Trajectories

Spring 2010 Newsletter of the ASA Comparative and Historical Sociology Section Volume 21, No. 2

CONTENTS

Corruption and Regulation in the Economic Sphere, New Research

Globalization, Corruption and Internal Violence

Jeffrey Kentor
University of Utah

Matthew Sanderson
Lehigh University

Matt Sanderson and I are working on a cross-national research project that explores the impact of global processes of the world-economy on corruption and violence in less developed countries (LDCs). Before giving a synopsis of this work in progress, however, a brief history of the research that set the stage for our current study may be helpful.

Background

Our work is an extension of earlier research on the impact of foreign direct investment on economic development in LDCs. This line of research began with Chase-Dunn's (1975) empirical work on the impact of foreign investment on development. Chase-Dunn found that stocks of foreign investment inhibited economic growth in less developed countries. Subsequent research by Firebaugh (1992, 1996), Dixon and Boswell (1996), de Soysa and Oneal (1999), among others, was inconclusive, with findings on both sides of this argument. Terry Boswell and I took a different direction in 2003, arguing that the question of the impact of total foreign investment on development was not a useful one; one more likely to obscure rather than clarify the complexities of foreign investment. We chose instead to deconstruct foreign investment, focusing on a new dimension that we referred to as *foreign investment concentration*; the proportion of a host country's foreign investment stocks owned by the single largest investing country. We found that, over time, foreign investment concentration slows economic growth in LDCs. In more recent work, our attention turned to foreign investment in terms of organizations rather than capital, focusing on the physical location of foreign subsidiaries of transnational corporations. We considered two aspects of these subsidiaries; the overall number of subsidiaries and *foreign subsidiary concentration*, defined as the proportion of a host country's foreign subsidiaries owned by the single largest investing country. In a forthcoming article (Kentor and Jorgenson 2010) we report two key findings 1) the overall growth of foreign subsidiaries accelerates economic development in LDCs and 2) relatively high levels of foreign subsidiary concentration inhibit the overall growth of foreign subsidiaries, thus slowing economic growth.

With this background now in place, we can proceed with an overview of our current work.

Globalization, Corruption and Internal Violence

Globalization has been associated with the rising importance of transnational corporations (TNCs) as significant actors in the global economy (Kentor 2005). The global expansion of TNC operations that began in earnest in the 1970s was, in part, facilitated by international financial institutions, which promoted widespread market liberalization and integration efforts under the rubric of what is now commonly referred to as the "Washington Consensus." Yet, as the first decade of the 21st century comes to a close, the results of liberalization and integration are quite uneven. In many LDCs, these efforts have failed to generate significant improvements in living standards and in some, the results have been calamitous. Indeed, many LDCs continue to confront significant impediments to development, among the most important of which are persistently high levels of government corruption and sustained internal violence (Gleditsch et al. 2002). These outcomes raise the question of whether there is a relationship between economic integration, corruption, and internal violence in LDCs.

Our current project explores the extent to which these phenomena are related. We focus explicitly on the role of transnational corporate expansion as a mechanism of global economic integration. These international headquarter-subsidiary linkages enable transnational corporations to incorporate host countries into global circuits of accumulation.

Previous research has produced only mixed results on the question of whether economic integration is on balance beneficial or detrimental to internal political outcomes in less-developed countries. Proponents contend that integration reduces corruption and internal conflict primarily by stimulating economic growth. From this perspective, foreign investment is an indicator of "openness" (Hegre, Gleditsch, and Gissinger 2003; Bussmann and Schneider 2007), which lowers the cost of capital, increases investment, and provides technical knowledge and access to modern technologies that are crucial for development. Critics contend that integration increases internal conflict in LDCs. From this perspective, foreign investment is an indicator of "penetration," or "dependency" and it further extends developed countries' dominance over LDCs. As foreign investment assumes a larger proportion of the domestic economy, it inhibits economic growth and promotes uneven development within the country (Dixon and Boswell 1996; Nielsen and Alderson 1995, Kentor 1998, 2000; Kentor and Boswell 2003), raising internal political conflict (Dixon and Boswell 1990).

The mixed results of previous studies may reflect a failure to consider the structural dimensions of integration. Previous studies frame the question in terms of the level, or degree, of integration. However, the theorized relationships between foreign investment and internal outcomes may be more accurately assessed in terms of the *structure* of foreign investment in a host country. Following previous works in this area (Kentor and Boswell 2003; Kentor and Mielants 2007; Kentor and Jorgenson 2010) we focus on the two recently identified aspects of foreign investment discussed above: "*foreign investment concentration",* the proportion of a host country's foreign investment stocks owned by the single largest investing country and "*foreign subsidiary concentration*", the proportion of a host country's foreign subsidiaries owned by the single largest investing country.

Because TNCs exert control over the investment function, foreign investment connotes a form of structural power. The ability to provide investment capital and generate employment endows TNCs with significant influence in LDCs, where resources are relatively scarce. The sheer scale of resources controlled by TNCs makes it possible for these organizations to influence political outcomes in host countries without explicit or concerted action. Moreover, the structural power of TNCs is enhanced in contexts in which foreign investment is more concentrated, in terms of both capital and location. In such contexts, domestic economic and political elites confront more integrated and unified corporate interests. The cohesiveness of these interests is further increased by the propensity of TNCs based in the same country to have denser ties among boards of directors (Kentor and Jang 2004, Carroll and Fennema 2002, Useem 1984). Thus, higher levels of foreign capital and subsidiary concentrations provide a platform for foreign corporations to exert greater influence over internal political-economic dynamics in host countries.

Foreign investment and foreign subsidiary concentrations exacerbate corruption by reducing state autonomy both from without and from within. From without, it strengthens the position of foreign actors vis-à-vis the state. From within, it strengthens the position of domestic elites with ties to foreign capital.

In these contexts, the state is weakened vis-à-vis foreign interests, as state autonomy is reduced and the state's capacity to implement policies that reflect citizens' collective objectives is constrained (Evans 1979, 1995). Foreign investment and foreign subsidiary concentrations exacerbate corruption by reducing state autonomy both from without and from within. From without, it strengthens the position of foreign actors vis-à-vis the state. From within, it strengthens the position of domestic elites with ties to foreign capital. By reducing state capacity and undermining state legitimacy, corruption promotes political instability and ultimately higher levels of internal conflict.

Our preliminary results from structural equation models using panel data from 1970 to 1995 support these hypotheses. Both concentration measures seem to exacerbate internal violence through direct and indirect causal pathways. Foreign investment and foreign subsidiary concentrations in 1970 directly increase levels of internal violence in 1995. Both measures also raise corruption levels in 1985, which in turn increases levels of internal violence in 1995. There is also an interaction effect between the two concentration measures. Countries with higher levels of both foreign investment concentration and foreign subsidiary concentration have higher levels of internal violence.

We would like to close by highlighting the following conclusions. Internal conflicts plaguing many less-developed countries are not solely, or even primarily, explained by domestic factors, but are instead associated with a particular form of global economic integration. Specifically, it is not economic integration *per se* but the *structure* of economic integration that seems to affect the prevalence of internal conflicts. Further, corruption is a significant mediator of this relationship. More broadly, our work encourages a re-framing of the conventional question of whether global economic integration is, on balance, positive or detrimental for development, towards more nuanced and potentially more fruitful questions that explore how the organizational composition and structure of globalization affect factors that impede or promote development outcomes.

References:

Alderson, Arthur S. and Francois Nielsen. 1999. "Income Inequality, Development, and Dependence: A Reconsideration." *American Sociological Review* 64:606-631.

Boswell, Terry and William J. Dixon. 1990. "Dependency and Rebellion: A Cross-National Analysis." *American Sociological Review* 55:540-559.

Bussmann, Margit and Gerald Schneider. 2007. "When Globalization Discontent Turns Violent: Foreign Economic Liberalization and Internal War." *International Studies Quarterly* 51:79-97.

Carroll, William K. and Meindert Fennema. 2002. "Is There a Transnational Business Community?" *International Sociology* 17:393-419.

Chase-Dunn, Christopher. 1975. "The Effects of International Economic Dependence on Development and Inequality: A Cross-National Study." *American Sociological Review* 40, 720-38.

Dixon, William J. and Terry Boswell. 1996. "Dependency, Disarticulation, and Denominator Effects: Another Look at Foreign Capital Penetration." *American Journal of Sociology* 102:543-562.

Evans, Peter. 1979. *Dependent Development: The Alliance of Multinational, State, and Local Capital in Brazil*. Princeton: Princeton University Press.

—. 1995. *Embedded Autonomy: States and Industrial Transformation*, Edited by Anonymous. Princeton: Princeton University Press.

Firebaugh, Glenn. 1992. "Growth Effects of Foreign and Domestic Investment." *American Journal of Sociology:* 98 (1) 105-30.

__. 1996. "Does Foreign Capital Harm Poor Nations? New Estimates Based on Dixon and Boswell's Measures of Capital Penetration." *American Journal of Sociology* 102:563-575.

Gleditsch, Nils Petter, Peter Wallensteen, Mikael Eriksson, Margareta Sollenberg, and Havard Strand. 2002. "Armed Conflict 1946-2001: A New Dataset." *Journal of Peace Research* 39:615-637.

Hegre, Havard, Ranveig Gissinger, and Nils Petter Gleditsch. 2003. "Globalization and Internal Conflict." Pp. 251-276 in *Globalization and Armed Conflict*, edited by G. Schneider, K. Barbieri, and N. P. Gleditsch. New York, NY: Rowman and Littlefield.

Kentor, Jeffrey. 1998. "The Long Term Effects of Foreign Investment Dependence on Economic Growth, 1940-1990." *American Journal of Sociology* 103:1024-1048.

—. 2000. *Capital and Coercion: The Economic and Military Processes That Have Shaped the World-Economy, 1800-1990*. New York, NY: Garland Press.

—. 2005. "The Growth of Transnational Corporate Networks: 1962–1998." *Journal of World-Systems Research* 11:263-286.

Kentor, Jeffrey and Terry Boswell. 2003. "Foreign Capital Dependence and Development: A

New Direction." *American Sociological Review* 68:301-313.

Kentor, Jeffrey and Yong Suk Jang. 2004. "Yes, There Is a (Growing) Transnational Business Community: A Study of Global Interlocking Directorates 1983-98." *International Sociology* 19:355-368.

Kentor, Jeffrey and Andrew Jorgenson. 2010. "Foreign Investment and Development: An Organizational Perspective." International Sociology 25 (3): 1-23.

Kentor, Jeffrey and Eric Mielants. 2007. "Connecting the Global and the Local: The Impact of Globalization on Civil Society," in Mark Herkenrath (ed.), *Civil Society: Local and Regional Responses to Global Challenges.* Muenster Germany: LIT Verlag.

Soysa, Indra de and John R. Oneal. 1999. "Boon or Bane? Reassessing the Productivity of Foreign Direct Investment." *American Sociological Review* 64:766-782.

Useem, Michael. 1984. *The Inner Circle: Large Corporations and the Rise of Business Political Activity in the U.S. And U.K.* Oxford: Oxford University Press.

The Clearing House: A Private Solution to Bank Panics

Lori Qingyuan Yue
Columbia Business School

Jiao Luo
Columbia Business School

Paul Ingram
Columbia Business School

The history of the U.S. banking industry is regularly dotted with short panics. During the one hundred years between 1814 and 1914, before the Federal Reserve was created, there were at least thirteen major panics (Calomiris and Gorton, 1991). The founding of the Federal Reserve did not stop bank panics. Just fifteen years after the Federal Reserve was founded, the Great Depression occurred. Many scholars argue that the panics during the 1930s were explicable by the pernicious role of the very institution, the Federal Reserve, that was created to stabilize the banking industry (Friedman and Schwartz, 1963) or, at least, by the elimination of preexisting institutions that would have limited the persistence or severity of the banking crisis (Gorton 1988). After the Great Depression, stricter government regulation and deposit insurance institutions were enacted, but they also have not been successful in preventing banking crises. Instead, on the basis of banking crises since the Great Depression, many scholars argue that government regulations that were designed to maintain depositors' confidence and prevent bank runs have served as the very culprit that creates the banking instability, falling under the curse of a classic moral hazard problem—insured actors tend to take more risks (Ely, 1999; Ahrens, 2008).

Given the obvious inadequacy of government regulations in preventing and eliminating bank panics, could private arrangements? Before the advancement of government regulations, the commercial banks in the U.S. had a private regulation system called the clearing house, a city-based self-governance among commercial banks. The historical background of this private regulation system is the ideology of community independence and the anti-branching legislations, which resulted in thousands of small, single-unit, and undiversified banks throughout this country. Local banks, sharing a commons of market confidence, were motivated to collectively avoid and lessen bank panics.

We investigated the efficacy of the New York Clearing House Association (NYCHA), the oldest and also the largest clearing house of the country, in affecting the survival and operational risk of the commercial banks in Manhattan from 1840 to 1980. Besides serving as an intermediate for check clearing among banks, the NYCHA offered emergency loans and provided deposit coinsurance during bank panics. Once a panic struck, the NYCHA organized an emergent loan committee that facilitated mutual lending and issued loan certificates to stressed members so that they could survive bank panics. The NYCHA also issued loan certificates directly to bank depositors in exchange for demand deposits. These small denomination loan certificates could be redeemed for cash in any NYCHA member banks after a panic. In this way, NYCHA provided deposit insurance for its member banks. We found that these cooperative efforts significantly reduced the failure rates of the NYCHA member banks, even after controlling for the selection bias of the NYCHA membership.

To prevent individual member banks from engaging in risky operations and taking advantage of the goodwill of others, the NYCHA adopted a set of monitoring strategies. Moreover, we found that social structures played an important role in ensuring these strategies were effective. Since the NYCHA was composed of a relatively small number of local banks, close monitoring was feasible. Moreover, the geographical proximity increased frequency of exposure and facilitated the formation of a high level of *closure* (Coleman, 1988; Uzzi, 1996). Generally, closure deters defection, because defectors are not only easily identified but also risk the loss of various types of community connections. Frequency of social interaction facilitates the formation of friendship, kinship, and status, which may motivate altruistic behaviors. Closure among elite bankers was especially important as they controlled the liquidity underlying the use of the clearing house loan certificates. Club affiliations had played an important role in consolidating the banking elites (Beckert, 2001), because New York City has traditionally been an immigrant city and the capitalist class has diverse origins (Kessner, 2003). We found that the NYCHA functioned better in reducing its members' failure rates and in reducing their operational risks when the elite bankers' club-affiliation network was denser.

At the end of the nineteenth century, as the national economy flourished, it became increasingly difficult for the city-based clearing houses to maintain market order. The Federal Reserve was established in 1914 to replace the city clearing house as the lender of last resort. The Federal Reserve institutionalized loan certificates and built a "discount window," through which the Reserve Bank extends liquidity to member banks. The NYCHA reverted to its initial function, clearing checks between member banks. The Federal Deposit Insurance Corporation (FDIC) was created in 1934 and provides deposit insurance and guarantees the safety of deposits in member banks.

Although these public institutions have functions similar to those of the clearing house, they are also different in important aspects. The Federal Reserve is governed by and implements regulation through public administration agencies, which may not only have an agency problem but also lack direct knowledge of the industry. Moreover, the operation of public institutions may lack flexibility. The enlarged scale of regulation makes it difficult for members to monitor each other. These conditions create opportunities for some member banks to free-ride on others by engaging into risky operations. Since the survival benefits derived from curbing bank runs may not overweigh the negative effects produced by risky operation, we found that banks affiliated with the Federal Reserve and the FDIC did not have significant survival advantages, but they were more likely to engage into risky operation.

Our investigation of the clearing house suggests that private institutions are not only viable but also may outperform public institutions under certain circumstances. The idea of organizing private cooperation is not an artifact of the nineteenth century political economy, but has emerged as a potential solution in the recent financial crisis. On the day Lehman Brothers announced bankruptcy, 10 major U.S. banks immediately organized an emergency loan fund to which each contributes $7 billion and from which they can tap if they experienced a Lehman Brothers-type crisis of liquidity (Andrews, 2008). This case exemplifies that even today private cooperation may still serve as a substitute for government action. Our research may inform debates as to just when such substitution may be effective or desirable.

References:

Ahrens, Frank. 2008. "Moral Hazard: Why Risk Is Good." in *The Washington Post*.

Andrews, Edmund L. 2008. "Fed Loosens Standards on Emergency Loans." in *New York Times*.

Beckert, Sven. 2001. *The monied metropolis: New York City and the consolidation of the American bourgeoisie, 1850-1896*. Cambridge, UK: Cambridge University Press.

Calomiris, Charles W., and Gary Gorton. 1991. "Origins of Banking Panics: Models, Facts, and Bank Regulation." Pp. 93-163 in *Financial Markets and Financial Crises*, edited by Glenn Hubbard.

Coleman, James S. 1988. "Social Capital in the Creation of Human Capital." *American Journal of Sociology* 94:95-120.
Ely, Bert. 1999. "Regulatory moral hazard: The real moral hazard in Federal Deposit Insurance." *The Independent Review* IV:241-254.
Friedman, Milton, and Anna Jacobson Schwartz. 1963. *A Monetary History of the United States, 1867-1960*: Princeton University Press.
Gorton, Gary. 1988. "Banking panics and business cycles." *Oxford Economic Papers* 40:751-781.
Kessner, Thomas. 2003. *Capital city: New York City and the men behind America's rise to economic dominance, 1860-1900*. New York, NY: Simon & Schuster.
Uzzi, B. 1996. "The sources and consequences of embededdness for the economic performance of organizations: The network effect." *American Sociological Review* 1996:674-698.

Book Symposia

Illiberal Politics in Neoliberal Times: Culture, Security and Populism in the New Europe by Mabel Berezin (Cambridge University Press, 2009)

Mabel Berezin's new book was the subject of an Author Meets Critics panel organized by Margaret Somers at the 2009 SSHA annual meeting in Long Beach, CA. What follows are the comments of the four critics (George Ross, John Agnew, Andreas Wimmer, and Dario Gaggio) and a response by the author.

Comments by George Ross
University of Montreal
Brandeis University
Harvard University

Mabel Berezin's *Illiberal Politics in Neoliberal Times: Culture, Security and Populism in the New Europe* (2009) casts a fresh new light on the French *Front National*. The book is both very brave and very good. It is brave because there is a vast literature on the FN and 'Right extremist' political parties in Europe more generally, making her move into such occupied territories a courageous act. And despite the library of research that she had to deal with—very well, in fact—mainly from electoralist political scientists and social movement specialists, Berezin makes a contribution that is novel and useful. "Populism," a term constantly used to discredit loud protest groups that elites do not like, has always been best studied by those with a historical bent. Berezin is a sociologically well trained social historian, and like the best of these, brings a unique sensitivity to aspects of history that political scientists have often overlooked.

The backbone of the book is a very special historical narrative of the FN in recent years. What makes it stand out is Berezin's sharp ethnographic eye for the cultural and sub-cultural sides of things. Groups like the FN feel very strongly about their issues. They are not the simple political utility maximizers that political science assumes parties to be these days. They trade in emotions as well as ideas and they are unusually sensitive to the performative side of politics. Jean-Marie le Pen, the leader of the FN who is now in his eighties, may come as close to a charismatic leader (and a crypto-fascist one at that) as one finds in French politics these days. He honed his skills in the French far-right of the 1950s whose base was social groups (often lower middle class and rural) who felt constantly betrayed by elites, sold out in the Indochinese and Algerian wars, ignored by Charles de Gaulle, deeply hurt by the wrenching effects of rapid economic modernization, and scorned by liberal priests and uppity young people, to list only a few of the more sensitive points. Le Pen, a former paratrooper in Algeria who supported *Algérie Française* and mingled enthusiastically with dangerous anti-democratic (and often anti-semitic) figures, has always been blessed with a great oratorical eloquence infused with harsh ironic mockery that can ably portray elite figures as arrogant manipulators constantly turning their backs on ordinary virtuous citizens. Even people who despise Le Pen know how good a rhetorical show he can put on. He is also an extraordinarily talented organizer who has been capable of integrating a legion of tireless true believing activists, seducing skilled apparatchiks, and finding lots of money to build, staff, and finance the Front. Moreover, he has had a nose for issues to peddle. After appeals about colonial betrayals wore out and France turned the corner to its modern prosperity, it was Le Pen who successfully tapped Europe's anti-immigrant hatred, the FN's stock in trade to today.

Berezin conveys all of this with great skill, speech-by-speech, campaign after campaign, different *Fêtes Bleu-Blanc-Rouges*, and other significant movement events. Her special sociological gifts are finding and describing the performative ways and tools which this kind of leader used to touch people through highly-charged spectacles he was able to generate. Reading the text one can feel almost viscerally the spontaneous and contrived expressions that initially electrified the raw nerves of the estranged and betrayed of the older French far right and, more recently, the estranged and betrayed former far left, orphans of the collapse of French Communism and the centrist politics of French socialists in power. Berezin places this dense story into a deeper comparative historical analysis, using the Italian case. The central concept here is the 'consolidation regime,' which refers to different European paths to national integration with their varying consequences for individual identities and their relationships with nations (see Table 2.2). Unusually strong states building on strong nations like France (in contrast to Italy) create powerful national collective identities. These identities, in turn, become fields for unending political disagreement around what the strong nation should be and what constitutes the most important threats to it. Identities are both 'scripts' and emotions, Berezin argues effectively.

Berezin does well at uncovering the strategies and shifts in the FN's development since it came first to prominence in 1983, when it won the Mayoral election in Dreux on a harsh anti-immigrant program. She traces the FN's events extremely well through to the traumatic moment when Le Pen came in second to Jaques Chirac in the 2002 presidential election and then played an important role in the victory of 'no' votes in the 2005 referendum to ratify the European Constitutional Treaty. There are a few oversights worth noting, however. In particular, she downplays the partisan and institutional contexts within which the FN has lived and which have helped shape its destinies.

France has been chronically multi-partisan, undoubtedly since political parties were first invented. This means that there have been hard right, loud-mouthed, and anti-democratic political formations around for a very long time, often as serious players. The FN is only the most recent iteration, therefore, and differs mainly from earlier contenders because it is now no longer polite to be openly against democratic institutions. The French Fifth Republic was carefully designed to constrain small parties like the FN away from destructive cacophony into simpler Left and Right coalitions, in part by creating a regime in which election to the very powerful presidency was the real key to power. This means that for a long time prior to all-important presidential elections the most plausible – usually centrist – parties whose candidates have the best chance of winning must try to build coalitions from their divided Left or Right families. These arrangements have consistently given more extreme parties, who often have had veto power over electoral success, a great deal of power over election prospects. Groups like the National Front (or the French Communist Party, to choose a historic example from the left) thus can influence programs and outcomes. These groups are significant not only for what they *are*, but also for what their existence implies for other key players and to the system.

This is not a trivial point and Berezin does not emphasize it enough. Let me give a few examples. The FN made its initial breakthrough in local elections in 1983, just after the Left in power had renounced its key campaign promises, turned to severe austerity, and was threatened with electoral collapse, in consequence. President Mitterrand, hoping to limit the inevitable success of the Right in the 1986 legislative elections so that he would stand a chance of re-election in 1988, changed the electoral law to introduce a form of proportional representation. His goal was to give the FN the possibility of winning a large number of seats away from the opposition center-right, which it did. This cut the center-right's majority down considerably, giving Mitterrand new space to rebuild his own position, and helped him win in 1988. It also gave the National Front a huge political success that it would not have had otherwise. Later, in the 2002 presidential elections, Lionel Jospin, the Prime Minister and Socialist candidate, ran a very bad campaign, neglecting the fundamental task of building a left coalition around his candidacy for the initial primary and instead running as if he were already in the final runoff against Jacques Chirac. This, plus the unpopularity of Chirac, again handed the FN a large electoral gift which it might otherwise not have had when Le Pen – barely – received more primary votes than Jospin. Similar things might be said about the 2005 referendum and the intelligent anti-FN strategy of Nicolas Sarkozy, who shrewdly co-opted the immigration issue from the FN in his 2007 campaign. The general point here is that the successes and failures of the National Front have often been determined well beyond its own intelligent choice of mobilizing issue – anti-immigration – and its own often brilliant leadership and mobilizing strategies. To use an analytical term central to scholars of social movements, the FN has also been a creature, sometimes fortunate, other times less so, of the "political opportunity structures" created by French institutions and the strategies and tactics of key players within them.

Berezin, looking for a deeper historical cause for the National Front's saga, seizes upon the growing significance of the EU and Europeanization in French national life. Here is where I disagree with her most. No one would deny the centrality of Europeanization for the French, from Jean Monnet and General de Gaulle through Nicolas Sarkozy. It is also obvious that a hard nationalist political formation like the FN will oppose the EU as a matter of course and use it as a central issue in campaigning whenever useful – as it did, for example, in the 2005 French referendum on the European Consitutitonal Treaty. Still, the primary stock in trade of the FN remains as it has always been, anti-immigration, as its longtime slogan 'French first' indicates. The FN was a pioneer in this ignominious struggle, targeting France's large North African Islamic diaspora. But it has not been alone, however. There are similar anti-immigrant nationalist parties all over Europe – Haider and the Austrian Freedom Party, List Pim Fortuyn in the Netherlands, the Swiss People's Party, Umberto Bossi and the Lega Nord, Pia Kjaersgaard and the Danish People's Party, the Vlaams Belang, the Norwegian Progress Party, the British National Party, and a number of others. These groups are different in their strength, their fit in national politics, what their national institutions look like, and the other issues they may espouse beyond nationalist xenophobia. Indeed most are also anti-EU, like the FN. But the overwhelming weight of their appeal is against immigrants.

If a deep cause is needed for all this beyond widespread European xenophobia, it might be wiser to seek it in the profound economic changes that have occurred since the crisis of the 1970s, which ended what the French called the 'thirty glorious years' of growth and reform after World War II. Mass unemployment and low economic growth have been preoccupying issues since then, feeding widespread insecurity in France. Globalization, including the out-migration of manufacturing to emerging markets, a newly tentacular financial system, and mass economic migration towards wealthier countries, has probably been the most important underlying process at work. In France, elites of left and right have been powerless to ward off the chronic threats of globalization to labor markets and social programs. Moreover, they, like everyone else, came to be infected by neo-liberalism, even if French statism sometimes masked this. The EU has been part of these globalizing processes, albeit usually following rather than leading. But the deep cause of the FN, if this is what we want to speculate about, lies somewhere else.

Comments by John Agnew

University of California, Los Angeles

Based largely on the growth of right-wing populism in France since the 1980s but with an intriguing supplement on the rise of a new right in Italy in a slightly later period, Mabel Berezin argues that rather than being primarily about xenophobic anti-immigrant politics, as in most previous accounts, the new European illiberalism is much more a direct result of "Europeanization." It is, therefore, a historical "surprise" springing from the perceived erosion of the postwar "world of security" provided by nation-states in the face of a growing and increasingly neoliberal European Union (EU) and not simply the recapitulation of older fascist, nationalist, and anti-foreign impulses. The argument relies on a series of fascinating and oft overlooked points I haven't seen connected previously. After noting these as best I can, I want to raise some questions that reading the book raised for me that could serve as elements in future debate over the origins and course of right-wing/nationalistic politics in contemporary Europe.

One of the book's central claims is that Europeanization has "compromised the bonds of democratic empathy and provided an opportunity for rightwing populists to articulate a discourse of fear and insecurity" (p. 8). This has happened not simply because ordinary, largely working-class, people have seen themselves increasingly disadvantaged materially relative to a new class of "Euro-stars" who can cash in on the new opportunities provided for those with the education and skills to profit from the new "borderless" Europe but also because a whole series of "events" have drawn symbolic attention to their plight. As against a theoretical logic that emphasizes political parties and social movements, therefore, Mabel Berezin sees key symbolic events with emotional resonance as crucial to the rise of the new rightwing politics. Events set in train a whole series of path dependencies that a focus on parties or movements would possibly miss. Unlikely election successes, surprisingly resonant populist speeches and gatherings, and the advent of EU directives subject to harsh public lampooning are examples of such generative events. At the same time, the political left entered into a well-noted era of disorientation and decline by lurching incoherently between "third ways" and older ideological commitments that, after the miners' strike in Britain in 1984, no longer appealed so much to either the old industrial working class or the new middle classes of metropolitan Europe. Historic events hitherto without much political reverberation in postwar Europe, such as the trial of the World War II Nazi administrator Klaus Barbie in 1987 and ten years later the trial of the Nazi collaborator Maurice Papon, also brought back memories of French collaboration with the Nazis and, in some cases, defensive reactions to them. In this context, the claim of the French rightwing party, *The National Front*, to represent neither left not right but all of "the French" took on a particular significance. So, just as borders across Europe were "thinning," so some national identities were undergoing a degree of "thickening" (p. 216). But this was much more the case in a country such as France with its "hegemonic consolidation regime" (a long history of a strong state and associated national identity) than, for example, in Italy with its "flexible consolidation regime" (incompetent state and weak national identity) or many Eastern European states with "brittle consolidation regimes" (bureaucratic states with weak national identities). It is not that these others have been without populist movements. But in these cases, as illustrated by Italy, there has been a need to stir up a national or regional identity upon which to base a new insecurity rather than simply presume a settled national one, as in the case of France.

The typical narrative about rightwing European populism as an unsurprising and straightforward reaction to foreign immigration is simply upended by this analysis. There should be no going back to that as a singular explanation.

The typical narrative about rightwing European populism as an unsurprising and straightforward reaction to foreign immigration is simply upended by this analysis. There should be no going back to that as a singular explanation. Questions do remain. I can suggest a number. For one thing, I have some trouble labeling all European rightwing parties and movements as "populist." The usage strikes me as too American in provenance. Some of the European movements are more conservative/national in orientation (e.g. the French National Front and the Italian National Alliance) whereas others come closer to the populist label (e.g. the Italian Northern League and the British UKIP) in their emphasis on government "corruption" and "elitism." I also think that foreign immigration is still of tremendous significance, particularly for the more populist and racist movements, not just as a trigger but as an issue around which to organize all their other positions on the economy and politics. The frequently expressed fear of "swamping" by immigrants emphasized by leaders such as Le Pen from the French National Front is instructive. So, even if the EU is implicated as enabling and encouraging population mobility across national borders, it is the immigrants themselves who are viewed as undermining the existing cultural order and threatening the economic interests of the locals. Certainly, the example of Switzerland lends pause to whole-hearted adoption of the Europeanization thesis as adequate in and of itself in explaining what is clearly in the Swiss case (recall, not a member state of the EU) much more a sense of the disturbance of everyday life introduced particularly by immigrants who have substantial cultural differences from the locals.

Beyond these questions about the overall adequacy of the "Europeanization thesis," I would also raise a few questions about the nature of the new illiberalism when compared to historic varieties of rightwing political movements, such as those widespread in interwar Europe. One is the extent to which the new ones draw on working-class support. Famously, interwar fascism was middle class in terms of its social base. The fear of socio-economic eclipse by a disreputable working class led to support for fascist and Nazi parties. This difference draws attention to the dramatic decline in the fortunes of leftwing politics in contemporary Europe since the 1970s. Mabel Berezin draws attention to this at the outset of her book but it seems to get lost in the subsequent analysis. The end of the Cold War and the decline of many of the industries in which leftwing parties were embedded probably explain some of the decline in support for the left. An important feature of old school European rightwing nationalism was its anti-Semitism. By and large the new movements are also not only significantly less anti-Semitic than their historic precursors, a number of them, like the radical right in the US, are philo-Semitic and pro-Israel. There is undoubtedly an interesting story here having to do with the model Israel provides as a militant "nation-state" in a hostile region but also with the fact that hostility to Muslim immigrants has largely replaced the older anti-Jewish sentiments.

There is undoubtedly a New Right abroad in Europe. Europeanization as described by Mabel Berezin provides an innovative theoretical reframing for understanding how this has developed since the 1980s. What remains in question is more its relative sufficiency than its overall necessity. That constitutes both a significant reframing and an important accomplishment.

Comments by Andreas Wimmer

University of California, Los Angeles

Events or occurrences?

There is much to be liked about Mabel Berezin's new book. It is accessibly written, and has an eventful pace that not only matches well with its theoretical program, about which I will say more further below, but also keeps the reader entertained. Before our eyes, a breathless sequence of dramas unfolds, a series of million-men protest marches in Paris and elsewhere in France, fired up by passionate speeches; surprising election results that shake the political establishment in France and Europe, the even more surprising outcomes of a soccer match feeding into the political dynamics, grand European institution building combined with petty electoral calculus and factional fights. All of this blends together into a fascinating narrative of the rise, the fall, a renewed rise, and the final fall of the electoral fortunes of Jean-Marie Le Pen's Front National in France.

The book is also to be commended for the high standards of craftwomenship; it is carefully written, with much attention paid to the details of how these various event chains link with each other, and a quite unusual restraint in dealing with potentially divisive issues. Most remarkably, the author never lets her disdain for Le Pen's National Front cloud her analytical vision and precision.

The mélange of historical narratives that form the core of the book is held together by a twofold analytical movement. The first is of a decidedly structuralist or perhaps rather institutionalist nature. The other analytical movement is narrativist and historist (not historicist), advocating for the causal importance of events that steer societies onto unforeseeable paths of political development. Let me briefly address the institutionalist argument first.

Berezin argues that to understand xenophobic populism, we need to first understand the institutionalized compact between states and societies in Europe. These compacts rely on the idea and practice of national solidarity. The nation is thus far more than a mere imagined community, as the proverbial formulation of Benedict Anderson has it. It is also an institutional reality: National citizenship binds the population together and into a contract of solidarity with a state. These corporatist, welfarist arrangements have emerged from the ashes of the European wars of the 19th and 20th century and have given Europeans, Berezin argues, a high degree of comfort, predictability and security in the post-war era. Right-wing populism emerges when this security becomes endangered because the national compact is eroding. According to Berezin, globalization and Europeanization, that is, the transfer of sovereignty from nation-states to the European Union, are the two forces that are responsible for this development. Conformingly, the rise of Le Pen's movement is set against the backdrop of the process of European integration and the globalization that came with the fall of the Berlin Wall.

It is easy to convince me of this line of argument, not the least because it parallels my own writings on populist and xenophobic movements that were published from the mid-nineties onwards (cf. Wimmer 1997). I would only raise a question mark regarding the exact mechanisms that trigger populist nationalism. I think Europeanization and globalization are too specific. There are right-wing populist movements in societies outside the European Union, such as the US, Norway or Switzerland, and there were right-wing populist movements before the recent advent of globalization or the empowerment of the EU, such as in Britain under Enoch Powell, in Switzerland under James Schwarzenbach and in Germany with the rise the National-Partei Deutschlands. The mechanisms that lead to the emergence of right-wing populism therefore have to be cast in more general terms. I have suggested looking at downward social mobility as the main driver of right-wing populism, and I think the empirical evidence to support this hypothesis is pretty solid.

But let me turn to the second and more important analytical movement that holds this book together. It consists in an attempt to re-cast our understanding of Le Pen's rise and fall as an eventful history in the Sewellian sense. This means that Mabel embarks upon a search for events that were recognized by participants as meaningful and dramatic, and that changed the structural conditions that gave rise to the event in the first place. Building on Sewell, Mabel frames events as "templates of possibilities" that open up various paths of possible futures that may or may not be taken by powerful social actors, a clear rebuttal of path dependency theory. I had difficulties with this argument and its execution in the analysis of Le Pen for three reasons:

First, this understanding of history as eventful, unpredictable, contingency-driven is the opposite of the institutionalist argument that I summarized above. You cannot say history is driven by large-scale, slow moving forces such as the emergence of certain institutional-ideological regimes and say that history is unforeseeably developing *à l'impromptu*, whimsically pushed into the future by autonomous agents.

Secondly and perhaps more importantly, I was never quite clear what the events were that had an eventful character. In the main body of the text, it seemed to me that the French victory in the soccer world cup, the fall of the Berlin Wall, and 9/11 were treated as the contingent events that shaped the trajectory of Le Pen's party. In the final chapters, however, the party congress in Strasbourg and the anti-Le Pen mobilization that it produced, the decline of the party's fortune after it split into two rival factions, and the subsequent surprise success in the Presidential elections of 2002 are the eventful events. Determining which events count as eventful is far from trivial, of course, since the idea of eventfulness is only meaningful if you can distinguish events from simple occurrences, the things that happen but are neither considered consequential by actors nor change the structural forces that operate in a society, but rather tend to reproduce them in new forms. So which of the candidate events meet this criterion of eventfulness?

Let me assume for now that the dramatic climaxes in the party's history represent the crucial events. But how far is this story eventful in the sense of the author? It seems to me that a much simpler reading of these developments, one that does not embrace the Sewellian notion of eventfulness, is at least as convincing. It's the story of the steady rise of a populist movement, driven by downward social mobility that the post-Fordist age ushered in France as elsewhere, and its fall as soon as its charismatic leadership is weakened. All weakly institutionalized, populist movements depend on leadership. They will not take off without leadership, and they will decline when leadership is split, or when leaders disappear. So let's review the history of the Front's rise and fall from this point of view and compare it to the Sewellian reading that Berezin is offering. Le Pen is the charistmatic leader that allows the movement to take off. This is undoubtedly connected to events, because Le Pen might as well not have been born or killed when fighting for the Foreign Legion against Vietnamese independence, but I doubt it is an eventful event by Berezin's own standards.

The counter-rallies by anti-Le Pen forces, such as in Strasbourg, strike me as quite uneventful too, since the rise of populist parties is accompanied by left-leaning counter-mobilizations wherever you go. When the party splits, the movement does badly in elections. That is an event indeed, since Le Pen and his second in command, Bruno Mégret, might as well have gotten along just fine and prevented a fratricidial electoral war. It is not eventful, however, because a split movement is always weaker than a unified one and nothing about the outcome of the subsequent elections is surprising.

The movement regains strength at the polls, in the presidential elections of 2002, mostly because the left was hopelessly split between various presidential candidates after the Plural Left alliance of Jospin had fallen apart. That's an event, because the left might as well have rallied around Jospin, but it did not. But it's a quite uneventful event since, again, it is unsurprising for a Presidential election that a candidate is weaker if many similar candidates are vying for electoral support.

The movement is in decline recently, because Le Pen is an old man now and the movement lacks a powerful, charismatic leadership figure and much of the political energy that the Front harnessed over the past two decades has now found a new political home in Sarkozy's center-right party. That's a story full of events and contingency, because Le Pen's youngest daughter might have been a more gifted populist than she is or Sarkozy might never have been born. It's very unsurprising, however, because we know that populism without charismatic leadership does not go very far.

While there are thus, as always in politics, plenty of contingent events that shape the course of history, I remain unconvinced that any of these events are indeed eventful enough to qualify as "templates of possibility" in Berezin's terms, moments of historical opening that changed the very forces that gave rise to the populist movement in France. The story is full of occurrences, to put my criticism in shortest possible terms, but seems to be quite uneventful.

Wimmer, Andreas. 1997. "Explaining racism and xenophobia. A critical review of current research approaches", in *Ethnic and Racial Studies* 20 (1):17-41.

Comments by Dario Gaggio
University of Michigan

First off, let me preface these comments by saying that I won't have much to contribute to Mabel Berezin's penetrating analysis of the rise and fall of Le Pen and the French National Front. Since I'm a historian of modern Italy, I will focus my comments on the shadow case of Italy and its treatment in Berezin's book, but I will also try and keep the European context in the foreground.

I found Berezin's focus on events a brilliant methodological move, which truly serves her well in pushing forward the debate on right-wing populism in Europe and beyond. By focusing on events as analytical sites, she manages to defamiliarize a well-trodden territory and bring in that element of contingency and, as she calls it, surprise, that can open up new vistas. Berezin usefully defines events as "templates of possibility," building on Bill Sewell's insights, and in so doing she draws attention to connections and imaginings capable of linking different social fields, rather than to causal chains of variables layered by type (the social, the economic, the cultural, etc.).

One of the major payoffs of this strategy is the reconceptualization of the ways in which right-wing populism has been "mainstreamed," a term which is not even deployed in the text. By focusing on events, Berezin is able to weave a narrative much more sensitive to the complex ways in which agency is distributed than most political analyses allow for. Thus, we move beyond economically or politically reductionist explanations that rely on visions of populism as an insular reaction to economic insecurity, as is the case in the otherwise excellent ethnographically oriented book by Doug Holmes, *Integral Europe*, as well as somewhat conspiratorial stories in which right-wing populists are "used" by more experienced politicians for devious electoral goals. By the same token, cultural change is not understood in isolation. And it is in the realm of political culture that the double methodological innovation of focusing on events and thinking comparatively really pays off. By drawing attention to the path dependent character of political culture, appreciated in its eventful interactions and feedbacks, Berezin for example is able to make admirable sense of the different degrees to which Le Pen and Gianfranco Fini have been absorbed into the French and Italian political systems. And again, she shows that there was nothing preordained or inevitable about any of these developments.

Having said all that, there are unanswered questions about event-focused methodologies. There is already a small library about the issue of event selection, about the relationships between events and the processes in which they are inscribed, about the criteria an occurrence must meet to become an event, and so on. Here Berezin leads by example, more than by explicit theorization, which I believe is a good choice. But I will raise a point or two about such methodological problems. For example, an event in the Sewellian sense (and Berezin seems to agree with this) must be recognized, emotionally charged, and narrated by the social actors involved. This often comes to mean a focus on events of national or international relevance, sometimes even capable of redefining what nationhood is all about (as is the case with ur-event of the storming of the Bastille). But what about events that reveal the contours of the nation only indirectly, through the ways they weave silences and define margins, rather than centers?

I would argue, for example, that the foundational event of Berlusconi III—the government coalition that was brought to power in the landslide election of April 2008—was one of these seemingly peripheral events. It seems to me that the baptism by fire of Berlusconi's new government took place in the country's underbelly, where the refuse—literal and metaphorical—of the social body came in full display. The dust had barely settled on the ballots when, on May 11, 2008, Minister of Interior Roberto Maroni, of the Northern League, stated publicly that "All Romani camps will have to be dismantled right away, and the inhabitants will either be expelled or incarcerated." Two days later on 13 May, in the same areas that had seen riots on account of an embarrassing garbage crisis, a mob used Molotov cocktails to raze a Romani camp in Ponticelli, a suburb of Naples. So here you have a government claiming to solve two problems, of providing two services at once, which are intimately connected in the imagination of the Italians—garbage collection and security from crime—and responding to a surge of concerns for "cleaning up" for *pulizia* rising from below.

In the wake of these and several similar incidents of mob violence, Umberto Bossi, the national leader of the Northern League and someone not famous for his compassion towards the plight of the southerners, stated that "People do what the state can't manage," while Minister of Interior Roberto Maroni also stated, "that is what happens when gypsies steal babies, or when Romanians commit sexual violence," referring to a gruesome case that had occurred in Rome the previous November. In a sense, this was indeed democracy at work. According to the results of a reputable poll conducted in May 2008, 68% of Italians want to deal with the "Gypsy problem" by expelling the lot of them. And indeed the riots were followed by a series of emergency measures targeting the nomadic population, as the Italian authorities and media routinely refer to the Roma and Sinti (even though they probably travel less than the average middle-class Italian). In particular, the "nomads" began to be fingerprinted by the thousands. No one knows how many people have been subjected to this (we do know that children have been especially targeted, both to "protect" them and make sure they get reached by largely imaginary social programs). We also have no idea where this information is stored and what its legal status is.

These Roma-specific measures were accompanied by a series of decrees, nicely wrapped into a "security package" and dealing with immigrants in general. Among the measures is the possibility of expulsion of all foreigners (including EU nationals) who have received a two-year prison sentence. Also, the status of illegal immigrant was criminalized and designated as an aggravating circumstance in the case of other crimes (illegal immigrants are to be punished more severely than legal residents for the same crime). Finally, now illegal immigrants can be detained for up to 18 months in another network of camps, called by the center-Left Centers of Temporary Permanence and Assistance, but now renamed Centers of Identification and Expulsion.

It goes without saying that many of these measures could be argued to violate national and international laws, and many NGOs as well as the demoralized opposition parties have cried bloody murder. On July 7, for example, the European Parliament passed a resolution against the fingerprinting of specific minorities. But more recently the European Commission has argued that this measure does not in fact violate human rights as defined by the EU, accepting the justifications of the Italian government. The Italian government, on its part, has reassured the international community and Italian residents that all Italians will be fingerprinted by 2010, so that the state of exception may become part of a newly defined state of normality.

All of this eventful activity was preceded by a barrage of televised coverage on the rise of crime, which is by the way belied by official statistics. And sure enough, on the eve of the elections, a plurality of Italian voters (21%) pointed to the fight against crime as the most important issue guiding their decisions. As a widely circulated newspaper article by journalist and political scientist Ilvo Diamanti has pointed out (and the data is by a reputable academic source), crime reports on the Canale 5 news, the most widely watched channel and Berlusconi's property, have declined by a full half since the electoral victory of the Cavaliere. Not sure what's happened of late to Romanian rapists and victimized Romani children, but they surely no longer populate the screens. Problems solved, perhaps? Who knows.

I am one of those Italian expatriates Berezin mentions in her book, who are dismayed at, and ashamed of, what is going on in Italy right now, and it's all too easy to get all indignant about these stories. I'm not focusing on this series of events to take issue with one of the book's central arguments, the necessity to look beyond immigration to account for the appeal of right-wing populism. And I also understand that there are important differences between France and Italy in this regard. I think that the book's intervention is indeed a very welcome corrective to the facile emphasis on the forever escalating racism and xenophobia of Europeans to explain pretty much anything these days, especially on this side of the Atlantic. I also think that the book's counter-emphasis on political economy is extremely urgent and compelling.

However, this story allows me to raise a couple of questions which I believe are pertinent and not completely clear to me from my reading of the book. These questions are connected but also distinct. The first deals with the meanings of security and insecurity, which, we all agree, lie at the very core of the populist right's appeal. What are we to do of the relationship between perception (or even imagination) and reality, and by that I mean above all security as defined by fear of crime, which calls for the state's repressive power, and security in the more concrete, real sense of access to jobs, public services, educational opportunities, etc., security in the political-economic sense, which used to be (and maybe still is) the territory of the political left? In the political debate, the Italian left sees itself as dealing with the bedrock of real values and concerns, while viewing the right as conjuring up irrational fears and unrealistic expectations. Indeed, it seems that the left has lost the war of imagination. The imaginings spawned by most events seem to carry a distinctively authoritarian accent these days, at least in Italy. Berezin's book points to a possible answer to this question. Part of the problem lies in the fact that the hands (and tongues) of the political left are tied by the barrage of neoliberal constraints posed by globalization and European integration, and therefore individual security from crime becomes the only "service" whose provision the state can claim to expand with credibility. But is that all there is to it? Couldn't this also become something of an alibi? Anyone who has an even remote knowledge of the Neapolitan left can easily imagine how comforting this answer may sound to the Bassolinos of the world, when they are confronted with the kinds of issues that are raised by the events I've recounted… In fact, it's at the very concrete, local level of day-to-day experience that the Italian left seems to have abdicated and vanished. A stroll in any of Rome's borgate will show what I mean by that.

And this leads me to my second question. What is European integration really about? As the example I've just recounted shows, the EU does not always speak with one voice, and it certainly doesn't always undermine national sovereignty. Indeed, one of Berezin's most interesting findings is that in the Italian context, in at least partial contrast with France, European integration may well be regarded as functional to the buttressing of national cohesion. Also, it's at least interesting to point out that the EU sanctioned the Austrian coalition government which included Joerg Haider in 2000 for the suspicion that it may have engaged in acts contrary to European values, but less than ten years later the Italian government gets away with an **actual** series of measures which even the most cautious observer would qualify as overtly discriminatory and racist. What does that say about the evolving relationship between the EU and right-wing populism and between national and supranational governance more generally?

Indeed, there is a tendency in the literature to look for the essence of European integration, for its ultimate meaning and function. And I'm not sure that's a productive search. Is the European Union about the promotion of neoliberalism? In some ways, of course, it is, and Berezin's book does an excellent job of highlighting some of the venues in which this takes place, from increasing labor flexibilization to tendentially deflationary economic policies. But clearly the EU is also about a massive project of re-regulation, especially in fields like agriculture, environmental policy, and even social policy (although to be sure the EU has pretty blunt teeth in that regard). Indeed, people in Italy are more prone to complain about the EU standards for motorcycle emissions, which have forced millions to get rid of their old mopeds, than about the consequences of the Growth and Stabilization Pact, of which they may well know nothing at all. There is of course an entire library devoted to making sense of the extreme messiness of European integration and supranational governance, but overall I would warn against viewing this process as only, or even primarily, the European face of globalization, carried out in function and at the service of international financial capitalism.

I would suggest that European integration is a highly contingent process itself, which responds to both pressures from below (or within) and to outside shocks. It may well be that the current financial crisis will significantly change things, much the way the crisis of the 1970s and early 1980s paved the way for the Single European Act and the creation of a single market. But this time the move might be towards more regulation, at least of capital flows and financial markets, rather than less. By the same token, what was unacceptable in 2000 for an Austrian government to even envisage has become OK for an Italian government to actually do. In sum, I would warn against the search for the iron logic of European integration, for I'm afraid there might not be any such thing.

To sum up, mine is a first of all a deep appreciation of Berezin's work. There is no question that the contours of the relationship between the market, state power, and civil society are being drastically redefined, and that right-wing populism offers a uniquely valuable prism to appreciate and assess these changes. In this sense, and in many others as well, we should be grateful for Berezin's painstaking work of mapping this complex and rapidly shifting landscape. But I suppose that mine is also a plea for an even more generous dose of contingency and unpredictability. Historians, we know, never get enough of those.

Response by Mabel Berezin

Cornell University

To begin, I want to thank Margaret Somers for organizing the SSHA "Author Meets Critics" panel and for assembling a distinguished group of tough critics! I want to thank the critics for taking the time to thoughtfully engage my work. Responding to their cogent and probing comments honors and challenges me. I am deeply appreciative that the critics found much to praise in *Illi-*

beral Politics in Neoliberal Times: Culture, Security and Populism in the New Europe. I will focus my response on their questions and criticisms.

The critics did an excellent job of summarizing several of the major themes and arguments of the book. For readers of *Trajectories* who have not read the book (although I hope that you will!), it is useful to briefly recapitulate its central tenets. George Ross astutely observed that it was "brave" of me to enter the world of contemporary European right politics. Historians welcomed me when I wrote my first book on Italian fascism, *Making the Fascist Self: The Political Culture of Inter-war Italy* (1997). Yet, I approached political scientists who protect their turf, whether it be European integration or political parties, with weapons drawn with some trepidation. Nonetheless, I forged ahead—convinced that the rightward turn in contemporary European politics could benefit from interdisciplinary eyes.

Right populist parties have been gaining electoral clout in Europe since the mid-1990s. Right wing parties are not new to European politics. What is new is that parties that analysts had viewed as extremist and fringe have attracted sufficient numbers of votes to become part of legally constituted governing coalitions. This is a surprising development. Post-war Europe prided itself on having learned the lessons of fascism and Nazism. The last threat to European democracy, Communism, collapsed when the Berlin Wall fell in 1989. What happened that allowed right wing parties to once more lurk on the European political landscape?

Noisy cadres of militants expressing extremist positions of various sorts distract from nuanced analysis of right populist parties. The electoral ups and downs of the genre of parties that constitute the *right populist moment* suggest that they are expressions of deeper social phenomena that explanations based on narrowly constituted analyses of party strategy and electoral behavior only partially capture. Political extremism of all stripes may generate violence and hatred—but it tends not to make electoral inroads. Electoral salience suggests *thin*, rather than *thick*, commitments. In contrast to the *thick* commitments that characterize xenophobic militants and ethnic nationalists, the ever variable *thin* commitments of disgruntled citizens are sociologically, culturally and politically important. *Thin* commitments make urgent the recalibration of the standard categories that analysts typically deploy to discuss the right.[1]

Illiberal Politics in Neoliberal Times argues that the accelerated process of Europeanization that includes political, economic and cultural integration is the core trans-European context within which the *right populist moment* emerged. It shows why and how market fundamentalism—the Archimedean principle of the neo-liberal project of the New Europe—creates social insecurities that right wing political parties exploit to their advantage. By analyzing the *right wing populist moment* in relation to the broader context of Europeanization and globalization, *Illiberal Politics* aims to unpack the political and cultural processes that evoked the *thin* commitments that characterize expanded support.

I designed *Illiberal Politics* as a "comparative historical sociology of the present." The book weaves together three stories: first, the story of the trajectory of the French National Front; second the story of European integration in France and in Europe writ large; and lastly, the contrasting story of the right in other European countries. Italy in the years between 1994 and 2007 serves as a shadow case that re-enforces some of the points made with respect to France and Europe more broadly. The book has three time frames that intersect with the analysis. The first time frame focuses upon France and the National Front in the years between 1997 and 2005—the year that French citizens rejected the European constitution. The second time frame that serves as a context for the right populist moment begins in 1994, the year of the first Berlusconi government in Italy. The broader time frame that serves as a reference point for the European context begins in 1980 when the post-war social contract began to unravel.

My remarks now focus upon the three broad categories that the critics addressed: first, Europeanization as my central explanatory proposition; second, a methodological challenge to my use of events; and third, coalition and strategy versus the politics of perception.

[1] Table 2.1 (*Illiberal Politics*, p. 41) systematically outlines the analytic strengths and weaknesses of typical approaches.

Consolidation Regimes and Europeanization: A Comparative Historical Approach

Ross challenges my emphasis upon Europeanization as a "cause" of the emergence of right populism and argues that he would attribute the success of the right, at least in France, to the economic distress that globalization creates. Agnew argues that I need to nuance my argument about Europeanization. Both Ross and Agnew claim that I need to pay more attention to immigration as a triggering variable. Wimmer agrees with what he labels as my "institutional approach" but argues that he does not see a mechanism that links Europeanization to the right. Agnew points to the failure of the left in Europe which has provided an opening to the right. Gaggio describes the Italian left as having "lost the war of imagination." Agnew points out that I raise the issue of the left but he would have preferred that I focus more upon it.

Illiberal Politics does not ignore immigration in its analysis. It simply weighs it differently. Migration, whether for employment, family reunification or political asylum, is an undeniable fact of past as well as present European experience. Immigration may be a necessary but it is not a sufficient condition to account for the contemporary right. As the book clearly shows (pp. 208ff.) in the case of France in the early 1980s, while groups such as SOS-Rascisme were busy organizing demonstrations against the National Front and the French media establishment was bemoaning Le Pen, the French state was busy designing laws that would seriously restrict immigration. While the right may have publicized the issue of immigration early on, the policy practices in European states around immigration in the last thirty years do not map exactly on whether a government is left or right.

Social scientists who study right populism in contemporary Europe frequently explain it as a xenophobic response to the increased presence of non-Western immigrants in diverse nation states. In these formulations, right populism is morally unfortunate but politically unsurprising. *Illiberal Politics* starts from the position that contemporary *right populism* represents a historical surprise not a political and social certainty. I argue that the emergence of the *right populist moment* in the 1990s and its intensification in various European venues must be analyzed as an unexpected rather than an expected or natural event. The presence of right populism poses a challenge to social science and commonsense assumptions about trans-nationalism and cosmopolitanism. In a multicultural Europe of acknowledged social and political expansion and increased cultural contact, right populism represents a recidivist contraction and turning inward that is puzzling.

I argue that the emergence of the *right populist moment* in the 1990s and its intensification in various European venues must be analyzed as an unexpected rather than an expected or natural event. The presence of right populism poses a challenge to social science and commonsense assumptions about trans-nationalism and cosmopolitanism.

Populism and European integration gained momentum during the nineties. As historical sociologists, we know that timing matters. If, as Ross argues, right populism was simply a response to increasing unemployment, then Europeanization should have provided an opening to the left. As we know, just the opposite occurred. The traditional European left has increasingly lost its voice in the years since 1992. European socialist parties are often technocratic and euro-friendly, and more importantly, they are losing elections. France and Germany, two major European players, have firmly established center-right governments. Anti-globalization groups such as ATTAC that began in France have provided a major critique of Europeanization. Although they represent a progressive vision, ATTAC is not affiliated with a political party, and as I show, often end up on the same sides of issues as the right.

Ross asks why we need to look for deeper reasons to account for the rise of the right. In posing this question, he overlooks the argument about the relation between nation and state that is a core theoretical contribution that *Illiberal Politics* offers. European integration is an instance of enforced social, political, economic and cultural trans-nationalism that challenges the standard prerogatives of the territorially defined nation-state. The accelerated pace of European integration dis-equilibrates the existing mix of national cultures and legal norms that governs those nation states. An unintended consequence of dis-equilibration is the weakening of the European social contract that threatens to make the national space "unfamiliar" to many of its citizens. "Unfamiliarity" has practical consequences and is more than simply a feeling of disorientation.

The modern nation-state is the institutional location of a relation between a polity and a people that provides security for its members. The institutions of the modern nation state legally inscribe individuals in the polity and society. Cultural practices from common language to shared norms cognitively and emotionally inscribe individuals in the polity and society. *Consolidation Regime* [*CR*] (Table 2.2, p. 50) is the heuristic that I develop in Chapter Two to capture the relation between national and state institutions.

CR is a relational concept that allows for variation. It is not a predictive model but rather a model of potential vulnerabilities that may become liabilities in specific historical circumstances. I argue that nations and states may be joined with varying degrees of institutional strength. I identify three types of *CR*s—the *hegemonic*, the *flexible* and the *brittle*. Each regime tends towards certain types of collective identities and is more or less vulnerable to certain types of threats. For example, France with a strong linking of state and national (think language policy and *laïcité*) institutions represents a *hegemonic CR*. With a strong national identity and corresponding institutions, France is vulnerable to external threats from McDonalds to Europe. France's *hegemonic CR* has left it in the paradoxical position of being both a founding member of EU and an ambivalent supporter.

*CR*s create national legacies—the dynamic experience of the relation between a people and a polity. Experience, *individual and collective*, is a temporal and cognitive phenomenon that consciously or unconsciously draws upon the past to assess the future. Experience creates a tension between imagined possibilities and perceptions of constraint. Social, cultural and monetary capital draws the boundaries of experience that permit individuals and groups to negotiate between institutions and culture. In the case of post-war Europe, the tension between culture and institutions was minimal. The post war European nation state was an arena that adjudicated risk for its members. Capital in all its dimensions was primarily national. "Social Europe" and the need to preserve it, a *pro-forma* comment built into integration discourse, is an acknowledgement of post-war social solidarity.

The mechanism that links the right and Europeanization that Wimmer requires is clearly stated in the book. In sum, the collective and individual experience of old Europe was solidaristic; the evolving experience of "new" Europe is individualistic albeit with a dose of ambivalence and nostalgia. In terms of my argument, "New" Europe, writ large, is an opportunity space for individuals and groups who are able to compete in trans-European economic, social and cultural markets. Yet, the experience of ordinary Europeans is still national—that is, their cultural and social capital, as well as their economic possibilities, are still firmly tied to the national state.[2] The disconnection between past experience and a European future that is oriented to the market rather than the collectivity is fueling a re-assertion of nation-ness that characterizes the right populist moment.

Methodological Queries: Events and Occurrences?

Wimmer engaged my methodology at length. Before engaging his discussion of events, I want to answer his question about whether one can combine an institutional and narrative analysis. My answer is a firm—yes! I do this in *Illiberal Politics*. Both methods inform and enrich each other and provide more robust answers to the

[2] See for example, Juan Diez-Medrano, *Framing Europe* (2003).

questions that I pose. In short, the institutional analysis, the *CR* provides an opportunity to develop hypotheses for future research—which I discuss at the end of the book but of course do not carry out. The narrative or event-ful analysis sheds a new angle of vision on the French case and contributes to the development of the *CR*.

I argue in the book that unraveling the puzzle that right populism presents requires historical, that is contextual, exegesis that shifts the location of analysis from individual or collective actors, whether voters or party operatives, to *events*. Contingent events, events that were unexpected and that emotionally engaged the national collectivity at all levels from the average citizen to political elites, form the basis of my analysis. I analyze populism through the lens of events that mark turning points in collective national perceptions.

William Sewell's work influences my approach, but I also depart from him in important ways.[3] First, the events that I chose to analyze do not change the course of history, such as the storming of the Bastille, rather they act as "templates of possibility"—that is, they enable individuals and collectivities to imagine a range of possibilities that they had not previously imagined. Only four events fall into this category in *Illiberal Politics*. The first is the 1994 election in Italy that brought the then post-fascist Gianfranco Fini into the government. That event signaled to Italians and Europeans that for the first time since World War II the right could be part of a legally constituted government. Although that election figures in the book in the shadow case of Italy, it does not merit an entire chapter—in part, because the event, important in the broader European context, loses salience in the Italian context—a longer story than can be captured here. The other three events are French: the 1998 regional elections; the 2002 Presidential election; and the 2005 rejection of the European constitution. I devote an entire chapter to each of these events.

Wimmer argues that I only consider occurrences because I seem to analyze so many events. I grant Wimmer his larger point but not his terminology. I clearly should have made a distinction between nodal events—eventful events—and the array of smaller events that occurred either prior to or after the "big" events that I focus upon. But this is precisely my methodological point. Rather than making a path dependent argument something that the book firmly rejects, I argue that eventful events derive their political meaning when we analyze them in the context of a wide range of events that are happening within a relatively similar time period.

Beginning in the 1980s, I map the trajectory of the National Front against the trajectory of French civil society, and the French state. Against these endogenous trajectories, I map the exogenous trajectory of the European Union. This method of comparing separate trajectories that are occurring during the same time period but are not path dependent embeds my three eventful events in a sea of other events. I then analyze the connections among all of the various events occurring in the same time period (Table 8.1, p. 210). For example, while standard analyses places the National Front on a downward trajectory from 1999, my method revealed that the moment of the Front's decline was actually the moment in which its ideas were gaining wide acceptance. In fact, the National Front, French civil society and the French state had remarkably similar ideas and practices with respect to EU and globalization. In the end, or at least the end in this book, the National Front's ideological success led to its political failure. Nicolas Sarkozy detached Le Pen's message from the messenger and ably defeated his Socialist rival, Ségelène Royal to win the presidency in 2007.

Collective Perception and the Politics of Emotion

Ross and Wimmer argue that there are simple explanations for instances where the right is politically successful. Success here is a relative term because, as I often point out, the European right is technically not in power, i.e., they are not yet running governments. Their political significance lies elsewhere. Ross notes that I do not take into account the French right's ability to influence coalitions—here, I beg to differ as I spend much time on that point in discussing the 1998 regional elections. The dire public effects of the 1998 regional elections were offset a few months later when France won the World Cup. The French state appropriated the public euphoria that the soccer victory generated. Politicians pointed to Zinedine Zidane, the star soccer player who was

[3] William Sewell, Jr. *Logics of History* (2005).

the child of immigrants, as a sign that despite the National Front, France was an integrated and multicultural society.

Ross and Wimmer analyze the first round of the French Presidential election of 2002 where Jean Marie Le Pen came in second place to the sitting president, Jacques Chirac. Both offer what I would describe as the standard political science story of Le Pen's "success" I also tell this story in Chapter 6 with some important differences. I view the 2002 presidential election as one of those eventful events where collective perceptions and emotions, not leaders, changed in important ways. Jacques Chirac, the center right President of the French Republic, and Lionel Jospin, the Socialist Prime Minister conducted a lackluster campaign primarily against each other. There were, however, candidates representing fourteen other political parties on the ballot.

Just about everyone who took note of such things in France, the media, the political science community, and the candidates themselves, failed to observe that Le Pen's ideas if not his person had been gaining strength—particularly his attacks on Europe, globalization and his defense of social solidarity. France's two round electoral system combined with two establishment candidates who were not all that far apart on many issues proved fortunate for Le Pen. Record numbers of citizens stayed home. The abstention rate was 28.4%. Citizens who bothered to vote registered protest votes and spread their votes among the other fourteen parties. Voila! Le Pen came in second place with 16.86% of the vote compared to Jospin's 16.18%. Le Pen went on to the second round. His presence on the ballot returned Chirac to the presidency with 82% of the vote.

Illiberal Politics documents this story in minute detail. The 2002 election is important for what it reveals rather than for its outcome. For those who do not follow French politics and only learned of this election from Wimmer and Ross's critique, one would think that French citizens yawned and went quietly on to the second round where they voted Le Pen into political oblivion. But that was not what happened. All of France erupted into a spasm of political fear and collective shame. Le Pen's second place was a "template of possibility" because it suggested that he **could** be President of France, not that he **would** be President of France. No one thought that he could ever win. What was at stake was the blow to national self esteem and the threat to France as protector of the "rights of man" and repository of Republican virtue. Demonstrations blanketed the country for the two weeks between the two rounds. The demonstrations in Paris against Le Pen on May 1 were larger than the demonstrations that had occurred when the Allies liberated France from the Nazis in 1944.

But that was not the end of it. April 21, the date of the first round, became a metaphor for political fear and collective emotion in France. In the years, between the 2002 and the 2007 Presidential elections both left, right and center invoked the date to argue for their political positions. A *Lexis-Nexis* search of French newspapers for that period revealed that "April 21" appeared as a metaphor in 908 headlines. For example, partisans of the European constitution urged French citizens to vote "yes" to avoid of repeat of the "shame of April 21." Politicians and pundits warned citizens not to repeat "April 21" when they voted in the first round of the 2007 presidential elections.

My response has become overly long which is a tribute to the cogency of my critics! I have tried to answer the broader questions that they raised, as well as the questions that grouped together. For this reason, I have not given as much attention as I would have liked to Gaggio's remarks on Italy. He does however end his comments with a question about the meaning of Europe as an entity. It is a good place to end my remarks also. The problem with working in the present is that events keep flowing. *Illiberal Politics* suggests that the significance of the right in some nation-states is that they serve to re-assert national identities and promote center-right political coalitions. The 2009 European Parliament elections were an important harbinger of political direction. In short, the center-right dominated; the left did extremely poorly; and in those nation-states with *brittle CR*s, far right politicians won seats. The eventful event confronting Europe at the moment is the global financial crisis. By all accounts, it is not only the extreme right that is questioning a commitment to a neo-liberal Europe—but that is surely the subject of another book!

Citizen Employers: Business Communities and Labor in Cincinnati and San Francisco, 1870-1816 by Jeffrey Haydu (Cornell University Press, 2008)

Jeffrey Haydu's new book was also the subject of an Author Meets Critics panel, organized by Isaac Martin at the 2009 SSHA. What follows are the comments of the four critics (Larry Isaac, Pamela Walker Laird, Ajay K. Mehrotra, and William Roy) and a response by the author.

Comments by Larry Isaac

Vanderbilt University

Citizen Employers is a lovely book, one rich in sociological theory and historical evidence woven together in a tight package. Haydu follows an important lineage of fine class formation studies in contemporary historical sociology (e.g., Katznelson and Zolberg 1986; Kimeldorf 1988; 1999; Voss 1993; Stepan-Norris and Zeitlin 2003). The book's framing draws a deft link between the power of citizenship as an ideology and practice during the late 19th century, one reminiscent of Karl Marx's addresses to workers as "Dear Citizens" and David Montgomery's (1993) *Citizen Workers*. At least for that historical era, the term 'citizen' elevated solidarity with the common good above seemingly narrow class interests, thus serving a key operation in forming a hegemonic position that appears to advance universal interests of all citizens while obscuring particularistic interests. Importantly, *Citizen Employers* differentiates itself from these prior studies by virtue of focusing on capitalist class formation instead of working-class formation.

The Puzzle and Core Solution

The historical-sociological puzzle which Jeff sets out for himself is this: Why did two capitalist classes, located in two comparable cities in the same country at the same time, differ so dramatically in: (a) their displays of unity; (b) civic ideologies; and (c) their dominant views and practices regarding labor unions. More concretely, why: a unified capitalist class following a "business citizenship" ideology and taking a strong anti-union line in Cincinnati; and a divided capitalist class following a "practical corporatism" and a tolerant approach to unions in San Francisco?

Haydu employs a methodological strategy that consists of: (a) going to the sub-national level; (b) selecting two municipalities that differed substantially on the outcome variable–bourgeois class formation in the tradition of Mill's indirect method of difference; (c) close sequential analysis of within-case path dependencies and contingent events where the processes are played out through narratives attentive to iterative problem-solving (Haydu 1998) done by business classes in those two cities; but (d) ultimately there are three cases, where cases consist in relatively homogeneous place (city)-time configurations: Cincinnati, postbellum years to WWI; San Francisco, postbellum years to around 1911; San Francisco, 1911 to WWI. Rather than following the conventional approach to U.S. exceptionalism which operates at the national-level, usually without any direct comparison with other putatively non-exceptional cases of working-class formation, Haydu locates his puzzle *within* the U.S. exceptionalism puzzle by focusing on sub-national processes of business class formation. By descending to the local/municipal level—the level at which key organizational and political actions formed during this historical period—he is able to demonstrate divergent bourgeois class formation processes; one that looks presumably like most of the U.S. (by way of Cincinnati) with its denial of the reality of class as it formed into one as "business citizenship," and on the other hand, one that is the exception within the U.S. exception (San Francisco), a grudging pragmatic corporatism that recognized unions as a fact of life and a force to be dealt with in local politics and industry.

The working-class is the central focus in analyses of class formation and in the American exceptionalism literature where one key puzzle is the absence of a strong organized working-class. Within the exceptionalism discourse—where the dominant strand finds the answer to American exceptionalism in its exceptional workers—there are some analysts who have concluded that what was truly exceptional about American political economy and class relations was not its workers, but rather its *employers* (e.g., Jacoby 1991; Voss, 1993; Lipold and Isaac 2009). Yet we have precious little attention paid to the formation of business classes. This is Haydu's central focus wherein he establishes an important and clear conceptual understanding of first what he means by *employer class formation* as the simultaneous double movement of (a) increasing separation *between* employers and workers, and (b) extended and intensified ties *within* various kinds of employer organizations. He finds that business class formation, largely among proprietary capitalists, took place through very different processes, conditions, and events in Cincinnati in contrast to San Francisco producing very different business class formations. By the middle of the second decade of the twentieth century, the differences in business class forms, ideologies, and practices began to dissipate as San Francisco came to look more like Cincinnati, and by extension the rest of the United States.

Drawing from an impressive variety of sociological literatures (e.g., class formation, social movements, neo-institutionalism, historiography on Gilded Age and Progressive Era, and historical sociological methods), Jeff weaves together a comparative tapestry that shows the dynamics of divergent bourgeois class formation in those two cities by employing three big concepts to organize this analysis: solidarities, identities, and transposition. Similar to working-class formation, employer class formation is about forming solidarities, overcoming divisions and narrow self-interests to align on the general interests of business as a class. Just as the forming of such solidarities is always an intra-class struggle for workers, so too for capitalists. Haydu insists that the latter is not automatic, nor can it be written off as never occurring, but is rather filled with serious challenges and is an outcome that is multiple, and shaped by complex contingent causal events and processes.

In Cincinnati intensified social unrest in the 1870s and 1880s served the republican alliance of wage earners and small capital, favoring a realignment of middle and upper classes under a "law and order" banner. Moreover, Cinnci's organizational class associations also reflected concerns like municipal corruption and economic competition from rival cities in addition to and perhaps more prominently than the "labor problem." Cincinnati employers formed a collective identity which Haydu calls "*business citizenship*," a cultural, civic leadership identity and position which was first forged in the political arena and then transposed into practice in the workplace. The business citizenship identity and practice was one which organized employers as a class while it simultaneously denied the existence of class. As such, Haydu sees the Cincinnati formation and ideology as largely representative of the U.S. as a whole: "In the absence of serious working-class political challenges or sustained, city-wide union threats, employers mobilized broadly around issues of civic order, not industrial conflict." (p. 37)

San Francisco followed a very different class formation trajectory among its businessmen. There a combination of small proprietary capitalists and a few over-bearing corporations kept alive a shared opposition to "monopoly" in San Francisco politics that formed a coalition of proprietary capital and skilled white labor. That cross-coalition was fueled by both an anti-monopoly stance and anti-Chinese sentiment. Because large corporations hired cheap Chinese labor, and low costs of entry led to Chinese-owned sweatshops, the Chinese were a common enemy of both white labor and business proprietors. This particular configuration gave rise to a dominant employer identity and practice that Jeff terms "*practical corporatism*," one that was rooted in a strong union position that proprietary capitalists could not and did not ignore. By 1911-1916, San Francisco's path dependent "practical corporatism" was derailed and a business citizen identity and practice similar to that of Cincinnati, and presumably much of the rest of the U.S., began to prevail there too. But for a number of decades, the exceptional U.S. case of class formation contained at least one local bourgeois class formation that looked similar to some Western European countries that admitted labor unions to the table of political power.

Critical Comments & Analytical Questions

Jeff has written a fine book, one which I have used and will continue to use and learn from. But the book also raises a variety of thorny issues, four of which I will address here. My first and second points are linked and focus on organizational paths to business class unification. Organizations are central to fostering class formation in Haydu's account and so too the sequence or path that Gilded Age/Progressive Era businessmen followed in their organizational activities. In other words, what kinds of organizations are highlighted and what sort of historical trajectory do they display in this study? How city-specific or general are these organizational forms and paths? By bringing in just one additional city-case, I suggest that important organizational forms of business class formation do not appear in Haydu's cases and that the sequence of organizational development in route to class formation is even more variegated and divergent than suggested by Haydu's comparison of Cincinnati and San Francisco. A third concern was the tendency to focus so deeply on San Francisco and Cincinnati almost to the exclusion of important national-level events that must have played in class formation, however contingently, in each of these cities. Finally, the exceptional case of San Francisco's relatively tolerant approach to unions (at least until about 1911-16) made me wonder if it was in fact exceptional in other ways too. I wondered, in particular, if business interests in that city shunned the use of physical violence against workers during strikes or if they followed the all-too-frequent pattern across much of the country at this point in history.

Comparative case selection via the indirect method of difference is put to good use by Haydu as he features these two divergent local business class formations. The theory and empirical substantiations based on these locales is quite convincing. But the argument could have been even more convincing if we knew more about the distribution of "business citizenship"—its making and possibly different organizational materials and paths in various economic, cultural, and civic organizations—across other cities in addition to San Francisco and Cincinnati. Here's where I was looking for the added leverage that might have been provided by augmenting the method of difference with the method of agreement, where the latter would be deployed to target another city's class formation process within the presumed vast sea of non-exceptional cities that comprised the macro-existence of "business citizenship" Jeff finds in Cinnci. This might be too much to ask of an already excellent and detailed study of two cities–you should have just had one more city. I realize how trite that might sound. But let me play this out just a bit with an additional case.

I recognize much of Jeff's Cinnci story in the historical terrain of Gilded Age Cleveland. For example, the collective identity of "leading citizens" and "business leaders" and "civic duty" with equations being drawn among these terms is all quite familiar, as is the important role of organizations–economic, cultural, civic–in unifying Cleveland's business class. So on the one hand, I was struck by similarities, on the other hand, there were significant differences between the "Queen City of the West" and the "Forest City of the Western Reserve," differences that add to and raise questions about just how representative the Cinnci *path* (not so much the business class outcome) really was. Two examples will illustrate what I believe to be a different path to "business citizenship" across two cities in the same state.

1. The developmental sequence of key organizations that served to unify the business class.

Jeff writes (p. 49): "The causal path from challenge to response runs through organization. Employers met new problems by organizing." For historical sociologists, *sequence* often matters, and it certainly does in Haydu's argument. The organizational sequence that Jeff identifies in Cinnci was largely one that started narrow in *economic trade associations*: During the late 1860s to 1870s, Cincinnati manufacturers formed trade associations (tobacco, leather, boots/shoes, horseshoes, iron, beer, furniture) at a rapid pace (p. 49). Built to deal with market crises and the labor problem, these trade associations were aligned by industrial function, which narrowed their constituencies to members within specific industries or trades. The more inclusive and important organizations from a class formative perspective came later, ostensibly organizations of "cultural enrichment" and "civic improvement." So, according to Haydu's account the sequence of organizational initiatives that mattered for business class formation in Cincinnati followed the narrow-to-broad pattern: trade associations→ cultural associations→ civic associations→ employers' association. Not only was sequence important here, but these were very distinct types of organizations, and only the last phase "employers' association" was both unifying of the business class and pointedly anti-labor.

There were two notable differences in Cleveland's business organizational trajectory. First, the organization that played the final general unifying role in Cinnci–the Employers' Association–came first in Cleveland in the form of the Cleveland Board of Trade (CBOT) which was founded in 1848 by 36 local businessmen to help their members operate their businesses more efficiently. The CBOT was largely a mercantile organization in the antebellum years, but had diversified to cover all major economic sectors of Cleveland industry by the 1870s. In 1880 there were 249 members covering industrial enterprises from banking to manufacturing to commerce to law to shipping and mining (CBOT, 1880). During the next three years it expanded to 274 members, and by 1893 when the association changed its name to the Cleveland Chamber of Commerce, it registered 1100 members. At least from the 1870s forward, the Cleveland BOT represented and unified capital from: merchants, railroad, manufacturers (esp. iron, oil), bankers, mining, lake shipping, construction, insurance, wholesale agents, self-designated "capitalists," and professionals (especially lawyers and politicians). As in Cincinnati, cultural and civic organizations came later, emerging between the 1870s-1890s. The difference is that these cultural and civic formations—mostly exclusive clubs—*followed* the formation of an extensively organized business association, rather than leading them as in Cincinnati.

What does this difference in business organizational trajectory mean? If Jeff is correct about the wide class formative influence of employers' associations, and I think he is, reasonably strong business class formation was achieved more rapidly and earlier in Cleveland than in Cincinnati. Cleveland business elites were using the language and collective identity of "leading citizens" before they had established much in the way of standard cultural and civic organizational infrastructure that was apparently required in Cincinnati. But how did any of this matter? Were the fortunes of labor, for instance, altered by the differential timing of business class formation in these two Ohio cities?

2. Synthesis of economic, cultural, civic organizational thrust inside a military shell.

But there was also a special kind of organization that blended all the elements of the organizational form that Jeff identifies in Cincinnati–but accomplished through functionally specific organizations, economic interest, cultural, civic, anti-union—into one organizational form in Cleveland. The years between approximately 1877 and WWI saw a generalized militarization of U.S. industrial cities. By *militarization* I mean the growth of military units in cities, parades of military formations in shows of strength, and a growing militaristic built environment evident in the increasingly common castellated military architecture in the form of armories to house units and their weaponry (see Fogelson 1987). This militarization process was not federal, but consisted instead of local militia organizations, of which there were two basic types: (a) On the one hand, there were the *state-organized militias*, regulated, financed and commanded by local-state officials and typically part of the nascent fledgling National Guard (e.g., Ohio National Guard, or National Guard of California), which in 1877 was still about a full quarter century away from being a truly integrated *National* Guard; and (b) On the other hand, there were the *independent militias*, which were organized, financed and commanded by private citizens. Following in direct response to the national labor revolt of summer 1877, two independent militias formed in the fall of 1877

Following in direct response to the national labor revolt of summer 1877, two independent militias formed in the fall of 1877 and the spring of 1878 in Cleveland—the First City Troop (FCT) and the Gatling Gun Battery (GGB). These two organizations were founded by upper-crust "leading citizens" of the city's business elite…

and the spring of 1878 in Cleveland—the First City Troop (FCT) and the Gatling Gun Battery (GGB). These two organizations were founded by upper-crust "leading citizens" of the city's business elite (about 110 men), and like the Board of Trade, these two organizations represented all major industrial sectors of the Cleveland business community and were comprised of the city's most wealthy businessmen—all living in the exclusive Euclid Avenue corridor known as "Millionaire's Row" (Isaac 2002).

The most salient point for present purposes is that the FCT and GGB did all the work of the various kinds of economic, cultural, and civic organizations identified by Jeff in Cincinnati, and more: they mobilized "leading citizens" for economic interests (e.g., intimidating and suppressing labor mobilizations) expressed as "civic duty," protecting community and city against what were characterized as terrorist uprisings threatening not only their business enterprises but ostensibly their neighborhoods as well. Under the guise of municipal civic duty, FCT and GGB members mobilized the most deadly armed force available, signaling the ability to suppress protestors while they practiced a manly duty that was lacking in their other affairs, and expressed a cultured good taste and refinement in the vast majority of the organizations' high society social functions.

These Cleveland citizen employers did all this organized as an armed bulwark of the business class while they repudiated the existence of social classes. They trained, purchased, and brandished military uniforms and arms to ostensibly do their manly civic duty as leading citizens of Cleveland. The elite Cleveland militias formed an important organizational step that was present there, but apparently missing in Cincinnati and San Francisco. These militias combined both: (a) the attributes of civic virtue (business citizenship) with (b) a very direct concern with the "labor problem," which motivated their initial formation. Moreover, the very same families and individuals that formed the FCT and GGB also populated the Board of Trade and the elite civic and cultural clubs of the city.

The FCT and GGB illustrate a distinctive organizational link in bourgeois class formation that occurred in some industrial cities during the Gilded Age. Thus, the Cleveland case tells us that (a) there was more than one organizational sequence to business citizenship style class formation in Gilded Age cities; and (b) private elite militias were multidimensional, serving the economic, cultural, and civic functions of other specialized organizations while projecting outward a military solution to the "labor problem" that could be addressed by leading citizens as a matter of civic duty and projecting inward a culture of manliness.

There is more to the militia development trajectory that parallels the broader class formation process that Jeff elaborates so well. Not only did some private elite capitalist militias form anew but others were made more class homogeneous as they recomposed their personnel sloughing off untrustworthy lower class members; still other organizations considered too untrustworthy, were dissolved altogether (Isaac 2010). The founding of new private elite units like those in Cleveland not only deepened business ties while separating them from wage earners, but also collectively armed the private citizens of one class while collectively disarming those of the other class.

The militarization of "leading citizens" was an integral part of business class formation in some cities. In such municipalities, citizen employers were quite literally "*citizen employer soldiers.*"

3. The role of national-level processes in city-level class formation.

While cities are good units, perhaps the best, for analyzing class formation during this historical period, I wondered how national-level processes and events may have reverberated in similar or perhaps different ways in Cincinnati versus San Francisco. Did the national-level labor uprising that began with a railroad strike in summer of 1877 have the same consequences for business class formation in these two cities? What about the Haymarket riot of 1886 and other major flashpoints of class struggle between 1870 and World War I?

After all, it was during this historical period that regional markets were being linked to together into national market networks. So if the sphere of circulation figures at all in capitalist class formation, and there is reason to believe it does (Marx, *Capital*, Volume II; Van der Pijl 1984), we might expect national level events to play an increasingly important role at least for those segments of capital that were most expansive in their production and circulation spheres (e.g., rails). This also raises serious questions about how we conceptualize capital and how capitalist class formation might be qualitatively different from that of working-class formation. A recognition of such a distinction is not immediately apparent in *Citizen Employers*. For to draw parallels in the difficulty of class formation for both capital and labor but not identify what is truly different, is a missed opportunity.

4. Virulent Anti-Laborism: The Exceptional Character of the Business Class in the U.S.

San Francisco is Jeff's exceptional island within the vast American exceptionalism sea—i.e., the city that followed a corporatist path for most of the Gilded Age and Progressive Era not greatly different from some Western European nations that are often held up as the baseline from which the U.S. is decidedly the exception. One of the key features of U.S. exceptionalism compared to most Western European nations is that what was really exceptional here historically was business (business-state relations), and not so much a peculiar working class. U.S. business demonstrated a virulent anti-unionism that is typically presumed to be much more severe than that encountered in Europe. Some version of this claim has been made by Jacoby (1991), Voss (1993), Lipold and Isaac (2009), and others.

So according to this position and Jeff's finding that San Francisco was a corporatist haven, a relatively labor-tolerant exception, one might expect to find less anti-labor violence in San Francisco than in Cincinnati. Why? Because Cincinnati had much earlier consolidated into business citizenship and an anti-labor cultural stronghold relative to corporatist San Francisco.

One way to gauge relative anti-labor violence across cities is to compare the number of workers killed in strike activities in each city. Between the end of the Civil War and 1920, the frequency of strike deaths was greater in San Francisco (n=20) than Cincinnati (n=3), by almost a factor of seven (data from Lipold and Isaac 2009). This is not the record one would expect based on the image of a corporatist, *relatively* labor-friendly San Francisco. So I am inclined to think, at least preliminarily, that while Jeff has found something different and important in San Francisco's class relations during the late 19th and early 20th centuries, there was still a strong willingness to use lethal violence against workers' collective action there, just as there was in other localities across the nation.

Significance

I will end by sounding several notes of significance. First, Jeff's analysis illustrates the weakness of exclusive reliance on broad country-wide explanations for class formation and arguments about American exceptionalism. There was a time when multiple and quite divergent paths to business class formation took place across America. Second, the *Citizen Employers* nicely demonstrates the fluidity and permeability between industrial, civic, and cultural spheres and how ideology and practice in one arena was effectively transposed to another. Third, although not automatic or easy, Haydu shows how class was effectively organized by business interests while the existence of class was simultaneously denied. Finally, *Citizen Employers* serves as an exemplar of fine-grained, locally-grounded, sequentially-sensitive, comparative-historical analysis and will most certainly stand as a touchstone for future studies of American business class formation.

References

Fogelson, Robert M. 1987. *America's Armories*. Cambridge: Harvard University Press.

Haydu, Jeffrey. 1998. "Making Use of the Past: Time Periods as Cases to Compare and as Sequences of Problem Solving." *American Journal of Sociology* 104 (2): 339-71.

Isaac, Larry. 2002. "To Counter 'The Very Devil' and More: the Making of Independent Capitalist Militia in the Gilded Age." *American Journal of Sociology* 108 (2): 353-405.

Isaac, Larry. 2010. "Policing Capital: Armed Countermovement Coalitions Against Labor in Late 19th-Century Industrial Cities." Forthcoming in *Strategic Alliances: New Studies of Social Movement Coalitions*, edited by Nella Van Dyke and Holly McCammon. University of Minnesota Press.

Jacoby, Stanford M. 1991. "American Exceptionalism Revisited: The Importance of Management." in *Masters to Managers: Historical and Comparative Perspectives on American Employers*, edited by S.M. Jacoby. Columbia University Press.

Katznelson, Ira and Aristide R. Zolberg (eds.). 1986. *Working-Class Formation: Nineteenth-Century Patterns in Western Europe and the United States.* Princeton University Press.
Kimeldorf, Howard. 1988. *Reds or Rackets? The Making of Radical and Conservative Unions on the Waterfront.* University of California Press.
Kimeldorf, Howard. 1999. *Battling for American Labor.* University of California Press.
Lipold, Paul F. and Larry Isaac. 2009. "Striking Deaths: Lethal Contestation and the 'Exceptional' Character of the American Labor Movement, 1870-1970." *International Review of Social History* 54 (August): 167-205.
Marx, Karl. 1967. *Capital.* Volume 2. International Publishers.
Montgomery, David. 1993. *Citizen Workers.* Cambridge University Press.
Ross, Steven J. 1985. *Workers on the Edge: Work, Leisure, and Politics in Industrializing Cincinnati, 1788-1890.* Columbia University Press.
Stepan-Norris, Judith and Maurice Zeitlin. 2003. *Left Out: Reds and America's Industrial Unions.* Cambridge University Press.
Van der Pijl, Kees. 1984. *The Making of an Atlantic Ruling Class.* Verso Press.
Voss, Kim. 1993. *The Making of American Exceptionalism.* Cornell University Press.

Comments by Pamela Walker Laird

University of Colorado, Denver

When I reread Jeff Haydu's superb *Citizen Employers* for this discussion, I was teaching "Theory & Practice of History," my department's required introduction for undergraduate majors. This bit of serendipity highlighted the benefits of thinking about a sociologist's successful methods for historical analysis. It also underscored differences between historians' and social scientists' goals and practices, reinforcing the importance of maintaining both disciplinary differences and conversations across them.

In every history class, I struggle to convince students that, while simple narratives can be fun to write and to read, good historical scholarship must do more. It must analyze. In contrast, students rarely resist the need for research in archives or other primary sources that solid historical work requires. That sort of digging is one of history's lures. Of course, it takes a lot of research to transform hypotheses into viable arguments; and evidence always requires revising even the best of hypotheses. Conversely, no amount of piling on of data will generate an argument. Social scientists' comfortableness with abstraction—with theory—can help historians strike a productive balance.

Novice and antiquarian historians often find historical treasures as they eagerly sift through their sources. Yet they, and even professional historians, are often somewhat at sea about the significance of their discoveries. Haydu's sense of what matters within his evidence and *why* it matters is exquisite. A small example among many such treasures in *Citizen Employers* comes from comparing the 1890 San Francisco and Cincinnati city directories. This seemingly dreary task revealed that the former placed employers' associations and labor unions within a single category, namely "Protective Associations," while the latter separated them, placing trade unions under one heading and employers' associations under another labeled "public bodies," along with benevolent societies and social clubs (175). By aligning this distinction with other patterns in his evidence, Haydu recognized that it reflected a very real difference in how the cities' businessmen believed which organizations represented responsible members of the polity. What might have been a mere "factoid" in another's hands makes an important point about whose citizenship mattered within the mainstream. Haydu's search for the dynamics of bourgeois class formation, and not just for stories about it, gave meaning to the directories' categorizations.

Deliberate thinking in terms of social dynamics, such as path-dependency, class formation, or social capital, as well as of structures, such as organizations and hierarchies, can guide and inspire historians. I can imagine social scientists asking at this point, how can one *not* think in these terms? The key here is the qualifier *deliberate*. Even in the most naïve storytelling, assumptions about how change or continuity happen can hold sway unnoticed by their authors. Asking my students to read social scientists—who wear their methods, hypotheses, and theories on their sleeves—helps them to generate and shape questions and directions for their own thoughts and research. In any case, I have been a fan for decades of what the social sciences offer historians. Thus, although *Pull: Networking and Success Since Benjamin Franklin* bore some historians' rebuke for "too much" social science, I eagerly noted the social science references in *Citizen Employers* for future projects.[4]

Given all of this, what more is there for me to say about *Citizen Employers*? The rest of this essay will address a few areas that highlight how the same book that shows the advantages of doing history through a sociologist's lens also points to why it can matter that sociologists and historians work differently. My first topic will look at historians as spoilers, and the second at what is missing from *Citizen Employers* from this historian's perspective. The third looks at how the goals and methods of producing history often differ for historians and social scientists, with some resulting frustrations.

Historians can get asked to parties to tell stories. We carry lots of them around with us and are always glad to share. But sometimes historians can dampen others' storytelling if they show off their hard-won factoids. No matter what story someone else tells, the historian in the crowd will know of something that happened earlier or elsewhere that can take the steam out of amateurs' stories. Even worse, historians often know when an entertaining story contains misinformation. Most of us avoid turning social gatherings into trivial pursuit contests, but sometimes avoiding temptation is hard. Of course, rarely in a social setting do those earlier or elsewhere events matter. In scholarship, on the other hand, they often do.

For instance, Haydu certainly knows about the violence of the nation's railroad strikes of 1877, the 1886 Haymarket Riot, the 1892 Homestead Strike, and the 1894 Pullman Strike. Some of these catastrophic events make cameo appearances in *Citizen Employers*, yet they hold no explanatory standing. Haydu makes a strong case for the power of local circumstances over the events and processes he investigates. Yet, national experiences, prefaced by the 1871 Paris Commune that struck terror into the hearts of "respectable" Americans, figured greatly in how businessmen and their peers worried about security and labor relations within their firms, industries, and cities, even their neighborhoods. The frightful events could not have been far from the mind of any businessman east of the Mississippi, although perhaps San Francisco's distance provided some insulation. Fears of class war rose to the surface with the unrest that began with the Panic of 1873 and continued during years of devastating violence that peaked in 1877 but did not end then. This national background must have heightened the local fears from Cincinnati's 1884 Court House riot that Haydu credits as a pivotal point there. In San Francisco, on the other hand, the rioters of 1877 attacked Chinese residents, not railroad properties, reinforcing Haydu's conclusions about racism as a dominating force there. (On the violence of 1877, see Bruce 1959; Painter 1987).

Looking even farther back, I could not help but wonder about the possible relevance of earlier patterns of American class formation. For example, two classic studies of antebellum polarization between employers and employees could be especially pertinent. Paul Johnson (1978) demonstrated how employers in the early nineteenth century abandoned centuries of tradition and no longer shared their homes, meals, and liquor with workers as they began to form what we now recognize as a middle class. Affluent families left grimy business districts for tree-lined neighborhoods where temperance and religious revivals could domesticate mainstream culture. Similarly, Mary Ryan (1981) showed the effects of religious fervor and rising affluence on families' worldly activities and ambitions before the Civil War. Among those who could, families sought to differentiate themselves according to respectability. Cultural transposition, which Haydu defines as "the application of a common cultural script across social settings" (17), did powerful work in antebellum America, setting new patterns across public and private spheres.

As rich in local history and interactions as are Haydu's analyses of Cincinnati and San Francisco, because of the lack of contextualization, the cities, their people, and the events seem to float, unanchored. This is not a criticism of doing local studies, and Haydu is quite right that "the local community remained the primary stage for class

[4] Not all historians resist social science. *Pull* received the 2006 Hagley Prize for the best book in business history.

formation among proprietary capitalists" in that era (23). Even nationally-prominent figures who had achieved wealth in the hinterlands tended to gather in the major metropolises to build their new class identities. Nonetheless, grounding Cincinnati and San Francisco within the nation's cultural and political trends and contexts could have deepened *Citizen Employers*' achievements even more.

My second topic—what I see as missing here—points to four items: narrative, women and families, national influences other than labor strife, and individual agency. Suggesting a stronger narrative does not contradict my earlier comments about history students (and antiquarians) who just want to tell stories. There are real benefits to helping readers follow the flow of events. For instance, adding dates for both quotations and events throughout would help connect lines of causality. As it is, events and quotations move in an out according to logic, not timing. Such insensitivity to sequence can be misleading or confusing. For instance, on a single page, quotations about unions as a target against which Cincinnati businessmen coalesced follow in this order: 1882, 1886, 1914, 1878, and 1894. Two of them are not identified in the text even by decade, which is especially misleading for the lengthy 1914 quotation (173). Dating would help, but neglecting the likelihood that conditions and attitudes changed between the occasions for these statements puts our sense of historical contingency and causation at risk. *Citizen Employers* is rich in historical development, including Haydu's argument for "historical reversal" in San Francisco between 1904 and 1919. Still, hanging evidence and arguments on time's passage could have avoided implying that events and time's passage do not affect people's attitudes.

Leaving out women and families from an analysis of transposition across work and politics fits Patricia Nelson Limerick's description of a "constricting coherence and clarity" that comes "at the price of an accurate reckoning with the complexity of history" (1997). In reality, much of the work of class formation is family-based, and therefore women's work. Men's families and neighborhoods intersected with their economic and political lives. Women's domestic and social lives included charities, church, temperance organizations, plus events that women created—and men attended—precisely for establishing class hierarchies. Addressing these intersections and interactions for Haydu's subjects would take the work of at least one other book, and I do not introduce these points as expectations. Nonetheless, a caveat about the relevance of domesticity, perhaps with some references to Thorstein Veblen (1899), Stuart Blumin (1989), Johnson, or Ryan, would have avoided giving the impression that class is a masculine domain. Many of bourgeois men's goals in those transition decades entailed raising or maintaining their families' property, propriety, and status, and their incomes set the limits of their families' social ambitions. Family ambitions were very much part of what drove many of the economic and political ambitions held by Haydu's *Citizen Employers*.

Other complications that could have influenced the processes that Haydu studied include additional national factors and processes. For instance, what were the effects of the 1895 founding of the National Association of Manufacturers in Cincinnati? Ohio governor and future president William McKinley addressed the convention's 583 manufacturing executives and trade association representatives. The NAM quickly became the leading organizer of the proprietary capitalists who are Haydu's subjects, and one of its goals was to draw the nation's businessmen together to define and pursue their common goals. It began publishing *American Industries* in 1902, just two years before San Francisco's "historical reversal." Could the outreach of this increasingly influential organization have influenced the changes in San Francisco? Other connections that may be pertinent include that William Taft, U.S. president from 1909 to 1913, was born in Cincinnati and was one of the important supporters of the U.S. Chamber of Commerce, founded in 1912. The Chamber's goals very much paralleled the NAM's. Perhaps these national developments helped to connect San Francisco with the majority of industrial cities that more resembled Cincinnati. Perhaps the dramatic reversal that Haydu convincingly depicts for San Francisco was not an entirely endogenous phenomenon.

Balancing structure and individual agency is not always easy. One of the many ways in which *Citizen Employers* provides a model for historians is its careful demonstration of the importance of organizations to historical processes. Individuals, even leaders, more often than not act on the polity, on history, through organizations and institutions, formal or informal. I continuously remind students that the most interesting historical questions ask less about why individual leaders do what they do and more about why other people follow them. Nonetheless, although Haydu brings us many voices, there are few actors, and I often wondered who was doing what. Apropos of this, in a concluding paragraph Haydu mentions Frederick Koster, whom I had noted earlier and wondered about. Did Koster have any links to national organizations? What was his connection with William Crocker, son of Charles Crocker of the Central Pacific? William co-founded the San Francisco Opera and chaired the Panama-Pacific Exposition. Advocates for Thomas Mooney, who was convicted for a key 1916 bombing but who appears in the index only with a reference to "San Francisco Preparedness day bombing," (265) attacked both Crocker and Koster as "pirates." Certainly Mooney saw a connection between the two businessmen, but all three men remain shadowy. Haydu briefly mentions the role of individual agency at the end (214; 237, n. 12), but it seems an afterthought. Unless we know something about actors, events seem to be autonomous; organizations, institutions, relationships, and mechanisms seem determinant. Do individuals participate in history as agents as well as representatives of various cohorts? Historians tend toward giving individuals too much agency. Social scientists and historians can guide each other toward more robust balances.

Many of the differences between historians and social scientists, and the resulting frustrations, follow from their differing goals for "doing" history that, in turn, lead to differences in methods. Historians, even analytical ones, want to produce a story, grounded in time and demonstrating change or continuity over time, with actors making—even agonizing—over decisions. Analysis does not usually trump telling the tale. Amongst social scientists, on the other hand, we find tales told in service of theoretical goals. The outcomes are powerful and compelling, in part because pursuing theoretical goals can narrow perspectives. In the case of *Citizen Employers*, the focus on attitudes and actions toward labor as the defining "relational" dynamic for class formation left little room for some of the factors I have introduced here.

All that said, I reiterate that *Citizen Employers* is a splendid work of history to which I will continue turning. It absolutely does *not* succumb to the most frequent failing for which historians critique social science work, namely the blithe use of a few secondary sources in service of substantiating some hypothesis. Quite the contrary. Haydu has earned his stripes many times over as an honorary historian. We historians should do so well at sociological analysis.

References:

Blumin, Stuart M. 1989. *The Emergence of th Middle Class: Social Experience in the American City, 1760-1900*. New York: Cambridge University Press.

Bruce, Robert V. 1959. *1877: Year of Violence*. New York: Bobbs-Merrill Company.

Johnson, Paul E. 1978. *A Shopkeeper's Millennium: Society and Revivals in Rochester, New York, 1815-1837*. New York: Hill & Wang.

Laird, Pamela Walker. 2006. *Pull: Networking and Success Since Benjamin Franklin*. Cambridge, MA: Harvard University Press.

Limerick, Patricia Nelson. 1997. "Has 'Minority' History Transformed the Historical Discourse?" *Perspectives* 35, no. 8: 1, 32-36.

Painter, Nell Irvin. 1987. *Standing at Armageddon: The United States, 1877-1919*. New York: W. W. Norton & Company.

Ryan, Mary P. 1981. *Cradle of the Middle Class: The Family in Oneida County, New York, 1790-1865*. Cambridge: Cambridge University Press.

Veblen, Thorstein. 1899. *The Theory of the Leisure Class: An Economic Study of Institutions*. New York: Macmillan Company.

Comments by Ajay K. Mehrotra

Indiana University, Bloomington

Scholars have long been preoccupied with chronicling the rise of class consciousness. But, for the most post, the traditional focus has been on the working-class. From Karl Marx to E.P Thompson, scholars have explored the structural and cultural factors that have shaped the political consciousness of industrial workers. Jeffrey Haydu's outstanding new book, *Citizen Employers: Business Communities and Labor in Cincinnati and San Francisco, 1870-1916*, joins a growing recent literature that moves the analytical lens away from workers and toward business owners. Like Martin Sklar and Sven Beckert before him, Haydu has given us a detailed and compelling social history of economic elites—a social history that is firmly grounded in the everyday experiences and ideologies of late nineteenth and early twentieth century leading capitalists in two major U.S. cities.

Haydu's central concern is to investigate why "employers in specific times and places adopt such different tools for denigrating—or, much more rarely, legitimizing—unions" (p. ix). More specifically, he seeks to explain the variation in industrial-relations that existed between two comparable modern American cities: Cincinnati and San Francisco. Evoking the Tocquevillian message about the importance of American voluntary associations, Haydu focuses on how diverse civic associations in these two communities shaped business ideology towards organized labor. Cincinnati's capitalists, agitated by political corruption and high taxes, gathered in civic organizations, Haydu argues, that solidified class solidarities around a particular notion of "business citizenship." This civic-minded class cohesiveness fostered a virulent anti-unionism. By contrast, San Francisco's economic elites were much more divided. Small and mid-sized proprietors eschewed unity with other capitalists and instead fostered cross-class alliances with white workers in the process of disparaging large factories and Chinese labor. As a result, San Francisco's business community, at least initially, developed a far more accommodating "practical corporatism," whereby certain business leaders were more willing to work with unions.

In narrating the historical variation in American industrial relations, Haydu has written a truly interdisciplinary monograph. It is interdisciplinary in its content, in its engagement with scholarly literatures, and perhaps most importantly in its methods of analysis. Although many histories of American industrial relations frequently privilege either material forces or cultural factors in explaining the rise of class consciousness, Haydu adroitly attends to both. He acknowledges, for example, that the late nineteenth-century economic cycle of boom and bust had a determinative influence on the civic ideology of Cincinnati businessmen. Whereas in the antebellum period the Queen City could boast of being the region's leading transportation hub and manufacturing center, the relative economic decline of Cincinnati compared to Chicago and Pittsburgh was evident by the 1890s. This comparative economic downturn had a profound impact on the city's labor militancy and the reaction of business leaders. The de-skilling of labor had an equally significant affect on bourgeois ideology. As Haydu explains, material changes on the shop floor "could also reshape capitalist alliances and identities, both by cutting vertical ties across class lines and by forcing employers to confront new problems of labor management—and, perhaps, to develop new collective solutions for those problems" (p. 47).

Yet, at the same time, economic forces by themselves do not tell the entire story for Haydu. Thus, he also explores how cultural institutions such as art museums and music festivals were pivotal both in helping business leaders with the boundary work of separating themselves from the working classes and in bringing like-minded economic elites closer together. Similarly, civic associations like the Cincinnati Commercial Club and the Business Men's Club not only reconfigured class alignments, they also helped facilitate a collective identity among business citizens, and framed how capitalists perceived the turn of the century "labor problem."

The interdisciplinary strengths of *Citizen Employers* can also be seen in the literatures with which the book is engaged. Haydu explicitly refers to the vast and growing sociological literature on social movement theory, class formation, and neo-institutionalism as he develops his argument about the variation in labor-capitalist relations. Highlighting the importance of social networks and group ties, Haydu shows how in San Francisco race played a major role in shaping conflicts arising from de-skilling and material transformations in the workplace. The prevalence of Chinese labor led white craftsmen and small proprietors to forge common ground and maintain cross-class ties that were not evident in Cincinnati, which had much starker class divisions. San Francisco's "practical corporatism" shows once again how race frequently trumps class in the history of American labor relations. Moreover, the focus on organizational contexts clearly shows the book's strong neo-institutionalist strand. Haydu takes seriously how all kinds of institutions, but especially voluntary civic associations, were critical to building common interests among public-minded businessmen, and how these interests in Cincinnati were transposed into a workplace ideology that resisted the demands of organized labor.

In the process of engaging with these varied types of scholarship, Haydu also employs an interdisciplinary set of analytical methods. The book, in other words, is not just an outstanding work of historical sociology; it is also an excellent example of comparative social history. The comparison, of course, is subnational, rather than transnational, but the former is a method of investigation that has a rich tradition in American social history. Unlike some types of historical sociology, which merely use old data to test current sociological theories, Haydu respects the foreignness of the past, as he attempts to transport his readers into a different historical context. In this sense, professional historians will be elated to discover that Haydu has carefully mined not only the secondary literature in American labor and business history, but also primary sources, including archival materials, surrounding his historical subjects. Although some critics may argue that Haydu could have done more with archival sources to uncover what rank-and-file employers in these voluntary associations believed and did, his interpretations of the ideas of business leaders is well supported by his careful work with primary sources.

It is not only in his blending of sources that Haydu pays careful attention to the past. He also attends to the messy mix of continuity and change that accompanies any reconstruction of past events and processes. Whereas some historical monographs focus on the past either as a snapshot, as a particular moment in time, or as a period of transformative change, an era of great rupture; *Citizen Employers* takes account of both the forces of continuity and change. His case study of Cincinnati demonstrates the stubborn persistence of a "business citizenship" ideology that took shape over time, but remained resolutely anti-union. At the same time, the "practical corporatism" of San Francisco's economic elite appears as a more contingent and historically-specific ideology, one that according to Haydu's narrative reverses course over time and comes to resemble in the early twentieth-century Cincinnati's anti-union "business citizenship." In this way, the San Francisco example is really two case studies in one; a pre-World War I San Francisco when a unique and more accommodating "practical corporatism" held sway as business leaders recognized unions through collective bargaining and consulted with them in municipal politics, and a post-WWI San Francisco when a stark reversal seemed to take place, as businessmen organized to characterize labor interests as antithetical to civic values, and as labor unions demobilized politically.

Citizen Employers makes a persuasive case for dispelling the conventional wisdom that turn-of-the-century American capitalists were everywhere and always anti-union. Still, there are portions of Haydu's account that could be clarified to support his arguments. One is in his use of political theory. The political ideology of republicanism, for instance, is an important theme throughout the book, but particularly in Part II (Identities) when Haydu examines the content of business discourse in his two case studies. Haydu convincingly demonstrates that republicanism was a multivalent ideology that permitted business leaders in Cincinnati and San Francisco to adapt portions of classical republican thinking to their particular social contexts. But at times it seems as if republicanism is doing too much analytical work; its malleability seems overstated. Why were businessmen in Cincinnati and San Francisco able to adapt elements of republicanism selectively? How were they able to extol the importance of civic virtue while at the same time diminishing the ideal of the mechanic as a respected member of the republic? These are critical questions that are identified in Haydu's study, but not fully explicated. Likewise, in contrasting republicanism with the resurgent classical liberalism of the Gilded Age and the corporate liberalism of the Progressive Era, Haydu appears to leave out a third form of American liberalism: the new liberalism of Walter Lippmann, Herbert Croly, and John Dewey. Portions of this strand of liberalism can be seen in San Francisco's "practical corporatism," but the Metaphysical Club was not the same as the National Civic Federation or San Francisco's Merchant's Association.

Another historical factor that also seems somewhat neglected in *Citizens Employers* is the legal structure. Haydu's exhaustive research acknowledges the role that the courts played in blunting labor militancy in Cincinnati, and how San Francisco's "Law and Order Committee" instrumentally turned to injunctions to combat the 1916 waterfront strike. Yet, Haydu depicts law and legal institutions as minor variables. He presumes that the legal framework was relatively constant across his case studies. That may be true if one focuses on how the courts were used to prevent and break strikes. But a more capacious view of law, legal institutions, and legal processes would suggest that voluntary civic associations and the varied character and strength of unions themselves may be a product of differences in legal structures.

Despite these minor omissions, *Citizen Employers* is an outstanding work of historical social science mainly because it takes temporality seriously; the formation of bourgeois class consciousness, for Haydu, is first and foremost an artifact of history, a result of complex developments over time. Social scientists frequently refer to such historical explanations by employing the theory of path dependency, which contends that during "critical junctures" or contingent moments particular decisions tend to have long-term consequences because subsequent events reinforce or "lock-in" the earlier contingent choices. Though Haydu evokes the path dependent metaphor, he does so with a healthy skepticism about the potential historical determinism implicit in any focus on particular "critical junctures." For Haydu, the historical development of capitalist class-conscious is defined less by pivotal moments of plasticity than it is by the incremental reinforcement of class ties, identities, and frames of analysis. As he explains, "initial forays into movement activities and organizations can lock in early choices by forging bonds and constructing identities that participants become reluctant to give up (p. 209)."

This is not to say that Haydu has ignored critical events. To the contrary, his focus on Cincinnati's March 1884 Court House riot, a widespread and violent reaction to the lenient sentencing of a murderer, shows how his study brilliantly braids single, salient events with longer-term historical processes. Rather than describing the Court House riot and its reaction as an overly-deterministic critical juncture, Haydu places the melee into a broader context, showing how it had deeper historical roots in the social dislocations of modern urban industrialism, as well as far reaching consequences in shaping the civic-minded identities of Cincinnati's business elites. Thus, the riot, in Haydu's hands, becomes part of a broader historical context whereby long-term structural forces help explain how possible options in dealing with industrial-relations are winnowed down and particular social alignments and identities are reinforced overtime.

Citizen Employers is certain to garner great scholarly attention and stimulate further investigations into the comparative-historical diversity of American industrial relations. Although the book's prose is at times dense, it should be a useful text for graduate classes in a number of fields, including sociology, history, and labor studies. Haydu has provided a concise and careful historical study of comparative class-formation that pulls back the analytical lens to show just how important life outside of the economic realm was to both employers and workers.

Comments by William Roy

University of California, Los Angeles

First, I would like to congratulate Jeff for writing such a terrific book. The theoretical sophistication, methodological care, historical sensitivity, and rhetorical grace are truly outstanding.

The role of businessmen in civil society has a long and honorable tradition, having drawn the attention of Marx, Weber, Durkheim, and Tocqueville, just to name a few. In mid-twentieth century American sociology, the issue was approached from the perspective of businessmen, asking whether the capitalist class was unified enough to hegemonically rule over central institutions, including the state. Scholars vigorously debated whether the American capitalist class could be considered a "class for itself" or merely a "class in itself," whether America and its cities were ruled by a power elite or a plurality of countervailing forces, and whether the fundamental structure of capitalism made direct rule by capitalists unnecessary or not to ensure the system's reproduction. In the late century, the perspective shifted to civil society as a whole, asking the circumstances under which civil society coalesced to the point that it could check concentrated power and provide a framework for a democratic society. The conspicuous ascendency of conservative capital in this country, the demise of communism abroad, and the flowering of cultural analysis in sociology shifted attention to the question of how civil society can develop and thrive.

Jeff Haydu's book is bringing these agendas together by asking what determines variation in the role that businessmen play in civic life. He does it by preserving the Marxist and Weberian orientation of mid-century interest in capitalists *as* capitalists and the more recent sensitivity to cultural nuance and contingency. Thus the title *Citizen Employers*, which links into one phrase the civic and economic sphere. But this phrase—citizen employer—is treated as a variable not a constant. The extent to which the term has historically made sense to people or might have been considered an oxymoron differed in the two settings he investigated. Thus, unlike many of the neologisms that populate academic discourse, this phrase suggests an agenda more than an explanation. The extent to which businessmen considered their roles as employers and their roles as citizens as part of the same cloth becomes an object of study. What are the structural features and cultural framings that make the role of citizen employer compelling or meaningless? It's an important and challenging question.

One of the most important issues addressed in this book is the relationship between the culture and identities that businessmen have toward the workplace and toward the public arena. Rejecting economic or political determinism, the book probes how ideals and the sense of self in work and in the public arena penetrate each other, though not in any fixed way. Even the strength of the boundary between work and the public varies and is something itself to be explained. Why businessmen might think of work and the public as two unrelated spheres or conversely might think of them as a single arena is an empirical question, one that he sets out to answer.

This is a superbly crafted book with sophisticated theory and strong method, but I want to raise three analytical issues. These are not so much criticisms as issues that the book raises and that I think a discussion can help illuminate more generally. The first issue concerns units of analysis and comparison. One of the book's strongest points is its exemplary use of the Millian comparative method to find generalizable explanations for what happened in Cincinnati and San Francisco. To what extent would those generalizations apply to larger units of analysis? Second, and this is the closest I have to a real criticism: How can our invocation of the concept of path dependency help explain both continuity and change? When we see continuity, it is easy to invoke the concept of path dependency. When we see change should we also ask, why path dependence doesn't apply? The third is a classic question of class analysis: to what extent and in what ways should the analysis of class formation for the capitalist class and working class parallel each other or differ from each other? Or to put it another way, what is specifically capitalist about class formation in capitalist societies?

How can our invocation of the concept of path dependency help explain both continuity and change?

Units of Analysis: From the local to the international

Haydu returns to a familiar terrain for American sociology, the city, the terrain that captured the imagination of such luminaries as Robert Park, Ernest Burgess, Robert and Helen Lynd, William H. White, and Floyd Hunter. Although I have not seen any figures to verify it, I would guess that before 1960, major social science journals included more articles with cities than nations as the unit of analysis.

For the time and place Haydu is studying, the U.S. in the late 19th and early 20th century, the individual city was the appropriate unit of analysis to study class formation of capitalists. Though the national consolidation of business and the corporate revolution was both a structural factor and a result of class formation at the national level, national class formation was still nascent at best (Roy 1984). Businessmen organized at the local level much more actively than at the national level. Except for issues that were explicitly national, such as tariffs, the relevant civic sphere for most businessmen, both structurally and culturally, was the city.

In addition to the historical suitability of using the city as the unit of analysis, there is a methodological advantage, especially for Haydu's well-executed use of Millian comparison. The strategy of Millian comparison that Haydu uses is the method of difference. In the method of difference, the analyst selects cases that are similar in most respects, but differ in terms of the dependent variable (Przeworski 1970; Ragin and Zaret 1983; Skocpol and Somers 1980). By analytically holding constant many of the factors that might be invoked to explain what is different between the cases, we can isolate the factors that vary and might explain the difference. By comparing cities within a nation, one holds constant all qualities that are uniform to that country such as the basic culture, level of development, national political system, etc. Haydu goes further, selecting cases that as cities share such factors as a similar manufacturing base. Haydu uses the comparative method skillfully. On one hand, he constantly compares back and forth, invoking the comparative logic to fortify his major thesis. This is used most effectively with his argument that the different racial structures and different responses to race in Cincinnati and San Francisco go a long way to explaining differences in class formation. One of the most interesting aspects of Haydu's analysis is the role that race plays. In San Francisco, the Chinese presence threatened both workers and businessmen, who formed a united front, thus alleviating the intensity of conflict between classes. The presence of Chinese workers and entrepreneurs in San Francisco split capitalists between modest sized firms and monopolies. Workers were also divided in their relationship to the Chinese. The result was a more or less united middle class against the Chinese from below and the monopolies from above. Racial conflict thus trumped class conflict. Counter-intuitively, class conflict became more visible, as capitalists affirmed the legitimacy of workers to participate in governance. While no less racist, Cincinnati businessmen could join together in a united front against workers. Their more complete unity made it possible for them to speak on behalf of the whole, rejecting working class politics as divisive. The analysis is as nice an example of the intersection of race and class as you'll see.

But the issue of race raises the question of generalizability to larger units of analysis, especially nation-states. The role of race clearly distinguishes the dynamics of class formation in Cincinnati from San Francisco. But it has been invoked to have the opposite effect in explaining the difference between the United States in Europe. Haydu emphasizes that class formation in San Francisco was an exception from the rest of the U.S. Class formation in San Francisco, he argues, was more like a European pattern. San Francisco's greater openness to unions, its willingness to recognize them and negotiate with them departed from the more general stance of total intransigence by most American capitalists. But one of the main factors makes San Francisco different from other American cities is a characteristic that makes the U.S. as a whole distinctly different from Europe—race. There is a certain unexplained irony in the fact that San Francisco's exceptionalism from the American pattern of class formation is based on a factor often invoked to explain America's exceptionalism from Europe. In some comparative analyses, race is invoked as a reason why the American capitalist was more effective in preventing working class power (Bonacich 1976). While it would be unreasonable to demand that Haydu explain why European capitalists were more accommodating than their American counterparts, his explanation for San Francisco's exceptionalism should be consistent with what happened in Europe. I can see several ways to address such an inconsistency: 1. The parallel between San Francisco and Europe was offered as an interesting coincidence more than an analytical generalization, an aside more than a part of the central argument. 2. We should not expect similar class configurations to always have the same cause. There are so many differences between American cities and European societies that any similarities will be due to many factors. 3. There are some interesting parallels to be found between the role of race in San Francisco and features of European society that can be gleaned from further analysis. The role of racial organization is much more complex than a simple variable that should have universal consequence. If this is true then Haydu's findings create an invitation for a richer analysis.

Path Dependency, Continuity and Change

One of the unresolved tensions is the invocation of path dependency. Our understanding of path-dependency in historical social sciences has become more sophisticated through the pioneering work of James Mahoney (2000; 2006) and Paul Pierson (2003; 2004) the concept of path dependency has evolved from an effective metaphor of continuity to a more fully understood mechanism. That is, we better understand the circumstances under which social relations are reproduced and those in which they change. Pierson's work is most explicit here, requiring that path dependent processes must involve identifiable positive feedback loops. Just to label patterns or relationships as path dependent does not constitute an explanation, but more of an agenda, requiring us to identify the mechanisms by which the patterns or relations are reproduced. When Haydu introduces his discussion of path dependence in relation to cultural frames, he wisely sets the criteria for when path dependence applies. "A path-dependent account of frame selection might begin by asking which inherited cultural tools from the past are effective in coping with current problems. Individuals, after all, do not so much *have* identities and frames as *use* them to situate themselves in their social worlds and to make sense of those worlds" (14). Haydu effectively shows the path dependent nature of republican thinking in Cincinnati and San Francisco, how businessmen in those two cities selectively drew upon the republican tradition to adapt to changed circumstances in very different ways.

The first section draws on social movement theory which treats all contenders as analytically equal. He shows that San Francisco businessmen were more accommodating to unions and more inclusive of the working class in politics. As mentioned above, the most important factor in this was the role of the Chinese in San Francisco. The second part focuses on the specific ideological content of class formation, drawing on theories of collective identity and framing to show how San Francisco businessmen thought of themselves more as pure businessmen in contrast to the Cincinnati business community's broader vision of the citizen employer. Thus the San Francisco businessmen were more comfortable with the notion of overtly class-based politics, whereas those in Cincinnati took on the role of guardians of the entire community. He labels the San Francisco frame "practical corporatism" focusing on the pragmatic attitude of accommodation with the unusually strong unions in that city. The Cincinnati frame he calls "business citizenship" a form of class consciousness that includes a strong sense of implicit group identity and a rejection of overt class politics. Thus Cincinnati was able to develop a far richer civil society with businessmen leading the arts, charities, and eventually progressive government. But the difference in the two cities belies an important continuity. Both drew on the same ideological foundations of American republicanism. Haydu emphasizes the continuities just as much as the divergences. The San Francisco businessmen embraced the conception of the honorable producer against the non-productive, including what they considered the slothful Chinese and the venal monopolist. The Cincinnati businessmen abandoned the emphasis on productive worth, while sustaining the virtue of public responsibility. In contrast to many historians who have argued that republicanism had been abandoned by the late 19th century, Haydu convincingly shows that it was alive and well. Even if embraced selectively, American business thinking in that period cannot be understood without it. Thus the consciousness and frames in the two cities came from the same path of Republicanism. I find the explanation original and convincing. But there is a glitch. One of the actors steps off the path. San Francisco businessmen after 1911 abandon their accommodation toward unions, push for the open shop, fight labor's representation in local government, organize violent vigilantes to maintain law and order, and discover a new degree of unity. Haydu repeats that this was not just a belated adoption of the Cincinnati model, but a pattern different from either Cincinnati or the earlier San Francisco. He calls it a third case, that he hopes will strengthen his analysis rather than weaken it. But I think this is an analytical cop-out, especially given the earlier attention to path dependency. His justification for making it a third case is that he tries to show that the same factors that explain San Francisco's earlier accommodation and attachment to the ideology of practical corporatism changed. The industries that relied most heavily on the Chinese declined, the trade unions were no longer threatened by Chinese workers, the workers' political party was weakened by corruption, and the business community faced new competition from other west coast cities. But what happened to the path dependent processes that had sustained continuity earlier? If we take path dependency seriously, we must justify when it applies and when it does not. We cannot declare that one case has been magically metamorphosed into another case and point to independent variables as though they are exogenous. Historical entities do not operate according to what Sewell has called experimental time (1996). This is not to say that path dependence should be a mindless explanation for continuity or that any change rebuts the existence of path dependent processes. Instead, we need concepts and models that explain both change and continuity.

What is Capitalist about Capitalist Class Formation?

My third issue is more of a genuine question than a criticism. What difference does it make that the people being studied were capitalists? There is one strain of thought that treats class formation in terms of the generic issue of group formation of any sort. What are the factors that make it possible for any group to recognize its interests, create cohesion and trust, and attain the ability for collective action. The social movement theory that Haydu draws on would explain the capacity for collective action similarly for any group, capitalist, working class, or for some practitioners PETA and Mothers Against Drunk Driving. There is another strain of analysis that insists that the processes of class formation for the capitalist class are inherently different for capitalists than other groups. One variation on the theme of inherent difference would see a structural bias in the system on behalf of capital that would imply that capitalists don't really need to organize to have their interests served (Block 1977). Another variation posits that the logic of collective action is inherently different in that capitalist interests are so much more obvious than working class interests, making capitalist class organization much less problematic (Offe and Wiesenthal 1980). A third variation on the theme of inherent difference would emphasize how such capitalist powers as the ability to withhold investment, the dilemma of competition vs. cooperation, the effects of property law, etc., give the capitalist class specific challenges and advantages in contrast to others.

Haydu skirts these basic theoretical questions by wisely investigating what capitalists were actually doing. He finds that capitalists were problematizing class formation and worked very hard to achieve it (see also Roy and Parker-Gwin 1999). But I'm curious to know if he thinks there is a fundamental difference between class formation in the working class and class formation in the capitalist class, and if so what it might be. For example, could the role that Cincinnati businessmen took in assuming the mantle of benevolent guardians of the whole community ever be taken by workers or unions? Could the corporatism in San Francisco that Haydu calls *practical* corporatism when adopted by businessmen, ever be anything but overtly ideological if adopted by workers or unions? If capitalist class formation can benefit from pragmatism more than militancy, can working class formation? I'm not sure Haydu's analysis can answer these questions, but perhaps they offer clues.

The extent to which businessmen participate in public life and the shape that business participation takes is one of the most consequential factors in shaping how public problems are addressed. For those that care about public issues to casually assume that business interests are easily identified, that elite actions are easily anticipated, or that business politics are just too boring to study is folly. Haydu's book shows how broad the range of business politics can be and how easily periods of openness can be closed. There are lessons well worth learning.

References:

Block, Fred. 1977. "The Ruling Class Does Not Rule: Notes on the Marxist Theory of the State." *Socialist Revolution* 7:6-28.

Bonacich, Edna. 1976. "Advanced Capitalism and Black/White Relations in the United States: A Split Labor." *American Sociological Review* 41:34-51.

Mahoney, James. 2000. "Path Dependence in Historical Sociology." *Theory and Society* 29:507-48.

—. 2006. "Analyzing Path Dependence: Lessons from the Social Sciences." Pp. 129-39 in *Understanding Change: Models, Methodologies, and Metaphors*, edited by A. Wimmer and R. Kössler. New York: Palgrave Macmillan.
Offe, Claus and Helmut Wiesenthal. 1980. "Two Logics of Collective Action: Theoretical Notes on Social Class and Organizational Form." *Political Power and Social Theory* I:67-116.
Pierson, Paul. 2003. "Big, Slow-Moving, and… Invisible: Macro-Social Processes in the Study of Comparative Politics." Pp. 177–207 in *Comparative Historical Analysis in the Social Sciences*, edited by J. Mahoney and D. Reuschemeyer.
—. 2004. *Politics in Time: History, Institutions, and Social Analysis*. Princeton, N.J. : Princeton University Press.
Przeworski, Adam Teune Henry. 1970. *The Logic of Comparative Social Inquiry*. New York: Wiley.
Ragin, Charles C. and David Zaret. 1983. "Theory and Method in Comparative Research: Two Strategies." *Social Forces* 61:731-54.
Roy, William G. 1984. "Institutional Governance and Social Cohesion: The Internal Organization of the American Capitalist Class, 1886-1905 " Pp. 147-71 in *Research in Social Stratification and Social Mobility*, vol. 3, edited by R. V. Robinson and D. J. Treiman. Greenwich, CT: JAI Press.
Roy, William G. and Rachel R. Parker-Gwin. 1999. "How Many Logics of Collective Action?" *Theory and Society*.
Skocpol, Theda and Margaret Somers. 1980. "The Uses of Comparative History in Macrosociological Inquiry." *Comparative Studies in Society and History* 22:174-219.

Reply by Jeffrey Haydu

University of California, San Diego

It's an honor to have my book evaluated with the care shown by Larry Isaac, Pam Laird, Ajay Mehrotra, and Bill Roy. They represent my purposes and arguments well, and the issues they raise are worth extended discussion. In organizing my response, I have corralled their main comments into three broad categories. The first category covers how I construct my account of business class formation in Cincinnati and San Francisco, particularly the ways my approach balances narrative and analysis and makes qualified use of path dependency. The second category includes potentially important influences my book slights. The final category encompasses questions about the extent to which the patterns of business formation I identify in my two cities may be generalized to other cases and other types of group formation.

I. Constructing Historical Arguments

This exchange began at an author-meets-critics panel at the 2009 Social Science History Association, an organization through which scholars can, at least briefly, bridge disciplinary differences. One of these differences, Pam reminds us, is the way we present our arguments. Historians usually weave analysis into narrative and make liberal use of illustrative stories. Sociologists are more likely to foreground their analyses. Some do so by resorting to "experimental" time (Sewell 1996), highlighting relationships among variables more or less detached from historical context. Others do so by telling "analytical narratives" in which causal relationships are embedded in events and unfold over time. But those analytical narratives still differ from the richer stories told by many historians in the way that raw materials (such as the things that real people do and say) are evaluated and grouped for presentation to readers. Pam offers a good example. I argue that Cincinnati employers in the period from the 1880s to WWI constructed a common public identity for themselves as "business citizens," and that this civic ideology was applied to demonize unions in certain ways. I came to that conclusion based on the preponderance of the evidence. But what bits of this evidence should I offer the reader, and how should I arrange the pieces? For my analytical narrative, the examples should illustrate the argument and line up with the temporal period identified in that argument. (I distinguish the 1878 quote from the others as representing, in some respects, earlier discourse.) To tie each quote to particular moments of time *within* that period might serve other worthy narrative purposes, but it would muddy the argument.

Pam makes a related point, that my account turns on structural conditions at the expense of actors' agency. Here I plead guilty, but only after distinguishing "agency" as part of a causal explanation from "agency" as an appealing narrative device. To follow Pam's example, Frederick Koster was a key leader of San Francisco's Chamber of Commerce as it made a decisive turn, in 1916, against the city's unions. Koster is an interesting character. There are stories that could be told of his background in San Francisco's business community and social circles. Telling those stories would have entertained many readers (and, alas, irritated some others: "why do I need to know *this*?"). But for my purposes, the key question is like that asked about Lenin: what if Koster had missed the train? I think that counterfactual has a clear answer. Koster's background was typical of one faction of San Francisco's business community that had long favored the open shop (the one bit of background I supply for Koster). Others from this faction held leadership positions in the Chamber of Commerce. And that organization, greatly expanded and revamped over the previous five years, gave San Francisco businessmen a solidarity and collective clout that they had not enjoyed before. If Koster had missed the train, others like him would have used these new resources in much the same way he did, exploiting new opportunities (above all, the Preparedness Day bombing of 1916) to attack unions.

My account, then, is a deterministic one more typical of sociologists than of historians. But the very fact that San Francisco class formation and industrial relations change course in 1916 points to the limits of my determinism. Here, Bill suggests, I seem to be forsaking path dependency as an explanatory model. In comparing Cincinnati and San Francisco, I make use of this model to show how the cumulative effects of economic conditions, race relations, union organization, and -- yes -- critical events, "locked in" different class alignments and class ideologies. Bill rightly insists that we be clear about where path dependency applies. My response is that it best applies to comparative questions like this one: why do similar cases, facing similar challenges, diverge over time? But when we look, instead, at successive paths in a single case, path dependency can hamper historical explanation because of the way in which it combines serendipitous switch points, where prior history exerts no causal influence, with a deterministic lockin over time, with no obvious way off the path, short of exogenous shock.

I want to soften up that determinism to allow for reversals, and I think it is a mistake to argue that if there is a reversal then it could not have been a real path to begin with. If we find the sorts of cumulative lockin mechanisms that path dependency suggests we look for, then the metaphor has served us well. And I think that is what happened in the "second" case of San Francisco. There we see a weakening of factors that had locked in earlier patterns -- racial cleavages becoming disentangled from relations among capitalists and labor's political power weakening, for example. But we also see a new series of developments, like the revamping of business organization and political realignment, locking in a different path. More subversively for path dependency, we find actors retrospectively configuring new paths out of inherited historical resources (Haydu 2010).

Among those resources are ideological ones. San Francisco's "second case" underscores that malleability in the republic tradition which Ajay finds exaggerated in my comparison of Cincinnati and San Francisco. Juxtaposing these cities showcases the selective uses to which businessmen in different settings put the ideological toolkits they inherited. The shift in employer rhetoric in San Francisco between the 1900s and the WWI era provides further illustration of this selective use of the republican tradition. (And so, I would add, do studies of *workers'* adaptations of republicanism [Gerteis 2007].) Of course, there are constraints on the opportunistic use of ideology. Highlighting the evils of parasitical monopolies (one republican theme voiced in San Francisco) had to be both plausible and in the interests of a substantial fraction of San Francisco businessmen. Thanks to the Southern Pacific Railroad, both conditions were met.

II. Omissions

Readers often wish books had included topics of particular interest to them. Authors, for their part, may err in their decisions about what to leave out. For any particular omission, there are two important tests: were there legitimate rationale for excluding the topic? Would its inclusion have made a substantive difference to the conclusions reached?

Ajay notes that I pay little attention to the law as a possible influence, beyond mentions of employers' strategic use of legal weapons against unions. And he is right about my rationale. The legal weapons available to employers in the two cases were similar, whereas there was a striking difference in how these employers put the law to use. To explain that difference, presumably we must look elsewhere. Ajay suggests that "a more capacious view of law, legal institutions, and legal processes" might reveal systematic contrasts between the cases. He may be right. If and when that more capacious view becomes more fully specified, we would be in a position to apply the second test.

Pam makes the important point that business class formation (and working class formation, Hanagan (1980) and others add) may be fostered by relations among families and through the activities and networks of women. The same point could be made about residence patterns, suburban country clubs, and etiquette rules. All have been shown to serve as vehicles for social closure against the working class (e.g Blumin 1989). All make only token appearances in *Citizen Employers* (although I do note women's prominent roles in the cultural institutions that helped differentiate Cincinnati's business class from the lower classes). How do my tests apply to the omission of a substantive focus on women and families? In terms of rationale, among the many organizations and networks that might have contributed to class formation, I gave priority to those that (1) *also* shaped businessmen's public persona and ideologies and (2) *also* fostered mobilization (whether for collaboration or for warfare) vis-a-vis labor. Those criteria directed attention to the civic organizations that loom so large in the book. Would inclusion of women and families have made a difference to my conclusions -- the second test? I don't think so, for both empirical and theoretical reasons. In terms of the archival evidence, there is material about family ties and women's roles in cultural associations, but it tends to be compartmentalized -- particularly in the "society" pages of local newspapers -- and to have been little remarked upon by businessmen active in local civic affairs and labor relations. By contrast, the networks and organizations that demonstrably did influence employers' collective identities and views of work were overwhelmingly male. Falling back from archives to theory, this pattern is also what one would expect. To use my book's language of cultural transposition, the institutional boundaries between family life, on one side, and civic identities and workplace ideology, on the other, were still quite high in the late 19th century. Or to put it another way, men's roles as business citizens blurred the boundaries separating civic leadership, cultural patronage, and workplace management. Women's roles drew a sharper line between the family and the realms of politics and work.

Larry and Pam also ask if the book is too narrowly focused on the local level, to the exclusion of national influences. They point to major events, like the violent industrial conflicts of 1877 and 1886, which are mentioned but play no real causal role. Pam adds that some national organizations, like the National Association of Manufacturers (NAM), may have contributed to San Francisco's reversal. Larry notes that developing national market ties may also have had a differential impact on each city's bourgeoisie. Although I don't use the same terms as Larry does, such networks do play a role in my account. One theme in the Cincinnati case is the bypassing of the city by developing transportation networks that were centered in Chicago. And part of the story of San Francisco's reversal involves both the end of its relative isolation from national competition and the rise of competing west coast shipping centers. On the other hand, some of the national organizations that might have influenced San Francisco labor relations, notably the Citizen's Alliance (a union-fighting body that worked closely with the NAM) were decisively repudiated by most local businessmen when they tried to rally San Franciscans to the open shop in the early 1900s. They got a better reception later -- *after* local conditions had changed.

Overall, however, Larry and Pam are right about the relative neglect of national influences. My main warrant for this neglect is methodological. I don't see these important national strikes as having been more or less influential in one city as compared to the other, and so they can't explain differences in business class formation. Pam and Larry's comments on this point did, however, alert me to a different sort of missed opportunity. Even if these national strikes did not have a differential *causal* impact, I could have used them to further illustrate the distinct business cultures of the two cities. My impression is that San Francisco businessmen interpreted the 1877 railway strikes and the 1894 Pullman dispute, in particular, according to their familiar template of evil monopolies (especially the Southern Pacific) and dangerous *Chinese* workers. Their Cincinnati counterparts, consistent with business citizenship, interpreted the same events more as serious civil disturbances rather than as battles between capital and labor.

III. Generalizing about Class Formation

Finally, there are a number of important questions raised about the broader applicability of my arguments. The "exceptional" case of San Francisco's practical corporatism, I suggest, resembled emerging patterns of labor relations in some European countries. Bill wonders if these similar patterns also had similar roots, and Larry asks if this picture of San Francisco is compatible with that city's high levels of industrial violence. As for Cincinnati's business class formation, to what extent does it look like other cases of bourgeois class formation? And is it as similar to *working* class formation -- or indeed to the sorts of group formation one finds in other social movements -- as my more theoretical discussion implies?

Citizen Employers does present San Francisco as exceptional in its class alignments and its industrial relations. In contrast to the country's open shop norms, in the early 1900s, a majority of employers recognized that labor and capital alike had distinct interests and that the public good was served if *both* sides were compactly organized and responsibly led. I hope I did not lead readers to conclude, however, that industrial peace followed. As I explain in the book, there was always an open shop minority. Member of this group sometimes took on workers who -- this being San Francisco -- usually were well organized. Hard-fought and protracted battles ensued. Even the practical corporatists got into scraps with unions, scraps which sometimes turned ugly. That high levels of industrial violence existed in San Francisco is not surprising. The important departure from national norms lies in employers' typical *goal* in fighting unions, which was not to smash them but to improve the terms of settlement. The unusual character of the city's industrial relations is better captured by other indicators, such as the extent of collective bargaining or the trades covered by industry-wide agreements.

By these indicators, San Francisco looks more like European cases such as England than it does like most other U.S. cities. Bill points to a puzzle in this comparison. One factor in San Francisco's exceptionalism, I argue, was racial cleavages that aligned small business and skilled white labor against the Chinese. Yet a common view among comparativists is that unions were more successful in Europe than in the U.S. because racial cleavages were *more* pronounced in the latter. So even if the outcomes in San Francisco and (for example) England were similar, must not the explanations for this differ? The answer is twofold. First, racial cleavages worked differently in San Francisco than in other U.S. manufacturing cities; second, there was more to San Francisco exceptionalism than racial cleavages. It was the particular way in which the Chinese were incorporated into key San Francisco industries like shoemaking and clothing -- as both employees of technologically advanced firms and as owners of small sweatshops -- that made them a threat to skilled white labor and proprietors alike. The more usual pattern was Cincinnati's, where African Americans in this period much less frequently appear in manufacturing either as workers or as employers. At least in northern states, the challenges of technological change typically pitted labor against capital, not whites against blacks. The path to San Francisco's exceptionalism also included other tributaries. One was that by the early 1900s, the local labor movement was powerfully organized at work and a potent independent force in municipal politics. This was crucial for beating back the open shop tactics of some employers and educating others as to the wisdom of settling disputes through negotiation. And *this* cause of San Francisco's un-American industrial relations can indeed also be found in quasi-corporatist European regimes (Robertson 2000).

Cincinnati, by contrast, has a more typical pattern of industrial relations, and *Citizen Employers* suggests that the character of business class formation found there is echoed in case studies of some other cities. How far would I push Cincinnati as "typical"? One point I make early in the book is how far we have come from Marx in studying class formation. Cultural practices and collective action have at times aligned with economic hierarchies. Sometimes we see a related, two-fold movement in which ties between members of different economic classes diminish and ties among those sharing similar class positions extend. But most scholars have cast off orthodox assumptions about *how* this happens, *where*, institutionally, it happens, or *whether* it will happen at all. By the same token, if class formation does happen, it will not always happen in the same way. In earlier U.S. cases cited by Pam, evangelical religion was a particularly important arena for bourgeois class formation. By the late 19th century, new divisions had emerged among capitalists. In Cincinnati, those divisions were bridged not through churches but through secular civic associations. Similarly, in the fascinating case discussed by Larry, Cleveland capitalists faced somewhat different challenges. Unlike Cincinnati, which was in decline, Cleveland was still in its boom years. Its more recent development may have minimized the frictions that occurred elsewhere between an urban gentry and the nouveau riche. And Larry is persuasive that in Cleveland, the extension of solidarity among businessmen happened through private militia. What these examples show are some generic dilemmas of class formation and the importance of non-economic relations as the arena in which capitalists drew together and cut their ties to workers. Cincinnati's business clubs and Cleveland's militia also display common organizational features which made them particularly effective vehicles for class formation. Both had a multivalent character: Cincinnati's business associations served the functions of genteel relaxation, cultural philanthropy, and civic reform; Cleveland's militia fused the roles of neighborhood association, defender of the public peace, social club, and union-fighting force. But beyond such general parallels, Larry is surely right that there are "multiple and quite divergent paths to business class formation."

To what degree, finally, is my account of business class formation generalizable beyond the bourgeoisie, to other groups or movements? Or to turn this around, as Bill does, is there anything distinctive about capitalist class formation? The answer seems as unavoidable as it is unsatisfying: it depends on what level of abstraction you happen to like. The model I use of the path dependent construction of collective identities is a very general one, potentially applicable to social movements as well as class formation. By the same token, it does little more than suggest what to look for in any given case and how to put the causal story together. Somewhat closer to earth, there are more substantial similarities between working class and bourgeois class formation. Case studies tell common stories of new challenges and threats, new opportunities, and new associational activities that break vertical ties and expand horizontal ones. The details of these challenges, opportunities, and associations, however, vary both between workers and capitalists and among cases of each. There is, however, one key difference between bourgeois and working class formation, at least in the era I studied. By and large, for workers, status closure (e.g., male workers denying job opportunities to women or keeping blacks out of unions) undermines class formation. In Cincinnati and many other industrial cities of the time, a smaller and more homogeneous bourgeoisie meant that status closure and class formation went hand in hand. Even that distinction, however, is a historically contingent one. We still need to look at more cases of business class formation -- something that I believe all participants in this exchange would recommend.

A final word of thanks to my critics and, from all five of us, to Isaac Martin for all his work organizing the original SHAW panel and facilitating this Trajectories exchange.

References:

Blumin, Stuart M. 1989. *The Emergence of the Middle Class: Social Experience in the American City, 1760–1900*. Cambridge: Cambridge University Press.
Gerteis, Joseph. 2007. *Class and the Color Line: Interracial Class Coalition in the Knights of Labor and the Populist Movement*. Durham: Duke University Press.
Hanagan, Michael P. 1980. *The Logic of Solidarity: Artisans and Industrial Workers in Three French Towns, 1871–1914*. Urbana: University of Illinois Press.
Haydu, Jeffrey. 2010. "Reversals of Fortune: Path Dependency, Problem Solving, and Temporal Cases." *Theory and Society* 39(1), January: 25–48.
Robertson, David Brian. 2000. *Capital, Labor, and State: The Battle for American*

Labor Markets from the Civil War. Lanham, MD: Rowman and Littlefield.
Sewell, William H., Jr. 1996. "Three Temporalities: Toward an Eventful Sociology." Pp. 245–80 in *The Historic Turn in the Human Sciences*, edited by Terrence J. McDonald. Ann Arbor: University of Michigan Press.

Identities

Editors' Note: In this section, comparative-historical scholars reflect on why they entered the subfield.

King-To Yeung
Princeton University

For eight hours a day you sit in the archival room, diligently unwrapping fragile documents and scanning them as quickly as possible. You wear a pair of white gloves, like a surgeon ready to scrutinize a hidden problem under the patient's skin. You copy verbatim the words of the dead and listen to their arcane lingo, trying to uncover the unmapped entrance into a world in which some strange social players once lived. This is how and where I enter the field—through doing things in the archival room.

In this peculiar place—a highly controlled and sanitized habitat—you and your research exist in pure harmony. The outside world cannot distract you; your pen, paper, camera, backpack, and all other vestiges of private property are confiscated before your entry into the sacred zone. Wi-Fi is banned as well. Alone with your own body, you come into direct contact with precious historical records that just a few days ago were mysteriously kept in storage, only seeing the light of day upon your special request. These rituals of the archival room make possible an utterly intimate relationship between you and the recorded past. No wonder that I feel such a sense of ownership of those documents which I merely hazily scan or roughly reproduce by hand.

I wish to think these pure—even sterilized—moments in the archival room are central to my joy and identity as a sociologist who does historical research. But quickly, even without any Bourdieusian cynicism, I realize that notion of scholarly purity is bound to be self-delusional. One only needs to look beyond one's research cubicle to see that what's really interesting, and sometimes more meaningful, lies somewhere else—in the many happenstances infiltrating the sacredness of research, in the idiosyncratic way each archive is organized, in the gulf between what one does or doesn't know and the insights of an experienced archivist, and, not least, in the unplanned, exponential growth of new research questions constantly prompted by emerging discoveries in the archives. Here is one such unremarkable—even irrelevant and impure—experience that nonetheless leads me to understand my work a bit better.

For the past two years, I have managed to locate certain late nineteenth-century American prison records in the official archives of a couple of western states. My research was exploratory, broadly focusing on inmates' infractions and prisons' internal disciplines. I am clearly a novice when it comes to American history, since my other long-term research had dealt with nineteenth century Chinese imperial bureaucracy and its reactions to social revolts (I still need to learn more about Lewis and Clark...). At any rate, one tangential connection between the Chinese case and that of the Old West seems to be their shared institutional obsession with punishment. The American prison documents I have located consist of wardens' daily entries about inmate infractions and the respective punishments, all recorded in big, heavy ledgers that apparently were kept to determine whether particular prisoners might receive early release or pardon. My sheer curiosity about the punishment system of the Old West sent me first to the Wyoming State Archives.

Unlike university collections, the Wyoming State Archives serves other, nonacademic functions, including, most importantly, the issuing of high school transcripts for state residents who have attended any Wyoming school. At first glance, this transcript service is clearly irrelevant to my own research. In fact, the other functionaries of the archival room did not initially affect my "pure" research rituals: I ordered the old prison ledgers, got my gloves, spread the heavy log books over a research desk, paged through the documents, took great pains to decipher old handwritings, and so on.

But gradually, the constant phone calls to the archivists made by people demanding their transcripts compelled me to see the archival room (and my own research) in a different light. Increasingly I became aware that the archivists sitting not far away from my research desk have spent much of their office hours responding, in person or over the phone, to citizens who utilize archival documents in very personal and private manners. People search for their genealogical roots in Wyoming, looking for their great-grandparents' names in old directories; they ask to reproduce photos of houses that once stood on their current properties; they spread out old maps to find out what has been changed in the ecology surrounding their own houses, perhaps wanting to renovate or sell them; and, not least, they pay a few dollars to get their high school transcripts for college applications. The personal utility served by these archival materials constitutes much of the archive's normal organizational activities. These historical projects seem to be vastly different (dare we say) from our impersonal and purportedly intellectual investigations of overarching historical changes, transformative events, the logics of nineteenth century punishment, and so forth.

As I became more attuned to the streams of phone calls that (quietly) found their way into the archival room, I also see that the boundary of my research is far less exclusive. In fact, I have been a part of the multiple problems the archival organization must solve daily. In a shared space, my project is connected to many others, including the endless high school transcript requests and people's genealogical search for their ancestors. These personal projects of private citizens probably have already shaped my research.

How so? I imagine that the ways the prison records are organized, kept, lost, or destroyed have something to do with the various functionaries and orientations of the Wyoming office, most of which might have little to do with the particular subject in which I am interested. When records get lost, for example, it is probably not because staff members are careless, but because there are more important things to which the staff must attend. It would be wrong for me to demand the archival room to organize itself according to my research agenda. A patient and experienced archivist once told me plainly, "When the old penitentiary closed down, they moved some of the records to us but threw away most of others." Or, at another site, I was told that "Documents disappeared when they traveled between the archival storage and the museum where they were displayed." These are neither reasons nor justifications for why things got lost; these are facts that describe the living and breathing archival organization.

Of course, this realization does not excuse us from making sense of the raw materials on which our craft depends, namely, the archival documents brought out from storage for our scrutiny. We cannot stop asking how, in what procedures and under what organizational constraints, these materials can possibly end up before us. Insofar as the archival room is a collection of activities—including all the private projects fellow citizens pursue with diverse interests—we need to reckon how the plethora of archival actions could have shaped our own research and altered the very explanations we seek to generate (especially when what is missing from the archives have something to do with the archival organization itself). As Michael Burawoy has said, organizational facts such as these are not pollutants to research, but form the substratum on which we should begin our research.

So the next time I enter the archival room, I will let the impurity of doing research reshape my thinking. Hopefully, in midst of many "irrelevant" activities, I may discover a more colorful identity in doing historical sociology.

The author would like to acknowledge the copy editing assistance from Andrew Grossman.

Joya Misra
University of Massachusetts, Amherst

I became a comparativist in my early childhood. Growing up the daughter of immigrants, my whole life was oriented toward comparisons, particularly across societies. My Bengali (Indian) father started almost every sentence with "In our part…;" my Swiss mother, always contrasted his experience with hers; and both (good Leftists) always made it eminently clear that the American way was not only not the only way, but a spectacularly poor way of organizing society. My own comparative experiences of growing up first in Shaker Heights, Ohio, then Shreveport, Louisiana, spending many summers with my mother's very large family in Switzerland, and being deeply engaged in Indian-American communities during the school year (Bengali lessons, Bengali dance, Bollywood films, or better yet screenings of Satyajit Ray movies), made me a comparativist long before sociology was a meaningful word for me.

High school was a high point. I was exposed to excellent courses in World and American History, as well as a truly fascinating class in law, politics, and society—I have the spiral-bound notebook for that course to this day. I found my niche. Just to lose it again in college, where I wandered. I majored in religion (comparative), minored in sociology, took lots of political science courses, and spent the vast majority of my time running the alternative college radio station. College held no allure, as my mostly working class friends were not in college, and the punk scene spoke far more directly to me and my concerns. But we were Marxists, and the alternative music of the 1980s analyzed society in ways that fit our experiences and spoke to our engagement in the anti-Apartheid movement and activism around ending American military involvement in Central America.

My parents tricked me into graduate school, of course, first wheedling me into taking the GREs (which I did after a long night listening to bands), next encouraging me to apply to graduate programs (why not?), and then telling me that I'd never have another chance to go to school for free (wrong!). But I figured that sociology would allow me to study whatever I wanted to, and I somehow found myself in a program filled with Marxist political economists, who took me seriously, despite my wild child ways. I couldn't have asked for better mentors.

Within the first week or two of graduate school, Terry Boswell asked me (his RA) to put together quantitative data on shipping during the 17th century from some secondary sources. Having come from a family of scientists, I almost immediately fell into despair, concerned about the poor quality of the data. Most likely annoying him, I interlibrary loaned the original Baltic sound toll data, translated the columns from the Danish, and gave him what I thought were much better, albeit still flawed data. My next move was to study the historical record, to get to know the period qualitatively, so that I could feel more confidence in the quantitative measures. I read, and I read, and I read. And I became more and more certain that I was gaining some insight into the development of the capitalist work-economy and the role of shipping in that world-economy. And somewhere in that reading, and that going back and forth between quantitative measures and qualitative historical records, I became who I remain today, a comparative historical researcher who constantly moves from qualitative to quantitative data, always trying to develop better understandings of historical processes.

Of course, I couldn't stay in the 17th century forever, and working with Alex Hicks, I became deeply engaged in the 20th century. While he is an excellent, sophisticated statistician, Alex's remarkable grasp of European history was stunning and gave me something to aspire to. Welfare state scholars are often focused on studying eighteen or twenty cases, so we learn everything we can about them. A researcher who studied the same eighteen people for a lifetime would likely know a remarkable amount about their cases too. So, once again, I read, and I read, and I read, and I've quite honestly, yet to come out of that phase. Understanding the emergence of the welfare state, and its development over the 20th century drew me in, especially since I'd grown up with parents so critical of the paltry American welfare state. As a feminist I was especially gleeful to be able to use these methods to trace the important contributions of women's movements to the welfare state – using both the power of QCA and the legitimacy of quantitative historical methods to argue that feminist historians were right—women's movements mattered, and in important ways. Over the years, I've used these methods to understand how class, race/ethnicity, nationality, citizenship, and gender intersect in modern welfare states. I've become less obsessed with origins, but remain deeply committed to understanding historical processes and weaving back and forth, between quantitative and qualitative methods, in order to make stronger, more convincing, more nuanced arguments.

When I look back, and forward, I am most grateful for the ability comparative-historical methods have given me, to not only theorize structure, but also culture; to not only think about the world in terms of equations, but also discourses. In my career, I've used almost every type of research tool sociologists claim—archival work, secondary analyses, interviews, ethnography, surveys, content analysis—feeling sure that by doing so, I can get closer and closer to making sense of the elusive and dynamic world around me. The explicit mentoring of comparative historical researchers, like Terry and Alex, who encouraged me to choose my method based on my research question, rather than vice versa, is what brings me to where I am today.

Publications

Books:

Andreas, Joel. 2009. Rise of the Red Engineers: The Cultural Revolution and the Origins of China's New Class. Stanford University Press.

Atasoy, Yildiz, ed. 2009 Hegemonic Transitions, the State and Crisis in Neoliberal Capitalism, London & New York: Routledge

Atasoy, Yildiz. 2009. Islam's Marriage with Neoliberalism: State Transformation in Turkey, London & New York: Palgrave Macmillan

Centeno, Miguel and Joseph Cohen, *Global Capitalism*. Polity Press 2010.

Dierkes, Julian. 2010. Guilty Lessons? Postwar History Education in Japan and the Germanys. London: Routledge.

Gates, Leslie C. 2010. Electing Chavez: The Business of Anti-neoliberal Politics in Venezuela and it is published this year (2010) by the University of Pittsburgh Press.

Lachmann, Richard. 2010. States and Power. Cambridge, UK and Malden, MA: Polity, pp. 234.

Mahoney, James. 2010. Colonialism and Postcolonial Development: Spanish America in Comparative Perspective; Cambridge University Press.

Mahoney, James and Kathleen Thelen, eds. 2010. Explaining Institutional Change: Ambiguity, Agency, and Power; Cambridge University Press.

Maynes, Mary Jo, Jennifer L. Pierce, and Barbara Laslett. 2008. Telling Stories: The Use of Personal Narratives in the Social Sciences and History. Ithaca, NY: Cornell University Press.

Rueschemeyer, Deitrich. Usable Theory: Analytic Tools for Social and Political Research. Princeton University Press 2009

Savelsberg, Joachim J. 2010. Crime and Human Rights: Criminology of Genocide and Atrocities. London: Sage.

Tiryakian, Edward A. 2009. For Durkheim. Essays in Historical and Cultural Sociology. Surrey (UK) and Burlington, VT: Ashgate Publishing Ltd. 360pp.

Articles:

Bodnar, Judit and Virag Molnar, "Reconfiguring Private and Public: State, Capital and New Housing Developments in Berlin and Budapest" Urban Studies 47(2)April 2010

Boyd, John, William Fitzgerald, Matthew Mahutga and David A. Smith. "Computing Continuous Core/Periphery Structures For Social Relations Data with MINRES SVD." Social Networks 2010 32:125-137.

Ma, Xuilian, Matthew Mahutga, David A. Smith and Michael Timberlake. "Economic Globalization and the Structure of the World-City System: The Case of Airline Passenger Data" Urban Studies, 2010 forthcoming.

Smith, David A. and Matthew Mahutga. "Trading Up the Commodity Chain? The Impact of Extractive and Labor-Intensive Manufacturing Trade on World-System Inequalities." Pp. 63-82 in Jennifer Bair (ed), Frontiers of Commodity Chain Research. Palo Alto, CA: Stanford University Press 2009.

Centeno, Miguel and Elaine Enriquez, Legacies of empire? *Theory and Society*, Volume 39, Numbers 3-4 / May, 2010 .

Chase-Dunn, Christopher and Thomas D. Hall. 2009. “Changement social et intégration des réseaux d’échange dans la longue durée” [Global Social Change and Integration of Exchange Networks in the Long Run]. Pp. 159 – 188 in *Histoire Globale, Mondialisation et Capitalismes* [Global History, Globalization and Capitalisms], Edited By Philippe Beaujard, Laurent Berger & Philippe Norel. Paris: Éditions La Découverte.

De Leon, Cedric. “Vicarious Revolutionaries: Martial Discourse and the Origins of Mass Party Competition in the United States, 1789-1848.” Studies in American Political Development 24 (1): 121-141

Eidlin, Barry. “‘Upon This (Foundering) Rock’: Minneapolis Teamsters and the Transformation of U.S. Business Unionism, 1934-1941.” *Labor History*, Vol. 50, No. 3, August 2009, 249-267.

Elias, Sean. 2009. (with Joe Feagin and Jennifer Mueller) “Social Justice and Critical Public Sociology.” *Handbook of Public Sociology*. Vincent Jeffries, editor. Lanham, MD: Rowman and Littlefield

Elias, Sean. 2009. “Comment to Michael Omi and Howard Winant.” *Contemporary Sociology* 38(5)

Elias, Sean. 2009. “Racial Discrimination, Origins and Patterns.” *Oxford Encyclopedia of Human Rights*. Dave Forsythe, editor. New York: Oxford University Press

Elias, Sean. 2009. “W.E.B. Du Bois, Race, and Human Rights.” *Societies Without Borders*, Special Issue on *Race and Human Rights: Critical Histories, Inquiries, and Futures* 4(3)

Hall, Thomas D. 2009. “Puzzles in the Comparative Study of Frontiers: Problems, Some Solutions, and Methodological Implications.” *Journal of World-Systems Research* 15:1:25-47 [Special Issue: Methodological Issues in Macro Comparative Research, Edited by: Jeffrey Kentor & Timothy Patrick Moran.

Hall, Thomas D. and P. Nick Kardulias. 2010. “Migration and Globalization: Long-term Processes in World-Systems.” Pp. 22-37 *Mass Migration in the World System: Past, Present and Future*, Political Economy of the World-System Vol XXXII, edited by Eric Mielants and Terry-Ann Jones. Boulder, CO: Paradigm Press.

Hall, Thomas D., Christopher Chase-Dunn and Richard Niemeyer. 2009. “The Roles of Central Asian Middlemen and Marcher States in Afro-Eurasian World-System Synchrony.” Pp. 69-82 in *The Rise of Asia and the Transformation of the World-System, Political Economy of the World-System Annuals*. Vol XXX, edited by Ganesh K. Trinchur. Boulder, CO: Paradigm Press.

Kumar, Krishan, "Nation-states as empires, empires as nation-states: two principles,one practice?", *Theory and Society* 39 (2), 2010: 119-43.

Naeshagen, Ferdinand Linthoe. 2009.“Folkets rettferdighet: Strafferett I senmiddlelalder og tidlig nytid” Collegium Medievale vol. 22, 2009, pp. 128-186.

Reifer, Tom, "Capital's Cartographer: Giovanni Arrighi: 1937-2009," New Left Review 60, November-December 2009, pp. 119-130

Sewell, William H. Jr., "The Empire of Fashion and the Rise of Capitalism in Eighteenth-Century France," Past and Present 206 (February 2010): 81-120.

Steinmetz, George, “Colonial Melancholy and Fordist Nostalgia: The Ruinscapes of Namibia and Detroit.” Pp. 294-320 in Julia Hell and Andreas Schoenle, eds., *Ruins of Modernity*. Durham, NC: Duke University Press, 2009.

Steinmetz, George, “Neo-Bourdieusian Theory and the Question of Scientific Autonomy: German Sociologists and Empire, 1890s-1940s.” *Political Power and Social The*ory, vol. 20 (2009), pp. 71-131.

Steinmetz, George, “The Imperial Entanglements of Sociology in the United States, Britain, and France since the 19th Century." *Ab Imperio*, *i*ssue 4 (2009): pp. 1-56.

Steinmetz, George, “Thirty Years of Thesis Eleven: A Survey of the Record and Questions for the Future.” *Thesis Eleven*, number 100 (2010): 67-80.

Tiryakian, Edward A. 2009. "Global Altruism: Some Considerations," pp. 409-27 in Vincent Jeffries, ed. Handbook of Public Sociology. Rowman & Littlefield.

Tiryakian, Edward A. 2009. "Durkheim's Reflection on the Crisis But Which One?" Durkheimian Studies Etudes Durkheimiennes, 15: 26-38.

Tiryakian, Edward A. 2009. "Modernity and the Second Return of Mechanical Solidarity," pp. 1-22 in Mohamed Cherkaoui and Peter Hamilton, eds. Raymond Boudon: A Life in Sociology. Oxford: the Bardwell Press.

Walker, Katherine. 2008. "United, Regardless, and a Bit Regretful: Confederate History Month, the Slavery Apology, and the Failure of Commemoration." *American Nineteenth Century History* 9:3, pp. 315-338.

Walker, Katherine. 2009. "Buried in the Unremissive Ground: Reading Richmond's Subterranean Signs." *Social Semiotics* 19:4, pp. 427-438.

Dissertations:

Elias, Sean. 2009. "Black and White Sociology: Segregation of the Discipline." PhD dissertation, Department of Sociology, Texas A&M University, College Station, TX

In order to present a more complete and accurate picture of the historical development of sociology and key themes in sociological theory, this dissertation addresses several fundamental omissions in sociology's history and thought. I explore 1) omission of an understanding of the centrality of racial meanings, race relations, and racism (race) in sociology; 2) omission of explaining ways race frames sociological viewpoints of influential, mainstream white sociologists; and 3) omission of the knowledge of marginalized black sociologists who form a counter-tradition with a counter-framework challenging key sociological beliefs of mainstream white sociologists.

"Re-Making Race, Class, and Nation: Black Professionals in Brazil and South Africa" Graziella Moraes Dias da Silva PhD 2010 Harvard Sociology DepartmentAdvisor: Michele Lamont

Abstract

Through the perceptions of black professionals in Brazil and South Africa, this dissertation explores the distinct and dynamic ways race and class interact across different national contexts. By contrasting the experiences of the invisible Brazilian black middle class and the evident South African black middle class, it contributes to the study of a significant and understudied group in two societies undergoing important changes. Two sets of questions guide this dissertation: First, how do upwardly mobile blacks in Brazil and South Africa perceive racism and discrimination in their countries? Second, how do the experiences of black professionals challenge the historical interface of race and nation in these two contexts? In order to address these questions, I rely on a census data, national survey studies, and 112 in-depth interviews with black professionals in Brazil and South Africa.

Socioeconomic and survey results show that, despite important improvements in racial inequalities, racism is continuously identified as a problem by large majorities in Brazil and South Africa—but particularly by the black middle class. Through the comparison of discrimination experiences, of folk conceptualizations of racism, and of the interface of racial and national identifications among black professionals in Brazil and South Africa, this dissertation identifies two contrasting antiracism narratives among black professionals in these two countries: homogenizing universalism in Brazil and universalized diversities in South Africa.

Zhang, Lu. "From Detroit to Shanghai? Globalization, Market Reform, and Dynamics of Labor Unrest in the Chinese Automobile Industry, 1980 to the Present." The Johns Hopkins University, Baltimore, MD. February 2010.

Abstract.

The rapid rise of China to become the world's largest auto vehicle producer and market made newspaper headlines at the end of 2009. Despite the extensive interest in the booming Chinese auto industry, little attention has been paid to the 1.7 million Chinese autoworkers who are "making" those headlines. This dissertation explores the current conditions, subjectivity and collective actions of the Chinese autoworkers, and how shop-floor, national and global processes have interacted in complex ways to produce the specific labor relations in the Chinese automobile industry. Specifically, I describe (1) the dramatic restructuring and re-composition of the autoworker labor force that has taken place since the mid-1990s; (2) the everyday experience of work on the shopfloor (the social life of the factory including the labor process, workplace hierarchy and relations with management, and the determination of wages); (3) the extent and type of grievances expressed by autoworkers; and (4) the extent and type of collective actions (resistance) that they engage in, and the sources of bargaining power on which they draw in these collective actions.

The dissertation is based on sixteen months of fieldwork from 2004 to 2007 at seven major automobile factories in six Chinese cities (Changchun, Shanghai, Qingdao, Yantai, Guangzhou, Wuhu), where I conducted in-depth interviews with 150 autoworkers, 30 managers, 20 factory party and union leaders, and 38 local government officials, labor dispute arbitrators, and Chinese labor scholars.

Awards

Congratulations to Julian Go. His book *American Empire and the Politics of Meaning: Elite Political Cultures in the Philippines and Puerto Rico* (Duke U. Press, 2008) was the co-winner of the Mary Douglas Prize for Best Book, given by the Culture Section of the ASA.

Congratulations to Ho-fung Hung, who has won the 2010 Outstanding Junior Faculty Award at Indiana University.

Congratulations to Enrique S. Pumar, who was elected Fellow at the Institute for Policy Research and Catholic Studies.

Announcements and Workshops

Announcement:
Edward A. Tiryakian has donated his professional correspondence of over fifty years to the Sociology Archives at Penn State University, a project under the guidance of Professor Alan Sica. His papers include extensive correspondence with Pitirim Sorokin, Talcott Parsons, S.N. Eisenstadt, and Robert Merton, among others.

Workshop:
Small-N Compass: Systematic Cross-Case Analysis at the ASA in Atlanta
Charles Ragin

Description:
The analytic challenge of case-oriented research is not simply that the number of cases is small, but that researchers gain useful in-depth knowledge of cases that is difficult to represent using conventional forms (e.g., representations that emphasize the "net effects" of "independent variables"). The researcher is left wondering how to represent knowledge of cases in a way that is meaningful and compact, but which also does not deny case complexity. Set-theoretic methods such as Qualitative Comparative Analysis (QCA), the central focus of this workshop, offer a solution. QCA is fundamentally a case-oriented method that can be applied to small-to-moderate size Ns. It is most useful when researchers have knowledge of each case included in an investigation, there is a relatively small number of such cases (e.g., 10-50), and the investigator seeks to compare cases as configurations. With these methods it is possible to construct representations of cross-case patterns that allow for substantial heterogeneity and diversity. This workshop offers an advanced introduction to the approach and to the use of the software package fsQCA. Both the crisp (i.e., Boolean) and fuzzy-set versions of the method will be presented.

Fuzzy set analysis is gaining popularity in the social sciences today because of the close connections it enables between verbal theory, substantive knowledge (especially in the assessment of set membership), and data analysis. Fuzzy sets are especially useful in case-oriented research, where the investigator has a degree of familiarity with the cases included in the investigation and seeks to understand cases configurationally--as specific combinations of aspects or elements. Using fuzzy-set methods, case outcomes can be examined in ways that allow for causal complexity, where different combinations of causally relevant conditions combine to generate the outcome in question. Also, with fuzzy-set methods it is a possible to evaluate arguments that causal conditions are necessary or sufficient. Examinations of this type are outside the scope of conventional analytical methods.

Symposium:
Junior Theorists Symposium. August 13, 2010, Emory University.

The Theory Section of the ASA invites you to attend its annual Junior Theorists Symposium, co-organized by Claire Laurier Decoteau (University of Illinois, Chicago) and Robert Jansen (University of Michigan). The conference brings together scholars at a relatively early stage in their careers who are engaged in original theoretical work as part of their ongoing research. This year's themes will be *The Practice of Theory*, *Culture and Action*, and *State, Politics, and Society*. Neil Gross (University of British Columbia), Michèle Lamont (Harvard), and Andreas Wimmer (UCLA) will serve as discussants.

The registration deadline is July 12. For a registration form, please email Robert Jansen at rsjansen@umich.edu.

In the next issue of *Trajectories*:

Methodological Pluralism in Comparative Historical Research

.

Contributions welcome: please contact the Editors at erikson@soc.umass.edu and isaac.reed@colorado.edu