way to tax incomes, a suggestion that first came up in the war of 1812 and which was then acted upon during both the Civil War and the Spanish-American War – a pattern that brings into question Prasad's dismissal of war as an important prompt for adoption of the income tax (pp. 254-55). The key contribution of agrarian populism was not to make the income tax "thinkable" but rather to add the political heft to get the amendment through Congress and ratified at the state level during a moment of unusual opportunity prior to the First World War: schism within the Republican party, a Democratic wave across many levels of government, widespread concern over the unprecedented fortunes of Rockefeller and Carnegie in particular, and the long-standing enthusiasm of former President Theodore Roosevelt and his allies for new funding to support the rapid expansion of American naval power.

This is a "yes, but" critique that is generated by the structure of the book as a whole, with its combination of "in comparative perspective" discussions that eliminate arguments and then a narrative that follows one element: agrarian populism. What this misses, in my reading, is systematic examination of the important interactions with some of the factors dismissed during the "in comparative perspective" portion of the analysis. For the adoption of the income tax, I suspect that it is the interaction of agrarian populism with the constitutional requirement that aggregate taxation by state be uniform in proportion to population. If the U.S. general government had the powers to tax property that John Brewer ascribes to the British state, then a progressive income tax would not have been so important as a method of capturing the benefits of economic growth that were concentrated in particular regions. Agrarian populism plus historical institutionalism seems the stronger explanation.

The need to "bring back in" some of the other factors also seems important to explain a second feature of the taxation regime: it is not just a system of income taxation, but one of income taxation with deductions. On this point, many of the central deductions don't have anything directly to do with farmers – instead they reflect the interests of those with property, income, and employees. Let me illustrate this point with a short history of the deduction that I know best: the charitable deduction which was adopted as part of the war revenues legislation during World War I. This change to the federal regulation and tax treatment of charitable contributions came as part of the debate over the financing of the war effort. Congress spent much of 1917 debating the War Revenue Act. The large questions concerned the character of the taxes to be imposed – on personal incomes, war profits, or consumption – as well as the progressivity of each of those taxes. The substantive political question was who would bear the financial costs of the war. Would these be imposed most heavily on the companies enjoying unprecedented prosperity due to the war mobilization? Or would the tax burden be distributed more broadly, impacting a wider range of citizens through both taxes on personal income and on consumer purchases? Early in the debate, the question of charitable contributions was addressed, prompted by telegrams from two financiers who explained that they gave significant donations to philanthropies, that these important philanthropies depended on such charitable donations, and that Congress might consider deducting such contributions from their gross incomes before the income tax was calculated. Such a deduction would be consistent with precedents set in state-level estate taxes and resonated with the exemption of various categories of educational, scientific, and benevolent organizations from federal corporate excise taxes.[1] The Hollis Amendment was passed and established the precedent for the charitable tax deduction which would become a permanent feature of the American income tax code, one that privileged charitable giving over other forms of private consumption.

In the debates over major exemptions and deductions, the lead voices are often of the wealthy, of business leaders who shaped the overall structure of the individual and corporate income taxes in important ways. My suspicion is that much of this effort was neither advocated by agrarian populism nor by direct opposition to it, but rather by a kind of "politics of the second best" made unavoidable within a capitalist democracy. Given that a majority might well be persuaded to adopt strongly redistributive policies (particularly at the turn of the century when concerns over the unprecedented fortunes of Rockefeller and Carnegie generated a cross-class concern with the problem of enormous wealth), businessmen opted to do what they could to increase the proportion of their tax vulnerability that could be extracted in forms over which they retained substantial control: individual and corporate charitable contributions, employee benefits, and so forth. Here, the relevant fight is not between agrarians and employers, but rather between "enlightened" employers and their counterparts more committed to a ruthlessly free market. (These are the forerunners of the corporate elite whose fracturing is now mourned by Mark Mizruchi.)

Prasad has made a masterful case that the trajectory of state development in the United States cannot be understood in the absence of attention to agrarian populism – that it is a necessary cause for producing the political world we have inherited. I am persuaded, but would argue that the specific paths of that influence need to be more carefully delimited, temporally constrained, and considered within combinatorial explanations.

The importance of thinking about the factors that matter in addition to agrarian populism is that this forces us to think about the moment when the direct effects of a causal factor fade out, when the great waves of grain cease to disturb American politics to a significant degree. As Greta Krippner has noted, the argument for the agrarian contribution to the American system of taxation is more compelling than that for the problem of credit. So historical sociologists need to ask: At what point do the chains of causal influence become too attenuated? When do the other factors and the interaction need to move to the fore in the explanation?

Monica Prasad provides her own powerful answer to this question and it is one that should be taken to heart by all those of us who practice historical sociology, with the emphasis on the second term. Reflecting on her argument, she explains:

> A state's approach to consumption is not an exogenous or ultimately determining factor but rather a moment when a causal chain that ends with a particular approach to consumption inaugurates a new causal chain that ends with a particular approach to the welfare state. This is a Durkheimian answer to the problem in historical social science of how far back in history to trace a causal chain. History

> produces sequences in which phenomena become sui generis, that is, no longer reducible to the events that brought them into being. (p. 250)

This is both a maxim for sociologists as social scientists and a challenge for historical explanation. Prasad has made a masterful case that the trajectory of state development in the United States cannot be understood in the absence of attention to agrarian populism – that it is a necessary cause for producing the political world we have inherited. I am persuaded, but would argue that the specific paths of that influence need to be more carefully delimited, temporally constrained, and considered within combinatorial explanations. Whether or not agrarian populism is the master key of American political development, however, is something that we will not know until we see that prize fight between Prasad and Katznelson – and anyone else who wants to jump into the ring, thinking themselves equal to this encompassing grasp of the scholarly literature and exceptional level of intellectual ambition and original insight. *The Land of Too Much* is a book of much too much for a single set of remarks and will bear fruit through many readings of its arguments.

Endnotes

1. Olivier Zunz, *Philanthropy in America: A History* (2012), pp. 86-88; "Hollis Amendment Exempts Charity Donations from Tax," *Red Cross Bulletin* (vol. I, no. 25, October 15, 1917), p. 4.

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Author's response

Monica Prasad
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It's a delight to get such a substantial engagement of my arguments. Elisabeth Clemens and Greta Krippner are ideal readers, taking the book on its own terms, engaging it rigorously, appreciating its accomplishments despite its shortcomings, and even taking the argument into new territory. I'm grateful to Michael Hout for organizing the ASA panel for which all of these comments were first prepared.

I am particularly glad to have the opportunity to revisit two issues that have been dominant in the reception of the book: the role of race, and whether the political economy I discuss really begins in the 1970s rather than in the New Deal or the postwar period.

In retrospect, I should have devoted a whole chapter to the question of race. Clemens cites Ira Katznelson's work to ask whether American welfare state exceptionalism is a result of southern Democrats in Congress introducing a racial bias into the welfare state. While Katznelson's book came out too late for me to engage it, the argument of the southern Democratic veto over the welfare state was made by Jill Quadagno thirty years ago (Quadagno 1984), and I discuss it briefly (Prasad 2012, 29-30).

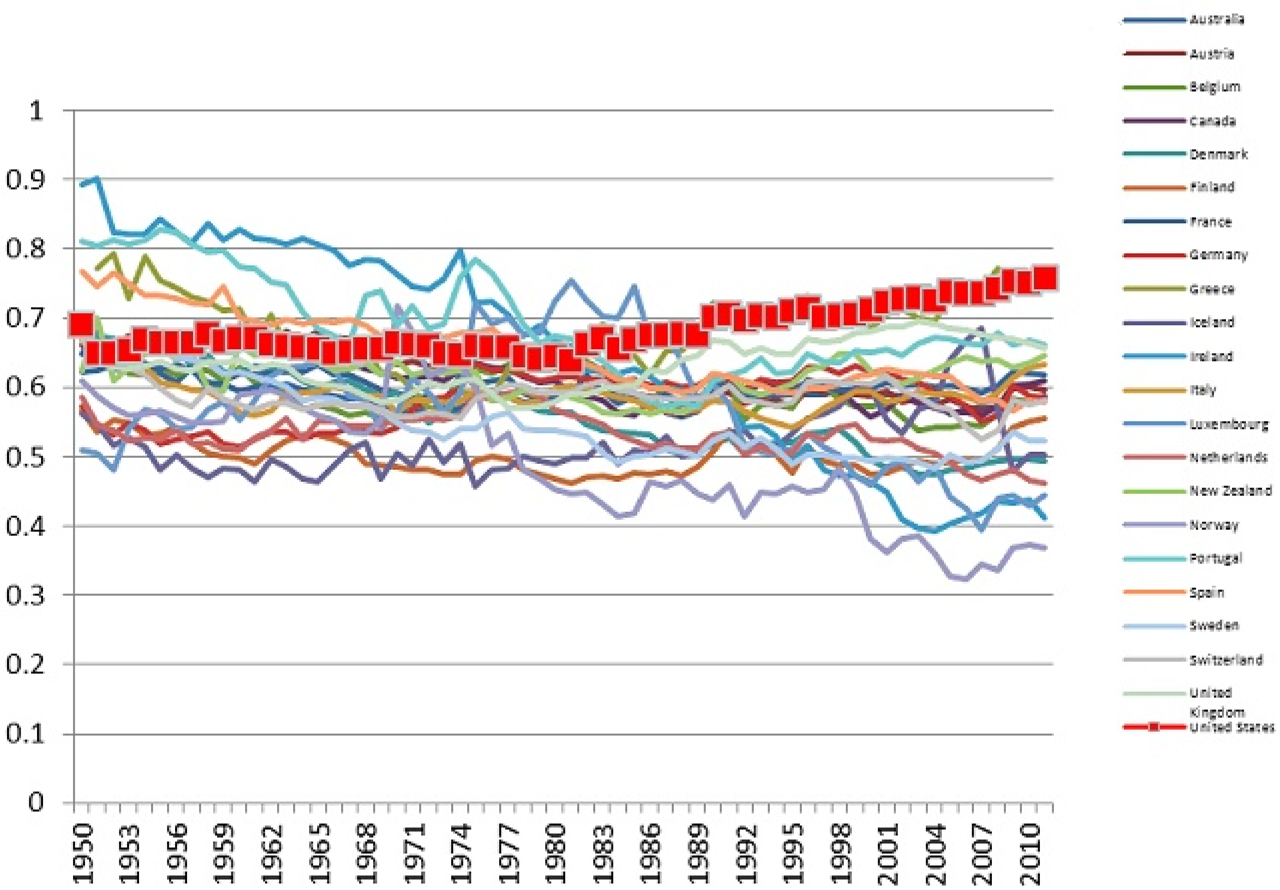
I find this argument unconvincing because the policies the southern Democrats in Congress influenced, such as minimum wage and social security pensions, are not the policies on which the U.S. looks different compared to other countries. For example, in comparative perspective the U.S. actually looks pretty good in terms of old age pensions, and from 1950 to 1983 the American minimum wage was higher, often substantially higher, than the French (Piketty 2014, 209), while Germany only established a minimum wage for the first time in 2014. The southern Democrat veto affected some welfare policies at their inception, but those policies later grew to match the welfare state in other countries. Rather, what makes the American welfare state different throughout the post-war period is, of course, the long absence of national health insurance, and on that issue the southern Democrat argument does not work because Roosevelt's health insurance proposals never got as far as Congress. (The usual understanding is Roosevelt censored himself because of the opposition of the American Medical Association, but as Antonia Maioni (1998) has shown, the strong opposition of Canadian doctors did not prevent health insurance from passing in Canada. Thus, I argue that to understand the long absence of national health insurance in the U.S., we have to understand the way in which the system of progressive taxation led to private rather than public provision of health care, as explained in detail in the book.)

While I won't address all the aspects of Clemens's thoughtful and provocative critique, let me briefly mention her point that "many of the central [tax] deductions don't have anything directly to do with farmers" but rather with businesspeople. The funny thing about the scholarly literature on tax exemptions is, no one asks why business leaders were asking for tax exemptions in the first place. The surprising answer, as I show in the book, is tax rates on business were so much higher in the U.S. than in other countries in the early part of the twentieth century. Businesses in other countries didn't need to ask for tax exemptions because they already had low tax rates. Businesses in the U.S. had to struggle to get tax rates as low as those in other countries. And why were American corporate tax rates so high? Because, of course, of the regime of progressive taxation instantiated by American agrarians.

Similarly, farmers are at the origin of the causal chain that ends with a credit economy. Krippner asks how Roosevelt's response to the banking crisis reflects agrarian influence. Agrarian influence is evident not in the shape of the response itself so much as in the need for such a response—that is, agrarian influence is evident in the restrictions against branch banking that created the crisis Roosevelt was responding to. In the book I compare the U.S. to Canada, which also had a great depression, and in which construction and industry were also hard hit. But in Canada, banking escaped the crisis, and scholars who have studied this episode think this was because of Canada's willingness to allow banks to branch across state lines, which the U.S. did not. And it was because of agrarians that the U.S. did not allow branch banking. So my causal claim for the U.S. is: no agrarians, no banking crisis, no need to revive banks by creating an infrastructure of mortgage credit. If not for activist farmers, we would not have the FHA. But the farmers are at the very beginning of the causal chain, not in the middle of it when Roosevelt is making his decisions as to what to do about the crisis.

Farmers do not explain everything, and I am not trying to develop a single-factor theory of history turning on farmers. But agrarian influence, which is in turn shaped by patterns of integration into export markets, explains substantially more about comparative political economy than scholars have appreciated.

Figure 1: Household Consumption as Share of GDP, 1950-2010

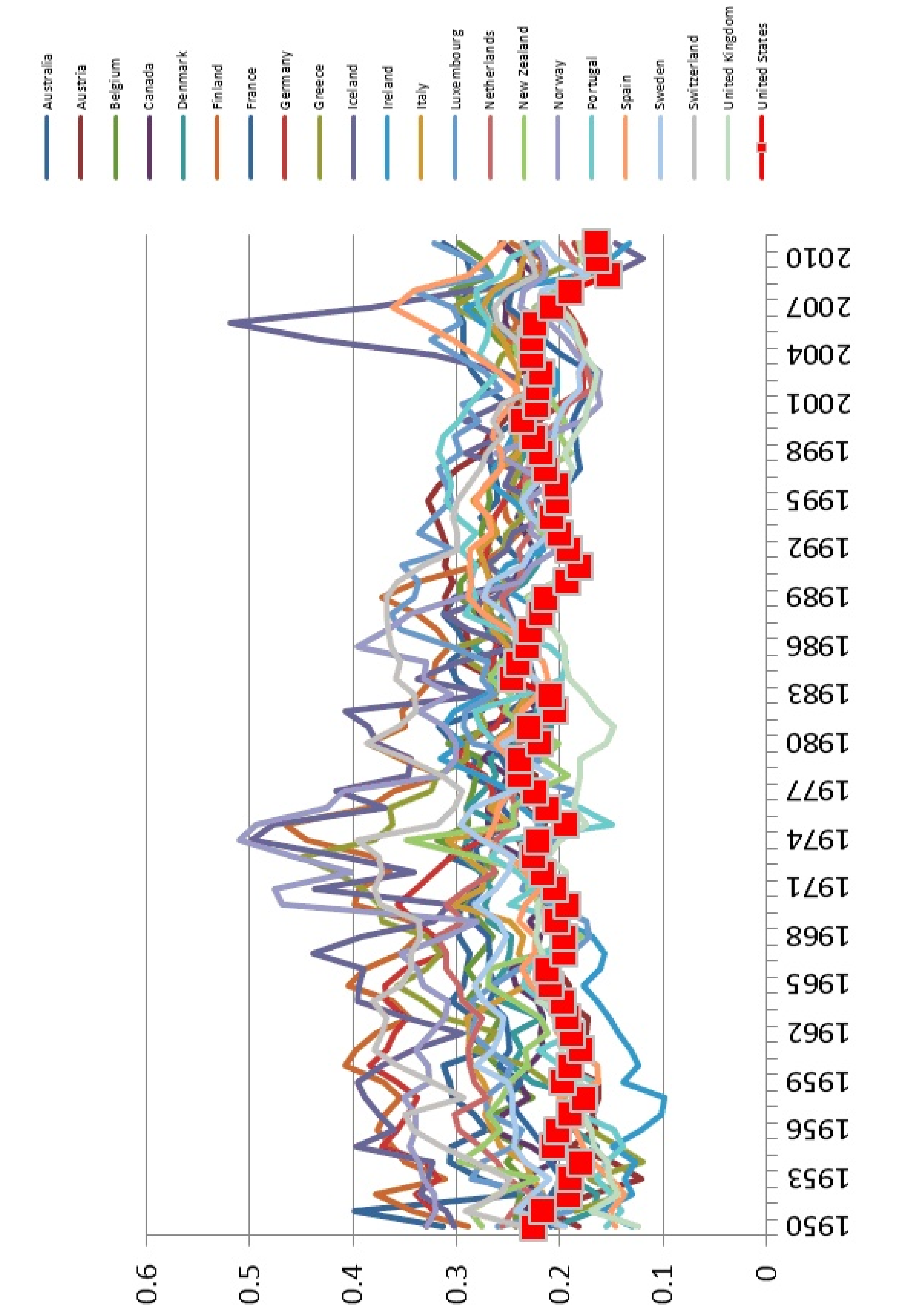


Source: Penn World tables

Krippner argues changes in credit markets of the 1970s were of a different order than changes in prior decades. But I am not claiming nothing changed in the 1970s. Rather, my argument is that to understand the changes of the 1970s, we need to understand the credit infrastructure in place in the post-war period. If we begin our investigations in the 1970s, we conclude that politicians stumbled around and developed what Krippner in her book calls "ad hoc responses" (Krippner 2011, 23) to the crisis of low economic growth and inflation of the 1970s, and these ad hoc responses added up to a pattern of easing credit. But if we examine the history of American political economy in earlier decades, we understand that these responses were not ad hoc responses at all—they were outgrowths of a political economy built on the premise of consumption, and which therefore made extensions of credit much easier than expansion of the welfare state.

For example, consider Figure 1, which shows household consumption as a share of GDP as far back as 1950 across the advanced industrial world, with the United States one of the leaders throughout the period. Spain, Greece, Portugal, and Ireland are even more heavily invested in consumption in the early years, and

Figure 2: Gross Capital Formation as Share of GDP, 1950-2010



Source: Penn World Tables

there may be some sort of Mediterranean exception to the story I tell. But at least when compared to the continental European and Scandinavian welfare states, the U.S. is astonishingly focused on household consumption long before the 1970s. The mirror image of this is Figure 2 (previous page), which shows gross capital formation as a share of GDP, a measure of investment. The continental European and Scandinavian countries have focused much more heavily on investment throughout the post-war period than the U.S., for reasons explained in the book. As this chart shows, and as I note in the book, the U.K. is a curious case, sometimes looking more like the other European countries and sometimes looking more like the U.S. But overall, there is a striking divergence in rates of consumption and investment throughout the postwar period that starts far before the 1970s.

If we only start our investigation in the 1970s...we end up concluding that even the left in the United States favors the market, and there are no other possibilities for politics because American culture excludes those possibilities. But if we look further back in history, we understand why the left in the 1970s favored financial deregulation: it was not an ideological preference for the market, but a pragmatic attempt to expand opportunities for those who had been excluded from a very particular kind of political economy.

America's consumption-focused economy was enabled by an infrastructure of credit established even before the war, as explained in the book. Understanding this prehistory helps to make sense of many things about the story that are otherwise puzzling, such as why financial deregulation was supported by the left as well as the right in the 1970s. Ralph Nader was in favor of financial deregulation, for example, and can't be assimilated into Krippner's argument of financial deregulation as the outcome of politicians' attempts to avoid blame for the crisis of the 1970s. Rather, he and other activists on the left favored financial deregulation because in the context of a political economy in which credit is how consumers achieve welfare—an equation set up during the New Deal—expanding credit is not only an ad hoc response and an attempt to avoid blame, but a way to incorporate less privileged citizens into the mainstream of American political economy. In short, what seems like an ad hoc response if we only look at the 1970s reveals itself to be a historical legacy if we look a few decades back.

As I note in the book, "Krippner and others are not wrong to focus on the 1970s… The economic crisis could have led to a conscious choice of a different path, and macroeconomic difficulties could have led consumers to change their behavior. Instead, debt levels rose to new heights while savings rates plunged. However, examining the history of credit before the 1970s helps to explain why this particular credit-oriented path was chosen in the 1970s—it shows the tradeoffs decision-makers faced, and the infrastructure in place that made some choices easier than others" (Prasad 2012, 197-198).

And this relates to Krippner's last point. If we only start our investigation in the 1970s, and we see even the left favoring financial deregulation, we end up concluding that even the left in the United States favors the market, and there are no other possibilities for politics because American culture excludes those possibilities. But if we look further back in history, we understand why the left in the 1970s favored financial deregulation: it was not an ideological preference for the market, but a pragmatic attempt to expand opportunities for those who had been excluded from a very particular kind of political economy.

More importantly, if we look further back in history, we begin to see the very strong strand in favor of financial regulation throughout American history, and that Americans have not always favored market solutions. Indeed, the whole sequence being discussed here starts with greater regulation of banks in the U.S. than in other countries. The U.S. had more stringent regulation of banks in the Glass-Steagall laws separating commercial from investment banking, as well as in the McFadden Act, which prevented branch banking; these are regulations that have been implemented only recently and rarely in other countries.

And this leads to the main point I hope readers take away from the book. One of the strongest legacies of the rise of the right under Ronald Reagan is to have made Americans forget our own radical past. Many of us in the academy implicitly seem to adhere to a Tea Party vision of American history, in which government intervention in the public interest is somehow seen as out of line with American values or American traditions. The most fascinating part of writing this book for me was discovering the extent to which this is false. Americans throughout the twentieth century were as vociferous about using the state in the public interest, and against capital and the market, as Europeans, but the forms this intervention took were very different in the U.S. because the U.S. was dealing with very different problems. In this book, I have given one possible explanation for this surprisingly interventionist nature of the American state. If readers are not convinced by this particular argument, they need to provide an alternative explanation for how there could be so much more intervention than any of our theories have suspected. I would be delighted if this book manages to kick off such a debate, even if my particular argument doesn't stand the test of time. If nothing else, I hope this book might get some of us in the academy to rediscover some of the fascinating and forgotten aspects of our own surprisingly radical history.

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Americans throughout the twentieth century were as vociferous about using the state in the public interest, and against capital and the market, as Europeans, but the forms this intervention took were very different in the U.S. because the U.S. was dealing with very different problems.

Book Symposium

The Emergence of Organizations and Markets

Princeton University Press

John F. Padgett and Walter W. Powell

Editor's Note: The following text is based on an author-meets-critics session organized by Bruce Carruthers for the American Sociological Association Annual Meeting in San Francisco in August, 2014. My thanks go out to Jim Mahoney, Brayden King, Kate Stovel, Woody Powell and John Padgett for agreeing to submit their comments to the newsletter, and to Andreas Wimmer for helping to make it happen.

The Emergence of Organizations and Markets: An Agenda-Setting Book

James Mahoney
Northwestern University

In *The Emergence of Organizations and Markets*, John Padgett and Woody Powell outline an extremely important agenda: they seek to develop new tools for understanding and explaining the emergence of new organizational forms.

Explaining true novelty in organizations -- or true novelty in anything else -- is one of the more difficult but more worthy undertakings that social scientists can pursue. It is especially worthwhile if the pursuit is undertaken in conjunction with empirical analysis. And while the theory chapters of this book are weighty in their own right, most of the book consists of empirical chapters that seek to explain emergence across quite diverse substantive topics.

At the heart of the book is a new framework for analyzing the emergence of new organizational forms such as these. The framework combines insights from social network analysis with insights from biochemistry, especially the biochemistry idea of autocatalysis. This is a fresh synthesis. The complaint about network analysis has always been the complaint about structural approaches more generally: it lacks a mechanism of transformation. It is not good at explaining change, much less emergence. This book seeks to overcome this structuralist bias and thereby allow for the explanation of emergence.

The key move that this book makes is to appropriate ideas and concepts used to explain the origin of life in order to make better sense of the emergence of organizations and markets. The analogy is quite interesting, and it goes beyond previous efforts to use evolutionary ideas from biology for the explanation of organizations. If currently influential evolutionary approaches to organizations draw heavily from the discipline of biology, this book draws more heavily from the discipline of chemistry.

For this reader, there is good news and bad news to report about this synthesis of network theory and biochemistry. It is mostly good news. One core piece of good news is that the approach has inspired the authors to develop some quite interesting and quite useful mid-level mechanisms of organizational genesis. In particular, the list of eight mechanisms of organizational genesis in chapter 1 is extremely helpful. These eight mechanisms are presented on pp. 11-26, and they make up the heart of the usable part of theory. I am not going to discuss all eight of them, but focus on just three of them.

One mechanism is *transposition and refunctionality*. This mechanism is the movement of a practice from one domain to another, and its repurposing to fit into the new domain. This is innovation in the sense of "a new purpose for an old tool." This is the most important mechanism in many of the empirical chapters of the book.

As an aside, this mechanism also appears to be the main mode of theory invention used by Padgett and Powell -- that is, they are transposing existing ideas from chemistry into the domain of organizations and sociology.

In presenting this mechanism, the authors set an agenda of research for others to take up. Some of the questions their framework inspires are the following. What kinds of agents are best at transposition and refunctionality? What kinds of organizations or environments are more likely (or less likely) to experience refunctionality. What are the normative implications attached to this mechanism? When will transposition help organizations meet their goals versus undermine their goals?

The next mechanism I want to discuss is called *incorporation and detachment*. This occurs when a part of one network is inserted into another network without detaching from its original network. You can think about this as two Venn diagrams that partially overlap. In fact, the book makes excellent use of just these kinds of Venn diagrams.

The agenda introduced by this mechanism in part involves exploring how learning and information dissemination occur in organizations. The mechanism suggests that once one network has partially penetrated another, it can spread new ideas to the penetrated network as well as bring back new ideas to its own network. What we need are hypotheses about the kinds of organizations that will allow for incorporation and detachment. Scholars need to ask: under what circumstances are we likely to see incorporation and detachment?

The book's theory explicitly brings in ideas of power and conflict, as can be seen in the mechanism of *purge and mass mobilization*. With this mechanism, the upper ranks of hierarchies are purged, and the bottom tiers are raised up to take their place. Stalin did this with the Great Terror.

Here the movement of ideas and new organizational forms can occur within a given organization or network. New organization emerges by eliminating old forms of organization and allowing marginalized actors to remake the organization. It is a kind of revolution from within. The key initiating source of the change is the actor who carries out the purge of the top. But really the key source of emergence is the marginalized actors who rise to the top after the purge. They bring the new organizational modes with them.

Again, this mechanism sets an agenda of research: What kinds of organizations are susceptible to purge and mass mobilization? Is it possible that purge and mass mobilization will end up reproducing prior organizational patterns? Said differently, when will purge and mass mobilization produce higher degrees of invention and innovation?

As a reader, I had some more general questions that I wanted to ask the authors. One concerns the relationship between this book's theory and field theory. The diagrams in this book often specify domains that might be thought of as fields. For example, in the discussion of purge and mass mobilization, there is a diagram of the Great Terror. In the diagram, on p. 22, one field seems to be the economy and another is the Communist Party. How do the authors feel about situating their theory as a kind of field theory?

Crucially, one does not have to understand the biochemistry roots of this argument to appreciate the basic Padgett-Powell model of economic production. The model is basically as follows: Firms are containers of skills. Skills are rules. Skills change products into new products. Trade involves the movement of products through firms, which can change skills. This model is useful for understanding the co-evolution and co-constitution of products and organizations.

Second, the networks in the diagrams tend to break things down into domains such as political, kinship, economic, military, and religion. I imagine that the kinds of domains or networks that one thinks are important will be heavily influenced by other theoretical considerations, such as whether one is a Marxist or not. Does the theory in this book have any advice for telling us how to determine the relevant and most important domains in a given substantive area? Would it be possible for two scholars to whole heartedly embrace the approach of this book but completely disagree with one another about the sources of innovation and invention in the same empirical setting?

Third, I wondered if the authors would be willing to say something about the relationship between this book and the earlier Powell and DiMaggio edited book, *The New Institutionalism in Organizational Analysis* (1991). Is this book about emergence, whereas the earlier book was about stability and change? Does the new framework in this book have things to teach us about the issues explored in the earlier book?

For me, the bad news regarding the new book is that the material on biochemistry, including even the core concept of autocatalysis, is rough going for social scientists. Autocatalysis is a bit like the concept of complexity: it is an umbrella label for something very important, but also something very hard to pin down in any exact way. Getting a handle on the concept is a bit like holding a ball of mercury. The concept is formally defined on p. 8 as follows: "autocatalysis can be defined as a set of nodes and transformations in which all nodes are reconstructed through transformations among the nodes in the set." The definition is not bad or wrong, but it is just hard to wrap one's mind around it, in the same way that it is hard to wrap one's mind around many definitions of complexity.

Crucially, one does not have to understand the biochemistry roots of this argument to appreciate the basic Padgett-Powell model of economic production. The model is basically as follows: Firms are containers of skills. Skills are rules. Skills change products into new products. Trade involves the movement of products through firms, which can change skills. This model is useful for understanding the co-evolution and co-constitution of products and organizations.

Moreover, one certainly does not need to have any background in chemistry to use and apply many of the key tools offered in this book. I think the eight mechanisms in chapter one are the core of those tools. The next step for the rest of us will be to develop further generalizations about how those mechanisms work in certain settings to stimulate innovation, invention, and emergence in organizations.

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Comments on *The Emergence of Organizations and Markets*

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The Emergence of Organizations and Markets is a fascinating and challenging book. Drawing inspiration from chemical models of autocatalysis, the bulk of the book presents a series of careful and dynamic analyses that trace how interlocking institutions can lead to reproduction, innovation, and invention in organizational form or substance. Unfortunately, the historical knowledge necessary to evaluate some of the case studies, and the biological vocabulary that provides the foundation for the modeling sections, are beyond the knowledge base of all but a few social scientists. Nevertheless, the book offers an exciting set of ideas, concepts, and examples that have the potential to push the study of networks and organizations in important directions. My comments in this short essay are intended to highlight several ideas that captured my imagination while reading this book, and to identify some of the more provocative threads that I believe merit additional development in subsequent research.

I begin with what I consider the book's mantra, "In the short run, actors create relations; in the long run, relations create actors" (p. 3). This insight, which can easily be traced to the relational sociology of Harrison White and his students, summarizes the powerful autocatalytic foundation for Padgett and Powell's approach to the study of the emergence of organizations and markets. The key insight here is that while actors meaningfully orient their behavior toward others, actors are, profoundly, the product of past relations - both those they may have personally been involved in, and other relations and systems of relations in which they and others are embedded. This reflects an important tradition in sociology, if one that is missing from much of our contemporary scholarship.

In fact, a quick perusal of what passes for sociology in much of the discipline treats actors as endowed with sets of characteristics (attributes) which – in many cases – have no history and whose meaning is unproblematic. Of course a few branches of sociology emphasize the constructed nature of all social material; together, these two poles remind us of the old over- and under- socialized 'man' debate. And so Padgett and Powell, like Granovetter before them, bring networks to the rescue. Yet whereas Granovetter emphasized the consequences of variability in network density, Padgett and Powell, echoing White, emphasize the temporal dimension of the problem.

For Padgett and Powell's mantra to drive a vibrant research agenda, it is necessary to move beyond treating it as an assertion, and consider instead a series of contextually specific questions that can be empirically verified. The chapters in this book provide some nice illustrations of how to do this, though many of them are a bit less connected to the core insight than one might like. At a more collective level, we should also begin to pose a set of more general questions about the relationship between actors creating relations and relations creating actors. Perhaps one of the most obvious questions is: what sort of time scale constitutes the short run, and what is the long run? Does the appropriate time horizon vary by setting, or situation? More sociologically, we must consider what we mean by relations creating actors. How do we know when actors are changed by their network? Most contemporary network methods still focus on measuring the presence and absence of ties, and these methods are quite poor at capturing changes in the salience or meaning attributed to interactions or relationships. (At the same time, if we simply choose to impute changes in meaning /value/salience as a result of change in structure at some aggregate level, we may miss the processes by which cognition and symbolic communication actually changes.) More qualitative strategies for understanding values, aspirations, and orientations might help, though such methods have proven difficult to effectively integrate with network structure in the cross-section, let alone over time.

My second observation is that the book offers a network version of Weber, in the sense that it emphasizes the transformative consequences of the intersection between spheres or domains of social life. Yet where Weber defined spheres of life substantively, here domains are reflected in (often self-sustaining) networks. In both approaches, an important source of organizational transformation is the collision between different spheres, collisions that may lead to adaptation, to importation, to inclusion, to homology, and so on. Most centrally for Weber, and for much of this book, is the essential feedback between political and economic activities, though the chapters organized by Powell expand this to include the modern educational realm.

Embedded in this insight is the notion that spheres (or domains, or networks) when stable may have a ‘logic’ and that interaction across spheres frequently interrupts the existing logic. Of course this language is not the language of Powell or Padgett; rather, it is the language much more familiar to students of organizations and institutions. And yet it seems that it is imperative to continue to specify, in particular contexts, how network structures generate and reproduce logics – where logics may be both material and symbolic. By carefully specifying the relationship between networks and logics, then we might begin to think more systematically about what happens when particular domains collide (and why some domains are likely to collide).

Some issues to consider on this topic: First, how central is the symbolic content associated with a domain (or a network)? In human systems collisions frequently trigger efforts to repair or replace the symbolic capital of networks – a process that no doubt impacts the sort of actors the network produces. So it seems that we need to attend to how these intersections of spheres impact networks at the symbolic or linguistic level as well as at a more material level. And second, are domains really that distinct in practice? As Padgett has previously helped us all appreciate, actual relations and institutions are rarely cleanly situated in one Weberian domain. When relations are multivalent, opportunities for borrowing and transposition may abound. However, the imperative of theory is that we offer more than a laundry list of possible mechanisms, and rather specify (or even predict?!) likely consequences of particular sorts of intersections. One way forward might be attempting to link particular logics with mechanisms as introduced in the book. For instance, networks that sustain a logic of complementarity may contain the sort of anchoring brokers that facilitate innovative, rather than transformative, borrowing.

Building on the idea that there is further room to theorize the conditions under which particular mechanisms operate, it strikes me that there are also opportunities to identify (possible) affinities between specific contextual or network/structural characteristics and particular mechanisms. In the opening chapter, Padgett and Powell briefly note that certain network structures might be more vulnerable to change than other structures, but they do not take the next step and consider how types of structural vulnerability might intersect with particular types of mechanisms. The closest Padgett and Powell come to explicitly linking network structure with a specific mechanism of origin is in chapters 9 and 10, which document transformations in the Communist party in Russia and China. In each instance, the crucial network feature is a dual hierarchy that facilitates the process of purge and subsequent mass mobilization. Yet it seems there is great potential for further development of the relationship between other network features and mechanisms of change.

Returning to the issue of how symbolic goods play a role in emergence, I found the book's emphasis on categorization to be particularly significant though still somewhat underdeveloped. It is well recognized that in relatively stable systems, shared approaches to categorization and classification are crucial for regularly getting things done (for instance, overlapping categorization schemes allow actors to find trading partners). In chemical systems, producing shared categorization schemes is relatively unproblematic since physical structures of molecules dominate. Yet classification and categorization are more complex in social systems where they involve cognition and language, phenomena that are less disciplined by material demands than in chemistry. Subtle (or not-so-subtle) shifts in classificatory rules within a population of actors may shift the value of particular inputs (or outputs), a mechanism that may well turn out to be the link between actors making relations and relations making actors. When commonly accepted categorization breaks down – often through endogenous drift or collision with other networks – the emergence of new forms is more likely. This process is nicely demonstrated in the series of chapters about the emergence of the biotech field, where resolution of classificatory incoherence differentiated regions in which biotech emerged from those where it did not. Yet because a key difference between chemical reproduction and social reproduction is symbolic language, it is imperative that we focus our microscopes on how symbolic shifts occur, and when they have transformative capacity.

Finally, I would like to briefly discuss the relationship between Padgett and Powell's project and the analytical sociology work spearheaded by Peter Hedström. Both of these approaches rely on mechanisms and agent-based models, but with vary different orientations. For Hedström and his followers, mechanisms are by definition specified at the micro/individual level, whereas the mechanisms identified by Padgett and Powell operate at the network, or meso-level. This makes sense, as analytical sociology tends to embrace the methodologically individualistic contention that explanatory accounts must make sense in terms of individuals' motivations. And yet if actors (and, presumably, their motivations) are fungible, then insisting on anchoring causality in actors' motivation may miss the important action.

Similarly, the role of agent-based models differs greatly between these two approaches. Whereas Padgett builds small and highly stylized models that emphasize the consequences of structure and interaction rules, Hedström's newer efforts at agent-based modeling rely on population-level registration data that contains variable-like data on masses of individuals. At its best, this latter approach allows analysts to describe mechanisms that are consistent with macro-level patterns, though it sheds little light on how the mechanism operates – let alone why one social arrangement might break down or be replaced by another. Padgett's work, in contrast, follows the model put forth famously in Schelling's tipping model (and further developed in the complex systems world), whereby analysis of the dynamics of a simple interaction model can yield great insight on the emergence of new and stable patterns.

I view this as an important book that offers a needed corrective to the variable/attribute centered approach that dominates much of American sociology. That said, I think that the long-term impact of this book depends on the extent to which others find ways to extract and develop some of the powerful ideas embedded within the dense pages.

While the differences in approach are striking, I worry that both rest on a laundry list of mechanisms generated in a rather ad hoc way from case study. Very little attention is paid to how mechanisms relate to one another, when a particular mechanism will become operative, or if there are key organizing principles (e.g., balance theory, hierarchy, or status orderings) that underlie the stabilizing or transformative effects of the family of mechanisms. In terms of the utility of agent-based models, there is great debate about how data intensive should agent-based models be. I am not convinced that models need be so rooted in detailed registration data, but I do think that while working within the complex systems framework it is imperative that all model objects be well specified, and that the number of moving parts be tightly coupled to either theory or an empirical puzzle.

In summary, I view this as an important book that offers a needed corrective to the variable/attribute centered approach that dominates much of American sociology. That said, I think that the long-term impact of this book depends on the extent to which others find ways to extract and develop some of the powerful ideas embedded within the dense pages. Luckily, there have been several engaging discussions of the book already, which provide an excellent resource for those seeking entrée into Padgett and Powell's way of thinking about organizational change. In order for these ideas to move beyond the 'trust me' phase, we need to focus on how to consistently operationalize the many concepts introduced here, and on how to measure relevant quantities precisely. For students looking for dissertations, I see great payoff in projects that will empirically evaluate some of the book's core insights across multiple contexts.

Comments on *The Emergence of Organizations and Markets*

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Reading this book, I was taken back to my junior year in college when I had organic chemistry in the mornings, one of the required classes for premed students. In the afternoons I sat in classes for my sociology major, including a complex organizations seminar where I read for the first time Dimaggio and Powell (1983) and Padgett and Ansell (1993). The tug-of-war for my attention was no contest. Isomorphism and Florentine political intrigue pulled me over to their side with little resistance, and I subsequently dropped the awful idea of becoming a medical doctor and tossed organic chemistry aside. And so here I am a few years later, reading a book by two of the scholars who lured me away from the natural sciences and suddenly I'm in the world of chemistry again. I came over to their side to get away from chemistry and somehow it found me again!

Holding the authors in such high esteem, I approached this book and the criticisms I will make of it with a bit of trepidation. As I see it, this book is the product of careers' worth of thought, theorizing, and painstaking analysis. Padgett, Powell, and their collaborators deserve praise for producing a big book at a time when we see fewer and fewer books such as this in sociological research. And I mean "big" in both a figurative and literal sense. Anyone who has had to tote this densely-packed book along with them on summer road trips, like me, will know just what I mean. But it's also a book that grapples with big ideas – perhaps the biggest problem that faces organizational and political sociologists.

Most of our theories are quite good at predicting/explaining stability and reproduction, but the real mystery is where novelty comes from. Why and when do new organizational forms emerge? How do new institutional arrangements get created? The real strength of the book is reorienting our gaze to the early stage processes of organizational and institutional genesis – when new forms are created through recombination and the transformation of relations between actors. Despite the big question, the answer they provide is elegant. Individuals and organizations can be quite cognitively/ culturally simple and still produce technological and organizational complexity *due to simple rule and role switching across multiple networks and accessing rich environments that sustain multiple skill combinations*. This view takes much of the invention out of the hands of the actors and into the process through which structural folds in overlapping roles/domains lead to recombinations and transformation of the nodes in a network.

The other strength of the book is the rich collection of case studies and the empirical diversity of those studies. Padgett, Powell, and their co-conspirators take us all over the globe and to different historical time periods to observe transformative moments. Examples of organizational genesis include the birth of Tuscan merchant banks out of Roman Catholic Church organization, the creation of the joint stock company during the Dutch revolution, the transformation of markets in post-Communist Russia, and the creation of hybrid life science joint ventures out of the university. If you thought you were simply getting a theoretical overview with no additional empirical analysis in this book, you thought wrong. The book's chapters are detailed and precise in their analytic approaches, assembling data in elaborate graphics, tables, and charts to illustrate the relational and organizational transformations at the heart of their stories. It's really a beautiful book to look at.

Now, let's turn to what I see as the major weaknesses of the book, the biggest of which is the analogy upon which the theoretical framework of the book is based. The book turns to chemistry for analogies to understand social life. This is an attempt to distance us from biological analogies that emphasize competition and selection but that do not offer much guidance in understanding the process of speciation, i.e., creating novel forms. The key concept is autocatalysis – the idea that change occurs through a self-sustaining process of reactions among nodes in a network. In chemistry, autocatalysis applies to chain reactions among elements that come into contact, leading to changes in the very product that was a part of the initial chain reaction. In social life, Padgett and Powell define autocatalysis as the transformation that occurs to all nodes in a network due to changes among certain nodes in the set. Usually autocatalytic networks are characterized by self-repair but in certain situations, where nodes overlap with other networks or where nodes are put to new uses, autocatalysis leads to the transformation of the entire set and consequently to the birth of a new social life form.

As social scientists we often use analogies from biology or chemistry to clarify and to focus our attention on processes and dynamics that would otherwise go unobserved. I asked myself two questions as I read this book: 1) is this a useful analogy for clarifying the creation of novelty? And 2) can we create from this analogy a more general theoretical framework about the origin of social life/novelty? I'm skeptical that the analogy of autocatalysis does either very well.

The analogy doesn't clarify. Instead it obscures the very processes they seek to understand. Once we get past the initial definition of autocatalysis, the book introduces a flurry of concepts, only a few of which seem directly tied to autocatalysis: structural folding, transposing, migration, and of course the more common concepts from social network analysis. If you've followed the works of these authors, or that of David Stark, you're probably already familiar with many of these concepts. The analogy of autocatalysis bears a heavy burden in trying to unify all of these concepts in an overarching framework. In all of the cases, especially the empirical chapters about biotech and life science firms, it was not apparent what value the analogy added. At times I felt like I was reading two books, and perhaps this reflects some tension in the writing process as well. The first book takes autocatalysis quite seriously and tries to theorize it as an actual process that we can observe directly in social life, and the other book is really interested in the mechanisms whereby novelty emerges. In these mechanisms-focused chapters, the concept of autocatalysis seems almost copied-and-pasted into arguments, rather than being their source of argumentation. Perhaps this added-on appearance reflects a more fundamental problem with the analogy. We don't need it, and it gets in the way of the analyses themselves.

Second, does autocatalysis generate new theoretical expectations or mechanisms for understanding the emergence of social life? I would say that it does not. Once we move down a level of analysis to the actions of the nodes themselves, the language of chemistry becomes pretty useless. One reason for this is that humans are not chemical elements; they are thinking, feeling actors. Autocatalysis does not generate a particular hypothesis about when nodes transform and when they do not. We need something more to explain why and when to expect novelty. And this is where mechanisms come in. The book lists eight of them, but there is no reason to think that we should be limited to just eight. As I understand it, mechanisms provide a way to bring energy to autocatalyzing systems. They are the node-level actions that inject a system in stasis with new energy that leads to transformation across nodes. To the point of the book, mechanisms are where genesis and novelty creation occurs. But the mechanisms don't follow logically from autocatalysis; rather, they are unique to empirical situations and vary by context. Some, like robust action, are derived from the existing literature on organizational genesis and others the authors arrive at inductively. The mechanisms end up being the primary causal explanations of novelty in their narratives. The problem with relying on mechanisms is that it doesn't really add up to a theory. Is a theory based on mechanisms really a theory at all?

I was struck throughout the book with the similarities to Charles Tilly's project of explaining change in political actors. Like Tilly, Padgett and Powell draw on network analysis to explain how identities transformed over time, leading to new kinds of actors and action repertoires. We can see some similarities to his accounts of the creation of new types of political actors – e.g., revolutionaries turned statesmen. Like Padgett's story about the creation of new types of economic exchange and politics in Florentine markets, Tilly saw similar changes in political repertoires in both Great Britain and France and claimed that they were the result of reconfiguring relationships into new network forms.

At the end of his career, Tilly weaned himself from overly-structural theoretical arguments about changes in actions, which led to his embracing of mechanisms. Tilly's *Dynamics of Contention* book with McAdam and Tarrow (2001) is illustrative of this approach. I would offer the same criticism of Padgett and Powell's book that many people at the time made of the dynamics of contention approach. Although identifying mechanisms is important to theory development, they are not by themselves a theory of anything, especially when they emerge inductively from the examination of historical case studies. Each historical case study seems to require a different set of mechanisms to explain how/why autocatalysis happened. Mechanisms may be universal but they are apparently limitless in number. How can we create a real theory of autocatalysis when it occurs through so many pathways or is contingent on so many different mechanisms?

A more fruitful approach, perhaps, would be to begin with a different set of premises. It is possible that we could arrive at the same mechanisms if we started with a bottom-up theory of novelty creation that took more seriously the human mind, motivations, interests, and struggles for power and status. I was surprised at how many times, as I read their chapters, these sorts of issues lingered under the surface. This more bottom-up, human approach wouldn't necessarily neglect the role of relations, but rather it would put the actor more squarely in the middle of creating and reconfiguring those relations.

Autocatalysis is attractive because it allows for the possibility of individual actors as an element in change and stabilization processes, but without having to carry over any of the baggage of psychology, decision-making, or emotion that distinguishes human actors from chemical compounds. Nevertheless, inevitably when we begin reconstructing stories about how a particular historical case unfolded we can't resist returning to the human-like properties that actors in these stories exhibit and inevitably shed some of the uncomfortable stiffness of the chemical analogy. For example, consider the mechanisms of refunctionality, conflict displacement, and incorporation. All of them depend to a certain degree on the calculations and motivations of the actors involved, the need to consolidate power and to maintain one's status position. The mechanisms derive from human and collective motivations to dominate, or at least to not be dominated by another group. The mechanisms do not derive from the process of autocatalysis as much as they are the transforming energy that ignites a change in a set of nodes. But without an understanding of the psychology and group dynamics of the nodes, you would never understand why in these situations, the nodes (read: humans) chose the particular strategy of action that they did.

I would like to take the theoretical machinery from this book as it describes actors as concatenations and retheorize it from the bottom up. On a more micro-level, I think there is more to be gained from incorporating the human mindset and passions into the creation of novelty. Consider the work of literary theorist Harold Bloom (1973; 1975), who I have always considered a sort of network theorist due to his emphasis on relations among literary figures. He argued that novelty stems from misreadings of past works of important literary figures. Misreading involves, first, borrowing from a predecessor – taking an idea that resonated in some way with your own understanding of the world – and second, reappropriating that idea, or willfully misinterpreting it, as a way to set yourself apart from your peers and predecessors.

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Through misreading, authors and poets both build on their literary forbearers but also distinguish themselves from those forebearers, and if the misreading is drastic enough, create something entirely novel. There is no biology or chemistry in this explanation at all, but yet it is squarely focused on how motivations and relations are intertwined and continually transform one another. In some cases, the motivation leads to an intended outcome, but in most cases novelty is an unintended byproduct of a local struggle with one's peers and predecessors.

Let me end by praising the book's emphasis on novelty. To me, creating something novel is at the heart of innovation and ultimately invention. I think one of the biggest takeaways from this book is to challenge us to consider new methods and theories for studying the creation of novelty. Padgett and Powell set us on the right path for uncovering new analytic and methodological tools for understanding this important outcome. Despite my misgivings about the chemical analogy, the weight of this big idea book will make it an influential tome in building a sociological understanding of novelty.

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Response to Critics

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I want to thank Kate, Jim, and Brayden for their thoughtful and thorough remarks, which are much appreciated. We also want to thank the audience, which has turned out in large numbers at 2:30 at the last session on the last day. This is quite gratifying. Now, Kate refers to the book's argument as a network version of Max Weber, and Brayden compares the book to Charles Tilly's efforts in *Contentious Politics* (2007). I am half-tempted to say thank you, and let's all go for a beer. That is very nice company to be in.

One of the questions asked by Brayden, as well as many others, is why chemistry? Why did we turn to chemistry for assistance in thinking about novelty? Can't we use ideas directly from sociology or literary theory? At the outset, fourteen years ago, we did not have our sights set on chemistry. We began a multi-year search reading a wide range of disciplines to see how scholars in different fields thought about the production of novelty. The "we" included John and myself, of course, but many others participated in our workshops at the Santa Fe Institute - Charles Sabel, David Stark, Doug White, Brian Uzzi, Bruce Kogut, Julia Adams, Lis Clemens, and Dan Carpenter, to name only a few. We also included many of our current and former students, and we were fortunate that Walter Fontana, Doug Erwin and Sanjay Jain, fellows at the SFI, joined with us.

There were many possible candidates. We read work in science and technology studies, most notably Peter Galison's powerful *Image and Logic* (1997), and related work on boundary objects. We looked at evolutionary game theory, as well as the so-called new Schumpeterian economics. There were numerous people at Santa Fe interested in power laws and the intersection of physics and computational social science, so that work received our attention. There was also emerging work in evolutionary and developmental biology. As we read these various texts and discussed them at great length, we looked for ideas that were fertile. John and I had a mutual commitment to pico-level historical data, and the close analysis of biographies and careers. For us, biography is a structure-producing mapping. Some of you will notice that the book is dedicated to Harrison White, and some of Harrison's best work drew on polymer chemistry, especially his ideas about wheeling and annealing. So work in chemistry on the origins of life had considerable appeal.

Our core theoretical commitment was to multiple networks. We simply are not the people that most of our theories suggest; people are bundles of different interests and identities, which change at different points in time and in different places. Most social scientists adopt an interest-based or identity-based view of the world. But people are multi-functional concatenations of different roles, which are often conflicting. Roles have interests and roles have identities, but we have to see people as bundles of divergent interests and identities, from which they toggle back and forth. If we see people as mixtures of roles and purposes at different times and spaces, that leads to analyzing multiple networks and their folding, rewiring, and disbanding through time.

...for us, autocatalysis is not chemistry, it is life, and it is fundamentally social. Autocatalysis helps us with our larger theoretical ambition that we are pursuing in our continuing work - a general theory of development that operates at multiple levels and has different rules, speciation, and selection at those different levels.

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Some of you may have noted that the cover of the book is a photograph of a cross-section of fossilized stromatolites. These were bacterial colonies formed 3.8 billion years ago, not long after the earth cooled. Stromatolites were the first life form, and are the earliest physical record we have of the origins of life. They were created out of a unique combination of an acidic ocean, a cooling earth, and mineral formations of serpentine structures from hydrothermal vents, which created a reactive environment where nascent RNA formed and life began. For us, the problem of emergence requires a focus on when flows of different elements intersect. The core question, then, is when do flows of networks become self-reinforcing or self-reproducing? Catalysis makes a project happen faster. Autocatalysis suppresses the noise of the surroundings, and more catalysts are created. This chemical view that we transport into the social world led us to think about how the coupling of roles in one domain reproduces relations in another, and to ask when the breakdown of authority in one domain might trigger change in another.

Kate Stovell asks a very good question, “How do we know when actors are changed by their networks, and how do we study this?” The mantra of the book is, of course, in the short run actors make relations, but in the long run, relations make actors. At the core of this view, which is fundamentally autocatalytic, is the idea of the network construction of persons through their biographies. We are searching for the transformative consequences of the intercalation of different spheres of life. This leads to an entirely different view of networks, not only as pipes and prisms, but as things that do transformational work. In this sense, what we are looking for - biography, politics, culture, social influences, the economy - is what passes through networks. Movement, not variable-centered frozen attributes, but networks through time.

How do we know when actors are changed by their networks? In John’s and my joint work, we had these remarkable moments in which we saw similar events, seven centuries apart, representing this kind of flow. In Renaissance Florence, as families tried to cement relations with rivals, they did so through the exchange of daughters and sons-in-law. We found the same phenomena in the contemporary life sciences, as molecular biology developed in its early days. Senior scientists traded graduate students and post-docs, cementing research programs and particular kinds of approaches. Similarly, we found compelling evidence in archival materials. A wonderful example came in comparing letters of credit from the early 1400s with licensing letters written in the early 1970s. I won’t do the long quotations here, but a short version is illustrative. A Florentine letter typically went, "Mio caro amico, because we have so many friendship, economic, and family ties in common, let me give you this loan as a gift. Perhaps down the road we can even become brothers and form a partnership." A gift here did not mean "free"; it meant business as reciprocal gift-exchange (Padgett and McLean 2011). A comparable letter from the Stanford University Office of Technology Licensing to a Bay Area startup biotech firm would read, “My Dear Colleague, Because of the many scientific and personal relationships between scientists at our university and your company, we do not believe it feasible to license this new recombinant gene technology to you. Instead, we propose to allow you to use it for free, but in the event a new medicine is eventually developed, we would ask for 3% of the royalties from that product” (Colyvas and Powell 2006). In both cases, such letters were very exclusive. A standard business letter would be sent, for example, to an established chemical or pharmaceutical company, asking for an annual payment.

In both Renaissance Florence and the early days of Silicon Valley, the realms of social relations – family and academe – were repurposed into business relations, transforming the business AND eventually flowing back to transform both the family and university science. Seeing these letters side by side, five hundred and fifty years apart, was quite an extraordinary moment. But it is not only multiple network data or archival data that can answer the question of when people are transformed by their network relations. Mario Small's ethnography of day care centers and hair dressers suggests how acquaintances get re-purposed to take on the roles of family members, and in so doing such crossings alter the character of hair dressing salons and day care centers in inner cities (Small 2009).

Jim Mahoney asked about the relationship of this project to my 'orange' book, *The New Institutionalism in Organizational Analysis* (1991), that Paul DiMaggio and I did back in the early 1990s. More generally, many people have asked about the relationship of our work to field theory. In a very important sense, the Powell and DiMaggio book, along with Theda Skocpol's and Peter Evans's, *Bringing the State Back In* (1985), were exemplars for John and me. Both of those books defined a research program, set an agenda for future scholarship, and have had healthy audiences. We aspired to do something comparable. But our new book is quite different from *The New Institutionalism*, and in some respects from field theory as well. The imagery of field theory is very much one of force fields from physics, and it carries a strong sense of alignment. You see this imagery when Bourdieu talks about a social field as like a football field, or the pitch, or when Fligstein and McAdam talk about fields with the analogy of nested Russian dolls. Our project is different, although we appreciate very much the insights from these scholars and they were among the materials we read in our search. (As one illustration, Bourdieu's notion of the habitus, or embodiment, has at its core a social learning model that suggests mastery of a small set of principles. He talks a lot about how skill is inscribed in play, and his image of European football is apt. If these skilled players had to think about what they were doing, it would disrupt the game. That is a beautiful illustration of flow.) Our project is constructivist too, but from the bottom up, not fixed things but things that are changing. We're interested in how micro-level interactions generate a sub-strata that is independent of its micro-origins. So rather than see networks like physical networks, and as fixed, restrictive forces, we want to think in terms of networks of possibilities. The term that Walter Fontana and others at SFI use is evolvability. Thus inconsistencies or cross-purposes are important for us. We are also much more mindful of how much innovation comes from people trying to hang on to what they have. Perhaps I learned this insight from John; it comes from a famous Italian novel by Giuseppe Tomasi di Lampedusa, *The Leopard*, which has a central theme that if we want things to stay the same, we have to change. So we too are constructivists who think about the social construction of persons, of categories of actors, and habits of mind. Rather than seeing domains as set and fixed, and institutions as top-down forces, we follow network flows to point us to which domains are the necessary objects of study.

Jim Mahoney likes our idea about the topology of the possible, but he wants to know what kinds of things can be recombined. That's a great question, one I have spent years thinking about. One way I approach it is to think about what considerations never appear on the table. So if we go back to the 1970s and 1980s and the dawn of the molecular biology revolution that created the biotech industry and the eventual transformation in both the pharmaceutical industry and university science, there were a number of organizational models that you don't see in the historical record. No one talked about turning the university into a factory for mass production of monoclonal antibodies. The older model of Bell Labs - that is of a large firm having an autonomous R&D unit - seems to have become discredited. No

U.S. firms thought very deeply about this. Few hospitals were willing to take on the task of becoming research-driven entities. And at the time, none of the early venture capital firms imagined themselves as incubators. So the creation of the small science-based start-up firm -- with a campus-like atmosphere and some modicum of freedom for scientists to explore, which many of you will recognize as now typical of startups in all fields today, was an unexpected result of amphibious scientists hedging their bets by keeping one foot in the academy and the other in this novel, risky world creating new kinds of companies. Our approach leads us to focus on these amphibians, who travel between different domains, and can reshape extant organizational forms for new purposes. The agenda, for both John and me, is to analyze these rare moments when border-crossings rebound to transform their domains of origin.

Let me move more quickly to several of the other comments.

Jim asked what kinds of agents are best at transposition and refunctionality. Can we develop any hypotheses about when transposition will help organizations meet their goals versus undermine their goals? In current work, Kjersten Whittington and I are trying to think about what kinds of organizations can be anchor tenants, and whether such anchors are always benevolent or whether they can be malevolent. We are also interested in what types of people are likely to be amphibians. In the scientific world we find that there are several kinds, either high-status university scientists, younger foreign scholars educated in the US, or frustrated middle managers in mainstream technology companies. What is common across them is they have very different time horizons than do their peers. I have started a project with Kathia Serrano-Velarde (University of Heidelberg) looking at the flow of academic scientists from computational social science fields into the social media industry. This seems to be a case of transposition and detachment at the same time, as their linkages back to the academy are being severed.

Several of you asked about our list of mechanisms and I take your question to be: by what principle is our list coherent? Is it exhaustive? Here I plead exhaustion rather than exhaustive. These ideas emerged from many, many years of work. Is it a complete list? Of course not. And perhaps it is even too long, as several might be combined. What we are trying for is a way of understanding the various processes by which multiple views can become stapled together, to offer an explanation that is adequate at the level of human meaning. Perhaps people would find the word process more palatable than mechanism, as the latter raises questions about our connection to Peter Hedstrom, James Coleman et al and more instrumental conceptions of human agency.

I want to close with a suggestion for the many younger researchers in the audience. One simple little idea that John and I often emphasize is that we need much more attention to verbs, rather than nouns. Most social science thinks about nouns, fixed things that you can attach a label to. Rather than labeling people, products, or institutions, we want to encourage people to use verbs and ask how these things come into being. Where do categories of thought and categories of actors come from? More attention to flows, we believe, will deepen and enrich social science.

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Response to Critics

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Like Woody, I want to begin by sincerely thanking our three commentators-cum-critics, Brayden King, Jim Mahoney and Kate Stovel, for their engaged and constructive reflection on our work. It is gratifying to see such thoughtful people take ideas seriously and appreciatively, whether or not they agree with our conclusions. All three of them have noted that ours was a "big book" in more than one sense. On the one hand, it is almost 600 pages, oversized with double columns, physically heavy even in paperback because of the care that Princeton University Press put into reproducing our 108 color diagrams. On the second hand, the range of topics covered in our book is almost ridiculous: (a) three chapters on the origins of life on earth, including simple chemistry models by us and others about that process; (b) four chapters on the emergence of capitalism and state formation in Europe, focusing on the cases of Italy, Netherlands and Germany; (c) four chapters on the fall of Communism in the Soviet Union and China, and post-Communist reconstruction in Russia and Hungary; and (d) six chapters on contemporary Silicon Valley, biotechnology and the life sciences. Scott Boorman in his review indeed called our book four books in one. And finally, it is "big" in the sense of trying to develop theory about a phenomenon not much analyzed or even discussed in the social sciences—namely, the emergence of novelty, in particular the emergence of novelty in "actors", be those people, organizations, markets or states. The task assigned to our three reviewers, in other words, was not a simple or an easy one. They deeply deserve the thanks they receive from Woody and me.

The comments of the three critics are not the same, but they overlap and are compatible in many ways. Rather than create redundancy by replying to each of the critics separately, I will proceed in my response by abstracting four questions that I think they all share, even though they emphasize different ones: (1) Why chemistry?; (2) Where is agency?; (3) Where is culture?; and (4) How to turn all this into researchable normal science? My reply will be organized into these categories.

Why Chemistry? (the question most emphasized by Brayden King)

Chemistry—and in particular the chemistry-based idea of autocatalysis—is used in this book in four ways: as a metaphor, as a formal model, as one-half of the answer to the question of the emergence of novelty, and as a theoretical framework for organizing our empirical work on historically dynamic networks and biographies.

As metaphor, I would insist that the contribution of "chemistry" to our book is profound: it deconstructs apparently solid objects into reproductive flows. In my talks, but not in the book, I often use the example of my nose. To me my nose appears solid and stable enough. But to a chemist my nose wasn't there a few years ago. Every cell and atom in it has died and been flushed in that time, only to be replaced and reconstructed afresh by new cells and atoms. Why does my nose seem the same in spite of the underlying chemical reality of its continual flux? Because it is an autocatalytic system, that's why, whose nodes in interaction (and not only nodes within the nose) reproduce the nodes. Autocatalysis is the chemical definition of life.[1]

Like chemists, we recommend that social structures be conceptualized processually as regenerative vortexes through time. In saying this, we are saying nothing more than that social systems are a form of life and should be recognized as such. Of course, social systems are more complicated in all sorts of ways than amoeba. We are not denying that obvious truism.[2] But at the existential level of understanding why social systems exist at all, it is more insightful as a first cut (I claim) to contemplate what we have in common with lowly amoeba that to fixate egotistically on how "superior" we like to think of ourselves as being.

More narrowly on the point of understanding novelty, a number of our critics have pointed out that autocatalysis by itself is insufficient for explaining novelty, even in our own empirical cases. That observation is correct, but that is not our argument. Our argument is that autocatalysis and multiple networks together are necessary to understand the emergence of novelty. Neither alone is sufficient; both, working together, are necessary. In our theory and in all of our cases, novelty at the level of invention[3] is produced by transpositions and recombinations of multiple networks. "Evolution" in our framework is not the recombination and selection of genes (or pseudo-genes like "memes"), as it would be in sociobiology. It is the recombination and selection of networks[4] —more specifically of the relational practices that comprise and generate networks. Where does autocatalysis fit into this multiple-network story? Multiple networks in the traditional SNA approach are too static; there is no motor driving reproduction, much less evolution, in an exclusively topological analysis. For us autocatalysis is that requisite motor. "Multiple networks" for us are coarse-grained representations of multiple autocatalytic systems, which overlay and interpenetrate one another. (Perhaps more specifically, networks are the historical residues or "reifications" of prior autocatalyses that have been inscribed into the "memory" of the present.) Therefore when we say "transposition and recombination of multiple networks," that is just our short-hand way of saying "transposition and recombination of multiple autocatalytic systems." The fact that each autocatalysis by itself leads to reproduction and stability, not to novelty, explains why the combination of internally self-regulating systems, when they become forced into contradiction or ambiguity through permutation ("historical contingency") frequently generate episodic or punctuated change[5] —just as Stephen Jay Gould argued long ago.

I appreciate Kate Stovel mentioning my formal agent-based models of production autocatalysis in chapter 3 of the book. Not too many sociologists are going to zero in on that. I take as a great compliment her comparison of my models of autocatalysis to the tipping model of Schelling, for indeed, quite similar to Schelling, my motivation for modeling is not to mimic reality, which for me means Florence—a goal I eschew because I know too much about Florence to insult her like that. Rather the purpose of modeling is to develop stylized logic machines that are capable of generating implications that were not intuitively obvious to their author. Examples in that chapter were my models' conclusions/hypotheses about the evolution of altruism as autocatalytic repair and about the impact of stigmergy (feedback between social networks and the physical environment) on the evolution of selfishness.

For present purposes, the most pertinent derivation from those models was that autocatalysis itself evolves toward multiple networks as chemistries become more complicated (namely, as transformational interaction possibilities increase). Out of a primordial soup of increasingly diverse interactions, multiple overlapping autocatalytric systems (a.k.a. multiple networks) emerged and differentiated in my agent-based models, even as they overlaid each other and stayed linked at multiple junctures. Perhaps others before me have concluded this in different language, but I would like to be remembered in part as someone who derived Durkheim's "differentiation of domains" simply out of chemistry.[6]

Brayden asks "why chemistry? why not literary theory?" or some other "more human" version of social constructivism. The comparative advantage of chemistry as a metaphor is that it immediately grants one access to a powerful and deep set of findings and models, at the cutting edge of science today, which one can use to help develop testable hypotheses about generative process and (evo-devo network style) evolution. But in no way am I opposed to literary theory. If literary theory can deliver payoffs like that, I say "bring it on." Pragmatically I am all ears; insights can come from anywhere. The problem in the social sciences is simply that I don't see many (any?) tools for addressing (or even asking?) Woody's and my core question about the emergence of novelty. Hence one is forced farther afield, like chemistry or literary theory. Until literary theory comes through to deliver the empirical bacon, however, I will continue to plumb for insight potential homologues between biochemical processes of classification and hybridity and social-science processes of cognition and multivocality.

To study novelty within the conceptual frame of life is to yank our individualistic minds out of their naturally egocentric gestalts toward the larger chain reactions of (transformational) flows into which all of our (heterogeneous) minds are linked.

Where is agency? (the most common question I have received from many, many sources)

Our answer to this question is always our mantra: *In the short run, actors create relations; in the long run, relations create actors.*[7] In other words, in any short-term time frame where individual actors can be presumed to stay fixed, Powell and I are methodological individualists—albeit more of Simon's "bounded rationality" variety. Since most of the social-science literature is methodological individualism, however, we choose to emphasize the longer-term side of this inter-temporal feedback across multiple time scales, where our theory is more original. To study novelty within the conceptual frame of life is to yank our individualistic minds out of their naturally egocentric gestalts toward the larger chain reactions of (transformational) flows into which all of our (heterogeneous) minds are linked. Our empirical case studies are littered with people who made a difference—Stalin, Mao, Bismarck, Cosimo de Medici, Deng Xiaoping, even Pope Urban IV (you've never heard of this last guy, but I guarantee that he too made a difference). Some might even say that our case selection is in fact biased toward "Great Men." To lower one's voice and intone the chant of AGENCY, however, is to completely miss the central point of our case studies. No matter how shrewd these historically important actors were—and unquestionably all of them were as smart as they come—the complexity of the systems in which they were enmeshed vastly exceeded their comprehension, much less their control.[8] For every success we can cite in their biographies, we can and do cite failures.

Two points are crucial in all of our case studies: (1) The consequentiality of "agency" lays not at the node of action/choice but downstream in the chain of reactions that unfolded from that choice. In our cases, the particular feature that over and over again made these chain-reactions both consequential and unpredictable at the same time was the catalysis of new interests and actors downstream, nonexistent at the moment of choice.[9] (2) The historical sources of any real actor's "agency"—that is, of any real actor's motivations, alternatives, and cognitive conceptions—do not come from our own imaginaries as analysts. They come from that person's learning within his or her own biography. Since that person's biography was constructed in turn by the social networks that reproduced through him or her, the history of the evolving system is itself inscribed into the micro as well as macro forces of its own transformation. All pieces for novelty and change are there in the path-dependent present; the almost unfathomable trick is how do they fit together, feedback, recombine, and tip through their interdependence.

Thus I respond to Brayden's plea for holding on to the human in the following way: You misunderstand us in thinking that we wish to abolish the human, turning everything into chemistry instead. That is far too literal a reading of what we are up to. In fact we want not to eliminate agency at all but to endogenize actors—by situating their emergence and evolution within learning from their own histories (both macro and micro). In other words, we want to open up the solid-object black box of agency, to look inside and to see how its components are moving through time, thereby constructing the "objects" we call actors, both at the time scale of biographical time and at the time scale of historical time. History is not separate from individuals; history works through and within individuals.[10]

What does this theoretical perspective imply for our particular operationalization of agency? Consistent with our theory of three types of socially distributed autocatalysis[11] flowing through people, thereby bringing them to life, our "actors" are conceptualized as composite sets of practices of three types: (a) production rules or skills, (b) relational protocols of how to form ties, and (c) linguistic-cum-cognitive categories or symbols. The main things left out of this characterization are purposes or goals. We make two points about that addendum—(1) goals are features of roles, not of individuals,[12] and (2) goals are our conscious (and thereby our limited) perceptions of the paths we are on.

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Thus at the most micro level we are "practice theorists"—in alliance, as far as micro foundations go, with Bourdieu and with Dewey-esque pragmatists. Our main complaints about these fellow travelers are that Bourdieu is too top-down when he turns to causality[13] and that pragmatists are so transfixed by creativity and flux as to be inattentive to macro stability. Were these weaknesses to be fixed, however, there would be much room for fruitful dialogue, which we welcome, between them and our own network autocatalytic approach.

Where is culture? (the question most emphasized by Katherine Stovel)

On this criticism, I mostly plead guilty. "Linguistic autocatalysis" is how our framework conceptualizes that multivalent (to the point of being vague) word "culture." This way of approaching culture emphasizes the living reproduction and reconstruction of words through conversation and action, and implies considerable fluidity and lability of language (and by implication conscious cognition) in active use. At the level of theory, in other words, we are open, not closed, to the topic of culture—especially when that can be represented empirically by semantic networks that can evolve.[14]

The reason for the relative lack of delivery, in the Padgett and Powell book, on this side of our theory is that linguistic change was not empirically observed to be an important causal driver in any of our case examples of organizational emergence, no matter how frequently linguistic change appeared as a lagging correlate.[15] In our cases, transposition and recombinations of biographies consistently seemed to be more consequential for organizational emergence than did transposition and recombinations of words.

That does not mean that other cases could not be found that illustrate better the leading, not the lagging, causal role of linguistic autocatalysis. Bill Sewell in particular has been persuasive in tracing the causal impact of linguistic autocatalysis[16] in driving the French Revolution. We simply need more cases like that to help us better to make the connection between linguistic autocatalysis and production and biographical autocatalyses. In the meantime, I have an agent-based-modeling project (with Jon Atwell at the University of Michigan) to model and explore the early evolution of communication and language—mostly at the level of social insects and animals—within autocatalysis models of production and biography. I welcome collaboration on this important outstanding issue.

How to turn all this into researchable normal science? (the question raised mostly by Jim Mahoney)

In the empirical cases in the volume, eight cross-network mechanisms of organizational genesis were discovered inductively: (1) transposition and refunctionality [Renaissance Florence and contemporary biotech], (2) anchoring diversity [life-science industrial districts], (3) incorporation and detachment [medieval Tuscany], (4) migration and homology [early-modern Netherlands], (5) conflict displacement and dual inclusion [nineteenth-century Germany], (6) purge and mass mobilization [Communist Soviet Union and China], (7) privatization and business groups [post-communist Russia and Hungary], and (8) robust action and multivocality [Cosimo de' Medici and Deng Xiaoping]. Jim Mahoney found this to be the best and most useful part of the book; Brayden King complained that a list of mechanisms does not a theory make.

Mostly Mahoney urges us to take the next normal-science steps. Understandably he wants to know when our various organizational-genesis mechanisms are more likely to be employed. And understandably he wants to know what the transformational consequences of those mechanisms are likely to be under various circumstances. Woody and I can't argue with these reasonable questions, because in fact they are also our own questions to ourselves. The challenge is that we don't yet know all of the answers. Our hope is that Padgett and Powell will not be alone in searching for these answers. Others, with different application domains in mind, are more than welcome to join us in parallel research to try to find the answers.

In lieu of answering Jim's questions as directly as he would like, I will confine myself here to specifying the outlines of what an "answer" would look like within the autocatalytic-network framework.

The first complication in analyzing open-ended evolving systems is scientifically to define what 'prediction' means in the study of historically contingent processes. Physicists and economists for the most part understand prediction to mean "convergence to equilibrium"—although the best of them recognize multiple equilibria and hence indeterminacy in their theories. "Convergence to equilibrium" will not do, however, for analyzing open-ended evolving systems where the rules for interaction change, because equilibria are calculated by iterating fixed behavioral and (especially) interaction rules. I don't want to go into an elaborate philosophy-of-science detour at this point, but I argue and hopefully demonstrate in the book (especially in chapter 9) that the best that scientific theories of open-ended evolution can ever do is to understand/derive the "trajectory space" of finite potential futures latent in a structure, rather than to predict exactly which historical path a social or a biological system will "choose."

Darwin thought similarly: his image of history was a branching bush. Given the complexity, contingency and stochasticity of actual history, Darwin never fooled himself into predicting that this critter or that would evolve. Understanding the structure of the branching bush was enough for him—which was good enough for him to change the scientific world.

How can our theory move toward our own goal of predicting or more modestly postdicting evolutionary trajectories (roads available), even if not of predicting actual histories (road taken)?

Compared with comparable discussions of speciation and organismal novelty that you can find in the evolutionary biology literature, the distinctive contribution of our own social-science-inspired approach is "multiple networks." In discussions with my biology and chemistry colleagues, multiple networks are what they find interesting and new—not autocatalysis, which they know already (what is new to them is old to us, and vice versa). All of the "organizational genesis mechanisms" alluded to by Jim are various processes of combining multiple preexisting social networks into something relationally new. Given this, the three moving parts in our theory are (a) "multiple preexisting social networks" (analogous to initial conditions, or to probabilistically predisposing independent variables or "IVs"); (b) "processes of combining" networks (the causal dynamic or motor); and (c) "relationally new" (the dependent variable or "DV"). I will discuss each of these in turn, starting with the DV.

(a) "DV": Relationally new. Ultimately defining an organizational case (or any type of case) as "novel" is a matter of historical sensibility and needs to be justified explicitly on those contextual grounds, not in the abstract. However, Powell and I do distinguish between "innovation" and "invention"—the former being a new object in its context, the latter being a new autocatalytic network that produces and reproduces that object. "Innovation" in our view (and more importantly in our cases) derives from transpositions of products, practices, people or language across autocatalytic domains. "Invention" in our view (and more importantly in our cases) derives from tipping from one autocatalytic network to another—often within domains, but occasionally more radically across domains, thereby refiguring those domains. Innovations (like biological mutations) are not really random; they have a "directed evolution" or "topology of the possible" pattern to the stochastic stream of them. This derives from the structure of multiple-network overlay or embeddedness through which they flow. Even if non-random, innovations are "a dime a dozen"; that is, they are voluminous, stochastic, and of high frequency. Sort of like quantum flux in our theory. Important perhaps to the short-term destiny of the carrier of that innovation, they are mere "perturbations" from the long-run perspective of the multiple-network system itself.

Building, testing and extending theory to us means doing careful, historically contextualized, and parallel case studies. An easy and lazy count of "adoption rates" won't do. This is because explanatory theory to us is about dynamic processes and generative mechanisms, not about correlations.If such intellectual labor limits the speed of our own theory's adoption, then so be it. We care more about the long-run anyway.

The real DV in our book is invention—namely, that small number of innovations that changed not just the local site of their use, but the broader topology of "ways things are done" in which they are embedded. Think industry evolution, not product evolution. Spillover, feedback, and tipping are the core network dynamics that need to be documented, to establish that our DV of "invention" has occurred. Having identified and process-traced a candidate "invention," the explanatory task becomes to understand what caused that original innovation to percolate through and to alter the multiple networks that sustain it. It also means to locate a control-group case, which is "close enough" according to some criterion, where nonetheless something different happened.

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(b) "IVs": Multiple Networks. Social network analysis as it is currently practiced was not as helpful to us as an outsider might think. There are the usual sociological criticisms about SNA being "too static" and "too reified." We agree with those criticisms, but feel that our own work and that of others is starting to make those complaints out of date. The weakness I am referring to instead is the focus of contemporary SNA on single networks, not on multiple networks. Ever since Harrison White and his blockmodels left the field, no one seems interested any more in measuring how multiple networks overlay and interpenetrate. SNA today is infatuated with big data and big networks, not with thick data and rich networks. That will make its future progress in the field of history even slower than it is now.

I don't have an immediate solution in mind for this problem with my subfield. But for Woody and I to move in the direction that Jim Mahoney wants us to go, we need better tools for characterizing in a systematic way our IV as well as our DV. Looking to chemistry (in particular to evo-devo) and to their carefully studied metabolic and genetic regulatory networks might once again prove to be a source of inspiration, but perhaps that is asking too much. At the very least they (unlike us) are onto the concept of catalysis, which lies at the heart of the issue of multiple-network intertwining.

(c) Causal motor: "Processes of Combining (and Reproducing)." Our critics are right to say that our eight organizational-genesis mechanisms were inductively derived from our cases, not deductively derived from some abstract model of autocatalysis. That does not mean that we have some rigid epistemological stance against models in favor of history, because we also use formal agent-based models. But it does mean that there is nothing fixed and magic about our number of eight; no doubt more multiple-network recombining or folding mechanisms will be found in the future. And it probably also does mean that even the mechanisms we have found eventually will be shown to be decomposable into more primitive operators that our histories have assembled into the collective "strategies" we see.

Let me defend, however, the value of induction, especially when the scientific goal is to study generative process, not static correlation. I will do so through two example mechanisms taken from my own research—multivocality and robust action, and incorporation and detachment.

Multivocality and robust action: It is true that my first study of Cosimo was a search for theory through narrative, not a "test" of some preexisting theory. That is also true of Obert's and my study of Bismarck in this book. It is also true of my analysis of Deng Xiaoping in this book. It just so happened, however, that these three cases inductively turned out to be members of a family—the "multivocality and robust action" family of organizational genesis. The contents of their histories and the content of their IVs and DVs are radically different, but they were similar in process. All three were cases of brokering or stapling together not just different multiple networks but contradictory multiple networks, more or less at war with one another. Oligarchs and new men in the case of Cosimo; democracy and autocracy in the case of Bismarck; and reform faction and the army in the case of Deng. Previous dynamics in these cases had already demonstrated that simply throwing these multiple-network IVs into the pot was not sufficient to generate anything stable, much less new. The mechanism or process itself of multivocality and robust action was crucial to the outcome—the "DV" details of which were quite different in any case (to wit: Renaissance elite in the case of Cosimo, German federalism in the case of Bismarck, and successful economic reform in the case of Deng.)

The methodological point here is that patient induction and comparison of carefully constructed rich case studies is another route to constructing theory. Potentially, induction is even a more fruitful route than statistical IV-DV correlations if the goal is to understand process and history.

My second example of induction is my other mechanism of *incorporation and detachment*. When I wrote this book (Padgett and Powell 2012), it is true that this mechanism really was just a generalization from a case of one—the case of medieval Tuscan merchant banks. I also did another case study—of early-modern

Amsterdam, where the stock market and joint stock company were invented. These both were not "examples of a preexisting theory" for me; they were just fascinating cases where for sure I could see "organizational invention" going on. I came up with a different tailor-made mechanism for Amsterdam, which I infelicitously labeled "migration and homology." It was not until the plane ride out here yesterday to the ASA, however, that I realized inductively that these two are also members of a processual family. Amsterdam's "migration and homology" really is just "detachment and incorporation," with Tuscany's "incorporation and detachment" sequence reversed. This is because in Amsterdam first there was a religious war (the Dutch Revolt) that detached vast population flows of Protestant merchants from the south of Spanish Netherlands and of Catholic merchants from the north of Spanish Netherlands. And then there was the massive incorporation of Protestant merchants from the south into northern governmental federations like Holland in order to make war through global trading. The unintended result was a brand new organizational form, the joint stock company, which inserted the more advanced mercantile skills and trading networks of the southerners into the regulatory crystallis of the northerners. This shrinks our eight mechanisms into seven, with variants in each family. Let's hope that future research continues this evolution in understanding.

Having just now perceived this homology—of process, not of IVs and DVs—I have much work to do in order to move toward "if, then" generalizations of the type that Jim is asking for. In our rush for scientific rigor, however, let's not forget that the patient inductive comparison of carefully done case studies was as much a part of Darwin's scientific method as was his occasional flash of theoretical insight from Malthus. Research-design courses in our home universities have far to go in teaching our next generation of students, as well as us, how to reason about and how to study causal process inductively, not just how to test pseudo-deductive hypotheses with IVs and DVs. There is no reason that we should prohibit ourselves from opening the black box of causal process to look carefully inside.

Endnotes

1. As discussed in chapter two, cellular enclosure and evolution are sometimes layered onto this chemical baseline definition, to produce more expansive definitions of life. But everyone agrees that chemical autocatalysis is a foundational component in the biochemical definition of life.

2. As far as how we conceptualize social systems to be more complicated than low-level chemical forms of life, we discuss three forms of social autocatalysis: (a) production autocatalysis, where products are reproduced through transformational ("technological") relations among products, (b) biographical autocatalysis, where people (specifically the production and relational practices they carry) are reproduced by social relations among people, and (c) linguistic autocatalysis, where words and other symbols are reproduced through conversational relations among words and symbols. While there might be other things as well, we thereby make the claim that economy, social networks, and language are three prominent examples of social forms of life.

3. See the book for our distinction between innovation and invention. To be simple-minded about it, "innovation" is change in the nodes; "invention" is change in the reproductive networks that construct the nodes. "Dime-a-dozen" innovations either spill over into their surrounding reproducing network to expand into inventions, or they do not, in which case pre-existing autocatalyses mostly select them away (although not entirely if they are incremental enough).

4. A similar move in evolutionary biology to make evolutionary theory more "networky" than the traditional population genetics is called "evo-dev" (i.e., the evolution of development). In biology circles, we are in alliance with evo-devo. The Social Science Research Council recently has created a new Working Group on History, Networks and Evolution, under my chairmanship, to explore commonalities and differences between biological and social-science conceptions of network evolution.

5. Although sometimes of course they can lead to

implosion and collapse, as they did for Gorbachev, unlike Deng. See my chapter 9 in the book for a detailed analysis of how network autocatalytic theory explains the divergent responses of the Soviet Union and China to the same reform program.

6. Perhaps that should not surprise us so much, because even amoeba have "differentiation of domains." This idea, I would argue, is a processual analogue to the more object-oriented concept of modularity. (I duly note, however, that the great Herbert Simon to his lasting credit defined his "nearly decomposably systems" operationalization of "modules" in network and frequency/energy of interaction terms. In spite of his brilliance, Simon missed the implications for the evolution of multifunctionality.)

7. Stovel is right to mention the Harrison White lineage of this mantra. The book is dedicated to Harrison.

8. "[Agents in the book] were part of but did not control the explosive events they stimulated… If 'agency' means induced intent and learning, then fine. But if 'agency' means the capacity to foresee and control complex chains of consequences, then no. Autocatalysis does not deny individual agency; it just endogenizes that as one time scale in life, interpenetrating with others" (Padgett and Powell 2012, 60).

9. This is why in times of turbulence, like our cases, rational choice is stymied: even the set of actors to strategize against has changed.

10. This is not inconsistent with "Coleman's boat." It is just that his hypothesized downward arrow of causation—from macro to micro—is rarely theorized in his rational choice tradition, where the upward arrow—from micro to macro—reigns supreme.

11. See footnote 2.

12. Cosimo de' Medici as ensemble individual didn't want to maximize profit; it was Cosimo de' Medici as banker that wanted to make profit. Likewise, Cosimo as politician wanted power, and Cosimo as father wanted status for his family, not Cosimo as a biological person. When pursuing multiple goals is made consistent by a world that made their multiple outcomes correlated, then it becomes mathematically possible to represent Cosimo "as if" he had a superordinate "utility" function. But when pursuing multiple goals is contradictory, because of zero or even negative correlation in their outcome variables, then cycles and situational switching behaviorally are observed. The assumption of "as if" maximization then becomes mathematically unviable because foundational axioms of von Neumann-Morgenstern utility theory are thereby violated. It is logically impossible to maximize cycles.

13. Questions have been raised about the relationship between Padgett and Powell's [and Durkheim's] "domains" and Bourdieu's "fields." Woody has addressed this already. My two cents are (a) that there is considerable consistency at the micro level in that both autocatalytic networks and fields ultimately are composed of reproducing practices ("habitus" in Bourdieu's terminology); but (b) that Bourdieu's "fields" are too exogenous and top-down in conceptualization, because they are founded on metaphors like "gravitational field" and "soccer field," which require an external force (like the sun or the state) to establish. "Domains as autocatalytic networks," in contrast, are bottom-up and emergent. This is not to say that "institutional logics" have no place in social analysis, but they should appear, it seems to me, at the end of the emergence causal chain, not at the beginning. Regulation kicks in to maintain autocatalysis after emergence has already unfolded. It was the error of functionalism to mistake the (equilibrating) consequence for the (genesis) cause. I would be delighted if social scientists treated "fields" simply as a shorthand for "autocatalytic networks," without all of the Foucault control overtones of "fields."

14. See the book by Paul McLean (2007), for an example of culturally oriented work very compatible indeed with our perspective.

15. "I stand with Harrison White [and with Herbert Simon] in concluding that, our Enlightenment pretensions notwithstanding, mostly we all play interpretive catchup with events, trying to respond to the jaggedness of the unpredictable twists of a vibrant and vast social world far beyond our comprehension" (Padgett and Powell 2012, 61).

16. This is our label, not his, of course. But from our perspective, that was exactly what Sewell was writing about.

17. Not that the latter could not be a useful step toward the former. A statistical estimation equation, no matter how sophisticated, is never itself a theory or even an explanation.

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Book Symposium

Waves of War

Nationalism, State Formation, and Ethnic Exclusion in the Modern World

Cambridge University Press

Andreas Wimmer

Editor's Note: The following text is based on an author-meets-critics session that took place during the American Sociological Association Annual Meeting in San Francisco in August, 2014. My thanks to Jack Goldstone, Mabel Berezin, and Andreas Wimmer for agreeing to write up their comments for the newsletter.

Comments on *Waves of War*

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In this deservedly acclaimed book, Andreas Wimmer has provided a feast of new data and vitally important analyses. In *Waves of War*, we see the completion of a trend away from class-based analysis that has characterized political sociology since the state-centered approach developed in the 1980s and 1990s; the "waves" that propel both state-making and war in this book are driven by ethnic groups seeking to build regimes based on national identities. You might say that Wimmer confirms Marx's worst nightmares about national interests trumping those of class, and thus creating continued conflicts that suspend the progress of working populations.

There are many findings that strike me as both sound and important, and where Wimmer's analysis reinforces conclusions supported by other quantitative and case-based work. One of the most significant is that regime type – whether democracy, anocracy, or dictatorship – is not a strong predictor of political violence; instead it is the rise of political struggles over ethnic power in states that precipitates most civil ethnic wars. This is a result that I strongly endorse, having been part of a multi-year team effort that arrived at essentially the same conclusion: that it is only when elite relations become polarized along ethnic or regional lines that partial democracies become likely sites of major violent conflicts (Goldstone et al. 2010).

I also applaud Wimmer's valuable thoughts on whether peace can be engineered – an issue that is of more than academic interest today as the US prepares to go to war yet again in Iraq to deal with a state that has fractured along ethnic lines. Wimmer is honest and pessimistic. He notes, first, that it is underlying social relations and not formal institutions that determine whether people feel their identities and interests are aligned with their government, and second that those underlying social relations are not easily changed. Wimmer notes that making governments more inclusive – Wimmer's solution for countering the centripetal forces of ethnic nationalism – is often resisted by dominant elites who see only a dilution of their power as a result. Similarly, ethnic groups who have been victimized in the past will rarely trust security and police forces dominated by an ethnic rival, no matter the rules under which they claim to operate. Inclusivity must therefore be pervasive throughout governance, and not merely a result of token inclusion at one level.

Perhaps most troubling but clearly demonstrated is that nationalism – a scourge that brought us two world wars and dozens of genocides and ethnic cleansings – is far from dead, and that it is even invigorated by democratic reforms that force issues of political identity to the fore (a point also made by Mann 2005). In Ukraine, Iraq, Syria, Yemen, Thailand, and many other places local nationalist aspirations clashed with old political boundaries to create a cauldron of competing claims to power and territory. In much of the world it may be many decades before people are able to act according to the liberal ideal of treating everyone as an individual with an equality of citizenship, with the latter including respect, access, and equal treatment under the law and in social relations. Governments, and even more, many social groups, continue to be discriminatory, exclusive, and distrustful of others in their own states.

In my own research, I have found that the percentage of the world's population living in societies that are both fully democratic and materially prosperous has hardly increased in the last 35 years. That is not because there has not been progress, especially in Latin America and eastern Europe. But that progress has been offset by persistent ethnic and religious conflicts, authoritarianism, and recurrent crises and democratic reversals in countries with faster-growing populations, leaving the world divided much as before.

There does, however, seem to be one rather large lacuna in Wimmer's comparative-historical analysis of nation-building and wars. Wimmer has sought to turn around Charles Tilly's famous claim that state-making is tied to wars; Wimmer instead insists that both modern nation-state making and wars are the result of nationalist aspirations clashing with older imperial state forms or with competing nationalisms. Yet the analysis completely overlooks another of Tilly's major topics – the role of revolutions in history.

In pointing out this omission, I am not simply asking for attention to a favorite topic, or to a tangential side issue. Revolutions have played a central role in the history of nationalism and nation-state building. The American, French, and Chinese Revolutions were crucial events in creating American, French, and Chinese nationalism. Indeed, by my count out of the 167 years of national state creation in Wimmer's data (adding the four pre-1800 cases he mentions in the text), 74 – nearly half of all nation-creation years – coincide with revolutions.

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This should not be surprising: Wimmer clearly says (p. 22) that "nation-states are created when a power shift allows nationalists to overthrow or absorb the established regime." "Overthrow" – that is, revolution – is the common mode by which a nationalist movement replaces a traditional imperial regime.

Yet the book has nothing to say about the relationship between ethnic conflict, state formation and revolutions. Astonishing! There is not even a mention of revolutions in the index. It is common enough for books on international relations to treat revolutions as obscure things within the "black box" of nations that can be safely ignored when talking about international war among states. But for a book arguing against that view, and making the case that internal political conflicts over power are critical to the onset of both civil and international wars, it is a very odd omission to neglect the literature on revolutions.

What would Wimmer have learned from bringing in the literature on revolutions (e.g. Skocpol 1979; Goldstone 1991, 2001, 2014; Goodwin 2001; Foran 2005; Selbin 2010)? Three things: First, state-changing politics are coalitional politics – cross-class coalitions are essential to large-scale political change. One reason ethnicity has the power that it does is that it IS an inherently cross-class identity. It thus competes effectively with more liberal civic nationalism, which often appeals mainly to urban or professional groups.

Second, mobilization is conscious and organized. It is not just a matter of a passive response to ethnic or nationalist institutions or interests. Wimmer's data analysis is rich indeed, and the correlations allow him to say that nation-making occurs when power shifts in favor of nationalists, and that such shifts occur when the established regime "is weakened by wars" or "if nationalists have had ample time to decry ethno-political hierarchies as instances of 'alien rule' and to mobilize followers" (p. 23). Yet we see very little mobilization in this book, and nothing about the interplay between nationalist leaders and followers. In Serbia, in Georgia, in Iraq, in Russia (Chechnya), and in China (Tibet) ethnic mobilization resulted from deliberate choices by political leaders to emphasize ethnic grievances and identities as a way to manage conflict and promote their power agendas. Furthermore, we learn nothing about the difference between ethnic mobilization for nation-building and ethnic mobilization for genocide. For Wimmer, Germany becomes a nation-state in 1871, as a result of Bismark's nationalist wars against Austria and France, but – oddly for a book on "nationalism" and "ethnic exclusion" – there is no mention of Nazism, Hitler, or genocide (none of which appear in the index, either). Not all nationalisms, even ethnic nationalisms, are the same.

Third, revolutions depend heavily on ideologies for mobilization and these ideologies affect post-revolutionary reconstruction. In some cases, revolutionary ideologies intentionally seek to replace or obliterate ethnic identities to create new national identities, as with India and Ethiopia and Tanzania and South Africa, or France in 1789 or the United States in 1776 (except for race). Yet in other cases, revolutionary ideologies promote and intensify ethnic identity – as in Serbia, Croatia, Kosovo, Ireland, Chechnya, Tibet, Xinjiang. Again, understanding how ideologies shape revolutions gives much more insight into how various nationalisms evolve and compete with other views of political identity.

Finally, a nit-pick, but a potentially important one. Research findings based on statistical analysis are no better than the data on which they are based. I find the data in many cases quite troubling. Why did the US become a nation-state in 1868, or Russia in 1905? I understand the formal logic here; the 14th amendment extended citizenship to all Americans regardless of race. But it did not give women the vote – if we don't care about women (who Wimmer explicitly sets aside in deciding on nation-state status), why care so much about racial minorities, or even majorities? Most white Americans believe the Constitution of 1789 created a state where sovereign power resided in the nation (We the people) – and that is supposed to be Wimmer's criteria. Similarly for Russia – yes, the Tsar issued a decree that created a Duma in 1905. But it was advisory, elected by a small fraction of the population, and did not change the character of the regime from absolute monarchy at all. It took the revolution of 1917 to do that. South Africa's whites believed that, despite apartheid laws, they had created a nation-state with citizenship rights and popular sovereignty; I don't think they would accept Wimmer's data-coding that South Africa only became a nation-state in 1994 with the post-apartheid constitution. It may have been a deeply flawed nation-state from the viewpoint of ethnic inclusion and civil rights; but those who created and fought for a South-African nationhood out of Boer and British nationalism would say they had created a nation-state far earlier. If we think minority/majority rights matter, then Britain only becomes a nation state after the reform act of 1832, not prior to 1800 as Wimmer would have it. Wimmer has Japan becoming a nation-state in 1868, with the Meiji restoration. But in fact the Meiji constitution that created a nation based on the Japanese people was not adopted until 1889. Prior to that, the Meiji oligarchs, like the Shogun before them, ruled in the name of the Japanese emperor.

China is dated a nation-state from 1911, the date of the Chinese Republican revolution. But after 1915 it was formally a dynastic empire again when Yuan Shikai made himself emperor, then dissolved into warlord rule. Chiang Kai-shek tried to revive the Republic, and arguably created a national state government in China in the 1920s, but he was at war with nearly half his population in the 1940s, eventually losing to the communists who finally established a people's republic in 1949.

Wimmer says that France became a nation-state before 1800, presumably as a result of the French Revolution that killed the King, installed the First Republic, and created a new national administrative and legal system. However, the First and even Second Republics were very short-lived; Napoleon I was a dynastic ruler, installing relatives as monarchs all across Europe and when overthrown was replaced by a return to the absolutist Bourbon monarchy. In 1830, France became a national constitutional monarchy; but that system was overthrown by Napoleon III. Napoleon III was an imperialist who sought to incorporate Algeria into France on equal terms with other territories without regard for French ethnicity; so one should perhaps date the creation (re-creation?) of the French nation-state from the start of the Third Republic in 1871.

Of course, in coding a cross-national data set, as I know from my own experience (Goldstone et al. 2010), one has to make coding rules and stick to them; and correcting these nit-picks might have no impact at all on Wimmer's statistical findings. However, it is a concern that so many of the cases that I know well seem to be miscoded; so it might be helpful to do a sensitivity analysis on some alternative coding rules (e.g. the date that a national language is taught in all schools, or the date that chief executive authority is no longer exercised by dynastic or imperial powers), to ensure the robustness of Wimmer's findings.

Wimmer has provided two great accomplishments: a new data set on ethnic power and state-making, and refocusing our attention on the role of ethnic nationalism in modern state creation. We might wish for a world of smooth democratization and universal human and civil rights; but that is not the world we have now. As Wimmer has shown, and has been shown true by current events in the Middle East and North Africa, we live in a world where ethnic nationalism and ethno-religious identities dominate state-making; and so it has been for the last few centuries.

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Comments on *Waves of War*

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In the last ten years or so, Andreas Wimmer has produced a body of work that ranges across topics from ethnic closure to social networks and reveals a comparative historical sociology that is as broad as it is deep. Wimmer's *Waves of War* (2013) is a particularly notable entry to his corpus of work. My discussion of *Waves of War* moves in three directions. First, my comments acknowledge the accomplishment that *Waves of War* represents and pinpoints where that accomplishment lies. Second, I take up the choice theoretic framework that Wimmer develops in his analysis. Third, a model is only as valuable as its potential application. I conclude by speculating on how social scientists might deploy Wimmer's analysis in future research.

Waves of War aims to re-theorize all of the major components of comparative political sociology—the state, nationalism and war. A bold formulation lies at the core of the book: nationalism is constitutive of modernity and its central political form the nation-state. Other political forms such as empires existed without the like-over-like or "identity" principle that is a core organizing principle of the modern nation-state. The upside of the modern nation-state is that it is an inherently more inclusive form of political organization than the organizational forms that preceded it; the downside of the modern nation-state is that it inherently predisposes towards war—hence the title of the book. Wimmer seeks to explain how and why the nation-state came to dominate modern political organization and how it spread from Western Europe to become a global political form. In short, *Waves of War* seeks to model and explain this "momentous transformation." Wimmer's project is deeply historical and raises questions that point to issues of temporality and sequentiality.

Each chapter of *Waves of War* engages in dialogue with major figures in comparative political sociology. The chapter on how the nation-state came together focuses upon the work of Charles Tilly and constructivist theories of nationalism. Wimmer finds lacunae in four standard analytic accounts of the development and diffusion of the nation-state.

He weighs Ernest Gellner's economic nationalism against the political sociology of Charles Tilly and Michael Mann. He contrasts Benedict Anderson's culturalist account of the growth and diffusion of nationalism with John Meyer's world polity theory. If, as Wimmer suggests, political sociology fails to provide a full account of the development of the nation-state because it does not give proper weight to the role of conflict then the International Relations literature on ethnic closure, violence and war might do a better job of explanation. With the exception of the game theoretic approach to ethnic conflict and war represented in the work of Laitin and Fearon (2003), Wimmer argues that realist and idealist versions of IR theory also fail to adequately explain multiple dimensions of the relation between conflict and political development.

Wimmer's problem is important, the "ethno-nationalization of war," that is the acceleration of conflict on all levels as the nation-state advances. The data sets upon which the book is based are vast, original, and remarkable in themselves. Wimmer's goal in *Waves of War* is to unite state formation, nationalism, ethnicity and war—topics that are empirically related but often treated as analytically separate—in one overarching analytic frame that marshals new data to answer old questions. He is careful to claim that he is not offering a new theory per se—just re-arranging the elements of existing theories so that they perform more analytic work. Wimmer's goal is to identify causal mechanisms that apply to more than one time period—which is the reason for the large data sets that he has constructed—data sets that enables him to treat all data points equally. In short, his data sets control for history and culture.

Wimmer's ambition is large and this demands that we subject his project to questions that are commensurate with this ambition. We have to ask does *Waves of War* succeed on its own terms? Does Wimmer's work complement in useful ways, rather than negate, the competing theories with which he engages?

To begin that assessment, we have to begin with the model that Wimmer develops—particularly the mechanism of political closure embedded in the model. According to Wimmer, the nation-state, unlike more traditional or feudal forms of political organization, is a contract among different competing groups of elites. These elites emerge as states begin to centralize and the degree of state centralization is proportionate to the capacity of a nation state to emerge. Modern states need money (taxes) and military (protection); they need non-elite members to enter a social and political contract with them. Nation-states "buy off" non-elites, the "people" with "public goods" (social welfare) and concurrently develop the like-over-like principle of cultural identity and attachment. To this point, Wimmer's argument bears some similarity to the one that Gianfranco Poggi advances in *The Development of the Modern State* (1979).

Wimmer's account departs from Poggi when he includes the variations that formal modeling mandates. In this account, nation states, and political organizations more generally, represent a negotiated equilibrium between elites and masses with room for variation depending upon how that negotiation plays out. The negotiation has multiple components: first, the four categories of collective actors (i.e., primary and secondary elites and primary and secondary masses); second, the degree of inclusion/exclusion of masses and secondary elites within the polity; third, the centralization of the state; and fourth, the strength of voluntary associations that move the cultural, like-over-like, project forward. These four categories yield three political forms. The first form is the standard modern nation-state (all collective actors included and a strong central state). France, among other countries, fits this model. The second political form is populism (no secondary elites; primary and secondary masses and an ineffective to weak state). Multiple countries in Latin America might fit this model. The last category is ethnic closure (only dominant elites; and primary masses coupled with a weak state). Various Eastern European countries might fit this model. The model is process oriented and answers the how question; but does not answer the why question: what makes the nation-state so attractive that it diffuses widely. Here Wimmer provides a novel answer. Nation-states spread because within diverse political spaces secondary elites (intellectuals, culture producers of various sorts) observe that nation-states work and these secondary elites take on the role of legitimacy entrepreneurs—who promulgate the new political form.

The architecture of *Waves of War* is worth noting. Wimmer shifts between two methodological strategies: first, the model building with its choice analytic mode of argumentation; and second, a more standard explanatory analysis based upon regression models. Wimmer puts his analytic model together brick by brick in a series of chapters that use standard explanatory logic with dependent variables and independent variables in regression equations that test different pieces of the overall analytic model. This is where the massive data sets that Wimmer has assembled come into play as he marshals a different data set for each chapter. These chapters are revealing in and of themselves. For example, one chapter demonstrates that democracy has no direct effect on either nation-state formation or, as I understood it, propensity to engage in war.

Wimmer's model is ultimately choice theoretic and the utility and strength of these types of models is the mechanisms that they reveal. They give us formal tools to apply to specific historical phenomena or events (for example, the discussion of French political development, p. 71). When Wimmer discusses the bargains that elites strike with masses and the political outcomes of these bargains, he is elaborating a formal mechanism that can provide an analytic frame that elucidates multiple contexts. A weakness of analytic models and mechanisms is that they tend to be a-historical and a-temporal, that is they attenuate the effects of context and culture. While *Waves of War* covers

A weakness of analytic models and mechanisms is that they tend to be a-historical and a-temporal, that is they attenuate the effects of context and culture. While Waves of War covers the entire modern period, the internal processes of change and development that contribute to thick cultures and continuities are not part of the analysis. As this is a book of comparative historical political sociology, the absence of history—in the form of context stands out. As the relationship between war, ethnic conflict and nationalism is the core of the book, I kept asking myself what we might learn if we applied this model to Putin and the Ukraine, to Gaza, or to ISIS.

the entire modern period, the internal processes of change and development that contribute to thick cultures and continuities are not part of the analysis. As this is a book of comparative historical political sociology, the absence of history—in the form of context stands out. As the relationship between war, ethnic conflict and nationalism is the core of the book, I kept asking myself what we might learn if we applied this model to Putin and the Ukraine, to Gaza, or to ISIS.

The strength of any analytic approach is its application. Wimmer spends the last pages of the book addressing the issue of globalization and the end of the nation-state—a concept that is much in vogue but which often lacks empirical specification. Nation states will continue (and hence wars) because Wimmer sees no institutional form on the horizon that can structure the kinds of negotiation between elites and masses that formed the core of the nation-state. Wimmer briefly mentions the case of the European Union as an example but only allots two pages to it. It is hard to ask an author who has already delivered such a compelling and meticulously researched book to write more—but Wimmer could have used the European case as a way to nail down his model.

If Wimmer's model is transposable at all, the European Union and its current crisis would be an excellent venue to test it in. The European Union does provide an institutional form but it has been unable to deal collectively with the challenges that the sovereign debt crisis which began in 2010 have posed. The principle response to the crisis has been a retreat to intense feelings of nationalism across the continent, the rise of xenophobic political parties and a refusal among citizens of member states to view each other in solidaristic terms. In the European case, primary and secondary elites have failed to negotiate with secondary masses (workers, persons who do no benefit from a transnational polity). Thus, for the most part, Europe is witnessing a regress to the national model.

Wimmer sees no institutional form on the horizon that might serve a global world in the same way as the nation-state served a more territorially restricted world. In contrast to Wimmer, I do see some global institutional forms on the horizon, although perhaps they are not the ones that we would necessarily welcome. For example, religion is absent from *Waves of War* even though religion has historically been at the core of much political conflict. Religion crosses borders and is institutionalized as Samuel Huntington (1993) has argued. A new political form could emerge that unites culture and economics, as opposed to culture and politics as the nation-state did. In the 2014 summer of Thomas Piketty, one could imagine a world governed solely by global finance through the institution of the market.

In the end, we have to ask does Wimmer succeed on his own terms. The answer is unequivocally—yes. Does *Waves of War* extend in useful ways the theories that it engages? My answer here is somewhat more nuanced. This "critic" will never be happy with the absence of history, narrative and culture in the analysis. I also would have preferred Wimmer to speculate more with his own model and to take it a bit more in a policy direction. Lastly, there is a danger with formal models, even with an analysis as data rich as *Waves of War* offers: that these models remain detached from the realities that they seek to describe.

In any project of this sort there is a direct relation between the level of criticism and the ambition and reach of the book. *Waves of War* is a magisterial accomplishment. It pushes the boundaries of each topic that it engages – topics that dominate the contemporary political landscape. For these reasons, Wimmer's *Waves of War* merits our attention and praise. I learned much from *Waves of War* and you will too.

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Author's Response

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I am deeply grateful to Professors Berezin and Goldstone for their careful reading of the book and their exceedingly generous assessment of its contributions. In an age when journal publishers ask us to summarize our articles in a tweet of 25 characters maximum and when books are sold online chapter by chapter, we cannot take it any longer for granted that our colleagues, even our reviewers, carefully read an entire book—especially a complex and hard-to-read one such as *Waves of War*. Since most of the readers will not be familiar with its content, I take the opportunity to summarize the gist of its argument first[1] —without referring to the multiple datasets and their statistical analysis that form the empirical core of the book.

To explain recent conflicts in countries such as Syria or Sudan, observers have been quick to point their fingers at proximate causes specific to our times: the power vacuum created by the end of the Cold War offered opportunities for rebels to fill the void; the recent globalization of trade flooded the developing world with cheap arms; rising global consumer demand generated new struggles over oil and minerals; jihadist groups spread using networks of fighters trained in Afghanistan and Pakistan.

Waves of War suggests that such explanations miss a bigger picture. If we extend the time horizon beyond the Cold War to include the entire modern period – from the American and French revolutions to today – we can see repeating patterns of war and conflict. These patterns are related to the formation and development of independent nation-states – a fact strangely ignored by mainstream International Relations scholarship that focuses on relationships between independent states, rather than the process and consequence of their emergence. Note that in contrast to Tilly, *Waves of War* is not concerned so much with the war-prone formation of modern, territorial states in early modern Europe, but with their transformation into national states ruled in the name of a people with more or less clearly identified ethnic boundaries and with the spread of this political formation around the world.

Waves of War thus lays the finger on how principles of legitimacy transform over time and with what consequences. Until the eighteenth century, empires, dynastic kingdoms, tribal confederacies, and city-states governed most of the world. This changed when nationalists introduced the notion that every "people" deserved its own government. They argued that ethnic likes should rule over likes. In other words, Slovaks should be governed by Slovaks, not the House of Hapsburg; and Americans by Americans, not the British crown. Over the past two centuries, in wave after wave of nation-state formation, this new principle of political legitimacy transformed the world. Nationalists adopted this principle because it promised them and the population at large a better exchange relationship with the state: an exchange of military support against political participation, of taxation against public goods. Wherever nationalists were powerful enough—mostly independent of global trends or colonial legacies—they overthrew or gradually transformed the old regime and established nation-states based on the like-over-like principle.

In most places, two distinct phases of conflict accompanied this transition. First, violence accompanied the creation of the nation-state itself. Roughly a third of present-day countries have fought violent wars of independence that united, if only temporarily, the diverse inhabitants of colonial or imperial provinces against their overlords. Second, many of the resulting nation-states endured even worse violence after independence was won because the like-over-like principle bred further conflict. Imperial governments had often recruited members of specific minorities into the colonial army and bureaucracy. (The classic example was the Belgian preference for Rwanda's Tutsi minority over its Hutu majority to staff the country's colonial administration.) In other former imperial dependencies, the elites of the more assimilated and educated groups controlled the post-imperial state's nascent bureaucracies and security apparatuses, a fact that other groups began to resent as a break with the like-over-like principle that was now firmly established as the new template of legitimacy. More important, many new governments lacked the political power and resources to reach out to the entire population and overcome the political inequalities inherited from the imperial past. This made nation building more difficult and ethnic patronage more likely. Large segments of the population thus remained politically marginalized.

> ***In short,* Waves of War *shows that the spread of the like-over-like principle and the formation of nation-states have been driving forces behind civil and interstate war...***

Whatever its origins, ethnopolitical inequality was perceived as a scandal once nationalism had been accepted as the guiding principle of legitimacy. This made it easier for opposition leaders to mobilize followers and stage armed rebellions against exclusionary regimes. Data from every country in the world since 1945 demonstrates a tight correlation between such inequality and conflict: an increase in the size of the politically excluded population by 30 percent increased the chances of civil war by 25 percent. Almost 40 percent of independent countries today have experienced at least one ethnopolitical rebellion since World War II. It is important to note that these countries are not more ethnically diverse than those at peace. It is therefore not diversity per se, *Waves of War* shows, but political inequality, that breeds conflict.

New nation-states are also more likely to go to war with each other than established empires or dynastic states were. Empires drew loose and often arbitrary borders with little regard to ethnicity. Nation-states, on the other hand, care about borders because these may divide a single national group across various states. This creates the risk that those who end up on the wrong side of the border are treated as second-class citizens in neighboring states dominated by other ethnic groups - another way that the like-over-like principle can be violated. Conflict between neighboring nation-states thus often erupts over territories where ethnic groups overlap or over borders that divide a single ethnic group. In the early 1990s, for example, the Serbian minority resisted integration into the newly founded state of Croatia. The government of Serbia, expecting that their co-ethnics in Croatia would be mistreated (and in pursuit of its own national unification project), intervened on their behalf. War between the two states followed, ending with the expulsion of the Croatian Serbs across the border.

In short, *Waves of War* shows that the spread of the like-over-like principle and the formation of nation-states have been driving forces behind civil and interstate war - a fact woefully absent from much of the literatures on civil and international wars, which remain focused on political economy mechanisms such as the economic incentives for rebels or the military-economic balance of power between independent states.

Goldstone notes the absence, in the narrative summarized so far, of an appropriate role for national revolutions, which often accompany the transition to the nation-state. This is an important point. Indeed, as he notes, many of the transitions have been brought about by revolutionary upheavals. I would go further and argue that even where the transition to the nation-state occurred gradually and in a negotiated and agreed manner, such as in Sweden or Botswana, the result is a profound re-configuration of the power structure, brought about by the new cross-class alliances that Goldstone emphasizes. In this broad understanding of "revolution", almost every transition to the nation-state is revolutionary—and the book is indeed about the causes and consequences of the national revolution, broadly defined, around the world. If we define revolution more narrowly, as implying resistance by the old regime and some collective mobilization (street protests, guerilla warfare, and so on) to overcome it, then it remains to be seen whether they do have consequences that are qualitatively different from non-revolutionary shifts in the power configuration. It would be easy to test—one would have to code every transition to the nation-state as either revolutionary or not (or a "degree of revolutionness") and then see whether this has consequences either for the subsequent power structure of for war proneness or both.

The international relations literature contains some hints that this might be the case for inter-state wars. Walt (1992) highlighted a possible link between a revolutionary change in the domestic power configuration and the possibility of interstate war (see also Maoz, 1989). He offered a classical neorealist argument, according to which "revolutions cause war by increasing the level of threat between the revolutionary state and its rivals and by encouraging both sides to view the use of force as an effective way to eliminate the threat" (Walt 1992, pp. 322–23). More recently,

...even where the transition to the nation-state occurred gradually and in a negotiated and agreed manner, such as in Sweden or Botswana, the result is a profound re-configuration of the power structure, brought about by the new cross-class alliances that Goldstone emphasizes. In this broad understanding of "revolution", almost every transition to the nation-state is revolutionary — and the book is indeed about the causes and consequences of the national revolution, broadly defined, around the world.

Colgan (2013) has argued that revolutions lead to international war because the leaders emerging from revolutionary turmoil are inherently less conflict averse and more politically ambitious.

From the point of view of *Waves of War*, I would argue that the threat to neighboring states' security (as argued by Walt) would be particularly pronounced if the revolutionary state emerges from a nationalist upheaval because a nationalist regime within an imperial or dynastic environment will often make claims to territory inhabited by co-nationals and corresponding trouble with the neighbors. Similarly, the political ambition of revolutionary regimes, emphasized by Colgan, would be especially marked, I argue, if it goes together with the nationalist project of re-drawing the boundaries of statehood in the entire region. It is, thus, an open empirical question whether or not revolutionary regimes emerging from nationalist movements do indeed have such consequences.

Goldstone's second, related point concerns the content of nationalism. Are "civic" nationalisms à la France and the United States inherently more peaceful than "ethnic" nationalisms, he asks, or are there even relevant distinctions between more or less violent nationalisms within these two types? I doubt that this will be so. The United States' supposedly "civic" form of nationalism had a decisively racial undertone—one fourth of the population was enslaved when the nation was declared independent—and it subsequently embarked upon an expansionist agenda that had very unpleasant consequences for the subjugated, expelled, and marginalized non-white peoples. The "ethnic" nationalism of China has not, as far as I can see, led to a similarly bellicose expansionism (leaving the Tibetan case aside). Everybody picks the examples that suit best, of course. It is an interesting question, and empirically quite feasible, to try to answer Goldstone's question in systematic ways. Of course, one would have to overcome very thorny definitional issues given that the distinction between ethnic and civic nationalisms is conceptually ambiguous, to say the least, as the US example makes clear (and as its early propagator later came to argue: Brubaker, 1999).

Theoretically, I doubt that some nationalist ideologies are inherently more violent—beyond the question of whether such variation can be captured by the civic vs. ethnic distinction. I would point to numerous transformations of nationalist ideologies (from the ideology of racial purity and superiority of the Nazis to the pacifist, anti-nationalist, and non-racial patriotism of contemporary Germany, for example). Ideologies matter, of course. The major ideological division relevant for war and peace, I submit, is that between nationalist and non-nationalist principles of legitimacy—at least at the level of abstraction and generalization that the book is aiming at and over the time period that it considers. Eastern Europe and the former Ottoman domains are especially prone to ethnic violence not because their nationalisms are particularly chauvinistic, but because they transitioned from empires that maintained ethnic diversity and heterogeneity at the local level, rather than slowly eroding it through (forced) assimilation as in France or (among whites) the United States, and because the new elites failed to incorporate minorities into the emerging system of alliances.

Goldstone's data concerns are legitimate and I am very glad that he raises these points—I have the deepest respect for his wide-ranging historical knowledge. Most of the "miscodings" that he mentions are, however, not miscodings given my definition of the nation-state as a government ruled in the name of a people of equal citizens without internal, legally enshrined divisions of status between them. The United States had legally sanctioned slavery until the civil war—the almost perfect antinomy to the idea of equality. Apartheid Africa similarly excluded de jure and de facto its majority black population. Whether American or South African whites thought they lived in a perfect democracy of equals doesn't matter that much, from the point of view of my definition, as long as the boundaries of the nation are not defined, constitutionally, in inclusive terms, but contain special provisions that define second-class citizenship (or no citizenship rights at all) for certain kinds of people. The issue of gender inequality, legally sanctioned in most countries well into the 20th century, is of a different nature and, while relevant for many aspects of modern statehood (e.g. Adams, 2005), it is only indirectly relevant for the core process of nation-state formation and war that the book is about (after all, there is no single mono-gendered state in the world nor has there ever been a war fought in the name of men or women).

On the more detailed codings: As is explained in the book, we code on what the constitution says about who rules in the name of whom, not whether or not a state lives up to (for example) the democratic principles enshrined in a constitution. Russia is therefore correctly coded, while we might have made a mistake in the case of Japan. As the book also explains in detail, we code only the first transition to the nation-state and not the reversals (Hitler's Germany, France's Napoleon, China's restored empire). If we do so, as mentioned in the book, the main results of the analysis do not change.

Statistical analysis is certainly a-contextual—it has to be to achieve its aims—but this doesn't mean that it cannot uncover cases and groups of cases in which contextual matters appear to make history work differently than "on average."

Berezin notes the absence of historical narrative in the book—and rightly so, because it explicitly assumes a non-narrative form. Readers who would like to follow threads of events and trends that intersect and produce particular configurations of contingency might be better served with Michael Mann's monumental four-volume *Sources of Social Power* (Mann, 1986-2013). *Waves of War* attempts to tease out, from the various historical threads and contextual colorings, the patterns that repeat—to remain in the carpet metaphor. The price is indeed, as noted in the introduction, a high level of abstraction and methodological de-contextualization: only those aspects of a particular war-prone configuration that are comparable, from the theoretical angle adopted by the book, to other configurations and that are captured by some data are relevant for the statistical analysis. Whether or not one prefers such a bare-bone skeleton of patterns over a richly flavored stew of contextual narratives is a matter of intellectual taste, rather than empirical accuracy or theoretical acumen. With hindsight, I think it would have been better if *Waves of War* had followed Berezin's advice and offered something for every taste. My new book on nation-building will try to do better and combine paired case comparison with broad statistical analysis of the sort that *Waves of War* is perhaps overly rich.

I do think, however, that *Waves of War* offers a little bit more of an attempt at delving into context than what Berezin makes it appear. To be sure, it is not the main concern of the book. But several chapters try to explore a) whether certain continents show different dynamics of nation-building than others, b) which groups of cases the argument applies to and which ones it doesn't (it discusses, for example, why the civil wars of Latin America of the 1960s and 1970s do not conform to the pattern found elsewhere), and so on. Statistical analysis is certainly a-contextual—it has to be to achieve its aims—but this doesn't mean that it cannot uncover cases and groups of cases in which contextual matters appear to make history work differently than "on average."

I am grateful for Berezin's suggestion to explore future alternatives to the nation-state in more depth. *Hélas*, I for my part find understanding the past so hard that to predict the future seems impossible. All we can do is to extrapolate trends, heroically assuming that mechanisms will remain constant and the same as in the past. Still, the exchange-theoretic argument at the core of the analysis lends itself to such an operation—the argument, that is, that political institutions and forms of legitimacy rest on specific modes of exchanging political, economic, and symbolic resources between state elites and the population at large. Let us further assume, as *Waves of War* does to explain the attractiveness of nationalism, that modes of exchange that leave the population worse off than in the past will appear less legitimate in their eyes and thus will be less stable. The European Union hasn't offered anything in public goods to the population (apart from financing infrastructure projects in the peripheries), including no security (there is no European army or police), nor does it tax the population directly or let it participate in its decisions (this has recently changed with the empowerment of the European Parliament). Conformingly, the sense of European identity has remained weak and its institutions are not perceived with much legitimacy. The current crisis in the European Union therefore doesn't come as much of a surprise from the point of view of the theoretical framework outlined in *Waves of War*.

Will religious identities replace national ones or will a non-political form of market control, centered on Wall Street emerge, possibilities that Berezin hints at? They may, but the transition to such a world will undoubtedly be painful and violent, given that neither macro-religious institutions (the Vatican, Al-Azhar University, etc.) nor Wall Street can offer a better exchange relationship to the population at large than can nationally defined states. If *Waves of War* is correct, then, the transition to a macro-religious institutional order or a completely unchained financial capitalism will meet organized resistance by large segments of the world population. ISIS, to be sure, represents an attempt to create a religiously and ethnically homogenous Sunni Arab state (a marriage between Wahhabism and Baathism, as it were) rather than to revert to a trans-ethnic, trans-religious empire such as the Caliphate that it pretends to re-establish. Thus, if one can read tomorrow's weather from today's sunset, not much of a post-national age seems to be on the horizon. But maybe the day after tomorrow?

Endnotes

1. Parts of this summary have appeared in the online version of *Foreign Affairs*, November 7, 2013.

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Comment

World Hegemonic Crises and Rising Tides of Secessionism

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There is a growing perception or awareness that the geopolitical configuration of the world we are living in is being challenged by a rising tide of nationalist, secessionist and irredentist movements. A quick glance at some of the key political developments of 2014 may suffice to explain why diverse forms of state-seeking nationalism have started to catch the attention of social scientists, media pundits and the general public once again.

In Ukraine, for instance, Euromaidan protests and the ousting of Yanukovych in the 2014 Ukrainian Revolution gave birth to a series of secessionist reactions in Crimea and Eastern Ukraine. These movements occurred in the context of a Russian military intervention in Ukraine following the fall of Yanukovych. In March 2014, Crimea held a highly controversial - and still disputed - status referendum, where the people of Crimea and Sevastopol were asked whether they wanted to secede from Ukraine and join Russia as a federal subject or to restore the 1992 Crimean Constitution. Following the referendum, Crimea declared itself independent and immediately requested to become a part of Russia. Although Russia claimed that it was a legitimate accession process, Russia's incorporation of Crimea was interpreted as an act of annexation, the first in Europe since the end of World War II. While Crimean Tatars furiously boycotted the referendum, pro-Russian secessionist forces in Donetsk and Luhansk provinces of Ukraine followed the Crimean model, proclaimed their republics and held referendums seeking legitimacy in May 2014.

Meanwhile, in June 2014, the self-proclaimed "Islamic State" in the Middle East declared the restoration of the *caliphate*, which was abolished in 1924. Using the power vacuum that emerged in the aftermath of the US-led invasion of Iraq in 2003, the Sunni extremists had already declared the establishment of the "Islamic State of Iraq (ISI)" in 2006. When protests against the Assad regime and the civil war started in Syria in 2011, the ISI expanded its operations to Syria and changed its name to "Islamic State of Iraq and the Levant" (ISIL) in 2013. After the declaration of the *caliphate*, ISIL started to call itself the “Islamic State”, affirmed its territorial claims in Libya, Egypt, Algeria, Saudi Arabia and Yemen, and launched a major assault to Kurdish territories in Northern Iraq and in Syria. As the already weak Iraqi state structure collapsed and ISIL forces advanced, the Kurdish regional government used this opportunity to acquire the disputed territories between the regional government and the Iraqi state (such as Kirkuk) and started to signal the possibility of an upcoming independence referendum. In June 2014, Mesoud Barzani told CNN in an exclusive interview that the time for Kurdish people to use their self-determination right has arrived. Although we do not know whether Barzani is really willing to push for Kurdish independence, a more radical development has been taking place in Western (Syrian) Kurdistan (aka Rojava). In the course of the Syrian civil war, Kurdish militia forces in Syria gained the control of the Rojava region, proclaimed their self-rule and gained the *de facto* autonomy of Afrin, Jazira and Kobané cantons. This multiethnic confederation – which is composed of Kurds, Arabs, Assyrians, Chaldeans, Arameans, Turkmens, Armenians and Chechens – declared its interim constitution in January 2014. Successful resistance of the Kobané canton against the ISIS forces in 2014 has not only helped this social revolution to be more visible in the international arena but also played a key role in the emergence of the pre-conditions of a unified Kurdish struggle by starting a rapprochement between rival Kurdish fractions and parties that operated in different geographies of Kurdistan.

Over in Scotland, an independence referendum to end its 307-year-old union with England and Wales was held in September 2014. Although 55.3% of the participants voted against independence, the referendum results showed that the support for Scottish independence increased from 30-35% to around 45%. This increase was recorded in the context of a historic 84.6% voter turnout, which is the highest in the history of the United Kingdom in any election or referendum so far. Following Scotland, in November 2014, Catalonia had a non-binding vote on independence, which the Spanish government tried to block. According to the results announced by the Catalan government, around 2.3 million people participated and 80.8% of them voted "yes" to both questions "Do you want Catalonia to become a state?" and "Do you want this state to be independent?" Another non-binding referendum - in the form of an online poll - was organized by Venetian nationalist organizations in March 2014. Although the way the referendum/poll was organized and its results - which indicate that 2.1 million Venetians (approximately 56.6% of all eligible voters) voted for independence – are highly disputed, this event successfully illustrates increasing aspirations by nationalist organizations to bring issues of "secession," "self-determination" and "national independence" back to the agenda of the masses and general public.

In this almost chaotic context, the international media – once again – turned its attention to rising or ongoing state-seeking nationalist aspirations in the world. Newspapers started to publish interviews with leaders and supporters of existing secessionist movements, to make maps of active state-seeking movements, to discuss potential referenda or declarations of independence and to speculate on what the world map may look like in the near future if these nationalist movements become successful. In addition to the ones discussed above, recent news reports cite Flemish and Walloon movements in Belgium, Basques in Spain, Corsicans in France, Welsh and Irish in the UK, Quebecois in Canada, Uyghur and Tibetan movements in China, Palestinians in the Middle East, South Yemeni movements in Yemen, Pashtun and Baluch movements in Afghanistan and Pakistan, Tuaregs in Mali, the Saharawi movement in Western Sahara, Somaliland and Puntland movements in Somalia, South Ossetia and Abkhazia in Georgia, Aceh and West Papua movements in Indonesia and various movements in Congo and Nigeria as active secessionist movements that have closely been watching these current developments. Needless to say, this is a highly heterogeneous yet still a partial list of existing state-seeking movements of the 21st century.

It is difficult to assess the world-historical significance of the current wave of state-seeking movements by looking at these selective, heterogeneous and anecdotal examples. However, these examples alone may suffice to illustrate that we are not living in a world in which forces of state-seeking nationalism have ceased to exist. Furthermore, current discussions of nationalism are strikingly different from those made in the 1990s. In the aftermath of the dissolution of the USSR and the Eastern bloc socialist federations, many scholars and media pundits had already declared that forces of liberalism, democratization and globalization would bring the final demise of secessionist nationalism. As the following anecdote from an article published in the *New York Times* in 2012 suggests, today the dominant perception is the exact opposite.

> It has been just over 20 years since the collapse of the Soviet Union and the last great additions to the world's list of independent nations. As Russia's satellite republics staggered onto the global stage, one could be forgiven for thinking that this was it: the end of history, the final major release of static energy in a system now moving very close to equilibrium. […] Now, though, we appear on the brink of yet another nation-state baby boom. This time, the new countries will not be the product of a single political change or conflict, as was the post-Soviet proliferation, nor will they be confined to a specific region. If anything, they are linked by a single, undeniable fact: history chews up borders with the same purposeless determination that geology does, as seaside villas slide off eroding coastal cliffs (Jacobs & Khanna, 2012).

Although it is too early to jump to the conclusion that another wave of state-boom will chew up the territorial borders of the existing world in the upcoming decades, these kinds of statements show a radical change in perception about the future of our world. In the course of the 20th century, predictions about the decline of nationalist movements have repeatedly been made. The end of World War II and the establishment of a new inter-state system in 1945, the success of decolonization movements in the 1970s and the dissolution of the Soviet Union in the early 1990s all revived expectations regarding the final demise of state-seeking nationalism. Ironically, almost every time it was declared that state-seeking nationalism was on the wane, a new upsurge was met with surprise. The revival of nationalism in Western Europe and North America in the late 1960s and the collapse of the USSR and socialist federations in the 1988/1992 period were among these surprises. So is the current wave of nationalism.

Dissipating some of the fog surrounding the current global wave of nationalist unrest – including its unusual geographical spread and curious simultaneity with interlinked social movements, wars and crises – is an important task and a major challenge for contemporary social scientists. Methodological perspectives, theoretical approaches and conceptual tools provided by comparative-historical sociologists are very useful for this task. My research takes a long historical perspective and explicates the complex relationship between periods of world-hegemonic breakdown/ transition (Arrighi 1994, Silver and Slater 1999) and emerging structural opportunities for state-seeking nationalist movements in the world (see Karatasli 2013). I show that during periods of world-hegemonic breakdown/ transition, (1) inter-state rivalries and warfare within the inter-state system increase, (2) social, political and economic crises intensify, (3) regional and social inequalities within existing states (or empires) deepen, and (4) frequency and strength of social revolts, rebellions and revolutions escalate. All of these interconnected social, political and economic processes, in turn, produce a favorable macro-structural climate for state-seeking nationalist organizations to mobilize the masses, especially in core and semi-peripheral regions of the world economy. There is a flourishing literature on nationalism which suggests that nationalist movements are more likely to take place when wars erupt (Tilly, 1990; Wimmer, 2013), states are riven by conflicts (Mann, 1993; Mayall, 1994; Skocpol 1979), inter-regional inequalities deepen (Hechter 1975; Nairn 1977) and other forms of social and national conflicts, revolts and revolutions take place (Beissinger 2002). Following in the footsteps of scholars who argue that these processes are more likely to take place during world-hegemonic transition/breakdown (aka "chaos") periods (Arrighi 1994; Arrighi and Silver 1999), I show how these conjunctures of world-history become very fertile for secessionist movements in the world.

The period from 1550 to 1648 – the transition from the Genoese-Iberian systemic cycle to the Dutch world-hegemony – was one such conjuncture. From the revolt of the seventeen provinces (aka the Dutch War of Independence) to the Catalan uprising and the Portuguese War of Independence, this era saw a number of state-seeking movements concentrated in the territories of the Spanish-Habsburg Empire. Another major wave of state-seeking nationalist unrest in the world took place during the transition from Dutch to British world hegemony (1776-1815). From the successful revolt of the thirteen colonies to various creole uprisings in Latin America, from Irish rebellion to Haiti revolution, state-seeking movements with interlinked wars, revolutions and rebellions spread to both sides of the Atlantic in this period. Likewise, the transition from UK to US world-hegemony - that started in the late 19th century, came to a peak during WWI and ended in the aftermath of WWII – coincided with the strongest wave of state-seeking movements and state-formation that world history had seen until then. Looking at these periods closely, one can see how wars, inter-great-power rivalries, a multiplicity of social, political and economic crises, and various forms of social revolts, revolutions and rebellions have historically contributed to the rise of state-seeking movements.

My research suggests that an analogous process has been unfolding in front of our eyes since the beginning of the crisis of US-world hegemony (Karatasli 2013: 343-392). This crisis started in the late 1960s, paradoxically deepened after the collapse of the USSR and it has escalated since the turn of the century. As the crisis unfolds, it gradually creates structural opportunities for secessionist, expansionist or irredentist state-seeking mobilization. From secessionist movements in Eastern Ukraine to the escalation of the Kurdish nationalist struggles and the independence referendums in Western Europe, current state-seeking movements have been utilizing a multitude of structural opportunities provided by increasing inter-great-power rivalries, inter-state wars, escalating social revolts and revolutions and/or increasing social, economic and political crises in an extremely complex set of ways.

It is important to recognize that these historically analogous periods and processes are not identical to each other. There is a major difference, for instance, in how inter-great-power rivalries unfolded and how they affected state-seeking movements during the Dutch and the British hegemonic breakdown periods. This process seems to be unfolding very differently during the current US hegemonic breakdown period. These sorts of differences are extremely critical for understanding the evolution of the modern inter-state system, emergent state-society configurations and changing forms of state-seeking (nationalist) movements.

Furthermore, this multitude of conflicts and crises do not create nations, nationalist sentiments or movements. They provide extraordinary contexts or environments under which state-seeking mobilization is more feasible than usual. For instance, while the long historical struggle for an independent Catalonia cannot be reduced to economic dynamics, Catalan nationalists see the Eurozone crisis as a historic opportunity for nationalist mobilization. As Joseph Vila d'Abadal put it in an interview: "Europe is tired of paying for the south and Catalonia is tired of paying for Spain. [...] No region in Europe pays 8 per cent of its GDP to the government. Probably *this is the best moment for us*. As Einstein said, the world only changes through crisis" (Charter, 2012: 35).

As this anecdote implies, different forms of crises (political, social, economic, etc.) create different opportunities for state-seeking nationalist mobilization in the eyes of nationalist political entrepreneurs. Today, while secessionist movements in Ukraine use Euromadian protests, the fall of Yanukovych and Russian interventionism as key opportunities, Kurds in the Middle East try to utilize the anti-Assad uprisings, the US-led invasion of Iraq or the ISIS siege for their cause. Of course, there is a significant diversity and unevenness in the temporal and spatial distribution of emerging structural opportunities for nationalist mobilization. Yet one thing is clear: As the unraveling of the US world-hegemonic order speeds up, more and more movements around the world start to believe that "*this is the best moment for us*." Or, as 20th century revolutionaries once put it "*The world is in chaos. The situation is excellent!*"

Editor's Note: The author is currently a Post-Doctoral Research Fellow of the Arrighi Center for Global Studies at Johns Hopkins University. His dissertation,* "Financial Expansions, Hegemonic Transitions, and Nationalism: A Longue Durée Analysis of State-Seeking Nationalist Movements" *recently received our section's Theda Skocpol Dissertation Award.

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Conference Report

Religious Wars in Early Modern Europe and Contemporary Islam

Institute for Religion, Culture, and Public Life, Columbia University; RESET Dialogues on Civilization; and the Graduate Center, CUNY

New York: October 23-24, 2014

Editors Note: This conference report was prepared by John Torpey, Director of the Ralph Bunche Institute for International Studies at the City University of New York. My thanks to John for submitting this for publication in the newsletter.

In conjunction with the Institute for Religion, Culture, and Public Life (IRCPL) at Columbia University and RESET: Dialogue of Civilizations (Italy), the Ralph Bunche Institute for International Studies (RBIIS) recently organized a two-day conference on "Religious Wars in Early Modern Europe and Contemporary Islam." The organizers, RBIIS director John Torpey and IRCPL director Karen Barkey, brought together scholars from around the United States and from across the Atlantic to make sense of the ways in which these conflicts resemble and differ from one another. The collaboration between the two institutions was a model for future endeavors, and included holding the first day's discussions at the Graduate Center and the second at Columbia's *Maison Française*.

The central purpose of the conference was to explore the extent to which the conflicts among Christians in early modern Europe and Muslims in the contemporary world are, in fact, driven by religious concerns, and thus to try to contribute to resolving the conflicts that exist today. At the same time, the organizers intended the conference to highlight the importance of the comparative method as an avenue toward understanding. The influential political sociologist Seymour Martin Lipset used to like to say, "He who knows only one country, knows none," because, without some comparative point of reference, it is impossible to say whether the phenomena one is observing are "typical" or "unusual." The comparison of these two periods of religiously infused violence was designed to clarify whether and how the two cases might be similar or different, and thus to illuminate the unique or the patterned nature of the conflicts in question.

The organizers sought to address such questions as: Is the conflict in the contemporary Muslim world so unusual, if Christians were doing the same things 400-500 years ago? Is this really a phenomenon that involves "Muslims," or is this more a matter of conflicts peculiar to a particular world region? To what extent are the stakes in the conflict "religious," as opposed to "political"? The conference brought together historians and social scientists in an interdisciplinary conversation to address these questions. Representatives of the two scholarly traditions do not always find it easy to talk to one another, as the historians tend to insist that everything is unique, while social scientists are chiefly interested in recurring patterns and generalizations. The conclave thus comprised a challenging endeavor that sought to benefit from the insights of scholars straddling area, period, and disciplinary divides.

The question of the relationship between the religious and the political was in many ways at the heart of the discussions. Some argued that there were too many ways in which "Islam," often referred to as the *umma* or worldwide Muslim community, holds together historically and elsewhere than the contemporary Middle East and South Asia for us to be talking about "religious wars" among Sunni and Shi'a in those parts of the world. Indeed, keynote speaker Chase Robinson, Distinguished Professor of History and President of the CUNY Graduate Center, challenged the conference participants to avoid "essentialist" conceptions of "Islam" that failed to do justice to the multifariousness and malleability of the Islamic tradition. He also noted that the very title of the conference proposed a comparison between a time/place ("early modern Europe"), on the one hand, and a rather short stretch in the almost 1500-year life of one of the world's great religious traditions ("contemporary Islam"). Notwithstanding certain questions about the categories involved in the comparison, however, conference participants engaged in vigorous and illuminating discussions about how to think about these two cases of major conflict, at least a good deal of which was religious in inspiration.

All of this raised a question regarding the very meaning of the notion of "religious war." With religion and politics often indissolubly intertwined, in what sense can one say that the conflicts were "religious" in nature? There are at least two different senses in which one might see wars as "religious." It might be the case that religious doctrines and their public status are what is being fought over; for instance, Protestants may be at odds with Catholics over whether or not they are free to practice their version of the Christian faith in public or not. Alternatively, it may be that religious identities, now functioning like ethnic identities, are the motivation behind many participants' involvement in the conflict, but not the subject of the conflict per se.[1] This scenario characterizes at least some of the fighting in contemporary Iraq: a number of Sunnis have taken up arms with the Islamic State group against a regime that systematically privileged the country's Shiites and disadvantaged the minority Sunnis. In short, the religious character of a conflict is not a straightforward matter of a conflict between representatives of different religious factions.

One of the major differences between the cases, the discussions revealed, had to do with the fact that the religious identities of the early modern European Christians were new and thus drenched with potential for conflict, while the religious identities (often) at odds in the contemporary Islamic world are very old – indeed, they originated from the problem of succession after Mohammed's death in the early 7th century – but this has by no means meant that Sunnis and Shiites have always been at each others' throats. New religious identities and their implications for early modern European politics were crucial causes of the conflicts in early modern Europe in ways that cannot be said to be the case in the contemporary Islamic world.

Yet Christians came, over time, to accept one another and to forswear deadly conflict arising from religious disagreements. Much weight in this development is attributed to the Peace of Augsburg of 1555, which first articulated the axiom *cuius regio, eius religio* ("whose the rule, his the religion"). This first peace treaty settling wars among Catholics and Protestants regulated the affairs only of Catholics and Lutherans, however; it took almost another century of bloody warfare, culminating in the so-called Thirty Years' War (1618-1648) to extend the understanding to those adhering to the Reformed (especially but not only the Calvinist) faiths. The Peace of Westphalia that brought these devastating conflicts to a close is widely regarded as having privatized religious faith and muted it as a cause of "domestic" strife. While the treaty had little directly to do with the idea of "sovereignty," it helped consolidate a burgeoning shift within Western Europe from a pattern of dynastic regimes marked by overlapping, cross-cutting forms of religious and political rule to a more coherent system of territorial nation-states.[2] Notwithstanding the shift to territorial states, the relationship between religion and politics remained close until at least the American and French Revolutions, which inaugurated forms of politics that were to be decisively separated from religion (in one case the divorce was friendly, and in the other it was notably hostile). The relationship between religion and politics has not been the same ever since.

Meanwhile, the conflicts among Muslims in the contemporary period are related to religion in complicated ways. *New York Times* columnist Thomas Friedman has written that there are three kinds of conflicts in the Islamic world today: 1) between Sunnis and Shiites; 2) between Sunni moderates and Sunni extremists; and 3) among different Sunni extremists themselves. These conflicts, which may have either of the characteristics of "religious war" outlined previously, are variously intermingled with more straightforwardly "political" conflicts. Hence the Sunni/Shi'a split is undergirded and (as a general rule) promoted by the regional great-power rivalry of Saudi Arabia and Iran. But the religious and national differences here are overlaid and perhaps exacerbated by an ethnic distinction between Persian and Arab. The ethnic (and indeed national) distinction plays a decisive role between Kurds and their oppressors, whether Arab or Turkish – despite the fact that both of them are Sunnis. Meanwhile, the threat of the Islamic State has brought Saudi Arabia and Iran together, in at least a limited fashion, against a common extremist enemy. This marriage of convenience reminds us that there is nothing "primordial" about the Sunni-Shi'a divide, even if it goes back, as a historical matter, to the very origins of Islam. In addition to these conflicts across the sectarian divide, Sunnis may also be at odds with each other in various ways. The rulers of a number of Gulf monarchies recently withdrew their ambassadors from Qatar because they believed that the tiny country was offering too much support to the Muslim Brotherhood, which Arab states have feared for decades as a serious challenger. The Brotherhood was, of course, the major force behind the Arab Spring in Egypt and its democratically elected leader, Mohammed Morsi, was overthrown by the military not long after he took power. Finally, the various factions battling Syrian leader Bashar al-Assad have hardly been "on the same page" as the conflict has unfolded. The jihadists in Syria – of which there are many, joined together in a substantial number of shifting militia groups – do not necessarily share the same goals with regard to the post-Assad future. For example, the Islamic State (aka ISIS) has been engaged in intense conflict with the Al Nusra Front in Syria over dominance in the opposition to Assad. ISIS is a renovated version of Al Qaeda in Iraq (with the addition of disaffected ex-Baathists – that is, supporters of Saddam Hussein), but has been disowned by Al Qaeda leader Ayman al-Zawahiri as too radical. Nonetheless, the Pakistani Taliban leadership has endorsed ISIS and its goals, notwithstanding the close relationship between themselves and Al Qaeda.[3] In sum, the divisions among Muslims over politics in the Middle East and South Asia are multiple, cross-cutting, and shaped by sectarian, national, ethnic, and great-power interests.

The entire endeavor was a vindication of the value of comparison in understanding social life and political conflict. The various papers and presentations enhanced our understanding of the myriad interconnections among religion and politics and reminded us that, even though these have changed from the time of the Peace of Westphalia, neither have they become as neatly separated as the French revolutionaries might have liked it. Religion and politics remain deeply enmeshed with one another, but not always and not everywhere, and it is possible to disentangle them for analytical purposes. The hope is that some sort of accommodation between religion and politics will allow those in the Islamic Middle East and South Asia to come to some more stable and satisfactory arrangement with respect to the religious pluralism that inevitably obtains in any country. But there is also some worry that there is no substantial social base for such an outcome, and that authoritarian leaders will continue to step in to regulate things when no other actor presents itself on the scene. That is a somewhat pessimistic conclusion, perhaps, but seems consistent with the facts on the ground.

Endnotes

1 See Monica Duffy Toft, et al., *God's Century: Resurgent Religion and Global Politics* (New York: Norton, 2011), p. 129.

2 "Medieval Europe had never been composed of a clearly demarcated set of homogeneous political units – an international state system. Its political map was an inextricably superimposed and tangled one, in which different juridical instances were geographically interwoven and stratified, and plural allegiances, asymmetrical suzerainties and anonymous enclaves abounded." Perry Anderson, *Lineages of the Absolutist State* (London: New Left Books, 1974), pp. 37-38.

3 Angelo Young, "Pakistan Taliban Pledges Support to ISIS Militants," *International Business Times*, October 4, 2014, http://www.ibtimes.com/pakistan-taliban-pledges-support-isis-militants-1699490.

Section News

ASA Joint Mentoring Event

Organized by the Comparative and Historical and Global/Transnational Sections

August 18th, 2014

Editor's note: the following event report was prepared by Nicholas Wilson, who co-organized the event with Richard Lachmann and Damon Mayrl. My thanks go out to Nick for submitting this report as well as to Andreas Wimmer for suggesting it for the newsletter.

On August 18th, 2014, the ASA Comparative and Historical and Global/Transnational sections held a joint mentoring event for the second consecutive year. The event, which was attended by 56 students and 20 faculty, paired small groups of graduate students with faculty who shared similar interests.

The event was organized as a happy hour and was held at the Cantina Lounge in San Francisco from 4:30 to 6:30. Students and mentors were asked to discuss professional development and research in small groups for the first half of the event, and then move on to general discussion if they desired to during the second half. Based on responses to an exit

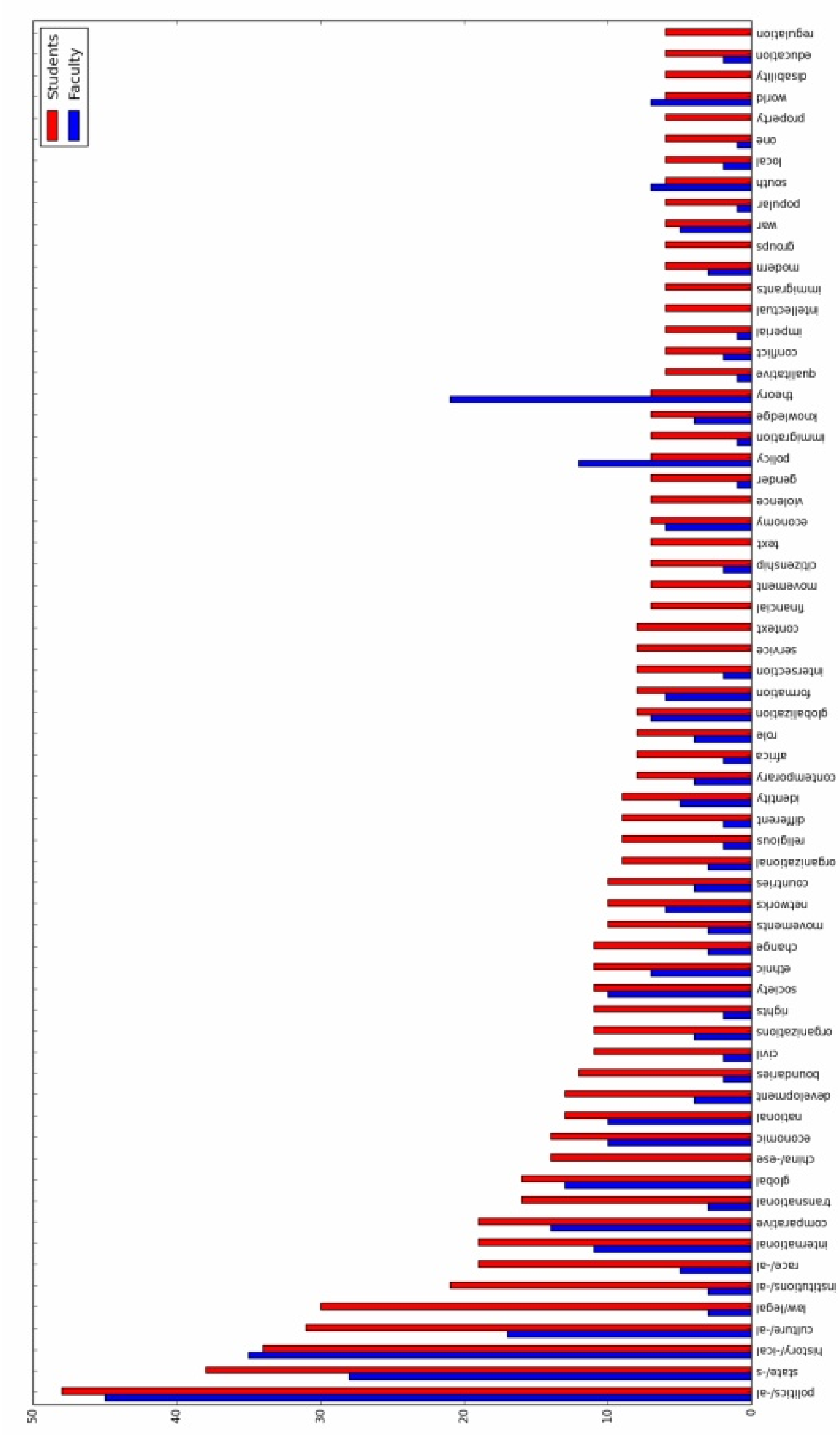

Figure 1: Student and Faculty Research Statement Word Counts

Source: Nicholas Wilson

survey, the event was well-received: of the 56 respondents to the survey, 55 rated the event either "very" or "somewhat" effective.

To participate in the event, students were asked to submit research statements, which provides a window into the interests of the participants. Figure 1 (previous page) summarizes the counts of non-trivial common words occurring more than five times in the research statements students provided and compares those counts to a similar tabulation of participating faculty research interests (as drawn from stated research interests on their websites).

Caution is warranted when interpreting figure 1 for two reasons. Participating students and faculty were not randomly selected: faculty volunteered or were asked by the events' organizers (Richard Lachmann, Damon Mayrl, and Nicholas Hoover Wilson); and students self-selected into the group by volunteering for the event. Second, simple word frequency counts do not adjust for the length of research statements - it might be true that people who mention "ethnic" in their statements also write systematically shorter statements than those who mention "politics" - nor do they distinguish between the contextual use of the relevant words (as might a topic model of the same statements.)

In spite of those cautions, the data in figure 1 is suggestive. That the event was held jointly with the global/transnational section is clear, as "transnational," "global," and "international" all appear frequently, while core concerns of historical sociology--"politics/-al," "state/-s," and "history/-ical"--are mentioned each more than thirty times. The difference in mentions between faculty and students of a few key terms is also striking: judging from student research statements, there is keen interests in "law/legal" and "institutions/institutional" topics, yet few participating faculty mention those terms; and in contrast, faculty mention "policy" and "theory" much more often than students (despite there being three times as many students as faculty providing statements.)

After the event, many students and faculty expressed a desire to repeat the event in the future. Accordingly, plans are underway for the ASA in Chicago in August 2015, and will be organized by Damon Mayrl, Julian Go, and Nicholas Hoover Wilson.

New Publications

Articles and Chapters

Bergesen, Albert J. 2014. "World War II: What Does It Tell Us about Future Great Power Wars?" in Christian Suter and Christopher Chase-Dunn (eds.) *Structures of the World Political Economy and the Future of Global Conflict and Cooperation*. Münster /Berlin/Wien/Zurich: Lit Verlag, pp 1-15.

Merriman, Ben. 2014. "Duels in the European Novel: Honor, Reputation, and the Limits of a Bourgeois Form." *Cultural Sociology*.

Mukerji, Chandra. 2014. "The cultural power of tacit knowledge: Inarticulacy and Bourdieu's habitus." *American Journal of Cultural Sociology*, 2(3), 348-375.

Pumar, Enrique S. 2015. "Fernand Braudel" in Turner et al (eds.) *The Wiley-Blackwell Encyclopedia of Social Theory*. London: Wiley-Blackwell.

Tiryakian, Edward and Jonathan Morgan. 2014. "Solidarity, Yesterday and Today," in Vincent Jeffries, ed., *The Palgrave Handbook of Altruism, Morality and Social Solidarity.* Palgrave/Macmillan, pp. 249-271.

Savelsberg, Joachim J. and Suzy McElrath. 2014. "Crime, Law and Regime Change." *Annual Review of Law and Social Science*, Vol. 10:259-279.

New Publications

Books and Edited Volumes

Mobilizing Democracy: Globalization and Citizen Protest

Johns Hopkins University Press, 2014

Paul D. Almeida

Paul Almeida's comparative study of the largest social movement campaigns that existed between 1980 and 2013 in every Central American country (Costa Rica, El Salvador, Guatemala, Honduras, Nicaragua, and Panama) provides a granular examination of the forces that spark mass mobilizations against state economic policy, whether those factors are electricity rate hikes or water and health care privatization. Many scholars have explained connections between global economic changes and local economic conditions, but most of the research has remained at the macro level. *Mobilizing Democracy* contributes to our knowledge about the protest groups "on the ground" and what makes some localities successful at mobilizing and others less successful. His work enhances our understanding of what ingredients contribute to effective protest movements as well as how multiple protagonists—labor unions, students, teachers, indigenous groups, nongovernmental organizations, women's groups, environmental organizations, and oppositional political parties—coalesce to make protest more likely to win major concessions.

Based on extensive field research, archival data of thousands of protest events, and interviews with dozens of Central American activists, *Mobilizing Democracy* brings the international consequences of privatization, trade liberalization, and welfare-state downsizing in the global South into focus and shows how persistent activism and network building are reactivated in these social movements. Almeida enables our comprehension of global and local politics and policy by answering the question, "If all politics is local, then how do the politics of globalization manifest themselves?" Detailed graphs and maps provide a synthesis of the quantitative and qualitative data in this important study. Written in clear, accessible prose, this book will be invaluable for students and scholars in the fields of political science, social movements, anthropology, Latin American studies, and labor studies.

The Power of Market Fundamentalism: Karl Polanyi's Critique

Harvard University Press, 2014

Fred Block and Margaret R. Somers

What is it about free-market ideas that give them tenacious staying power in the face of such manifest failures as persistent unemployment, widening inequality, and the severe financial crises that have stressed Western economies over the past forty years? Fred Block and Margaret Somers extend the work of the great political economist Karl Polanyi to explain why these ideas have revived from disrepute in the wake of the Great Depression and World War II, to become the dominant economic ideology of our time.

Polanyi contends that the free market championed by market liberals never actually existed. While markets are essential to enable individual choice, they cannot be self-regulating because they require ongoing state action. Furthermore, they cannot by themselves provide such necessities of social existence as education, health care, social and personal security, and the right to earn a livelihood. When these public goods are subjected to market principles, social life is threatened and major crises ensue.

Despite these theoretical flaws, market principles are powerfully seductive because they promise to diminish the role of politics in civic and social life. Because politics entails coercion and unsatisfying compromises among groups with deep conflicts, the wish to narrow its scope is understandable. But like Marx's theory that communism will lead to a “withering away of the State,” the ideology that free markets can replace government is just as utopian and dangerous.

Seeing Through the Eyes of the Polish Revolution: Solidarity and the Struggle Against Communism in Poland.

Brill and Haymarket Press, 2014

Jack M. Bloom

In 1980 Polish workers astonished the world by demanding and winning an independent union with the right to strike, called Solidarity--the beginning of the end of the Soviet empire. Jack M. Bloom's *Seeing Through the Eyes of the Polish Revolution* explains how it happened, from the imposition to Communism to its end, based on 150 interviews of Solidarity leaders, activists, supporters and opponents. Bloom presents the perspectives and experiences of these participants. He shows how an opposition was built, the battle between Solidarity and the ruling party, the conflicts that emerged within each side during this tense period, how Solidarity survived the imposition of martial law and how the opposition forced the government to negotiate itself out of power.

This book is published in Europe by Brill Press and in the U.S. as a paperback by Haymarket Press.

Contention and the Dynamics of Inequality in Mexico, 1910-2010

Cambridge University Press, 2014

Viviane Brachet-Marquez

This book details how contentious politics - everyday as well as exceptional, local as well as national - that took place in three communal villages of Mexico alternately reproduced and reshaped inequality. Narrated and analyzed as instances of the general process of contention, these events took place during three key periods of Mexico's history: the 1910–20 revolution, the Cold War period from the 1950s to the 1970s, and from the 1980s to the present. Together, these episodes of contention build and test a theory of the making and unmaking of inequality in theoretically ideal conditions, illustrating the dynamics of this all-pervasive facet of social organization.

Between Monopoly and Free Trade: The English East India Company, 1600–1757

Princeton University Press, 2014

Emily Erikson

The English East India Company was one of the most powerful and enduring organizations in history. *Between Monopoly and Free Trade* locates the source of that success in the innovative policy by which the Company's Court of Directors granted employees the right to pursue their own commercial interests while in the firm's employ. Exploring trade network dynamics, decision-making processes, and ports and organizational context, Emily Erikson demonstrates why the English East India Company was a dominant force in the expansion of trade between Europe and Asia, and she sheds light on the related problems of why England experienced rapid economic development and how the relationship between Europe and Asia shifted in the eighteenth and nineteenth centuries

Though the Company held a monopoly on English overseas trade to Asia, the Court of Directors extended the right to trade in Asia to their employees, creating an unusual situation in which employees worked both for themselves and for the Company as overseas merchants. Building on the organizational infrastructure of the Company and the sophisticated commercial institutions of the markets of the East, employees constructed a cohesive internal network of peer communications that directed English trading ships during their voyages. This network integrated Company operations, encouraged innovation, and increased the Company's flexibility, adaptability, and responsiveness to local circumstance.

Between Monopoly and Free Trade highlights the dynamic potential of social networks in the early modern era.

Denial of Violence: Ottoman Past, Turkish Present, and Collective Violence against the Armenians, 1789-2009

Oxford University Press, 2014

Fatma Muge Gocek

While much of the international community regards the forced deportation of Armenian subjects of the Ottoman Empire in 1915, where approximately 800,000 to 1.5 million Armenians perished, as genocide, the Turkish state still officially denies it.

In *Denial of Violence*, Fatma Müge Göçek seeks to decipher the roots of this disavowal. To capture the negotiation of meaning that leads to denial, Göçek undertook a qualitative analysis of 315 memoirs published in Turkey from 1789 to 2009 in addition to numerous secondary sources, journals, and newspapers. She argues that denial is a multi-layered, historical process with four distinct yet overlapping components: the structural elements of collective violence and situated modernity on one side, and the emotional elements of collective emotions and legitimating events on the other. In the Turkish case, denial emerged through four stages: (i) the initial imperial denial of the origins of the collective violence committed against the Armenians commenced in 1789 and continued until 1907; (ii) the Young Turk denial of the act of violence lasted for a decade from 1908 to 1918; (iii) early republican denial of the actors of violence took place from 1919 to 1973; and (iv) the late republican denial of the responsibility for the collective violence started in 1974 and continues today.

Denial of Violence develops a novel theoretical, historical and methodological framework to understanding what happened and why the denial of collective violence against Armenians still persists within Turkish state and society.

Searching for the Spirit of American Democracy: Max Weber's Analysis of a Unique Political Culture, Past, Present, and Future

Paradigm Publishers, 2014

Stephen Kalberg

The ongoing "crisis of American democracy" debate is the topic of this new book. By referring to Weber's long-term perspective, it provides rich new insights and also offers powerful explanations for the particular contours of today's American political culture.

Kalberg draws upon Weber to reconstruct political culture in ways that define America's unique spirit of democracy. Developing several Weber-inspired models, the author reveals patterns of oscillation in American history. Can these pendulum movements sustain today the symbiotic dualism that earlier invigorated American democracy? Can they do so to such an extent that the American spirit of democracy is rejuvenated? Kalberg forcefully argues that facilitating political cultures is indispensible if democracies are to endure. He then explores in his concluding chapter whether Weber's explanations and insights can be generalized beyond the American case.

Globalizing Knowledge: Intellectuals, Universities and Publics in Transformation

Stanford University Press, 2014

Michael D. Kennedy

Globalizing Knowledge introduces the stakes of globalizing knowledge before examining how intellectuals and their institutions and networks shape and are shaped by globalization and world-historical events from 2001 through the uprisings of 2011–13. But Kennedy is not only concerned with elaborating how wisdom is maintained and transmitted, he also asks how we can recognize both interconnectedness and inequalities, and possibilities for more knowledgeable change within and beyond academic circles. Subsequent chapters are devoted to issues of public engagement, the importance of recognizing difference and the local's implication in the global, and the specific ways in which knowledge, images, and symbols are shared globally. Kennedy considers numerous case studies, from historical happenings in Poland, Kosova, Ukraine, and Afghanistan, to today's energy crisis, Pussy Riot, the Occupy Movement, and beyond, to illuminate how knowledge functions and might be used to affect good in the world.

European Glocalization in Global Context

Palgrave Macmillan, 2015

Roland Robertson (editor)

This highly original work applies the relatively new concept of glocalization to contemporary Europe. The opening and concluding chapters by Roland Robertson deal with Europeanization in an innovative way as a highly problematic process in relation to the changing position of Europe in the global arena. The individual authors are each experts in their own field, they include Ewa Morawska on migration; Debra Gimlin on beauty contests; Andrea Esser on European TV; Christopher Kollmeyer on democracy; Victor Roudometof on European Christianity; Franciscu Sedda on the European imaginary; and Paolo Demuru on Brazilian football. These chapters make a concerted effort to relate European developments to the world as a whole, this being a highly neglected aspect of European studies. The range of this book's coverage, its theoretical innovativeness, and, not least, its discussion of European extremism renders it a unique contribution to the field in comparison with existing scholarship.

Racism, Class and the Racialized Outsider

Palgrave Macmillan, 2014

Satnam Virdee

Racism, Class and the Racialized Outsider offers an original perspective on the significance of both racism and anti-racism in the making of the English working class. While racism became a powerful structuring force within this social class from as early as the mid-Victorian period, this book also traces the episodic emergence of currents of working class anti-racism. Through an insistence that race is central to the way class works, this insightful text demonstrates not only that the English working class was a multi-ethnic formation from the moment of its inception but that racialized outsiders – Irish Catholics, Jews, Asians and the African diaspora – often played a catalytic role in the collective action that helped fashion a more inclusive and democratic society.

Inside China's Automobile Factories: The Politics of Labor and Worker Resistance

Cambridge University Press, 2014

Lu Zhang

In *Inside China's Automobile Factories*, Lu Zhang explores the current conditions, subjectivity, and collective actions of autoworkers in the world's largest and fastest-growing automobile manufacturing nation. Based on years of fieldwork and extensive interviews conducted at seven large auto factories in various regions of China, Zhang provides an inside look at the daily factory life of autoworkers and a deeper understanding of the roots of rising labor unrest in the auto industry. Combining original empirical data and sophisticated analysis that moves from the shop floor to national political economy and global industry dynamics, the book develops a multilayered framework for understanding how labor relations in the auto industry and broader social economy can be expected to develop in China in the coming decades.

A 20% discount is available for this book until 31 December and can be claimed here: www.cambridge.org/ICAF2014.

Section News

Announcements

Call for Papers

The Laboratory for Comparative Social Research (HSE) announces a call for the 5th LCSR International Workshop "Social and Cultural changes in cross-national perspective: Subjective Well-being, Trust, Social capital and Values" which will be held within the XVI April International Academic Conference on Economic and Social Development of the National Research University Higher School of Economics on April 6 – April 10 in Moscow.

The workshop aims at developing empirical quantitative comparative (cross-country and cross-regional) studies in social science. Participation at the workshop is possible via entering the LCSR research network. The main purpose of the research network is to attract young scholars to work on their own projects under the guidance of the LCSR experts (Ronald Inglehart, Eduard Ponarin, Christian Welzel).

The application deadline for entering the network is January 15, 2015. The notification of acceptance will be given by February 1. More information on the workshop can be found here:

http://lcsr.hse.ru/en/announcements/138758256.html

Grant Announcement

Professor Larry King (University of Cambridge) was awarded €3 million from the European Union's Horizon 2020 Fund for his grant "Digital Whistleblower." This project will employ computerized data mining of digital public procurement records to generate objective measures of corruption at the level of individual contracts, which can be scaled up to the organizational, sectoral and the country level. This data will be collected and analysed in all member states of the European Union. This will create objective measures of corruption that are comparable over time and across different sectors and countries. Among the many uses of this data, researchers and civil society actors will be able to measure corruption at the level of individual organizations, allowing them to observe how corruption changes over time in response to policies like deregulation and privatization.

Book Awards

Confucianism as a World Religion: Contested Histories and Contemporary Realities

Princeton University Press, 2013

This book, written by section member Anna Sun (Kenyon College), has been honored with the following book awards this year:

The Best Book Award from the Sociology of Religion Section of the American Sociological Association.

The Best First Book in the History of Religions Award from the American Academy of Religion.

PhDs on the Market

Rachael J. Russell
University of California, Irvine

Constructing Global Womanhood: WINGOs, Women's Ministries, and Women's Empowerment

My dissertation integrates findings from political sociology, international relations, social movements, and cultural sociology 1) to understand the evolution of women's global civil society expanding initially from the West since 1870, 2) to explain structural expansion in state concern since 1960 to include a women's ministry, particularly in non-West nations, and 3) to explain women's institutional power outcomes cross-nationally since 1960. Research questions asked are: How has women's global civil society evolved over time? What is the effect of women's global civil society on structural expansion in social concerns of the state to include women? And, finally, does a global women's civil society devoted to empowerment, along with state infrastructure designed for empowerment, actually give women more power across all nations? Based on neo-institutional theory, I argue that world society is a locus of messages regarding women which are diffused to nation-states through links to international organizations. Both women's empowerment and national institutional incorporation are cultural constructions from world society that diffuse to nation-states through international organizations and have increasingly come to define legitimacy of nation-states. Because the international non-governmental and governmental solutions to women's empowerment are culturally constructed, and many times based in Western imaginations of women's empowerment, there is likely to be decoupling between intended solutions and actual power outcomes for women, particularly in the non-West and United Nations-designated Least Developed Countries (LDCs). Using archival comparative-historical methods, I first analyze descriptive statistics on women's international non-governmental organization (WINGO) foundings and national memberships, along with an Exploratory Factor Analysis of organization categories on a sample of 183 WINGOs over the period since 1870, offering evidence of expansion and change in the structure and discourse of world society devoted to women. Second, I analyze rate of women's ministry establishment across all nations since 1960 in an Event History Analysis (EHA), showing positive and significant effects of national WINGO membership and LDC status, supporting world society theory. Lastly, I analyze women's national institutional power outcomes across all nations since 1960 using EHA methods considering social power as measured by female tertiary enrollment ratios, economic power as measured by women's labor force participation rates, and political power as measured by percentages of women in parliament.

Committee: Evan Schofer (chair), Catherine Bolzendahl, David J. Frank, and Ann Hironaka

Research Interests: Quantitative Methods; Cultural and Political Sociology; Global and Transnational Sociology; Gender; Religion; Comparative-Historical

PhDs on the Market

Yao Li
Johns Hopkins University

Informal Norms and Protest Space in China—Why the Chinese Regime Remains Stable despite Rising Protests.

My doctoral dissertation focuses on the question: Why does the Chinese regime remain resilient amid mounting social protests? This phenomenon is at odds with expectations of previous scholarship which often links rising protests in authoritarian states with regime decline. I argue that it is critical to distinguish two types of protests: regime-engaging and regime-threatening protests. In regime-engaging protests, both the state and protesters accept the legitimacy of the other side and are open to negotiation; whereas in regime-threatening protests, both authorities and protesters reject the legitimacy of the other side and close the door to negotiation. The two kinds of protests are ideal types and a protest may move from one to the other. Yet the distinction matters: regime-engaging protests help maintain regime legitimacy and resilience, whereas regime-threatening protests undermine them. Based on a dataset of 1,418 protest events that I generated, I conducted binary and multinomial logistic regression analysis to show that regime-engaging protests are prevalent in the country. Further, seven case studies of regime-engaging protests demonstrate that informal norms of contention play a role in regulating actions of both authorities and protesters and guiding both sides to work on resolving conflicts through dialogue not force. By contrast, three case studies of regime-threatening protests exhibit a vicious cycle of conflict escalation and pose a great challenge to the regime. Overall, my research has developed a conceptual model of regime-engaging and regime-threatening, which can be employed to monitor the trajectory of protests not only in China but also in other authoritarian regimes.

Committee: Joel Andreas (chair), Ho-Fung Hung, Lingxin Hao, Erin Chung, and William Rowe

Specializations: social movements, political sociology, international development, comparative-historical sociology, and China studies

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