



Comments on Greta Krippner, Capitalizing on Crisis: The Political Origins of the Rise of Finance

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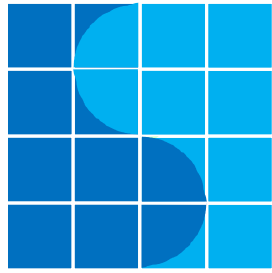
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Book Symposium (Krippner)

Editors' Note: Greta R. Krippner's Capitalizing on Crisis: The Political Origins of the Rise of Finance (Harvard 2011) was the subject of an Author Meets Critics session at the Social Science History Association meeting in Boston in November 2011. These are the revised comments from Frank Dobbin, Isaac Martin, and Bill Sewell with Greta Krippner's response. We would like to thank Ho-fung Hung for organizing and guest-editing the symposium.

Capitalizing on Crisis: The Political Origins of the Rise of Finance

by Greta R. Krippner

(Harvard University Press, 2011)

Comments by Frank Dobbin

Harvard University

Greta Krippner submitted *Capitalizing on Crisis* to Harvard University Press at the end of the summer of 2008, before the Great Recession began. I read it as a member of the press board, and we all thought Krippner had a career ahead as a psychic. There had been rumblings from economists, like Simon Johnson at MIT, about the rise of finance and its dominance of the American economy, and there have been some rumblings about the growing frequency of crises since the deregulation of a generation ago, in the work of David Moss at Harvard Business School for instance. But, and this is hard to remember through the fog of the last 3 years, in 2008 we still thought we understood the crises of recent memory. Crony capitalism in East Asia set off one crisis, and that wasn't of our doing. Greed set off the Enron/Worldcom/Tyco debacle of 2001, and Sarbanes Oxley was going to fix that through better accounting.

What made this a great read in the fall of 2008 was not that it diagnosed the recent crisis, but that it detailed the conditions for the rise of that crisis, but stopped in 2001. Greta Krippner saw the historical roots of this crisis before we knew there would be a crisis. She detailed the conditions that invited crises.

The big picture is that to deal with economic crises since the 1970s, Washington introduced broad regulatory reforms that set the stage for a series of economic crises. These regulatory reforms ushered in the financialization of the American economy. We saw a rapid shift in the core business of the United States, from manufacturing not to service so much as to finance per se. As Simon Johnson pointed out, when the market peaked in 2001, finance accounted for 40% of profits in the American economy.

Krippner's analysis takes us through three main policy shifts that produced this unprecedented growth of financialization, by which she means not only the staggering rise of the financial sector, but

the growth of financial activities in non-financial firms. The business unit that was G.M.'s loan department, the General Motors Acceptance Corporation, in many years made more money for the automaker than automaking did. Likewise for Ford's and General Electric's financing divisions. The economy has increasingly grown via new-fangled financial instruments and securitization. You can now bet not only on hog futures, but against low income households paying off their mortgages, and for the continuing popularity of the Beatles backlist. Krippner's second chapter details how this happened brilliantly. Her colleague at Michigan, Jerry Davis, had done some of this in his 2009 book, *Managed by the Markets*. But where Davis largely describes the change, Krippner traces its political roots.

It would be tempting to build an argument about the policy decisions that encouraged financialization around a conspiracy theory, or a narrow power theory, attributing blame for growing, structured, volatility in the economy to Wall Street titans. Krippner instead makes a sophisticated historical argument. The fact is, as Krippner shows, political expediency rather than power politics produced the key policy changes, and the effects of those changes were not anticipated by anyone. No one planned the rise of finance, speculation, and bubbles, and the top

1/10 of 1 percent did not design it with malice aforethought. Today's elite came of age in the 1970s, when the star CEO took home a million dollars a year, and lived to see a world in which the star hedge fund manager took home five billion.

Instead of a story of conspiracy, Krippner tells a story of an increasingly complex economy, with political leaders who made decisions based on expediency that piled up one on another to alter the basic dynamics of the economy. Perhaps the most sobering lesson from the book is that we live in an economy so complex that neither scientists nor soothsayers can predict the effects of one regulatory choice or another.

“Krippner ... makes a sophisticated historical argument. The fact is, as Krippner shows, political expediency rather than power politics produced the key policy changes, and the effects of those changes were not anticipated by anyone.”

Krippner shows us that from the 1970s the state dealt with ongoing distributional conflicts and particular economic crises by changing regulations in ways that promoted financialization. The story is less about power politics than about efforts to maintain state legitimacy and to prevent unruliness from below. To this extent the book echoes Raghu Rajan's *Fault Lines*, written after the crisis, which ties the crisis (which he can take credit for predicting in earlier work) more narrowly to federal efforts to expand home ownership to preclude class backlash in the context of growing inequality. Krippner likewise takes up the theme of politicians attempting to preclude political crisis resulting from the explosion of inequality, but she explains financialization more generally, not just the mortgage crisis, and traces it to a series of political decisions, rather than to congressional efforts to appease the working class with mortgages.

The core chapters address key federal policy changes that set the country on a new course. Based on primary archival research, these chapters contribute not only a new interpretation of events, but original research on the debates behind key policy shifts.

Washington made 3 moves that promoted financialization. It deregulated financial markets in the 1970s. From the 1980s, it encouraged global flows of capital to the U.S. And the Federal Reserve altered monetary policy beginning in the late 1970s. First, the sustained economic crisis of the 1970s threatened to produce political strife. The oil crisis, economic stagnation, and a record cost of credit hit the middle and working classes hard. Deregulation of financial markets would free up flows of capital across different sorts of markets, reducing mortgage rates but also unleashing greater competition for investment. The foundation was set for financialization.

Second, in the 1980s the Reagan administration unwittingly opened up global capital flows, and became addicted to foreign credit. This happened as the Fed, under Paul Volker, sustained high interest rates when global credit flows were in their infancy. The high interest rates, and security, offered by Washington drew unprecedented flows of capital from abroad, which allowed Reagan to cut taxes without making equal cuts to

spending. Reagan thus overcame the impending fiscal crisis of the state, and investors were soon accustomed to thinking globally. A global financial market was on the rise.

Third, by the early 1980s, the Fed moved away from a strategy of closely controlling the supply of credit, and largely abandoned adjustments based on the money supply. Reluctant to take sole responsibility for the health of the economy, Washington moved toward allowing markets to determine the availability of credit. Without any central decision-making capacity, markets turned out to be credit boosters. This led to an unexpected expansion of credit.

Where would we be without all of these policy shifts? While it is clear that political expediency was the motive behind each of these changes, it is equally clear that without these changes, the United States would not have seen the rapid move toward financialization that it has seen. Washington might not have been able to tame the political discontent tied to the stagflation, and sky high interest rates, of the 1970s. More generally, the global economic system might not have been globalized. China might not own us.

Krippner shows that politicians and bureaucrats are constrained by considerations of legitimacy and by political pressures even in what look like cold calculations about how to manage the economy. There are no decisions that are apolitical. At the same time, the changes that public policy wrought were largely inadvertent, and so if indeed the financial sector has now captured Washington, that change is a consequence, not a cause, of the regulatory shifts that led to the growing economic and political power of finance.

Krippner modernizes Polanyi's agenda brilliantly, showing us that the economic world we see before us is not necessarily the best, and certainly not the only, option we have. But Krippner goes beyond Polanyi, whose theme was that a nascent commercial class pushed for an end to policies that advantaged the gentry, and the opening of markets. She shows us that what has happened in recent years is not really a *deregulation* of financial markets, but a *reregulation* of markets, with new divisions separating financial from non-financial institutions, new mechanisms for establishing interest rates, and new boundaries on credit markets. These changes

may all look like the opening of markets, but the state has not retreated so much as made decisions to handle political crises that had unanticipated consequences. We have a new set of regulations, not the absence of regulations. While we think that the economy evolves according to its own logic, in fact it changes in large measure in response to changes in political structures.

I end with two questions. Not to suggest that Krippner should have written the book I would have written, or done more in this book, but to suggest that the book begs for more research, both at the meso level and at the macro level.

One question at the meso, organizational, level. While Krippner points out that the studies of the shareholder value movement, which also purport to explain financialization, set political decisions aside and do not explain them, she doesn't fully assess how much of the change was wrought by the big public policy shifts she points to, and how much by change in how corporations operate. How much of the change can we apportion to the rise of the shareholder value paradigm of corporate management? It is a complicated question, but let's take the example of corporate debt. The opening up of credit that Krippner documents helped corporations to take on substantially more bond debt. Corporate debt nearly doubled after the 1970s as a proportion of equity. This came about for a variety of reasons, but the shareholder value paradigm supported debt as a way to create a market for corporate control and as a way to leverage the value of equity by returning to shareholders not only profits made on equity, but profits made on borrowed money. The corporate bond and junk bond markets thus fueled the growth of Wall Street for much of the period, because it was investment banks that were issuing and managing the debt, and handling the acquisitions, spinoffs, and bankruptcies that erupted when firms had trouble paying off debt. We need to better understand how much the public policy shifts that Krippner

describes, and how much the shareholder value revolution that Michael Useem and Gerald Davis describe, contributed to financialization.

One question at the macro level. Krippner gives us an historical account of the rise of policy decisions such as financial deregulation and Volker's interest rate spike, and traces how they contribute to financialization. Her broad argument is that the state faced fiscal and legitimacy crises, and these reactions were politically expedient. But seen from abroad, these reactions were peculiarly American. In another political context, where the state has historically played a different role in the economy, we might have seen very different solutions. In what ways were these solutions constrained by American state capacities, and by the American conception of the role of the state in the economy? Would a state like Germany or France or Japan, in which the state had historically played a greater role in regulating credit, interest rates, and international flows of capital, have responded to America's crises quite differently? Perhaps American political culture and structure made these policy choices not just possible, but likely.

Comments by Isaac Martin University of California, San Diego

Greta Krippner's *Capitalizing on Crisis* is an important study of the rise of finance in the United States. It combines a careful quantitative description of financialization, defined as "the growing importance of financial activities as a source of profits" (2011: 27), with a richly detailed historical narrative that purports to explain financialization by describing how policy makers confronted with a structural crisis made a series of decisions that radically increased the profit that could be made on financial activities. I came away from the book convinced of the most general argument—that

"It combines a careful quantitative description ... with a richly detailed historical narrative that purports to explain financialization by describing how policy makers confronted with a structural crisis made a series of decisions that radically increased the profit that could be made on financial activities."

financialization occurred in response to a general social and fiscal crisis—but also convinced that we need more research to be sure whether this book has identified the correct agents and causal mechanisms by which the crisis caused that social change.

The first chapter of the book is given over to documenting that financialization occurred. It provides a rigorous conceptual discussion of financialization, and presents a variety of possible indicators of the concept. Krippner delved deeply into the data, and her Chapter 1 makes a strong case that the apparent turn to finance is not an artifact of the particular indicator chosen, nor is it just one aspect of a broader post-industrial shift in the economy. It is a real shift. I think the book will be seen as definitive on this point. Before reading the book, I thought financialization was an unnecessarily big word. After reading this book, I see financialization as one of the most important social changes of the late twentieth century in the United States.

The book also offers an innovative theoretical account of financialization that, in broad outlines, I find plausible. The most general argument of the book is that the path to financialization was paved by public policy. In particular, Krippner argues that policy makers deregulated financial activities in order to allay a series of crises that beset the advanced capitalist economies beginning in the late 1960s—a social crisis associated with class conflict, a fiscal crisis of the state, and a legitimation crisis. She also points out that these were facets of the same underlying crisis, the basic problem of how to divide up resources when economic growth slows down. As the allusions to O'Connor (1973) and Habermas (1975) might suggest, the fundamental logic of this argument is broadly Marxian. It rests on the premises that capitalist economies are characterized by competing resource demands between broadly defined social groups; that this conflict can be allayed but not resolved by economic growth; that the conflict between contradictory demands comes to head when growth slows; that political actors respond to the crisis by setting up new institutional rules that will help get growth started again; and that the new rules work for a little while, but ultimately just set the stage for the next crisis. That plot will sound familiar to anyone who has read

Capital, and the logic and assumptions are quite close to those of the “social structures of accumulation” school (see Kotz, McDonough and Reich 1994).

But is this argument correct? Was the crisis a fundamental cause of financialization? The book offers case study evidence that the intertwined crisis of growth, public finance, and governance was much on the mind of policy makers who deregulated finance in the late twentieth-century United States, but it would take comparison to test whether the crisis was actually a cause of financialization. The book does not provide such a comparison. It does provide the necessary conceptual building blocks for a comparative analysis, however, and a brief exercise in applying Krippner's measures of financialization to international data increased my confidence in the argument of *Capitalizing on Crisis*.

Figure 1 illustrates the data that persuaded me. The figure presents time series of the ratio of financial-sector profits to non-financial sector profits for twelve countries—including all countries for which data prior to 1990 are available in the OECD STAN database. Though Krippner's preferred measures of financialization are based on American national accounts and published tax data sources that are idiosyncratic to the U.S., this is a reasonably close approximation of one of her preferred measures of financialization (cf. Krippner 2011, Chapter 1, Figure 8) and can be calculated from internationally harmonized data published by the OECD. The “financial sector” for the purpose of this graph refers to OECD industry category 6574 (Finance, Insurance, and Real Estate); and “profits” refer to the STAN variable “gross operating surplus” (GOPS), which is not adjusted for depreciation, nor for financial accounts payable and receivable. In order to facilitate comparison, the graphs are ordered from the greatest ratios (left) to least (right), and from steepest slopes (top) to shallowest (bottom).

If Krippner's argument is correct, then we should expect to observe financialization quite generally throughout the OECD, since the social, fiscal, and legitimation crises of the 1960s and 1970s were also quite general. And that is indeed what we observe. The available data series start in the 1980s, and they reveal a more or less secular increase in the ratio of financial-sector to non-

financial-sector profits almost everywhere. The United States stands out in this group of rich countries for having achieved the greatest degree of financialization the most rapidly. But the scope of the process and its pace in the United States appear to differ in degree, not in kind, from the scope and pace of financialization in most other rich countries. The ratio increased somewhat more slowly in France and Denmark than in the U.S., but attained about as high a level. It increased just as rapidly in Germany and Japan as in the U.S., though from a lower baseline. In short, if we accept this measure of financialization, then we must conclude that almost all of the most developed capitalist economies underwent financialization in the late twentieth century. The near-ubiquity of financialization seems consistent with the view that it was driven by some fundamental process common to these developed market economies, and the late-twentieth-century fiscal crisis of the state is a likely suspect.

The comparison also exonerates other likely suspects that might be inconsistent with Krippner's account. For example, the trend looks basically similar despite finance policy regimes that were very different in this period. It happened in liberal market economies and coordinated market economies. It happened in economies with strong welfare states and weak welfare states. It happened in places where neoliberals took power early and places where neoliberals never quite ran the show. It happened regardless of the partisan coloration of government. And so on.

The comparative data also give us something quite close to a natural experiment. There was one rich democratic country that escaped the fiscal crisis of the state in this period by the lucky expedient of discovering oil. That country was Norway. And—apart from the banking enclaves of Switzerland and Luxembourg, which did not financialize only because they were already so dependent on finance—Norway appears to be the only rich democratic country that did *not* undergo financialization in this period. The ratio of financial to non-financial sector profits started low and decreased.

In short, although the book did not present the kind of comparative evidence that would be necessary to convince me of its fundamental claim, it did give me the conceptual tools to go see for

myself; and I came away from the data convinced that the fiscal crisis of the state in late-twentieth-century developed capitalist economies was indeed a fundamental cause of financialization.

But how, exactly, did crisis lead to financialization? When it comes to identifying the relevant actors and causal mechanisms, the book offers a Tocquevillian argument that I found initially plausible, but ultimately somewhat less convincing. The basic argument is that policy makers, rather than, say, financial elites, were the key decision-makers. The narrative does make a strong case for this view. It spells out in careful detail who did what, naming names, and presents detailed evidence about why those people at that moment thought that was the thing to do. (The main actors in the narrative are not social classes, or class-based organizations such as labor unions or industry trade associations. They are voluntary interest groups and public officials whose actions have unintended consequences. In this respect the book belongs to the great tradition of comparative historical sociology that poses Marxian questions and gives Tocquevillian answers. I thought at some points as I read that the book could just as well have been called *The Old Regime And The Financial Revolution*.)

The general picture of the policy process seems true to me, or at least it suits my prior assumptions. Policy makers in the narrative appear to be short-sighted problem solvers, whose primary motivation is blame avoidance, who often lack information about how the world works and who operate according to ad hoc theories rather than stable or coherent ideologies. They sometimes respond to lobbying but they also worry about re-election and about doing their jobs; they are neither dispassionate technocrats, nor consistent neoliberal ideologues, nor puppets of capitalists. This general depiction is all well supported by the data in the book.

But the evidence presented in the book did not convince me that the particular actors and decisions identified in the narrative were, in fact, the crucial ones in the rise of finance.

In part, I think, I came away from the narrative unconvinced because the first part of the book had so thoroughly and effectively sold me on Krippner's preferred measures of financialization, which are all continuous variables. The

financialization of the American economy, as depicted in the first chapter of the book, thus appears to be a continuous trend that has been underway since the late 1960s. The bulk of the historical narrative, however, concerns discrete events—and most of them are discrete events that took place well after financialization was underway, such as the Depository Institutions Deregulation and Monetary Control Act (DIDMCA) of 1980, the 1984 decision to permit the Treasury to issue bonds in a form that was appealing for institutional investors, and the 1994 decision by the Federal Reserve to commit publicly to a specific interest rate target. There are certainly reasons to think that these decisions might have contributed to the further financialization of the U.S. economy. But they do not correspond to any obvious thresholds, turning points, or points of inflection in the graphs of financialization presented in Chapter 1. (Some of them—particularly those in the mid-1980s—*may* correspond to points of inflection in some of the graphs; at least, I first thought so, but on second viewing of the graphs I thought not, and now I think is hard to tell one way or another just by eyeballing a trend line. There is no clean or obvious discontinuity in the data. Perhaps some time series analysis could have helped to separate signal from noise, so that we could figure out which kinks in the graph are the turning points in the rise of financialization that are most in need of narrative explanation.) In effect the book offers us a turbulent and eventful narrative explanation of what appears to be a more or less continuous trend.

In part, too, I was not sure whether I agreed with the narrative explanation because at several key points I was not sure what sort of explanation was actually on offer. The book is generally silent on the relevant counterfactuals. For example, is the implied claim that the ratio of financial to non-financial profits would have risen less if, say, Congress had not passed the DIDMCA? How much less? (How much less would it have had to increase in order for us to say that our economy did not undergo financialization?) Such questions suggest themselves at several points in the narrative, at least to a reader like me.

But that is all just to say that *Capitalizing on Crisis* is a book well worth thinking hard about and arguing with. It should be seen as an agenda-setting

book, and I hope it will be. There is obviously room here for more research to help fill in the picture of financialization. This book convinced me that we need that research urgently.

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Figure 1
The changing ratio of financial-sector to non-financial-sector profits in twelve rich countries



Source: Author's calculations from OECD STAN database (see the text for details).

Comments by William H. Sewell, Jr.

University of Chicago

The first thing to say is that Greta Krippner's *Capitalizing on Crisis* is a splendid book—one from which I have learned an enormous amount. It takes on a controversial, complex, and technically difficult subject and develops a marvelously clear and convincing argument—all in a surprisingly brief compass. Her chapter establishing the quantitative outlines of the financialization of the American economy (which obviously took a lot of painstaking work) is extraordinarily clarifying. As far as I am concerned, this is the neatest, most irrefutable, and most definitive empirical demonstration of financialization we are likely to see.

She then proceeds to use that same laser-like intelligence to trace out the specifically *political* path by which financialization happened—the

legislative, regulatory, and policy decisions that led from the highly-regulated state-steered financial regime of the immediate post-war decades to the deregulated, free-

wheeling, market-steered financial regime that crashed so spectacularly in 2008. She disentangles and anatomizes in succession the piece-meal but cumulatively revolutionary deregulation of the US financial regime from the late 1960s to the early 1980s; the surprising but opportune flood of world capital into the US in the 1980s as a consequence of the Volker shock and its aftermath; and finally, the evolution of the monetary policy of the Federal Reserve toward increased reliance on markets from Greenspan's appointment in 1987 to the onset of the financial meltdown of 2008. The result is a political history of the rise of finance in the United States that is unlikely to be surpassed. It's a brilliant achievement.

I have nothing but admiration for the clarity and persuasiveness of this political story. But I do think that Krippner's decision to concentrate so single-mindedly on the purely American and purely political and regulatory story leaves some important questions unasked and some possibly important connections and contexts under-examined. The rest of my comments will attempt

to smoke out some of these unasked or under-examined questions.

Characteristically, Krippner indicates forthrightly what her explanatory strategy will be and contrasts it to previous explanations of the rise of financialization: those that see it as repeating the speculative dynamics that have plagued capitalist economies since the 17th century (think of Charles Kindleberger 1978); those that emphasize the development of a business ideology of "shareholder value" (think of Gerald Davis 2009 or Neil Fligstein 2001); and those that see financialization as a repeating phase that characterises periods of declining hegemonies in the capitalist world-system (think of Giovanni Arrighi 1994). The first two she criticizes for essentially ignoring the importance of politics in generating financialization. Krippner criticizes Giovanni Arrighi, who certainly emphasizes the importance of *global* politics in the periodic rise of

financialization, for being vague about the actual political mechanisms by which financialization comes about. It is these political mechanisms that she aims to supply for the

"[T]his is the neatest, most irrefutable, and most definitive empirical demonstration of financialization we are likely to see."

contemporary American case.

Krippner's strategy is quite familiar to me as a historian. It actually reminds me of the kind of strictly political history that dominated the field when I became a history major, way back in 1961. (At this time, social and cultural history were just beginning to emerge in American history departments.) The strategy of the political historians was to focus relentlessly on the doings of key political actors, bringing in the larger context only when and where it impinged directly on the central story. The result, when well done, was precisely the sort of clarifying and disentangling account that Krippner gives us in this case. It's worth noting that a common theme of such histories—as of Krippner's—is that the historical outcomes are rarely the consequence of any considered plan on the part of any of the protagonists, but rather emerge from the improvisations, blunders, and compromises of actors making the best of unmasterable circumstances. History, this seems to imply, is contingency all the way down, without any clear

direction or generalizable shape. It is a curious irony that a contemporary young sociologist impatient with the grand but all too vague claims of such disciplinary colleagues as Arrighi should turn to exactly the kind of narrow but sharp empirical investigation of politics that made me, as a young historian, wish to reach for the larger theories and generalizations I found in the social sciences. So, following out this unexpected crossing of generational and disciplinary paths, let me suggest some issues, most of them having to do with underlying dynamics of capitalism as a global social system, that seem to have escaped Krippner's political-historical net.

First, I was struck by how often in Krippner's account it was the effects of financial innovations that induced the political actors to change regulatory frameworks. Even in the highly regulated environment of the 1960s and 1970s, such novel financial instruments as certificates of deposit, Eurodollars, securitization of mortgages, money-market funds, or interest-bearing checking accounts seem to be constantly popping up, frustrating the regulators and forcing them to relax one regulation or another. Hyman Minsky (1986), whom I think Krippner dismisses rather too quickly, argues cogently that there is a tendency inherent in capitalism for financial entrepreneurs to continually develop new products that maneuver around existing regulatory frameworks. (Of course, we've been treated to particularly spectacular examples of this undermining effect of financial innovation in the past decade or so.) Although Krippner doesn't emphasize this aspect of her story, on my reading of her text, this seemingly perennial aspect of the historical dynamic of capitalism emerges as a significant driver of the political story she tells.

Second, Krippner insists that financialization was not a conscious creation of her political actors, but rather an unanticipated consequence of their decisions. This is an important corrective to the rather vulgar Marxist accounts of, for example, David Harvey (2005) or Gerard Duménil and Dominique Lévy (2011), who assume that it was capitalists who drove deregulation from the beginning. But granting Krippner's point, it seems clear to me that once the initial deregulation got under way, the capitalists who benefited from it used some of their rising wealth to increasingly rig

the political and regulatory system in such a way as to further reinforce their advantage—as Jacob Hacker and Paul Pierson's *Winner-Take-All Politics* (2010) brilliantly documents. If financialization was an unintended consequence of piece-meal efforts to deal with a rolling economic crisis, the creation of the 1 percent vs 99 percent society we Americans now live in was fully intended. (I'd also like to put in a plug for Lévy and Dumesnil as inspired data-hounds. I think they're among the few whose quantitative dissection of recent economic trends rivals Krippner's.)

Third, the world or international context of the American developments drops almost entirely out of sight in Krippner's account, except when some action from outside the United States directly requires the American actors to respond. Thus, Krippner mentions in passing the development of a Eurodollar market, which, in the 1960s and 1970s, made it possible for American businesses to get around the limits imposed on interest rates in US banks (p. 67). But she doesn't tell us that Eurodollars were only one instance of continuing financial innovation going on in European and world financial markets—innovations that were of course beyond the reach of US regulators but that formed a crucial and continually evolving context for American decision makers throughout Krippner's period. Even chapter 3, entitled "The Reagan Administration Discovers the Global Economy," tells us very little about what was actually happening in the global economy. Rather, it emphasizes that the Reagan administration learned—much to its surprise—that foreign (especially Japanese) investors would fund spiraling American deficits once Fed chairman Volker's tight money policy resulted in record high interest rates in the United States—thus launching the debt-fueled bubble economy that lasted right up to 2008. There is, for example, no mention of the dizzying rise of London's City (let alone Hong Kong or Singapore or the Cayman Islands) as a global competitor of Wall Street; no mention of the Plaza accord of 1985, in which representatives of the governments of the US, Japan, the UK, Germany, and France agreed to sharply depreciate the dollar against the Yen and the Deutchmark; hardly a hint that financialization was, by 2000, a characteristic of essentially all the advanced

capitalist countries, not merely the United States; no discussion of the rise of China and the other “emerging economies,” which have so sharply changed the balance of economic and financial power in global capitalism since the mid-1990s.

In her discussion of Arrighi in her introduction, Krippner indicates that she does not “take issue with the *theoretical* argument that financialization is a property of the world capitalist system” The problem with this argument, she indicates, is that it operates at too high a level of abstraction, making unclear the actual mechanisms that made financialization possible. Indeed, she affirms that Arrighi’s “notion that financialization offered a ‘solution’ to the crisis of the 1970s is an intriguing idea—and [she continues] *one that directly informs my own research*” (p. 13-4, emphasis mine). This led me to expect that once she had specified the mechanisms of the American case, she would cycle back in the conclusion of her book to Arrighi’s argument and give us some account of how her discoveries about the US case might help us to understand better the macro-evolutions that Arrighi traces. But in fact her concluding remarks remain entirely on the US scale—arguing that as a consequence of the 2008 meltdown, the US now must face up to the difficult political choices that financialization postponed for some thirty years, perhaps even forming some version of the “public household” that Daniel Bell (1973) had called for in the 1970s. But as desirable as it would be for the United States to get its public household in order, one wonders how far this would go toward solving the problems that face us and the rest of the thoroughly globalized world we now inhabit.

In sum, I would like to invite Krippner to use her accumulated knowledge of the US case and her laser-like analytical intelligence to say more about four issues: (1) the importance during her period and into the future of the long-standing tendency of capitalist financial systems to produce continuing financial innovations; (2) the successful efforts by very wealthy capitalists since the 1980s to rig the American political and regulatory system to their advantage; (3) the significance of changes in the world capitalist system for her story about American financialization and of American financialization for the dynamics of the world capitalist system; and (4) what the US might be able to do now, both domestically and as the still

hegemonic power in the capitalist world, to move the national and global public households in a more positive direction.

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Reply to Critics Greta R. Krippner University of Michigan

It’s extremely gratifying to have one’s work read so carefully by three such esteemed scholars.¹ I am very grateful to have received such perceptive and challenging comments, and I would also like to

¹ Jennifer Klein of Yale University was also a participant in the original author-meets-critics session held at the annual meeting of the Social Science History Association in November of 2011. I am grateful to Daniel Hirschman for valuable feedback on an earlier version of this comment.

express my appreciation to Ho-Fung Hung for organizing this exchange.

This may be one of those things that I shouldn't admit in public, but it took me a very long time to discover what this book is really about and also why I wanted to write it. This makes it extremely stimulating for me to learn what others think the book is about (and indeed to consider what it could have been about had I not written it in the way that I did). But before I engage my critics, I'd like to briefly describe the book's arguments as I've thought about them during the years I've been working on this project. Of course, it's easiest to note the things the book is not. As Bill Sewell observes, *Capitalizing on Crisis* is not a book about recent transformations in the global economy, nor does it offer a comparative study of

"It was Polanyi who observed that the attempt to sever the economy from politics in market society was a singular historical departure—a development that made the democratic forms of capitalism that were institutionalized in the twentieth century particularly vulnerable to episodes of crisis."

financialization across advanced industrial nations. It is not a book about the class politics undergirding the rise of finance. Perhaps most shockingly of all, it is not really a book about finance, although finance is its subject matter. Rather, this book is about the perennial tensions between democratic politics and market economies, and the way in which attempts to contain if not resolve those contradictions in the late twentieth century launched our society on a path that led quite inadvertently to the dramatic expansion of financial markets, with far reaching consequences that we are still coming to grips with as a society today. While there has been some speculation as to whether I am a Marxist or a Tocquevillian, I think this theme marks me clearly as a Polanyian. It was Polanyi who observed that the attempt to sever the economy from politics in market society was a singular historical departure—a development that made the democratic forms of capitalism that were institutionalized in the twentieth century particularly vulnerable to episodes of crisis.

More specifically, my book argues that the turn to finance in the U.S. economy in recent decades originated in the state's attempts to avoid

distributional conflict as the long period of postwar prosperity came to an end beginning in the late 1960s and 1970s. In this respect, the turn to finance—or financialization, the term I use in the book—can be regarded as a kind of successor to inflation. When robust growth in the American economy stalled, inflation initially served to disguise this development, allowing Americans to feel richer than they in fact were and thereby avoiding distributional conflict. But only for a time.² Eventually, the jig was up, and as

Americans' tolerance for inflation wore thin, policymakers faced the prospect of having to assume responsibility for directly allocating resources between competing social priorities. At each such juncture, policymakers made a fateful choice: they passed this unpalatable task to the market, first by deregulating domestic

financial markets, then by tapping into global capital markets, and finally by innovating new methods of implementing monetary policy that allowed policymakers to conceal their responsibility for unfavorable economic outcomes. In each such case, the political cover offered by the market also involved a loss of control over policy outcomes, unleashing a dramatic expansion of credit, as well as introducing a great deal of

² As Albert Hirschman (1980) observed, as long as inflation remained at relatively low levels, it served to dissipate distributional tensions. This reflected the fact that inflation created a game of "leapfrog" in which it was never totally clear who was winning and who was losing. For example, a trade union that obtained a favorable wage settlement from employers momentarily secured an advantage, until these higher wage costs translated into higher prices, eroding the real value of the goods and services that the wage could purchase. Once these price increases became generalized across the economy, workers whose real wage had decreased would push for another wage increase, and the process began again. This cycle could repeat endlessly, with each group securing only temporary gains, and yet the sequence of moves and countermoves tended to vent distributional conflict (see also Goldthorpe 1987). Of course, once inflation increased beyond a certain threshold, the consequences of price changes for distributional outcomes became clear, and inflation exacerbated rather than eased underlying social tensions.

volatility into the economy, both of which created propitious conditions for the turn to finance. In this sense, I suggest that the financialization of the U.S. economy was not a conscious policy objective, but an inadvertent result of the state's attempt to solve other problems.

Now, in response to some of the issues raised by my critics, I want to be clear that in putting forward this argument, I offer less an explanation for financialization than an *interpretation* of it. My purpose in the book is not to provide a tight causal account of the financialization of the U.S. economy, and indeed I am skeptical that such an account could be successful. As the growing literature on this subject has made clear, this is a multiply-determined phenomenon involving developments in markets and firms as well as the state actions that are the focus of the book. Given this complexity, Frank Dobbin would like to know how to assess the relative contributions of state policies and changes in managerial practices that have been the focus of most of the literature to date. I will disappoint Dobbin by acknowledging that I cannot conceive of a way to partition the variance between firm, state, and market in a rigorous manner. I'll also reveal my Polanyian colors again by suggesting that even were such an exercise possible, it would perhaps miss the point. While I've trained my attention on the state for purposes of analysis in the book, the actions of state policymakers and market actors were deeply intertwined as financialization unfolded: the changing regulatory environment shaped firm actions at every juncture (and was in turn shaped by them). I tend to refer to state policies as "creating conditions conducive to" rather than "causing" financialization not to be evasive, but precisely to acknowledge this intertwining.³ In short, my objective is not to argue *against* accounts that give greater attention to developments inside firms than I do, but rather to place such accounts on a firmer foundation by explicitly theorizing the state actions that made the reorientation of firms to

financial markets in the post-1970s period both possible and likely.

For related reasons, I am not troubled by the lack of a tight coupling between the policy changes that I indicate as most important and the empirical evidence that I present for financialization. But I do disagree somewhat with Isaac Martin's characterization of how the policy changes I highlight line up with the empirical evidence I present for the financialization of the U.S. economy. The book identifies three main policy changes as particularly consequential for the subsequent development of financialization: 1) the deregulation of domestic financial markets occurring over the 1970s and culminating in passage of legislation in 1980; 2) the growing dependence of the U.S. economy on foreign capital inflows to finance deficits beginning in the early 1980s; and 3) the radical change of course of U.S. monetary policy initiated with the so-called "Volcker shock" in 1979. In short, my narrative suggests that the key changes were in the 1970s and early 1980s, which on both measures I develop is consistent with the timing of the beginnings of financialization. Moreover, the most remarkable feature of the data I present in support of financialization is not each and every gyration in the two time series, but rather the evidence for a dramatic change between the structure of the economy in the 1950s and 1960s and the structure of the economy in the post-1970s period. Martin himself acknowledges this reading of the data when he notes that the financialization trend is basically continuous, but he worries that I explain a continuous change by referring to discrete events. I'm not sure I see the problem here: the discrete events I deal with in the book are policy changes that, *once enacted*, had durable effects on the structure of the American economy. In this sense, I do think the historical narrative and the empirical evidence are telling the same story.

Martin also would have liked me to exploit comparative evidence from other national economies in order to make my argument more convincing. While I find Martin's examination of the comparative data on financialization broadly informative, there are a number of reasons why I did not undertake the kind of comparative analysis Martin recommends. One is that the cases are not independent, limiting the usefulness of the causal

³ To avoid confusion, I should note that acknowledging the co-constitution of state and market actions in this manner does not amount to a claim that the *interests* of state policymakers and financial elites are identical. Indeed, I am critical of the lurking instrumentalism that I find in some of the literature on precisely this point (on which more below).

inferences that Martin would like to draw in support of my argument from the comparative evidence he presents. In pursuing financialization at home, the United States was reorganizing global capital markets abroad in ways that changed the terms on which other economies were integrated into global financial markets (and here I regret not attending more fully to these issues, as Bill Sewell observes in his comment). There is also the fact that firms in other countries likely emulated highly profitable American business practices once financialization was underway in the United States. Thus, the presence of financialization in other countries need not reflect the same underlying political economy that created conditions conducive to financialization in the U.S. case.

An even more fundamental reason that I did not undertake this sort of comparative analysis, to reiterate what I've said earlier, is that my interest is less in providing a tight explanatory account of financialization than it is in understanding how the turn to finance can be understood in light of domestic politics around distribution in the United States in the decades since the 1970s. For this, comparative evidence would be illuminating, but it would involve not a quick and dirty configuration of cases to test variables, but rather detailed (and admittedly old fashioned *pace* Sewell) historical investigations of the actions of key policymakers and how these actions intersected with domestic political developments. Such investigations I suspect would confirm Frank Dobbin's hypothesis that the particular response I describe to the crisis of the 1970s was a uniquely American one, although I have not myself delved into the relevant empirical materials to be able to assert this definitively. On this score, I can only plead exhaustion and hope that other scholars have more stamina than I!

This brings me to Bill Sewell's comments. Sewell observes, perceptively, that financial innovation is a perennial feature of capitalist economies and wonders why financial innovation doesn't get more explicit theoretical attention as a "driver" of financialization in my account. Sewell is correct that financial entrepreneurs are continually innovating around regulations, and this is indeed part of my narrative. The reason financial innovation remains undertheorized in my account is that to me the important question is not

whether innovation occurs—as Sewell observes, it is a constant—but when and why regulators give up the game, allowing the innovation rather than bringing errant innovators back under the umbrella of regulation. Stated somewhat more pointedly, the critical issue from my perspective is how the inflation of the 1970s—a response to unresolved distributional conflict in our society—made reining in financial innovations occurring during that decade politically unpalatable to regulators. That said, I do wish I had paid greater attention to one innovation in particular in my account—the development of securitization, which in hindsight assumes greater importance than my limited attention to it suggested. I will say that I am confident that had securitization been more central in my narrative, my overall argument would have only been strengthened. In this regard, Sarah Quinn's (2010) carefully researched account of the history of mortgage securitization is congruent with my argument, showing how securitization emerged out of the convoluted budget politics of the Vietnam War, allowing the state to sidestep difficult political choices in the context of a new era of austerity.

Sewell also wonders whether I am too quick to dismiss the role of finance capitalists if not in originating the policies I discuss than in reinforcing them once they were in place. Perhaps. But I think accounts of the rise of finance have often erred in the other direction by overstating the power of the financial sector. This is easy to do if one is looking at the size of campaign contributions coming from Wall Street, which are truly staggering. But what these analyses sometimes overlook is the internal differentiation of the financial sector. The financial sector is not monolithic and internal opposition between various sectors of the industry has in many instances undercut the ability of the financial sector to act as a coherent political actor. Take the repeal of the Glass Steagall legislation separating investment and commercial banking as an example. Repeal of Glass Steagall took over thirty years to accomplish—not because the financial sector did not devote an enormous amount of money to reforming banking legislation—but because investment banks and commercial banks, among other players in the industry, could not agree amongst themselves first

as to whether repeal should occur and then how repeal should be implemented.

My point here is not to deny that finance capitalists exert a great deal of influence over politics—I believe that they do, and their influence has almost certainly grown in the wake of the repeal of Glass Steagall. My purpose in writing the book as I did was to write about aspects of financialization that aren't captured very well from a narrow interest-based account. State actors had reasons for pursuing policies that created an environment conducive to financialization that had little to do with pleasing financial executives and everything to do with navigating the tension between maintaining democratic legitimacy and meeting market imperatives. Generally, policymakers have managed these tensions by “pulling forward future resources into present consumption,”⁴ whether through inflation, the turn to finance, or other kinds of “sequential displacements,” to borrow the term Wolfgang Streeck (2011) uses in his recent article in the *New Left Review*. But all such maneuvers are inherently self-limiting as they do not resolve the underlying distributional conflicts that give rise to these moves.

And so we find ourselves at the current moment confronting a crisis that is perhaps more daunting in its political than in its economic dimensions—as the spread of the crisis from the American mortgage market to Europe's sovereign debt markets makes abundantly clear. What to do, Sewell asks? The solution to our current difficulties, Sewell implies, must be constructed at the global as well as the national level, and I agree. Without a reconstructed international financial architecture that subjects international capital flows to some controls, there is no limit to the size of global imbalances that can build up in the system, fueling credit expansions and contractions that whipsaw national economies. But I would add to this that there is no technical fix, no matter how well conceived, to the underlying problems that led us to financialization. In this sense, an adequate response to our current quagmire requires attention to the normative underpinnings of market society. In particular, we must answer questions about who ultimately will pay the price of restoring weakened

economies back to health, and then questions about who gets what in societies that must live within more finite resource constraints than has been the case in the recent past. If there is a broad lesson here, it is that when markets substitute for politics, we are in trouble.

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⁴ Streeck (2011), p. 12.

Book Symposium (Mahoney)

Editors' note: James Mahoney's Colonialism and Postcolonial Development: Spanish America in Comparative Perspective (Cambridge 2010) was the subject of an Author Meets Critics session at the Social Science History Association meeting in Boston in November 2011. These are the revised comments from Mara Loveman, Nitsan Chorev, Richard Lachmann, and Dan Slater with Jim Mahoney's response. We would like to thank Richard Lachmann for organizing and guest-editing the symposium.

Colonialism and Postcolonial Development: Spanish America in Comparative Perspective
by James Mahoney
(Cambridge University Press, 2010)

Comments by Mara Loveman
University of Wisconsin – Madison

In his advance praise for *Colonialism and Postcolonial Development*, Timothy Wickham-Crowley calls it an “epic of a book.” Knowing Jim Mahoney and his previous work, I won’t say I was surprised to see this verdict. But it did have the effect of setting my expectations at the outset pretty darn high.

This is an extraordinarily ambitious book. It tackles a huge and fundamental question: Why are some former colonies so much better off today than others? Or put another way: What explains relative levels of development among post-colonial countries? The book aspires to nothing less than developing a general theoretical answer to this question that applies broadly across all cases, while also generating sufficient causal explanations for the particular outcomes of individual cases. The book treats no fewer than 15 countries – all the nation-states that emerged from former Spanish colonies in the Americas – minus Panama, Cuba, and the Dominican Republic – plus, as an

extension and preliminary test of the theory, the British Colonies and Portuguese America. The sheer volume of secondary scholarship mastered and synthesized as a prerequisite to the systematic comparative-historical analysis deployed to develop and refine the general theory is frankly rather mind-boggling.

A starting premise of the book is that to explain why post-colonial countries have uneven levels of development today requires locating the causes of their *initial* differences in levels of development (p.8). The focus on initial differences is warranted for the Latin American cases, the book argues, because relative levels of development around the time of independence have been extraordinarily persistent ever since.

To identify the original sources of uneven development in Latin America, the book tackles two primary explanatory tasks: First, it seeks to identify the factors that explain variation in levels of colonial settlement. Second, it seeks to identify the factors that explain variation in postcolonial levels of social and economic development.

In very broad strokes, the explanation for differing levels of colonialism rejects theories that focus only on geographic or demographic characteristics of the colony, on the one hand, or only on the characteristics of the colonizing power, on the other. Instead, the theory focuses on the institutional “fit” between the colonial power and the attributes and institutions of the colonized territory. The explanation for variation in post-colonial levels of economic and social development, in turn, focuses on the interaction between the prior level of colonialism and the political economy of the colonizing power. Through fine-grained comparative and historical analysis, these basic arguments are fleshed out to identify necessary and sufficient conditions for different levels of colonialism under different types of colonial powers, and for different consequences of level of colonialism for subsequent trajectories of development.

The theoretical argument advanced in the book also triggers a rethinking of the historiography of the individual cases. As just one example of this: the analysis of the Argentine case fundamentally revises the standard chronology of Argentine economic and social history, locating the moment of Argentina’s economic takeoff relative to rest of

the region in late colonial period rather than later in 19th century (129). This is quite a significant challenge to the received wisdom about the economic history of Argentina. Yet in the text this is noted more or less in passing; the novel insight is ‘tucked in’ along the way to development of the general theoretical argument. The stated goal of the book is to develop and test a general theory, not to reassess the historiographies of fifteen countries. But such historical insights should be highlighted as a major contribution of the book nonetheless. There are a fair number of these intriguing and provocative historiographic nuggets nestled into the narrative. (It’s almost as if they were deliberately planted there for others to come by and pick them up later, to follow wherever they may lead.) At times the truncation of the historical narratives leaves the reader with curiosities unsatisfied. But the omission of much historical detail is clearly not an oversight; it’s a sign of the author’s remarkable restraint.

This restraint was of course critical to the task of developing a *general theory* of the relationship between colonialism and post-colonial development. Even readers who are wary of the very term ‘general theory’ will be compelled to acknowledge that the argument is masterfully crafted and difficult to refute. The analysis is clear, parsimonious and compelling. The pieces of the puzzle all fit neatly together (though maybe just a bit too neatly, which is an issue I’ll return to momentarily). Counterarguments are identified and addressed. The story is very much path determinant, yet there is room for some historical contingency, especially for a subset of “underdetermined” cases. This is major contribution that changes how we think about the relationship between colonialism and long-run social and economic development.

And yet, some nagging questions remain.

I suspect these are things that Jim has already thought of and thought through in the process of writing this book. In which case, I’m interested to hear how and why he chose to deal with each of these issues in the way that he did.

“Even readers who are wary of the very term ‘general theory’ will be compelled to acknowledge that the argument is masterfully crafted and difficult to refute.”

1. Does the theory hold up if you take regions or provinces as cases instead of nation-states? The units of analysis in the models are the countries that came into existence as such after independence (with some borders determined later). Each country is coded as ‘core’, ‘semiperiphery’ or ‘periphery’. Yet within each country, as recognized in the narrative, you can also identify ‘core’, ‘semiperiphery’ and periphery.’ Indeed, this is done for the case of Brazil, and in a preliminary way for the case of Guayaquil in Ecuador. It seems like the general model of the relationship between level of colonial settlement and post-colonial development should hold for units smaller than countries. Indeed, wouldn’t this be a more rigorous test of the theory than extending it to other empires? Or if not necessarily more rigorous, still a very useful test that could help tease apart the

institutionalist-materialist causal factors from the geopolitical ones? Was the decision to stick with countries as units of analysis purely a pragmatic decision, driven by the types of data available? Or was the decision also theoretically driven? Does the nation-stateness of the cases

matter for how we understand the links between colonialism and post-colonial development? Put slightly differently, does it matter for the theory that the boundaries delineating the units of analysis are not just geographic/territorial boundaries, but *political* boundaries and political boundaries of a very specific – *national* – kind.

2. A related question is whether the theory presumes or requires us to treat the cases as independent. Is it possible that the relationship between cases at a given moment in time is itself a ‘factor’ that gets set early on, and then contributes to the stability of the relative positioning of cases thereafter? If we made the relations between pairs or triads of countries themselves the units of analysis, how might the story change? Further, how does the current model equip us to consider the relationships between the cases and other significant actors that are not currently an explicit part of the theory – like the interventionist U.S., which played a much more direct role in shaping

development possibilities and cutting off possible alternative trajectories in some countries than in others.

3. What is the role of ethnicity or race in the general theory? In the first stage of the argument, ethnicity is construed primarily as a demographic matter – e.g., a large number of indigenous people in country X at a given moment favors the development of labor-intensive extractive industries relative to more sparsely settled regions. The size of precolonial indigenous populations matters because of what it reveals about relative levels of precolonial institutional development and prospective “fit” with institutions imposed by colonizers. In the second part of the argument, the significance of ethnicity for the theory seems not so much demographic as cultural: it is the cultural gulf between European-descendent populations and indigenous or African-descendent populations (or more directly, the racist disdain of most of the former towards the latter) that puts the brakes on any momentum to extend social development benefits beyond elite sectors of the population. Of course, it’s possible for race/ethnicity to play a role in the argument as both a demographic and cultural factor without contradiction. But the different ways of construing the significance of ethnicity for the general theory are never explicitly laid out as such. I’d like to better understand how Jim thinks about the relevance of racial or ethnic distinctions in the Americas for his general theoretical argument. Is it the demography of race/ethnicity that matters as a factor in the explanation of variation in long-run outcomes? Or is it the social organization of culturally different populations, and/or the prevailing ideologies of race, that determine the material significance of demographic conditions for long-term development? What are the underlying processes or mechanisms through which racial or ethnic differences – or beliefs about such differences – play a role in the causal argument?

4. At the end of the day, who are the agents in this story? One of the things I really like about the argument and approach is how collective actors are conceived. I appreciate the historical institutionalist approach to thinking about who the collective actors are in this story and where they come from:

“a historically grounded institutional theory of colonialism and development needs to examine how specific institutions and institutional complexes put whole groups of individuals in similar positions vis à vis the flow of resources. From these common positions, collective actors are born. These actors may then become critical forces in shaping productive activity and development outcomes, even long after the demise of the original institutions from which they were first assembled” (p.20). The key actors in the model and their interests do not pre-exist the institutions and social relationships that constitute them. Collective actors and their interests are constituted by the institutional environment. Thus it becomes critical to pay attention to the political economies of colonizing powers at the time a given territory is colonized, because this will decide the kinds of institutions implanted in the colonies (p.23). And critically, the institutions in place – including the rules of the game – will in turn shape the collective actors whose orientations and behaviors determine not only the short run prospects but also, Jim argues, the long-run fate of the colonies.

The pivotal collective actors in the model turn out to be the merchants: it makes all the difference for long-run development whether the institutional environment at the time of colonization constitutes and supports entrepreneurial, free-market merchants (liberal colonizers) or monopolistic, resource hoarding merchants (mercantilist colonizers). I sometimes thought this institutionalist understanding of collective actors wasn’t taken far enough. In the model, the character of the merchants – liberal or mercantilist – is determined by the character of the colonizing power at the time. This keeps things neat – there are two basic kinds of merchants. But the reality was likely more messy: the interaction of the colonizing power’s political economy and existing colonial institutions would no doubt yield a continuum of hybrid liberal-mercantilist merchants, with varied ties to other elite actors like government office holders, religious authorities, and landed elites, generating competing and sometimes contradictory interests. The dichotomous characterization of the key collective actors in the model does not really seem to suffice.

A second way in which I would have liked the institutionalist conceptualizations of collective

actors to go further is in the treatment of the ‘subordinate’ collective actors: indigenous peoples, and in some cases African-descendants. Jim writes: “The institutional creation of subordinate and elite actors ... is relevant because these were the collective forces who shaped development outcomes later in the colonial period” (54). The exploitative relations with colonizers is understood to *constitute* indigenous communities as collective actors. In contrast to the developmental or obstructionist merchants, however, the subordinated collective actors, once constituted, do not end up doing much ‘acting’ in the model. They feature more prominently as ‘givens’ at different stages in the causal story. Though there are a few exceptions, for the most part, they are conceived as one of the factors that constrain or enable the initiatives of the dominant collective actors. Treating these groups as true collective *actors* would entail incorporating consideration of their interests, motives, and outlooks alongside those of the history-making merchants. This could include a more rigorous consideration of how the interests of dominant *and* subordinate collective actors were constituted, at least in part, through interaction or negotiation or conflict with each other. In sum, a more robustly relational conceptualization of collective actors seems a natural and productive extension of the institutionalist perspective adopted for the analysis. Such an extension could provide more leverage to explain variation in levels of social development across countries with large indigenous populations post-independence. Such an extension might also contribute to developing a more satisfying explanation of the ultimate trajectory of ‘underdetermined’ cases, like Chile, Paraguay and Costa Rica. (The current factor highlighted as pivotal in those cases – war – seems rather ad hoc and insufficient to hold the weight of the argument in that portion of the account).

Again, I suspect Jim has already pondered these issues and made deliberate choices in resolving them. So I raise these questions largely out of genuine interest to learn how he thought about these and related decisions in the process of writing the book. To craft such a powerful, parsimonious and compelling argument out of the chaotic mess of historical reality for fifteen countries over hundreds of years necessarily means many paths

were left untaken. I’d like to hear Jim discuss some of the decisions he made as he waded through the massive historiography – how he decided what was foreground and what background, and what could be left out of view altogether. Were there indeed paths not taken? Were there others you traveled down for awhile only to retreat part way (and at what cost)?

I’ll finish by quoting an opening line from the preface of the book: “Comparative-historical analysis achieves its potential when it generates new theoretical insights of broad utility *and* novel understandings of particular cases.” Jim has shown us by example what it looks like when this potential is fully realized. It is an extraordinary accomplishment. *Colonialism and Postcolonial Development* is indeed an epic of a book.

Comments by Nitsan Chorev Brown University

In *Colonialism and Postcolonial Development*, Jim Mahoney has generated an impressive and novel theory regarding the impact of colonial legacies on countries’ levels of development. This is a general theory, but drawing on a plethora of evidence provided by 15 case studies of Spanish colonialism in Latin America, the argument is remarkably sensitive to variation, including the identification of distinct paths of development.

Given the complexity of the theory developed in this beautifully crafted book, this review simply identifies the five central arguments and responds to each of them individually.

First, the type of the colonizing state matters for the prospect of future development. Mahoney argues that different colonial states established different types of institutions, which had a long-term effect on the territories’ subsequent economic and social development. Concretely, he differentiates between mercantilist colonialists (like Portugal) and liberal colonialists (like Britain). The type of colonialism practiced by the same state could also change over time. Crucial for Mahoney’s analysis, Spain was mercantilist under the Habsburg monarchy, until 1700, but turned liberal under the Bourbons.

Whether a society was colonized by a mercantilist or a liberal colonial power mattered greatly because, according to Mahoney, mercantilist institutions *inhibited* development while liberal institutions *encouraged* development. Mahoney explains that this was not due to the institutions *per se* as much as it was due to the power configurations and elite compositions they created. Mercantilist institutions established merchant and landed elites with vested interests that were in conflict with policies that would encourage development. Liberal institutions, in contrast, created commercial business elites with vested interests that, on the contrary, supported the needed policies.

Here, Mahoney rightly emphasizes the constitutive role of institutions in making elites and he then assigns the responsibility of historical change to those actors rather than the institutions that have created them. This “bringing the actors back in” approach is an important contribution to institutionalist analysis. Historical institutionalists argue that institutions make elites; Mahoney adds the significant insight that it requires labor for those elites to maintain their position. This argument could have been further enriched if Mahoney also analyzed the political and economic strategies that enabled the elites to maintain their power over decades, even as old institutions deteriorated and were replaced with new ones. Especially in conditions of failed development, what allowed merchant and landed elites to maintain their domination and not be replaced with (or, in other cases, become) a commercial elite?

Mahoney’s second argument is that colonizers did not pay equal attention to all territories when they established these institutions. Institution building was more intense in some places but less intense in others. As a result, colonized societies can be differentiated based on whether they were at the center, semi-periphery or periphery of the colonizing project.

What factors affected the level of colonialism of a given territory, namely, whether it was part of

the center, semi-periphery, or periphery? Mahoney shows that one necessary condition for developing institutions at the core was *existing* institutional conditions. Mercantilist colonizers preferred territories with highly differentiated institutions, which made it easier for them to collect tribute and to exploit available labor for resource extraction. Liberal colonizers, in contrast, preferred territories with sparse population and proximity to ports.

The suggestion that the level of colonialism depended on the institutional conditions in place is an important contribution to the analysis of colonial expansion and to economic expansion more generally. There is an understandable tendency in the literature to focus either on the characteristics of the external powers (e.g., Spanish vs. British colonizers, American vs. British empires, Chinese vs. Indian foreign direct investment and so on) *or* on the characteristics of the affected societies (e.g., geographical conditions, mineral wealth, or size of the population). The interplay between the two dimensions is a particularly fruitful analytical strategy that may resolve many questions regarding diversity in the types of intervention and, as Mahoney suggests, diversity in the outcomes of interventions as well.

Given the theoretical potential of such an approach, it is possible that Mahoney’s analysis does not go far enough. While referring to the “interplay” between the interests of the external powers and the local conditions in place, the analysis provides little attention to the impact of institutional and other original conditions *once the level of colonialism is established*. The narrative offers, then, less a genuine analysis of an interaction and more an identification of mediating factors that influenced the decisions of colonizers. As soon as colonizers decided on a level of interest in a given place, the colonized societies more or less disappear from the analysis. But it seems likely that the conditions in place have continued to play an independent role in the development trajectory of a territory even after the new institutions were put in place. (Mahoney suggests as much in his

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analysis of social development, but not in his analysis of economic development, as I discuss below).

According to the third argument, because not all colonized societies experienced the same level of institution building, the negative or positive effects on economic development – themselves based on whether colonial institutions were mercantilist or liberal – were not experienced to the same extent by all societies. Because mercantilist institutions had negative effects on economic development, high levels of colonialism in the core territories resulted in low levels of development. In contrast, low levels of mercantilist colonialism in the periphery kept the possibility of development open. In turn, because liberal institutions had positive effects on economic development, high levels of colonialism resulted in high levels of development. Low levels of liberal colonialism did not have the same development outcomes.

This is a beautifully crafted argument. However, while Mahoney presents it as useful for explaining the trajectory of all colonized societies, independently of whether they experienced low or high levels of colonialism, it necessarily works better as an explanation for cases of high levels of colonialism than for cases of low levels of colonialism. In cases of intense colonialism, the institutions that were put in place by the colonizers created the elites that later determined the trajectory of development. In cases of low levels of colonialism, however, no such institutions were established, and the possibility for development could not be determined by the colonial legacies but was rather left open.

This explanatory imbalance is partly disguised by the fact that Spanish colonialism offers a hybrid type of colonialism, starting with mercantilist institutions and then layering, on top of that, liberal institutions. This means that the fate of some of the colonized societies with low mercantilist institutions was determined by the fact that liberal institutions were later established. It is only in cases in which neither mercantilist nor liberal institutions were established that the possibility of contingency is explicitly revealed. More generally, the hybrid nature of Spanish colonialism means that we cannot really find the possible outcomes of “pure” Spanish cases. (The last empirical chapter,

on Portuguese and British colonies, partly compensates for that).

In turn, Mahoney concludes that the presence or absence of wars best explains the trajectory of development in the “contingent” Spanish cases – namely, cases in which colonized societies had low levels of both mercantilist and liberal institutions. There’s a surprising theoretical disconnect between the path-dependent and institutionalist emphases of the overall argument and the treatment of wars as an exogenous force. The theoretical framing is already complex, but I still wish that Mahoney incorporated into it a view on the origins of these wars, particularly the role of precolonial and colonial legacies – including institutions and elites.

The fourth argument moves from asking about the institutional origins of economic development to the institutional origins of social development. The analytical distinction between the two types of development as potentially independent of each other is important. I was left with some questions, however, regarding the empirical conclusions. According to Mahoney, social development depends not just on the level of economic development achieved, but also on the size of the indigenous population. The sparser the indigenous population, the higher the level of social development. This argument raises two questions. First, since the size of the population also plays an important role in Mahoney’s explanation of economic development, it may undermine his attempt to offer an independent explanation to the origins of social development. According to Mahoney, the size of the indigenous population had a positive effect on the level of mercantilist colonialism, but a negative effect on the level of liberal colonialism. Hence, sparse indigenous populations led both to more economic development and to more social development even if different mechanisms were involved. Moreover, the direct role that Mahoney assigns to the size of population in explaining social development raises the question of whether it is entirely justified to limit this “size of population” factor in explaining economic development to its impact on the type of institutions in place. The second question raised by the argument on social development concerns the actors identified as contributing to economic and to social development, and therefore the mechanisms linking colonialism to later outcomes. According to

Mahoney's account, elite interests explain economic development, while the indigenous population and the presence of ethnic conflicts serve to explain social development. From a political-economic perspective it seems likely that elite interests would have an impact on social development and that the size of the indigenous population, and certainly the presence of ethnic conflicts, would affect economic development.

Finally, according to the fifth argument, the long-term implications of colonial institutions are remarkably enduring. According to Mahoney, the only factors that could lead to change in a country's relative level of economic development are "ruptures" such as civil wars, revolutions, and, at least in the case of Venezuela, the discovery of oil. As I mentioned earlier, Mahoney does not suggest that the endurance of the outcome is due to the endurance of the institutions that made them happen. The institutions in place have certainly changed. Rather, it is due to the capacity of the elites to maintain their privileged position in the economy. However, this "path-dependent determinism" could be questioned. On the one hand, the general argument of the book suggests that colonial institutional arrangements lock countries into a particular level of relative development (although not absolute levels of development). On the other hand, as Mahoney suggests in the conclusion, whether certain institutional arrangements lead or do not lead to a high level of economic development depends on the larger global economic context. So, for example, mercantilism worked during one period, liberalism during a later period, and, following liberalism, state-interventionism was the established road for national economic success. This leads to a puzzle. If states manage to maintain their relative economic position across different types of "global economic contexts," it suggests that states manage to move from one economic strategy to another. This also suggests that if the existing elites stayed in power, they must have transformed their economic strategies or otherwise would not have been able to maintain their relative position in the world economy. This seems to somewhat contradict the notion that the long-term implications and legacies are enduring; certainly it at least requires clarification as to what gets endured that permits countries to maintain their

relative economic position but prevents them from changing their *absolute* economic position.

These five arguments offer a remarkably rich way of thinking about colonial legacies as well as the role of exogenous factors in influencing countries' economic and social development more generally. Maybe most importantly, the book suggests that scholars should look at the internal diversity of categories of foreign influence (not all colonial powers are the same, not all empires are the same, not all types of foreign direct investment are the same, etc.) and that scholars consider how even the same type of foreign influence may have different effects at the local level depending on the existing conditions in a given setting. These are important lessons, and a very good reason for scholars, including non Latin-Americanists, to read this book.

Comments by Richard Lachmann University at Albany, SUNY

Jim's book is designed to explain why some former Spanish colonies achieved relatively high levels of development after independence while others did not. His great innovation is to show systematically how the complexity of precolonial institutions affected the nature of colonialism and hence the degree of development under colonialism and then after independence. However, this book does more. It traces the development of each territory colonized by the Spanish through four stages: the social institutions right before the Spanish conquerors arrived, the structure created in the first, mercantilist phase of Spanish rule, the somewhat altered state of colonial government under liberal Bourbon rule in the eighteenth century, and finally the post-independence social order. Jim's careful reading of each country's history led him to the realization that for some countries, most notably Chile and the countries of Central America, there was yet another moment of structural transformation caused by nineteenth century wars. Jim explains how war allowed Chile and Costa Rica to make developmental strides that would not have happened in the absence of war, although in very different ways. In the case of Costa Rica its insulation from wars in the rest of Central America allowed for the consolidation of

liberal government. For Chile war was both an economic stimulus and the way in which it added lands rich in nitrates to its territory. For the rest of Central America war brought reactionary elites to power that retarded economic and social development up the present and into the foreseeable future.

For the other Spanish American countries, their relative positions were locked in after independence. Argentina's famous decline from one of the richest countries in the world in 1920s to its current positions at well below the levels of even the poorest Western European countries is in line with the overall decline of Spanish America in those decades. Argentina and Uruguay remain at the top of the Latin American hierarchy, just where they were 100 years ago. Their long-ago trajectories as Habsburg mercantile peripheries that became core liberal colonies of Bourbon Spain still shape their continental and world positions and their levels of social development.

Jim writes: "Level of colonialism is important in its own right as an outcome to be explained. But in this study, level of colonialism is also of interest as a cause of postcolonial development" (p. 27). I will leave it for others to comment on Jim's analysis of postcolonial development. I will focus on Jim's explanation of levels of colonialism, and especially on his analysis of the transition from mercantile to liberal colonialism.

Mercantile colonialism established a particular type of polity, a system of exploitation that used coercive methods of labor control to extract raw materials (most importantly precious metals, but also agricultural products). The specifics of labor control varied depending on the social structure the initial conquistadors encountered. While the system of rule varied, the ruling colonial elite shared a crucial characteristic across all the colonies: a tight linkage, which in practice amounted to a fusion, of officials, clerics, landlords, and merchants. What varied across colonies was the size of the elite and how firmly they were able to embed themselves in the conquered societies. The bigger, richer, more settled, and more complex the precolonial society, the more deeply the mercantilist colonial elite was able to plant itself. Where the elite was spread thin, as in peripheral Argentina, the Bourbon crown found empty spaces – both geographic and

structural – in which it could insert new liberal elites. However, where the mercantile elites were dense neither the crown in Spain nor indigenous peoples in America had any real openings to challenge them.

Here Jim goes a long way to solving the central mystery of colonialism. How did a few thousand Europeans dominate millions of Americans, Africans and Asians? How could Europeans extract resources from and exploit labor in colonies with only the limited number of personnel and relatively weak military forces available? The usual answer, which others have made about British India and Africa, is leverage. Europeans enlisted local rulers to do their dirty work and for the most part relied on existing systems of revenue extraction and labor control. Jim goes well beyond that existing answer, which is presented either at such a general level that it doesn't say much more than the superficial summary I just offered, or is so steeped in the details of a particular case that it can't be used to understand variations among colonies. What Jim has done is find a systematic way to differentiate precolonial societies and use the differences among them along several dimensions to explain different Spanish and, in a comparative chapter, British and Portuguese strategies for controlling the peoples and lands of the colonies the Europeans conquered. In essence, Jim argues that differences among types of colonialism or among forms of imperial rule are made when colonizers arrive at and conquer indigenous peoples. They are not shaped beforehand by metropolitan politics and culture.

The structures of rule created in the first moment of colonization mattered, as Jim shows for subsequent economic development, and also for the room imperial rulers back in the metropolitan capital enjoyed to restructure colonial rule. This matters for the transition from mercantile to liberal colonialism. Jim finds that the stronger the mercantilist rule in a colony, the more entrenched the elite, and the less effect Bourbon reforms had. Only in peripheral regions were the Bourbons able to create new liberal merchants elites that made possible rapid development, most notably in Argentina, which had been only barely colonized under the Habsburgs.

In this way, Jim makes an important contribution to the long debate, among historians

of Spain, over how much the transition to Bourbon rule in the 18th century mattered. That debate until now has been largely about Spain itself. The dominant view is that liberal reforms didn't amount to much within Spain and therefore had little effect in the Americas. Jim challenges that view. His main argument is that liberal reforms had a varying effect. Where colonial elites were few and far between, and especially where there were not dense complex native polities to which they could connect their rule, the Bourbons back in Spain had room for maneuver. They could set upon new colonial centers, empower new corps of merchants, and stimulate economic development, as in Argentina.

My one criticism of Jim's analysis is that he gives too much weight to Bourbon bureaucratic reforms. Intendants, as we see in Jim's specific accounts of each colony, had widely varying effects. Their bureaucratic organization, administrative training, and relative incorruptibility in fact accomplished little. Spain disrupted or bypassed old elites less through administrative fiat and more by finding geographic locales where old elites were weak and creating opportunities for new elites to form. Where the old elites were tightly integrated, intendants and new merchants either withered or ended up in business as junior partners to the old mining-landlord-clerical-administrative oligarchs.

This book explains in a more rigorous way than ever before why it is so difficult for lands with dense, advanced polities that were colonized by Europeans to ever escape from a peripheral position. Mercantilist core colonies never could achieve higher economic development because during their time under mercantilist rule, elites were established that could not be eliminated by liberal reforms. Liberalism mattered ultimately mainly for the former peripheries, by creating an opening for a new commercial elite in Argentina. For much of the colonial world, liberalism arrived too late.

Comments by Dan Slater

University of Chicago

There are basically three kinds of books in the social sciences. More than 99% of them are what I

would call, for lack of a better term, *ordinary* books. And that's OK. These books tell us something new or provide new evidence about some domain of the social world. Luckily the social world is boundlessly fascinating, so a great many of these books make major and lasting contributions in their domain of interest. Even books that don't last long or don't attract much attention add to our stock of social knowledge as we navigate a complicated and tumultuous world. They also allow most of us to find lasting employment in the academy. Thank goodness we have so many ordinary books.

But I want to focus my remarks today on the other two types of books, both of which belong in the less-than-1%. These are truly landmark books, which raise the bar for us all, which are impossible to ignore, and which will rest at the center of our bookshelves and be assigned in seminars for a very long time to come. In case it wasn't already obvious, I would put *Colonialism and Postcolonial Development* (or CPD) in this less-than-1% category, without hesitation. (Not to worry, though. I'm not threatening to unleash an "Occupy Mahoney" movement. This is the kind of 1% that the 99% wants to emulate, not expropriate.) In a word, CPD is extraordinary – that is, extraordinary. My colleagues in this symposium have been telling you at great length why.

I plan to take a slightly different tack here, however. I wish to ask: *what kind* of extraordinary is CPD? In my opinion, the key dividing line among the tiny subset of truly extraordinary books lies in what they strive for. Some strive for *greatness*. Others strive for *perfection*. Even before rereading Jim's acknowledgments, which conclude with an endearing confession of chronic, paternally-inherited perfectionism, it struck me that CPD is as close to a perfect book as I can recall reading. But I want to argue that it is closer to being a *perfect* book than it is to being a *great* book. I also want to argue, however, that – despite the obvious and inconvenient constraint that the book is already in print – it is not too late for Jim to make CPD a greater contribution than it already is.

Rather than distinguishing perfection and greatness in the abstract, let me offer a concrete example. Jim's favorite book, in fact: Skocpol's *States and Social Revolutions*. Looking back, I doubt that very many of us would deny that this is

a great book – even one of our greatest. It offered a new theoretical perspective on one of the biggest questions of our age, or any age. It made a profoundly provocative argument. But it was also far from perfect. It ignored a lot of important things. It was full of holes and unanswered questions. People couldn't wait to argue with it, to build upon it, or to tear it apart (probably in some cases literally). But nobody could even dream of ignoring it, if they wanted to talk about revolutions. For all its assertive and self-confident tone, *States and Social Revolutions* reads more like the first word in an argument than the last word.

But the book was also stifled by a certain perfectionism. It tried very hard not to be wrong, or to cross into terrains where it might be proven wrong. It set very tight scope conditions around its arguments. Only great social revolutions, only non-colonized agrarian empires, etc. But then something interesting happened. Several things, actually. Iran. Nicaragua. People Power. And rather than bunkering down behind her restrictive initial scope conditions, Skocpol came out to fight – and to play. She extended her argument into new empirical terrains, adjusting but never abandoning her state-centric theoretical approach. She smacked Sewell back when Sewell smacked her, but she also started taking ideology more seriously as a structural variable. This is why I find it more fun today to read her *Social Revolutions in the Modern World*, circa 1994, than her original book, circa 1979. As time went on, Skocpol abandoned the quest for perfection, and in so doing produced a theoretical armature that was even closer to true greatness. (With a fair amount of help from the great Jeff Goodwin, I might add.)

So what about Jim? What about CPD? This book's perfectionism can be seen on every page in its impeccable analytical craftsmanship. It leaves no stone unturned. It modestly accepts the limitations of its central framework. It does not try to convince you that types and levels of colonialism neatly explain much more than half of Spanish America's 15 cases. It "takes sides" at a metatheoretical level, in Jim's insistence that institutions are distribution devices rather than coordination devices (p. 15). But as I will detail in a moment, it does not "take sides," or even totally come clean on whether he thinks one should or

should not take sides, on some of the biggest theoretical questions at the heart of his masterwork.

In short, it is not the kind of book that makes me want to yell out: "I disagree!" As Mara Loveman rightly put it, "the argument is masterfully crafted and difficult to refute." Indeed, I am literally unsure whether Jim is wrong about *anything* in CPD. Like Mary Poppins, this book is practically perfect in every way. Yet this perfectionism comes at the cost of avoiding some dust-ups: the kinds of dust-ups that made Skocpol's work on revolutions less perfect but more great. As a result, there is a way in which CPD reads like the *last* word in an argument, not the *first*.

I don't suspect that Jim would intend such a thing. So in the remainder of my remarks, I would like to point to three areas where I would love to see Jim "take sides," or tell me why we have been thinking about "sides" in all the wrong way. To my mind the main and perhaps only significant flaw in CPD doesn't lie in anything Jim says, but in some big theoretical conversations it either elides or misses. In his future work on colonial legacies and development, I would love to see Jim do what Skocpol did, and help us all become more engaged theoretical conversation partners.

The first two conversations relate to development. Development is both a process and an outcome, but CPD primarily treats it as an outcome. Ironically, the "postcolonial development" in Jim's title does not refer to how postcolonial countries develop at all. It refers to the legacies of *colonial* development as a process on postcolonial *levels* of development as an outcome.

This begs the question of why countries develop, in general. In the context of colonial Latin America, Jim sounds an awful lot like Douglass North. Property rights are the key. Liberalism is the answer. Recall Richard Lachmann's big takeaway line, that liberalism simply came too late to most of the colonial world. Considering how much ink and even blood have been spilled over the relative importance of states and markets in national development, this claim is enormously consequential, if it's indeed what Jim means to say. But it's not as clear as it should be, since CPD ultimately embraces a kind of radical equifinality in which markets seem to be the key in the 17th and 18th centuries, and states seem to be the key in the

20th and 21st. Would even Gerschenkron draw so sharp of a temporal distinction? In sum, I would love to hear more about how Jim's mountain of historical evidence might prompt us to reassess our theories of property rights, state intervention, and development – by which I mean the *process* of development.

More could also be said about CPD's lessons for theories of the rentier state and "resource curse." Understandably, Jim is more concerned with assessing the direct role of mineral wealth in shaping levels of colonialism than levels of development. But of course there are reams of works (if perhaps no great works) that inquire into the effects of resource wealth on economic diversification and growth. This exact theme pops up repeatedly in Jim's case studies. CPD cites some of this literature, as it cites North on property rights, and cites an array of scholars on developmental states. Yet broad citation cannot take the place of deep conversation. Much as Mara Loveman saw "nuggets nestled in the narrative" on particular cases, I saw a similar pattern in how Jim engages with these key literatures. I am ultimately left too unsure whether the Spanish American experience broadly supports, negates, or complicates these theories, and how.

My third and final plea for deeper theoretical conversation is the one I'm most surprised I need to make. So, a quiz question for the reader: what great scholar's "research tradition" did Jim so brilliantly review in his edited volume with Dietrich Rueschemeyer? There were several, but the most memorable is Barrington Moore. No one knows Moore better than Jim. So where is Barrington Moore in CPD? The case studies and even the theory discussion repeatedly invoke the distinction between commercial elites and their anti-commercial landed brethren. Yet I could find Moore nowhere in the endnotes, which number nearly a thousand.

But Moore is everywhere in the text itself. Especially on the point where Jim is maybe most cautious about theorizing, on the "mechanisms of perpetuation" through which developmental legacies persist (see p. 227). By my reading, the key mechanism in CPD is what Jim calls

"entrenched mercantilist actors" (50) or "powerful mercantilist coalitions" (118). If Moore, like Marx, thought bourgeois revolutions required "sweeping away the feudal rubbish," Jim is arguing that development requires sweeping away the *mercantilist* rubbish.

Might this be why war, revolutions, commodity booms, and state power all deflect developmental trajectories? Because they sweep away the mercantilist rubbish? Or to be more precise: they only deflect developmental trajectories *when* they sweep away the mercantilist rubbish? In this light, these additional factors would not be fully alternative explanations to Jim's, but additional historical forces that unleash his core causal mechanism. In Nitsan

Chorev's terms, this would reduce the "analytical detachment" between his original explanation and these subsidiary claims. Does this unifying explanatory

maneuver make sense, or go too far? I think it could make for a terrific debate.

So, to conclude: Books *anticipate* objections when they are trying to be *perfect*; they *incite* objections when they are trying to be *great*. CPD anticipates brilliantly, but it incites insufficiently, at least for my taste. If Jim's own legacy in the study of colonialism is to transcend the tremendous achievements of CPD itself – as we all must certainly hope and anticipate – there is still ample opportunity for Jim to complement his father Elmer's obsession with perfection with Skocpol's yearning for greatness.

Reply to Critics James Mahoney

Northwestern University

Historical Explanation and Theory Development: Reply to Loveman, Chorev, Lachmann, and Slater

"Comparative history grows out of the interplay of theory and history," Theda Skocpol once noted, "and it should in turn contribute to the further enrichment of each." My work on *Colonialism and Postcolonial Development* was written with

"Books anticipate objections when they are trying to be perfect; they incite objections when they are trying to be great."

the goal of both enriching our general theories and advancing our historical understanding of colonialism and its long-run developmental legacies for particular cases. How well I achieved these goals is the subject matter of the preceding commentaries from Mara Loveman, Nitsan Chorev, Richard Lachmann, and Dan Slater. I am grateful to each of them for sharing their views on the ways in which I have – and have not – succeeded in advancing new theoretical agendas and generating valid explanations of processes of colonialism and postcolonial development as they actually occurred historically.

Mara Loveman provides the kind of comments that an author of a book can only hope to receive. Her discussion of *Colonialism and Postcolonial Development* is sophisticatedly appreciative, and she praises aspects of the book that I like best too. Loveman views the theory as “clear, parsimonious, and compelling,” yielding an empirical argument that is “difficult to refute.” She applauds the aesthetics and craftsmanship of the book as well as its engagement with vast secondary literatures. She calls attention to and sees as a contribution the fact that the argument “triggers a rethinking of the historiography of the individual cases.” She is right that these “historiographic nuggets” were “deliberately planted there for others to come by and pick them up later, to follow wherever they may lead.” Bless Mara Loveman for emphasizing the historiographic contribution of *Colonialism and Postcolonial Development*.

“And yet, some nagging questions remain,” Loveman writes in pushing me to think more deeply about four questions. Her first question concerns whether the argument holds up at subnational levels of analysis. As she suggests, the individual case narratives make many subnational comparisons. These comparisons are used to explain important variations *within* countries – e.g., northern versus central and southern Mexico; coastal versus inland Ecuador; western versus eastern Bolivia; and interior versus littoral regions in Argentina. I agree with Loveman that a useful further test of the general theory would be to draw on subnational comparisons in a more systematic way than I did in *Colonialism and Postcolonial Development*. I chose to focus mainly on the country level because I believe that national states

and the territories they claim to control have been the dominant political units of the international system. In turn, because I was so preoccupied with presenting the argument clearly at this national level of analysis, I did not to call central attention to my subnational findings when developing the theory and summarizing the empirical findings. Rightly or wrongly, I worried that drawing too much attention to the subnational comparisons would lead readers to lose sight of the “big picture,” which concerns the cross-national comparison.

Loveman’s second question asks “whether the theory presumes or requires us to treat the cases as independent.” The *general theory* does assume the independence of cases. The fact that this independence is ultimately a fiction is one of several reasons why I suggest that the general theory is simply a starting point: it must be supplemented with other theoretical principles in the explanation of actual cases. I see the flexibility built into the overall explanatory framework as one of its great strengths. Thus, in the case analyses, I always started with the general theory but then brought in other relevant case-specific considerations (including interrelationships among cases) as needed. I anticipated the kinds of additional considerations that would need to be incorporated with the discussion of theoretical principles in the first chapter.

“What exactly is the role of ethnicity or race in the general theory,” Loveman wants to know with her third question. For example, Loveman asks for more discussion of what exactly it is about indigenous people (e.g., their culture, the racist ideologies to which they are subjected, their social organization) that links their relative size to social development outcomes. In my historical account, the fundamental causal culprit is colonial institutions of economic exploitation. Indeed, the precolonial size of the indigenous population was linked to levels of colonialism in the first place because it shaped possibilities for economic exploitation. From large-scale colonial exploitation emerged large-scale impoverished, spatially concentrated, and denigrated indigenous communities. The extreme poverty of these communities was the most immediate source of their “contribution” to poor social outcomes at the national level. But that poverty was a byproduct of

colonial institutions, and it was reinforced through a host of other mechanisms, including cultural ones such as racism. A full account of such reinforcing mechanisms was far beyond what I could accomplish in the book. But I think the case narratives offer a starting point for other comparative researchers who want to take up this task.

Finally, Loveman asks about the agency of the main collective actors in the argument: elite merchants and indigenous communities. For the merchants, she worries that a distinction between liberal and mercantilist traders is not sufficient. Yes, I agree that these merchants actually fell along a continuum. In cases where the historiography is especially deep, such as Peru, I was able to make nuanced distinctions about types of merchants. But for many other cases, I lacked sufficient empirical detail to make fine-grained points. As additional historical research is carried out on colonial merchants throughout the region, it may eventually become possible to do more of what Loveman rightly wants. With respect to the indigenous communities, Loveman asks why I did not say more about their interests, motives, and outlooks. My case narratives do feature some of these matters: the strategies used by kuracas when negotiating with Spanish authorities, the motivations of forasteros for leaving their homes, and various techniques of cultural preservation. But I had to work with the existing historiography, which is still disappointingly underdeveloped on this topic for most cases. We need studies of colonial indigenous communities as history makers and not simply history takers for a wider range of regions.

Nitsan Chorev's encompassing and thought provoking comments point to areas where I might have omitted important considerations or should have addressed certain themes at greater length. While Chorev appreciates my discussion of the institutional constitution of elite and ethnic actors within colonial Spanish America, she suggests that I should have said more about the reproduction of those actors once colonial institutions were changed or removed. I certainly see Chorev as setting an agenda for future research. My argument explored the original creation of specific types of elite and subordinate actors and the ways in which

they helped bring territories to their initial levels of development. Yet more could and should be said in future analyses about the evolving institutions that reconstituted those actors once they were brought into being.

As Chorev notes, I argue that colonialism was not fate for all territories in Spanish America. Several territories were only weakly colonized, and their outcomes depended on events in the decades that followed independence. I think that Chorev will agree that the general theoretical model anticipates these "contingent" cases rather well (though she may not be pleased that colonial history and the theory allow for such contingencies). The theory tells us when colonialism will bring countries to specific levels of development and when it will leave outcomes underdetermined and open to postcolonial occurrences. For the Spanish American cases in which outcomes were underdetermined, the book presents a supplementary explanation of the nineteenth-century events (especially warfare) that sorted them into different levels of development. With this additional layer of analysis, I try to offer a "complete" explanation of outcomes across all the cases in Spanish America. The supplementary explanation is indeed different from the general theory. But this difference tracks the historical record, and it must be acknowledged if one seeks valid explanation.

Chorev raises other questions about the role of the indigenous population in shaping economic and social development. In my argument, the institutional organization of the precolonial population is a crucial cause of level of colonial institutional implantation. In turn, colonial institutions influence economic development through the constitution of specific elite actors. Chorev argues, however, that the size of the postcolonial indigenous population may have had a major *direct* effect on economic development, especially through ethnic conflict, independent of my mediator variable. Yet I find little evidence for this claim. The effect of the indigenous population on economic performance during the colonial period was filtered through colonial institutions and shaped economic development only indirectly via influence on elite constitution. This reality can be most easily appreciated by looking at peripheral cases such as Honduras and Paraguay, where the

indigenous population was small but economic development did not commence for want of the right kind of elite actors. To the extent that the indigenous population and ethnic cleavages had direct development consequences, these effects were manifested mainly in social developmental outcomes (e.g., levels of education, health). Even here, however, the indigenous population was not the sole cause of social development outcomes. Economic development itself – an outcome centrally generated by elite constitution and activity – also mattered in the ways formally summarized in the book.

Finally, Chorev ends by asking questions about the endurance of relative levels of national development despite changing modes of global economic organization. She is correct that I do not offer a fully developed theory of the evolving mechanisms that sustained relative levels of development across time. My goal was documenting this persistence (something that had not been done very well) and then identifying the causes of the initial positioning of countries within the enduring hierarchy of development. Future research on mechanisms of perpetuation can hopefully benefit from this argument and stand on the empirical foundation built in *Colonialism and Postcolonial Development*.

Richard Lachmann's comments address a different aspect of the argument: the causes of variations in the extent to which Spain (and other European colonizers) settled and implanted institutions in their colonial possessions. I am grateful to him for focusing on this part of the argument. For my book is nearly as much about the causes of variations in levels of colonialism as it is about the effects of those variations on long-run development.

Lachmann finds much to like in my explanation of colonial variations, especially the way in which it emphasizes the interactions between the institutions of the precolonial society (i.e., level of precolonial institutional complexity) and the institutions of colonizing society (i.e., mercantilist vs. liberal) as causal determinants. His central concern revolves around my treatment of the political reforms carried out in Spanish America under Bourbon rule. He writes that my account "gives too much weight to Bourbon bureaucratic reforms" in the theoretical discussion

(though less in the actual case studies). The account in the book (see pp. 44-46) follows classic works such as Mark A. Burkholder and D. S. Chandler's *From Impotence to Authority: The Spanish Crown and the American Audiencias, 1687-1808* (University of Missouri Press, 1977) and John Lynch's *Spanish Colonial Administration, 1782-1810: The Intendant System in the Viceroyalty of the Río de la Plata* (Athlone Press, 1958). Any further debate with Lachmann would require exploring the evidentiary basis for his doubts about my empirical treatment. Ultimately, the stakes of this disagreement are not high, since both Lachmann and I concur that the Bourbon reform was crucial mainly for economic reasons, especially providing new opportunities for trade within the New World.

Dan Slater's entertaining but important remarks are built around a useful distinction between the validity/quality of a specific argument (i.e., degree of perfectionism) and the larger theoretical and agenda-setting implications of an argument (i.e., degree of greatness). On these dimensions, Slater argues that my book is nearly perfect but falls short in its greatness. Far be it from me to disagree with him about the level of perfectionism achieved in *Colonialism and Postcolonial Development*. Instead, I will focus on his remarks about the larger theoretical implications of the book.

To make his points, Slater compares *Colonialism and Postcolonial Development* to Skocpol's *States and Social Revolutions*. Skocpol's famous book was unusual (arguably singular) in the degree to which it influenced scholarly debates and set important intellectual agendas in the field. Slater is right that perceived imperfections in Skocpol's work "provoked" many scholars to refute to her argument. He is also right that Skocpol's strong stances on weighty analytic matters put her at the center of the theoretical controversies of the day. No one could afford to ignore Skocpol – and no one wanted to ignore her either.

Although Skocpol's work profoundly influenced me, I never saw myself as writing for the kind of large audience in the social sciences for whom Skocpol's work struck a chord of one kind or another. When I imagined my reader, I am not too ashamed to confess, I had in mind a select set of scholars appreciative of excellence in

craftsmanship and the pursuit of valid knowledge via macrocausal analysis. In Slater's terms, I had in mind scholars who strove for and could recognize perfectionism in the field of macrocausal analysis. I knew full well that this group was a minority within the social sciences. But it was the group who I cared about the most and to whom I wanted to speak most directly. And to speak to them in the way that I wanted, I wrote a book that would not and could not trigger the kind of reaction of *States and Social Revolutions*.

My book, of course, is not without an explicit and extensive discussion of important theoretical matters. I develop and defend a distributional approach to institutions. I weigh in on key debates about the role of geographic conditions in promoting (or not) development. I take on leading theories of colonialism, including especially those that have gained influence within the discipline of economics. I offer a vision of stability in relative levels of development not present in the literature. I address issues concerning the historical evolution of the world economy as a whole and its influence on patterns of colonialism and development. Yet my goal throughout is less to provoke debate than to show how certain theoretical literatures can inform principles of analysis that are necessary for the valid explanation of the outcomes examined in my book.

Slater would have liked me to have done even more, and he suggests some literatures that I might have engaged at greater length, such as work on property rights, the resource curse, and Moore's famous argument about commercial elites. It is possible that discussing these and other theoretical literatures at greater length would have increased the market for the book (and thus its "impact"). But given my explanatory goals, I was not mainly speaking to the issues addressed in these additional literatures. And utilizing them was not crucial for

the development of the theoretical principles that drove my main argument (though I concede that Slater is probably right that I should have discussed Moore at greater length). In macrocausal analysis, one must resist the temptation of addressing issues and controversies that are really only tangential to the explanatory goals at hand. While avoiding these temptations may come at the expense of theoretical impact, it is essential if one seeks maximum results on Slater's dimension of level of perfectionism.

When compared to my book or any contemporary work of macrocausal analysis, Skocpol's *States and Social Revolutions* enjoyed important opportunities in terms of being able to shape the theoretical landscape. For the literatures that she had to engage were – by today's standards, at least – “big but easy targets”: intra-societal modernization approaches to the international system, pluralist and reductionist Marxist views on the state, and an array of simplistic, ahistorical theories of revolution. Skocpol chose not to engage directly the sophisticated comparative-historical arguments about revolution of the time, such as Moore's *Social Origins of Dictatorship and Democracy* (1966) and John Dunn's *Modern Revolutions* (1972). She could avoid this engagement because these comparative-historical works were not the leading theoretical orientations in the 1970s. But I had to work – as we all have to work now – in a post-*States and Social Revolutions* environment in which more nuanced perspectives are leading orientations that cannot be ignored. The legitimate targets of the past are today nothing more than artificial straw men.

It is possible that our generation will yield a new work of macrocausal analysis that matches the greatness of *States and Social Revolutions*. In the meantime, though, there is nothing wrong with settling for a perfect book.

“The legitimate targets of the past are today nothing more than artificial straw men.”

Member Award

Congratulations to **Elizabeth Popp Berman** (University at Albany, SUNY), whose book *Creating the Market University: How Academic Science Became an Economic Engine* was awarded the 2011 President's

Book Award by the Social Science History Association. “The prize rewards an especially meritorious first work by a beginning scholar. ... Entrants will be judged on the criteria of scholarly significance, interdisciplinary reach, and methodological innovativeness, within the broad category of monographs analyzing past structures and events and change over time.” Further information can be found at: www.ssha.org/awards/award-winners/157-2011-presidents-book-award-winner.

Journal Announcements and Calls for Papers

American Journal of Cultural Sociology

Editors:

Jeffrey C. Alexander, Department of Sociology, Yale University, USA

Ronald N. Jacobs, Department of Sociology, University at Albany, State University of New York, USA

Philip Smith, Department of Sociology, Yale University, USA

From modernity’s onset, social theorists have been announcing the death of meaning, at the hands of market forces, impersonal power, scientific expertise, and the pervasive forces of rationalization and industrialization. Yet, cultural structures and processes have proved surprisingly resilient. Relatively autonomous patterns of meaning—sweeping narratives and dividing codes, redolent if elusive symbols, fervent demands for purity and cringing fears of pollution—continue to exert extraordinary effects on action and institutions. They affect structures of inequality, racism and marginality, gender and sexuality, crime and punishment, social movements, market success and citizen incorporation. New and old new media project continuous symbolic reconstructions of private and public life.

As contemporary sociology registered the continuing robustness of cultural power, the new discipline of cultural sociology was born. How should these complex cultural processes be conceptualized? What are the best empirical ways to study social meaning? Even as debates rage around these field-specific theoretical and methodological questions, a broadly cultural sensibility has spread into every arena of sociological study, illuminating how struggles over meaning affect the most disparate processes of contemporary social life.

Bringing together the best of these studies and debates, the *American Journal of Cultural Sociology* publicly crystallizes the cultural turn in contemporary sociology. By providing a common forum for the many voices engaged in meaning-centered social inquiry, the AJCS will facilitate communication, sharpen contrasts, sustain clarity, and allow for periodic condensation and synthesis of different perspectives. The journal aims to provide a single space where cultural sociologists can follow the latest developments and debates within the field.

We welcome high quality submissions of varied length and focus: contemporary and historical studies, macro and micro, institutional and symbolic, ethnographic and statistical, philosophical and methodological. Contemporary cultural sociology has developed from European and American roots, and today is an international field. The AJCS will publish rigorous, meaning-centered sociology whatever its origins and focus, and will distribute it around the world.

Our first issue will publish in the first quarter of 2013 but accepted articles will appear earlier online. Submissions will be anonymously reviewed.

For more information about AJCS, and to access our online submission system, please visit our website at www.palgrave-journals.com/ajcs/.

Comparative Sociology

Comparative Sociology, founded in 2001, is an international scholarly journal dedicated to advancing comparative sociological analysis of societies and cultures, institutions and organizations, groups and collectivities, networks and interactions. The journal publishes theoretical and empirical work on all aspects

of comparative sociology, and welcomes both quantitative and qualitative work. Comparative Sociology is sponsored by RC 20 (Comparative Sociology) of the International Sociological Association and published by Brill Academic. Articles are indexed in SCOPUS, Sociological Abstracts, and the Social Sciences Index. Manuscripts may be submitted through Editorial Manager at the journal's web site (<http://www.brill.nl/comparative-sociology>). For questions, contact the editor, David Weakliem (University of Connecticut).

Member Publications

Berman, Elizabeth Popp. 2012. *Creating the Market University: How Academic Science Became an Economic Engine*. Princeton, NJ: Princeton University Press.

Bonastia, Christopher. 2012. *Southern Stalemate: Five Years without Public Education in Prince Edward County, Virginia*. University of Chicago Press

Chorev, Nitsan. 2012. *The World Health Organization between North and South*. Ithaca: Cornell University Press.

Fox, Cybelle. 2012. *Three Worlds of Relief: Race, Immigration, and the American Welfare State from the Progressive Era to the New Deal*. Princeton: Princeton University Press.

Go, Julian. 2011. *Patterns of Empire: the British and American Empires, 1688 to the Present*. Cambridge University Press.

Goldberg, Chad Alan. 2011. "The Jews, the Revolution, and the Old Regime in French Anti-Semitism and Durkheim's Sociology." *Sociological Theory* 29(4): 248-71.

Gorski, Philip S., David Kyuman Kim, John Torpey, and Jonathan VanAntwerpen, eds. 2012. *The Postsecular in Question*. New York: New York University Press.

Haglund, LaDawn and Rimjhim Aggarwal. 2011. "Test of Our Progress: The Translation of Economic and Social Rights Norms into Practices." *Journal of Human Rights* 10: 1-27.

Halfmann, Drew. 2011. *Doctors and Demonstrators: How Political Institutions Shape Abortion Law in the United States, Britain and Canada*. University of Chicago Press.

Hough, Phillip A. 2011. "Disarticulations and Commodity Chains: Cattle, Coca and Capital Accumulation along Colombia's Agricultural Frontier." *Environment and Planning A* 43(5): 1016-1034.

Hough, Phillip A. 2011. "Guerrilla Insurgency as Organized Crime: Explaining the So-Called 'Political Involution' of the Revolutionary Armed Forces of Colombia," *Politics and Society* 39(3): 379-414.

Hough, Phillip A. and Jennifer Bair. 2012. "Dispossession, Class Formation and the Political Imaginary of Colombia's Coffee Producers over the Longue Durée: Beyond the Polanyian Analytic." *Journal of World-Systems Research* 18(1): 30-49.

Lamont, Michèle and Nissim Mizrachi, eds. 2012. "Special Issue: Responses to Stigmatization in Comparative Perspectives: Brazil, Canada, Israel, France, South Africa, Sweden and The United States." *Ethnic and Racial Studies* 35(3).

Reed, Isaac Ariail. 2012. "Analytical Sociology: Appreciation and Ambivalence." *Sociologica* 1/2012.

Rhomberg, Chris. 2012. *The Broken Table: The Detroit Newspaper Strike and the State of American Labor*. New York: Russell Sage Foundation.

Tuğal, Cihan. 2012. "Fight or Acquiesce? Religion and Political Process in Turkey's and Egypt's Neoliberalizations." *Development and Change* 43(1): 23-51.

Woodberry, Robert D. 2012. "The Missionary Roots of Liberal Democracy." *American Political Science Review* 106(2): 244-274.

Section Awards

Theda Skocpol Best Dissertation Award for 2012

Winner

Stephan Bargheer, Max Planck Institute-Berlin

"Moral Entanglements: The Emergence and Transformation of Bird Conservation in Great Britain and Germany, 1790-2010."

Runner-up

Damon Maryl, University of California, Berkeley

"Secular Conversions: Politics, Institutions, and Religious Education in the United States and Australia, 1800-2000."

Bendix Prize for Best Student Paper Award

Co-winners

Carly Knight, Harvard University. "A Voice but Not a Vote: The Case of Surrogate Representation and Social Welfare For Legal Noncitizens Since 1996."

And

Diana Rodriguez-Franco, Northwestern University. "Internal Wars, Taxation, and State Building."

Barrington Moore Best Book Award

Winner

Yang Su, University of California, Irvine. 2011. *Collective Killings in Rural China during the Cultural Revolution*. New York: Cambridge University Press.

Honorable Mentions

Gail Kligman, UCLA and Katherine Verdery, Graduate Center–CUNY. 2011. *Peasants under Siege: The Collectivization of Romanian Agriculture, 1949-1962*. Princeton, NJ: Princeton University Press.

And

James Mahoney, Northwestern University. 2010. *Colonialism and Postcolonial Development: Spanish America in Comparative Perspective*. New York: Cambridge University Press.

Charles Tilly Best Paper Award

Winner

Nicolas Hoover Wilson, University of California, Berkeley. 2011. “From Reflection to Refraction: State Administration in British India, circa 1770-1855.” *American Journal of Sociology* 116 (5): 1437-77.

Honorable Mention

Hazem Kandil, UCLA. 2011. “Islamizing Egypt? Testing the Limits of Gramscian Counterhegemonic Strategies.” *Theory and Society* 40(1): 37-62.

Please join us at the reception at the ASA Meeting in Denver on Friday, August 17 with the Section on Global and Transnational Sociology from 6:30-8:00 where we will give out the awards. Congratulations to the winners! Many thanks also to our Committee Members who had hard decisions to make.

Section Election Results

Council Chair-Elect 2012

Andreas Wimmer, UCLA

Council Members – 3 Year term begins in 2012

Emily Erikson, Yale University

Isaac Reed, University of Colorado at Boulder

Section Members on the Job Market & Recently Defended Dissertations

Sinem Adar
Brown University

Understanding Nation-State Formation and Its Everyday Exclusionary Implications on Non-Muslims in Istanbul and Alexandria, 1920-early 1970s

My dissertation explores a seeming puzzle in the integration of non-Muslims to Turkish and Egyptian nation-states. In Turkey, an adamantly secular nation-state, non-Muslim minorities decreased from about 20% to 0.1% of the population, while in officially religious Egypt, they (most of whom are Coptic Christians) still account for almost 15% of the population. What is it about these two trajectories that brings together secularism and almost complete religious homogeneity in one case (Turkey), and official religious ideology with so much more diversity in the other (Egypt)? What are the implications of these different trajectories on the legal and social integration of non-Muslims in two post-Ottoman port cities, Istanbul (Turkey) and Alexandria (Egypt), between 1920s-1970s? Building on historical ethnographic data of a 15-month period of fieldwork in these cities, my dissertation demonstrates that strong state consolidation in Turkey during the first two decades of the republic under a strenuous nationalist regime of assimilation ensured the legal integration of non-Muslims. However, under increasing popular nationalist pressure, the social integration of non-Muslims became difficult, and as a result many left Istanbul starting from the late 1950s-early 1960s. In Egypt, on the other hand, increasing popular nationalist mobilization as a reaction to the British indirect colonial rule (between 1920s and the late 1940s) led to extensive state consolidation in the aftermath of the 1952 coup d'état. It is in this context that legal and social integration of non-Muslims (except Coptic Christians) in Alexandria became more and more difficult starting from the 1940s, leading to their gradual emigration from the city.

Dissertation Committee: Gianpaolo Baiocchi (Chair), John Logan, Patrick Heller, Karen Barkey (Columbia University)

Research Interests: Comparative Historical Sociology, Political Sociology, Urban Sociology, Nationalism, Nation-state Formation, Category- and Boundary-making, Politicization of Religion, Race and Ethnicity, Citizenship, Urban citizenship, Civil Society, Social Movements, Democratization, Welfare State and Social policy, Historical Research Methods (particularly Historical Ethnography), Middle East and the Balkans, Turkey, Egypt

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Andrew Dawson
McGill University

State Authority Structures and the Rule of Law in Post-Colonial Societies: A Comparison of Jamaica and Barbados

My dissertation examines the social determinants of the rule of law by comparing Jamaica and Barbados, two countries with many similarities, but with divergent outcomes concerning the rule of law. The research takes a comparative historical approach, specifically investigating the origins of the divergence of the rule of law between Jamaica and Barbados by focusing on the late colonial period (1937-1966). Using new data collected from archival research, state legitimacy is identified as the key factor that helps explain the

divergent trajectories of the rule of law in Jamaica and Barbados post-independence. Going beyond state-based explanations of the rule of law, the analysis suggests that the rule of law not only depends on characteristics of the state, but also on characteristics of society and the fit between the two.

Dissertation Committee: Matthew Lange (Chair), John A. Hall, Axel van den Berg

Research Interests: Political Sociology, Sociology of Development, Political and Ethnic Violence, and Comparative Historical Sociology.

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Cristián Doña–Reveco
Michigan State University (Sociology and History)

In the Shadow of Empire and Nation: Chilean Migration to the United States since the 1950s

Migration is a critical event in the life of individuals and families. This event is defined by biographical and historical conditions, and it is part of a complex process within which the actual decision to migrate is a pivotal point. This complex process involves multilevel forces that impact personal and familial decisions and actions at every stage of the migration process: before the migration, while migrating, during the incorporation into the receiving society, and during possible return migration to the country of origin. Through the Chilean emigrants oral histories and memories, migration data (census and visas) and archival research, my research will uncover the particularities of the relationships between the biographical aspects of the decision to migrate and the structures and historical frameworks within which these decisions take place. In my research I position the emigrant at the center of the migration experience while maintaining the relevance of macro structures. I analyze the macro (the state within the world-system), meso (social institutions) and micro (individual biography) inter-related components of post World War II Chilean migration to the United States, particularly to Illinois and Michigan. I compare the emigration in four historical periods in Chilean history; the modernization before 1973, the exile (1973-1982); the neoliberal dictatorship (1982-1990); and the neoliberal democracy (post 1990). This migrations occurs within a context of continuous social, political, and economic change in the country of origin, Chile; a context that is shaped by global policies enacted by the country of destination, the United States.

Dissertation committee: Brendan Mullan (Co-Chair), Alesia Montgomery, Stephanie Nawyn (Sociology); Peter Beattie (Co-Chair), Leslie Moch, and Edward Murphy (History)

Research Interests: I am interested in the relationships between the emigrants and their state of origin. My research intersects macro-historical dynamics, demographics, and memory constructions of the Southern Cone of America (Argentina, Chile and Uruguay) post 1930. I am also interested on the pedagogical aspects of using cinema in teaching Sociology and History.

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Laura R. Ford
Cornell University

A Case of Semantic Legal Ordering: The Emergence and Expansion of Intellectual Property

In my dissertation, I undertake an historical and comparative investigation of the emergence and expansion of intellectual property (patents, copyrights, trademarks, and trade secrets). The historical and geographic sweep of the dissertation is broad, ranging from guild protections in medieval European city-states to the English Statute of Monopolies (1624), patent and copyright laws of the early French Revolutionary period, the World Intellectual Property Organization, and the America Invents Act (2011). From a theoretical perspective, I seek to accomplish two things: (1) to formulate a thesis about the causal process through which formal law makes a difference in social relationships, and (2) to show how that causal process either complements or conflicts with contemporary North American sociological theories. I argue that formal law makes a difference in social relationships through a causal process of “semantic legal ordering,” focusing on the ways that legal interpretation shapes institutions, organizations, and intentions.

Dissertation Committee: Richard Swedberg (Chair), Mabel Berezin, Stephen L. Morgan

Research Interests: My other interests include (1) sociological theory, especially classical theory, (2) economic sociology and sociology of law, (3) sociological, political, and cultural history, (4) counterfactualist methods and the identification of causes in sociological theory, and (5) a broad range of “law and society” topics, particularly in relation to the contemporary welfare state.

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Lindsey Freeman
New School for Social Research

Longing for the Bomb: Atomic Nostalgia in a Post-Nuclear Landscape

Can nostalgia for an atomic past, not only serve to spark remembrance for a bygone era, but also lead to critical thought that could affect social change now and in the future? Often connected to a sense of home or home-ness, nostalgic sites are places where we imagine that we fit, a community of which we are a part that circumstances have separated us from, but to which we long to return. If this is nostalgia, are there people that actually long for the atomic bomb, for the imagined hearth of the uranium production plant? In places that owe their existence to the dawn of the Atomic Age, the answer is yes. It is often forgotten that nuclear energy and weapons programs created real communities, in such places history, memory, and nostalgia meet in a landscape both lived and imagined, where they converge in ways that are sometimes unexpected and contradictory. It is at these points of tension where critical thought can emerge. Two decades after the close of the Cold War, a space has opened up for the renegotiation of atomic memories. Compared with the contaminated materials of the Atomic Age, historical memory has a much shorter half-life; in light of this fact questions about the nuclear past, present, and future are immediate and pressing. Through Oak Ridge, Tennessee, one of the three secret cities created for the Manhattan Project, I engage in an ethnography of the American relationship to the rise and decline of the Atomic Age, an ethnography of failed atomic utopianism and atomic nostalgia as they are engaged in the post-nuclear present.

Dissertation Committee: Vera Zolberg (Chair), Jeffrey Goldfarb, Oz Frankel, Elzbieta Matynia

Research Interests: Historical Sociology, Cultural Sociology, Sociology of Memory, Sociology of Science and Technology

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Kevan Harris

Johns Hopkins University, 2012

(2012-13 Post-Doctoral Fellow at Princeton University, Near Eastern Studies Department)

The Martyrs Welfare State: Politics and Social Policy in the Islamic Republic of Iran

My dissertation asks two questions. First, why has the post-revolutionary state in Iran endured for decades despite war, social conflict, and economic turmoil? Second, was 2009's "Green movement" in Tehran a negation of the events of 1979 or was it a lineage of the Iranian revolution itself? Drawing on sixteen months of fieldwork from 2007-2011 in several Iranian provinces, including interviews with government officials and participant observation of the Green movement itself, I argue that the post-revolutionary state in Iran remained resilient because its state-building project intertwined with a welfare-building project. I examine how the Islamic Republic created and relied on a set of welfare institutions which channeled the social mobilizations of the 1979 revolution and 1980-88 war with Iraq into a warfare-welfare complex. This broadened the social base of the state while also constraining its capacity for top-down projects steered by state elites. A subsequent attempt within the Islamic Republic to create a developmental project, one with its own loyal educated and technocratic cadres, expanded the middle class through social welfare policies. Yet this developmental push, common to many middle-income countries, generated new expectations among the population for upward mobility, changed livelihoods, and an alternative cultural/political order. I argue that the large 2009 popular mobilization was an outcome of the various and conflicting lineages of state-building efforts by the Islamic Republic of Iran, and the response to those efforts by newly empowered social classes.

Research Interests: My future project, originating from the Iranian case, will situate the ongoing uprisings in the Middle East within various development trajectories of middle-income states in the global South. I also have two projects related to the historical sociology of development. First, I am pursuing research detailing the paradox of parallel processes which contribute a global wealth gap and a global welfare convergence. Second, I examine the contradictions of various developmental strategies, such as industrial "upgrading" and resource extraction, as they produce "adding-up" problems at the global level.

Dissertation Committee: Giovanni Arrighi, Beverly Silver

Damon Mayrl

University of California, Berkeley, 2011

Secular Conversions: Politics, Institutions, and Religious Education in the United States and Australia, 1800-2000

Although sociologists have increasingly abandoned the assumption that secularization is an inevitable byproduct of modernity, they have yet to develop a compelling account for why otherwise similar modern countries nevertheless accord religion substantially different roles in public life. I engage this problem by examining how the United States and Australia came to develop contrasting policies toward religious education in the late twentieth century. Despite many political, constitutional, and demographic similarities,

and despite sharing nearly identical sets of policies at the end of the nineteenth century, the two nations evolved distinctive and novel arrangements governing religious education in the years after World War II. Drawing on insights from institutional theory and historical sociology, I account for these divergent “secular settlements” by detailing how three common political processes (religious conflict, professionalization, and state-building) were differently refracted through each nation’s distinctive administrative, judicial, and electoral institutions. American political institutions constituted a “permeable state” which facilitated the progress of these processes, while Australian institutions constituted an “insulated state” which inhibited them. Based on this analysis, I develop a novel “political-institutional” approach to secularization which argues that variations in secularization stem, not simply from broad modernizing trends or the self-interested calculations of political leaders, but instead from the interaction of multiple general secularizing processes and particular political institutions. And I reveal that, both by mediating political and professional conflict and by actively calling into being the very actors who subsequently seek more secular policies, the state is a key factor in explaining variation in secularization.

Jules Naudet

Postdoctoral Fellow, Centre Maurice Halbwachs (ENS-EHESS), Paris, France.

Comparative Analysis of the Experience of Upward Intergenerational Social Mobility in the United States, in France and in India (in French)

My dissertation proposes a comparative analysis of the experience of upward social mobility in the United States, in India, and in France. It is based on approximately 150 interviews conducted among people from modest backgrounds who achieved prestigious positions in the higher ranks of civil service, in the private sector and in academia. These three countries are often cited as paradigmatic cases by sociologists who try to theorize the links between social mobility and social stratification systems. The United-States are thus typically perceived as the archetype of an open society characterized by few obstacles to mobility and by social statuses considered as achieved. Conversely, India is frequently described as the archetype of a closed society marked by the weight of the caste system and by social statuses considered as ascribed. Between the model of a closed society and that of an open society, French society seems to be more structured by the notion of social classes that continues to shape the analysis of its system of stratification. These three models are deeply rooted in sociological thought and they influence the way these three countries are apprehended. The first thread of my dissertation questions these categories of international comparison by drawing on the empirical research conducted in these three countries using the same protocol of investigation. The second thread consists in a discussion of the conceptual tools that are most often used by sociologists to understand the experience of upward social mobility.

Dissertation Committee: Marie Duru-Bellat (Sciences Po), Christophe Jaffrelot (CNRS), Michèle Lamont (Harvard University), Marco Oberti (Sciences Po), Serge Paugam (CNRS, EHESS), Olivier Schwartz (L’université Paris V).

Research Interests: I am currently involved in a new comparative research focusing on self-segregation among upper-class neighborhoods of Paris, Delhi and São Paulo. We more particularly try to understand how the wish to live in these highly segregated neighborhoods can be explained by specific representations of the poor.

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Matthew Nichter

University of Wisconsin – Madison

Rethinking the Origins of the Civil Rights Movement: Radicals, Repression, and the Black Freedom Struggle, 1930-1970

I argue that a mass movement for African-American equality had begun to emerge by the mid-1940s, largely under the auspices of labor unions and leftist political parties. However, the repression of radicals during the McCarthy era delayed the emergence of this nascent civil rights movement and weakened its ties to the labor movement. Notwithstanding these discontinuities, I also demonstrate that many activists with backgrounds in the Old Left struggles of the 1930s and 1940s played key leadership roles in the resurgent civil rights movement of the 1960s. These findings challenge canonical analyses of the origins of the civil rights movement, and shed new light on the historical roots of contemporary racial inequality.

Dissertation Committee: Erik Olin Wright (Chair), Pamela Oliver, Chad Alan Goldberg, William P. Jones (History)

Research Interests: Comparative and Historical Sociology, Political Sociology, Social Movements, Race, Labor, Political Economy, Theory, Philosophy of Science

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Shiri Noy

Indiana University – Bloomington

Globalization, International Financial Institutions and Health Policy Reform in Latin America

This multimethod dissertation uses the case of health sector reform in Latin America to test the thesis that international financial institutions (IFIs) have used their coercive financial power to uniformly impose neoliberal policies in developing nations. I use cross-section time-series models to examine the overall impact of IFIs on health spending. I then draw on evidence from 300 policy documents and over 100 interviews with policy makers and stakeholders in Argentina, Costa Rica and Peru to account for cross-national variation in health policy reform. To date, I have three main findings. First, contrary to conventional wisdom, international financial institutions have little effect on health expenditures in Latin America. Second, IFI policy prescriptions are neither uniformly applied across countries, nor are they strictly “neoliberal.” Neoliberal concerns with market efficiency, privatization and individual responsibility are discussed in tandem with a state-responsibility discourse on equity and poverty-reduction. Third, institutional arrangements such as degree of decentralization and state autonomy and capacity – that is, whether the state formulates clear goals for the health sector and whether it is able to carry those goals to fruition – shape the extent to which IFIs are able to influence health policy reform in Argentina, Costa Rica and Peru. This research contributes to our understanding of the process of transmission of IFI policy prescriptions and their reception, negotiation and implementation by developing countries’ governments.

Research Interests: Political Sociology, Sociology of Development, Globalization, Health Policy, Comparative Methods, Latin America

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Jung Mee Park
Cornell University

International Legal Norms and Domestic Politics: The Transformative Effects of 19th Century Bilateral Treaties

In my dissertation, I examine 19th century bilateral treaties as they pertain to the development and standardization of international law globally. After writing treaties with Western states (US, Great Britain, Germany, etc.), East Asian countries (China, Japan, and Korea) adopted new legal terminologies, radically reorganized, and institutionalized new models of statehood. During this time, China's status within Asia declined, Japan emerged as a world power, and Korea, a once sovereign nation, became a colonial site. For the dissertation, I constructed a dataset of 228 treaties involving 123 unique dyadic relations for countries from Europe, Asia, North America, and South America. The treaties were coded for legal, diplomatic, political, commercial, and social provisions. Over time, the concluded treaties corresponded to specific categories such as arbitration, consular, delimitation, and extradition treaties to handle various claims. My analysis shows that intra and inter-regional tensions shaped treaty provisions and determined whether the treaty was symmetrically beneficial or asymmetrically beneficial. Treaties tended toward mutual benefits by the early 20th century as inter-regional tensions declined. My analysis also explores how the treaties allowed foreign nationals to establish lasting educational, scientific, and religious institutions in East Asian countries.

Dissertation Committee: David Strang (Chair), Mabel Berezin, Katsuya Hirano (History)

Research Interests: I previously wrote on the history of Christianity (particularly in Korea), religion and nationalism, post-colonialism, and sociology of culture (particularly American musical theatre). Currently, I am writing a paper on the dyadic network ties in international diplomatic exchanges from 1817 to 2005, which examines the stability of symmetric and asymmetric ties.

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Oren Pizmony-Levy
Indiana University – Bloomington

Testing for All: The Emergence and Development of International Assessments of Students' Achievements 1958-2008

International assessments of students' achievements (IASA) – such as Trends in International Mathematics and Science Study (TIMSS) and Program for International Student Assessment (PISA) – appear to be a vital catalyst in the globalization of education. Currently, one-third of all countries participate in these assessments. Still, empirical research on the IASA is less extensive than might be expected. My dissertation investigates the emergence and global diffusion of IASA over the past five decades. My point of departure is neo-institutional theory and its application to globalization; I extend this theoretical framework by exploring processes taking place at both global and local levels. Using archival research and interviews with 45 key-informants, I demonstrate how the field of IASA has developed in two phases. In the early decades (1960s-1980s), actors working in the field framed their work in terms of academic and intellectual endeavor (e.g., official reports were guided by specific research questions). Since the mid-1990s, however, actors working in the field frame their work in terms of global governance and auditing of educational systems (e.g., official

reports include more ranking tables and less research questions). I explain this development by examining the central role of the United States in the field of IASA. Furthermore, using original quantitative dataset, I show how regional and global factors, rather than national characteristics, affect the likelihood of countries to participate in IASA.

Dissertation Committee: Brian Powell (Co-Chair), Margaret Sutton (Co-Chair), Arthur Alderson, Heidi Ross, Pamela Barnhouse Walters

Research Interests: Comparative Sociology / Education, Sociology of Education, Political Sociology, Environmental Sociology, LGBT Studies, Quantitative and Qualitative Methods, Social Networks

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Jennifer Rosen

Northwestern University

Political Institutions, Development Thresholds, and Women's Political Representation

Jennifer Rosen's dissertation offers a new explanation of cross-national and over-time variation in levels of female political representation. She shows that key causal mechanisms have different –even contradictory– effects on female representation across countries with diverse socio-economic histories. Using a nested analysis that combines quantitative and qualitative methods, she systematically examines the interaction between political institutions and economic development in mitigating or reinforcing social inequalities. She pays particular attention to women's political empowerment in African and Latin American post-conflict societies. Results indicate that the specific kinds of political institutions that enhance female political representation are radically different in developed vs. less developed countries. Hence, institutional designers need to take into consideration the economic context of a country in order to promote more balanced political representation for women.

Dissertation Committee: James Mahoney (Chair), Monica Prasad, Jeremy Freese, Alberto Palloni

Research Interests: Her research interests focus on the intersection of politics, gender, and international development, as well as the use of innovative social science research methods. Jennifer has a forthcoming article (sole author) on the topic of women's representation in *Political Research Quarterly*.

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Ashley T. Rubin

University of California, Berkeley (Jurisprudence and Social Policy)

Accounts of the Separate System: Organizational Legitimacy and Eastern State Penitentiary, 1829-1930

How do officials in perpetually vulnerable organizations seek to protect their organization's legitimacy? An understudied organization, prisons are perpetually vulnerable as they employ problematic technologies to achieve ambiguous (often conflicting) goals that are often difficult to evaluate. I offer a longitudinal study of an extreme case: Eastern State Penitentiary (1829–1930). One of the first American prisons, Eastern

represented an experiment with a new, untested technology and no systematic means of evaluation. As a state-run organization, it often faced funding problems that led to embarrassing gaps between theory and practice. Most importantly, Eastern's "separate system" of inmate confinement was an exceptional practice in an increasingly isomorphic penal field, subjecting it to intense field-wide criticism. Drawing on a range of public and private, ephemeral and regularly produced primary-source documents, I demonstrate that organizational officials utilized organizational accounts rooted in institutions that intersected the penal field. By signaling their adherence to legitimate norms, values, and understandings, they sought to compensate for the legitimacy they lost through their deviant formal structures. Although officials often relied more heavily on these accounts in times of explicit threats to the autonomy of the prison, the accounts ultimately became institutionalized over time, becoming part of the formal structure of the organization making it at least in part isomorphic with its environment.

Dissertation Committee: Malcolm Feeley (Chair), Cybelle Fox, Calvin Morrill, Jonathan Simon

Research Interests: (Formal) Social Control, Organizational Theory, Law and Society, Historical Sociology, Methodology

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Hiroe Saruya

University of Michigan

Democracy and Protests in Japan: The Development of Movement Fields and the 1960 Anpo Protests

My dissertation examines contestations and practices of democracy during the 1960 Anpo Protests—the massive social movement that coalesced to combat the revision of the U.S.-Japan Security Treaty in 1960. The dissertation looks at the transitions that occurred within the U.S.-enforced democratic sphere from 1945 through 1960, during and after the occupation. The research is based on 18 months of fieldwork, including archival research and about 100 in-depth interviews with former protest participants. By focusing on intellectuals, student protest groups, and workers, I analyze how and why each of these three groups developed its own movement prior to their convergence in the Anpo Protests in 1960. I draw upon Pierre Bourdieu's concept of field to show how each group developed its own movement field, and how actors therein engaged in struggles in accordance with the specific rules and practices within that field. I argue that the 1960 Anpo protests were not a single coherent movement, but rather an aggregation of different kinds of social movements, each of which was internally comprised of distinct movement dynamics. I then analyze how the 1960 Anpo protests served as a political opportunity for each of these groups to achieve previously determined political goals specific to their group. Finally, I show that the practice of democracy, forged during the post-World War II period, provided a shared context that served to coordinate their collective protests.

Dissertation Committee: George Steinmetz (Chair), Genevieve Zubrzycki, Kiyoteru Tsutsui, Michael Kennedy, Jennifer Robertson

Research Interests: Comparative Historical Sociology, Political Sociology, Classical and Contemporary Theory, Ethnicity and Nationalism, Social Movements and Social Change, the Sociology of Japan and Asia

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Ritchie Savage

The New School for Social Research

A Comparative Analysis of Populist Discourse in Venezuela and the United States

My dissertation investigates the way in which political discourse is structured in order to appeal to the people. Through an analysis of speeches and articles covering Betancourt's Democratic Action, Chávez, McCarthyism, and the Tea Party, I argue that there is an essential structure to populist discourse revealed in references to the 'opposition' as a representation of the persistence of social conflict. In the discourses of these politicians and social movements, references to the opposition are posed against a 'founding moment of the social,' which serves as a collective memory of the origins of democracy and the strive for freedom or liberation. With evidence provided that this binary structure is present in all of the aforementioned cases, I conclude that populism is a case of a universal discursive formation, which can emerge in administrations, social movements, and ideologies with vastly different characteristics. I then utilize this definition of populism to reveal that instances of populism, which once proved to be exceptional phenomena within modern forms of political rule, are now becoming part of the institutionalized structure of democratic politics, evidenced by a number of cases taken in comparative-historical perspective.

Dissertation Committee: Orville Lee (Chair), Andrew Arato, Sarah Daynes, Federico Finchelstein

Research Interests: My other research interests include an ongoing inquiry into the role of 'language' as an analytic construct in the social sciences and how it has been deployed within social and cultural theory in such a manner to create a fundamental set of recurring antinomies between its structuralist, psychoanalytic, Marxist-historical, performative, and phenomenological applications.

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Ben Scully

Johns Hopkins University

Development in the Age of Wagelessness: Labor, Livelihoods, and the Decline of Work in South Africa

My dissertation examines how the decline of formal wage labor in South Africa has shaped both the developmental strategies available to the state and the political strategies available to trade unions. South Africa has experienced one of the highest levels of unemployment in the world over the past decade. The consensus among a wide range of scholars is that such levels of unemployment have produced an economic and social divide between the country's remaining formal wage workers and the un- and underemployed "wageless" majority of the labor force. My dissertation research draws on an analysis of national household surveys and NSF-sponsored I conducted in 2010-2011. Using this data, I argue that divisions between workers and the wageless are not as sharp as is often assumed once we look beyond individuals' workplace experiences and into their households and kinship networks. The vast majority of South African households rely on livelihood strategies that combine multiple sources of income from both wage and non-wage sources. Instead of a socioeconomic divide, there is widespread interdependence between workers and the wageless. My findings point to broadly utilized sources of livelihood—such as land, small enterprises, and government social spending—which can be the focus of effective and popularly supported developmental strategies, even if economic policy is not successful in broadly expanding formal wage labor. My work also highlights the possibility for unions in countries like South Africa to expand their political constituencies by taking up

issues beyond the workplace which have direct economic impact on their members and the unorganized majority.

Dissertation Committee: Beverly Silver, William Martin (SUNY-Binghamton), Rina Agarwala

Research Interests: Economic Development, Labor and Social Movements, State Welfare Policy, Comparative Historical Sociology.

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Kristen Shorette

University of California, Irvine

Fair Trade Certified: The Institutionalization of Nongovernmental Regulation of Global Markets

My dissertation research, supported by the NSF, examines the uneven rise of Fair Trade Organizations (FTOs) as market-oriented social justice organizations. Using original data on all current and former FTOs, and time series and panel regression analyses, I examine (1) the rise of FTOs over time, (2) cross-national variation in the concentration of fair trade (FT) producer organizations across the global South, and (3) cross-national variation in the amount of FT goods consumed within developed countries. Examining proliferation of FTOs since 1960, I find that the rise of these organizations is not simply related to global inequality and environmental degradation, but that these issues become problematized with the rise of new world cultural norms supporting equality, human rights, and environmentalism carried by INGOs. Further, FTOs grow as economic liberalization increases, suggesting the applicability of the Polanyian double-movement to the global level. Examining concentration of FT producers across the global South, I find evidence not only of top down diffusion via international organizations and colonial legacies but also of lateral diffusion processes via networks of Peace Corps volunteers. This agentic model of diffusion is consistent with work in economic sociology that highlights the importance of social ties for economic activity. Finally, the examination of cross-national FT consumption patterns reveals the relevance of organizational structure over individual altruism, whereby the widespread and mainstream availability of FT goods most strongly predicts the amount of national consumption. Overall, my research identifies the cultural and structural underpinnings of global markets, and the importance of non-state actors in their governance.

Dissertation Committee: Nina Bandelj (Co-Chair), Ann Hironaka, Evan Schofer (Co-Chair)

Research Interests: In addition, I conduct cross-national and over-time comparative research on how cultural, political, and economic forces influence a variety of outcomes in the areas of the natural environment, human health, and human rights. I am particularly interested in engaging both world society and political economy perspectives.

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Sourabh Singh

Rutgers, The State University of New Jersey

Dynamics of Political Field Structure in a Democratizing State: India, 1947-1984

In my dissertation research, I have empirically examined Bourdieu's claim that the ontological unity between field and subjects simultaneously creates conditions for both field reproduction and transformation. I have investigated the logic of field reproduction and transformation by studying changes in the relational positions of 4,000 parliamentarians within the Indian political field from 1947 to 1984. My data sources are the 'Data Handbook on Elections in India,' 'Who's Who of the Indian Parliament,' and published biographies of prominent Indian parliamentarians. To illustrate changes in the structure of the Indian political field, I have used Network Analysis, Multiple Correspondence Analysis, and Logistic Regression modeling, as well as biographical descriptions of parliamentarians' everyday political interactions. An article discussing my study of the transformation of the relational structure of the Indian political field in the late 1960s and the resultant rise of Indira Gandhi in Indian politics, in spite of her being a symbolically devalued female politician, is forthcoming in *Theory and Society*. Also, a manuscript based on my study of the formation of the particular political habitus of Gandhian-era parliamentarians resulting from their differential exposure to various capitals of the Indian political field during the Nehruvian era is currently under review. I am currently preparing a manuscript based on my study of the friction between the evolving relational structure of the Indian political field and the Jayaprakash (JP) movement in the mid-1970s, which led to the only authoritarian interlude in the history of postcolonial Indian politics.

Dissertation Committee: Paul McLean (Chair), Ann Mische, Ethel Brooks

Research Interests: I am interested in exploring Bourdieu's discussion of the symbolic power of state by studying how the emergency state in India (1975-77) lost its symbolic power. I am also interested in exploring the ethical foundation of Bourdieu's field theory by scrutinizing the presence of symbolic violence in violent and non-violent protest strategies.

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Michelle Smirnova

University of Maryland, College Park

The Construction of "We": The Russo-Soviet Anekdot in a Cultural Context

The way by which nationality and citizenship are codified in law or used by political entrepreneurs to mobilize populations is different from how individuals make sense of themselves. Although sharing a particular attribute or physical connection offers some sort of relational identity, it is the product of belonging both to a category and network of individuals in addition to the feeling of belonging which produces a bounded groupness (Tilly 1978; Anderson 1983/2006; Brubaker 2000). It is often difficult for historians to get at such feelings of groupness or nationness except through means of self-identification (a labeling process), but I believe that the Soviet Russian anekdot—a politically subversive joke—provides an intimate view into the perspective of the Russian people living under the Soviet regime. The anekdot serves as a discourse of cultural intimacy (Herzfeld 1999), in that it serves to deface or expose the public secret (Tausig 1997) that Soviet citizens are prohibited from voicing. It also serves to reify the top-down definition of an "imagined community". Beyond its transgressive properties, politically subversive texts like the anekdot articulate the details of an intimate set of knowledges that insiders "are taught not to know". In my dissertation I look at how the characters and narratives construct (1) the boundaries of "we"—who belongs and who does not by exploring how different groups are "marked" in the anekdoty, (2) how the collectivity

negotiates their understanding of leaders, institutions and State propaganda as a means of rejecting or reifying aspects of Soviet power, and (3) what sort of collective memory and identity is conveyed through the expressions of the public secret, nostalgia and/or regret. The anecdote reveals power dynamics at multiple levels: within the family, between ethnic groups and geographical regions, and between people and state. Together these multiple identities and relationships express a form of collectivity among Russians.

Dissertation Committee: Meyer Kestnbaum, Patricia Hill Collins, Melissa Milkie, Patricio Korzenwitz, Vladimir Tismaneanu

Research Interests: My broad theoretical interests are upon collective identity and memory, nationalism, culture and discourse. I have worked at the Census Bureau on several projects pertaining to perceived racial and ethnic identities among US residents and about levels of trust in the Federal government.

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Nicolás M. Somma

University of Notre Dame, 2011

(Assistant Professor of Sociology, Pontificia Universidad Católica de Chile)

When the Powerful Rebel. Armed Insurgency in Nineteenth-Century Latin America

By assuming that insurgencies come “from below” – i.e., are launched by exploited and deprived social groups – existing theories are not suited for explaining insurgencies “from above” – i.e., led by political, social, and economic elites. When most insurgencies come “from above”, why do insurgency levels vary across countries? In nineteenth-century Latin America insurgencies of this kind were very common. Therefore, I compare two countries with high insurgency (Colombia and Uruguay) and two with low insurgency (Chile and Costa Rica) during the century after independence (ca. 1820-1920). Three factors help explaining variations in insurgency levels. The first one is the strength of the ties between peasants and landowners. Strong ties allow landowners to use selective incentives for mobilizing their subordinates into rebel armies. Conversely, weak vertical ties reduce selective incentives and obstruct mobilization. This complements theories about insurgencies “from below”, which assume that weak vertical ties increase insurgency. The second factor is the timing of consolidation of the central state. Early state consolidation increases the costs of insurgency and leads government opponents to engage in other strategies (elections, informal agreements, and/or military coups). Conversely, late consolidation encourages an early resort to insurgency, which becomes self-reinforcing and persists even after the state consolidates. By emphasizing the timing of state consolidation I complement political opportunity and state breakdown theories, which overlook how past events shape outcomes across time. The third factor is the type of party system. Two-party systems simplify the process of blame attribution, allow the party in power to exclude its opponent, and encourage leaders to emphasize extreme positions for capturing the support of small and highly militant electorates. This increases polarization and boosts insurgency. Conversely, because in multi-party systems parties are unable to govern alone, they are encouraged to engage in flexible electoral and congressional alliances that decrease polarization and therefore insurgency. Consistent with this argument, in Colombia and Uruguay vertical ties were strong, central states consolidated late, and two-party systems polarized, leading to high insurgency. In Chile and Costa Rica vertical ties were weak, states consolidated early, and multi-party systems did not polarize, leading to low insurgency.

Mark D. Whitaker

University of Wisconsin – Madison, 2008

Ecological Revolution: The Political Origins of Environmental Degradation and the Environmental Origins of Axial Religions; China, Japan, Europe

Most argue environmental movements are a novel feature of world politics. I argue that they are a durable feature of a degradative political economy. Past or present, environmental politics became expressed in religious change movements as oppositions to state environmental degradation using discourses available. Ecological Revolution describes characteristics why our historical states collapse and because of these characteristics are opposed predictably by religio-ecological movements. As a result, origins of our large scale humanocentric 'axial religions' are connected to anti-systemic environmental movements. Many major religious movements of the past were 'environmentalist' by being health, ecological, and economic movements, rolled into one. Since ecological revolutions are endemic to a degradation-based political economy, they continue today. China, Japan, and Europe are analyzed over 2,500 years showing how religio-ecological movements get paired against chosen forms of state-led environmental degradation in a predictable fashion. I argue that the formation of unrepresentative political clientelism/jurisdictions is responsible for environmental degradation. The process of environmental degradation is argued to be caused by unrepresentative state elite organizational changes in environmental and social relations for their own short-term political economic benefits though with bad long-term consequences.

Dissertation Committee: Joseph Elder, Frederick Buttel, Daniel Kleinman, Charles Halaby

Research Interests: Environmental Sociology, Comparative Historical Sociology, Political Sociology, World Regional Sociology, Information Society, Social Stratification and Inequality, State/Cultural Interactions, Social Welfare and Quality of Life Research, Comparative Development, Social Movements, Sociology of Science/Medicine, Consumption and Material Choices as a Politicized Infrastructure, Comparative Constitutional Engineering Effects on Representation and Sustainability.

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Daniel Williams

(Visiting Assistant Professor, Carleton College)

Citizens, Foreigners or Germans? The State and Persons of Immigrant Background in the Making of Membership in Germany since 1990

My dissertation examines recent changes in citizenship in contemporary Germany and their impact on understandings of nationness and belonging. It combines a historical-comparative analysis of how citizenship policy at the level of the state has changed since 1990 with interview and ethnographic data drawn from immigrants and the children of immigrants about their understandings of citizenship and self-identification as German. Previous scholarship has shown that nationness has been a key category for the making of citizenship policies. A similar relationship may be posited for individuals, who may relate citizenship and nationness. Furthermore, understandings of citizenship and nationness which are institutionalized in the state may inform the understandings of persons of immigrant background. Since 1990, access to citizenship in Germany has become more liberalized for persons of immigrant background. Contrary to scholarship emphasizing nationally-specific traditions of citizenship, as well as convergence theories based on global or universal norms around citizenship, this dissertation shows that these changes

after 1990 are explained largely by political parties' narratives about immigrants and foreigners, Germany and the nation, and the meaning of citizenship. In the context of the liberalization of citizenship policies, the majority of immigrant-descended individuals do self-identify as German, largely based on their everyday cultural practices and through language. However, they simultaneously articulate a sense of non-Germanness through appearance, name and other markers of descent. Additionally, persons of immigrant background tend to disconnect citizenship from Germanness. They tend to view their citizenship as neither a means to, nor reflection of, Germanness.

Research Interests: Comparative Race and Ethnicity, International Migration, Intersectionality, Qualitative Methods, Global and Transnational Sociology, Culture

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Xiaohong Xu
Yale University

Revolutionizing Ethos: Making 'New Men' and New Politics in the Chinese Revolution

Based on an empirical study of the Chinese Revolution, my dissertation argues that the revolutionary process can best be understood with reference to the dynamic triadic relationship among civil society, competitive party politics, and evolving state institutions. I investigate the organizational emergence of the 'new men' who made their way from civic activism into politics, and the process in which these Communist revolutionaries developed a new organizational ethos and diffused it into civil society and eventually into the party-state. Based on extensive use of archival and historical materials and interviews, I discover that Chinese Communism emerged from youth activist organizations with strong sectarian ethical culture; their agenda of social transformation was fused with a group ethos derived from this sectarian base. Their rise in the political arena disrupted the weak parliamentary politics of the time, and reconfigured the relationship between civil society and party politics. Finally, I examine the formation and consequences of the resulting Maoist political culture: its resurgent sectarian ethics fostered a highly disciplined cadre crucial for its rise to power yet also incurred organizational dynamics within the Party which, after the 'new men' took power, frequently led to policy disasters.

Dissertation Committee: Julia Adams, Philip Gorski, Peter Perdue, Steve Pincus

Research Interests: My next project will draw on organizational theory and network analysis to analyze the transformation and reproduction of cultural institutions and cultural elites in contemporary China in order to understand why the major political rupture taking place in 1989 has given way to political resilience in the following two decades.

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Other Information

The Comparative and Historical Sociology Section currently has 657 members, down 7 (about 1%) since last year. The ASA Section average is down about 4%. We also have 31.4% of our members who are students.

The ASA Section average is 30%.

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Please email your information to Robert Jansen, CHS Web Editor:
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Contributions to Trajectories are always welcome: please contact the editors at atesaltinordu@sabanciuniv.edu and seio@hawaii.edu.