TRAJECTORIES

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Editors' Note: We'd like to take this opportunity to announce that with this issue, we are completing our two-year term as editors, and will be handing over the newsletter to the next editorial team of Isaac Reed and Emily Erikson. Please direct future email correspondence regarding the newsletter to Isaac and Emily (isaac.reed@colorado.edu, erikson@soc.umass.edu). We are grateful to the many section members who agreed to contribute to the newsletter during our tenure, and to all of you who couldn't contribute but who responded graciously to our emails! -- GK & NC

Into the Maelstrom: Comparative-Historical Sociologists on the Financial Crisis

For this issue of Trajectories, we asked comparative-historical sociologists Bruce Carruthers, Sarah Quinn, and Greta Krippner to reflect on various aspects of the unfolding financial crisis.

When Rocket Science Misfires

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In recent decades, Wall Street became one of the biggest employers of mathematicians, physicists, and other so-called "rocket scientists." These new employees were prized for the technical and quantitative skills that allowed them to model, design, simulate, and evaluate the new financial instruments and derivative products that a newly deregulated financial sector was eager to produce and sell. The combination of deregulation and financial innovation with rocket science proved very lucrative, both for firms and their employees. And yet, everything came crashing down starting in the summer of 2007. Assets proved toxic, "black swans" flocked to Wall Street (i.e., ex ante improbable events began to occur with improbable regularity), liquidity disappeared, and generally investors realized that risks and values had been systematically and substantially misunderestimated. Among other things, this crisis involved a failure of collective cognition.

Failure in settings where technical expertise is shaped by strong organizational imperatives has been studied by sociologists like Charles Perrow, Lee Clarke, and Diane Vaughn. The latter's careful analysis of the space shuttle Challenger explosion revealed that the underestimation and even concealment of risk (forms of what retrospectively looked like "deviance") could become part of an organization's culture and informal standard operating procedures. What distinguishes the current crisis is a kind of "distributed cognition," dispersed cognitive practices that occurred between financial organizations as well as within them, and which involved individuals using a variety of technical devices (computers, graphic-user-interfaces, communication networks, formulae, ratings, algorithms) to interpret and make sense of their decision environments. Coupled with an industry-wide hubris stemming from Wall Street's ability to recruit smart, successful and high-status individuals, and then to shower them with riches, the stage was set for tragic overconfidence in quantitative prowess.

Early diagnoses of the subprime and related crises have uncovered a couple of ways in which financial engineering failed. Rocket scientists unleashed their quantitative skills on massive data sets, and the ones conventionally used to estimate the default probabilities associated with subprime mortgages covered a period starting in 1998. This meant that for many years their data sets did not include a sustained episode in which home prices dropped. And it seems that no-one thought to see whether their empirical data excluded any relevant scenarios, or how robust their estimates were with respect to that kind of a shock. Now we realize that subprime default rates are very sensitive to the trajectory of home prices (especially loans with "teaser" interest rates), and that price declines send default rates through the roof. In other respects as well, the last decade has been quite atypical.

Structured finance added complexity and (at first) liquidity to old-fashioned and pretty well-understood home mortgages. Through securitization, underlying sets of mortgages, loans or other assets were pooled together and the cash-flows that they generated were divided into ordered layers ("tranches") that had different priority claims to the cash-flow: the highest tranches were paid first, the lowest paid last. Securities were issued against each tranche (collateralized debt obligations, or "CDOs" for short), and so the highest tranches got the highest ratings from credit raters. Sometimes, the securitization process was applied again to produce second-order CDOs ("CDO^2") where the underlying asset was a CDO rather than a mortgage or loan. Well it turns out that it is very hard to estimate exactly the default probabilities associated with these more complex instruments, especially the second-order ones. And the errors magnify with each new level of complexity so that a slight under-estimation of default rates for a CDO becomes a big error for a CDO^2. Nevertheless, structured finance performed an alchemical transformation by turning a pool of assets with an average BB rating (for example) into one with an AA average rating. With the seal of approval provided by a high rating from Moody's or Standard and Poor's, investors eagerly bought up the new products.

Secured finance only worked because of how it exploited the central role played in modern financial markets by the credit rating agencies. These private for-profit entities are essentially unregulated and have for a century issued opinions about the risks associated with particular financial instruments. They started first with railway bonds, then included corporate bonds, and now assess almost everything under the sun. Their judgments are cast in precise ordinal categories, ranging from "AAA" at the top (for the S&P system) down to "D" at the bottom. How they create their ratings and what information they use is, of course, proprietary. But in markets characterized by significant asymmetries of information, the demand for ratings is substantial. Some have criticized the business model of the rating agencies, pointing out that securing revenue from the companies being rated sets up an obvious conflict-of-interest. However, there is no public oversight at this point and so without regulatory reform there is little chance that rating agencies will be held accountable in any thoroughgoing fashion.

Securitization also undermined the incentive for mortgage originators to gather sufficient information at the earliest stages of the lending process, when a borrower makes an application. An old-fashioned savings-and-loan institution making a 30-year mortgage loan knew that such a loan would have to generate 30 years of payments going from the borrower to the lender, and so took appropriate precaution when deciding whether to lend or not. With securitization, the loan ends up in someone else's hands, and is soon out of the originators'. Not only did this weaken the vigilance of mortgage originators (it is well-known that underwriting standards declined in recent years), but it made it very difficult to renegotiate mortgages once the borrower got into financial trouble. With an old-fashioned mortgage, borrowers could pay a visit to their old-fashioned lender and try to refinance or somehow alter the terms of the deal. Outright foreclosure is an outcome that everyone wants to avoid. But with securitization, mortgages are pooled, sold, sliced and diced so many ways that it is no longer clear who a troubled debtor should talk to. Securitization has made it much harder to conduct the negotiations necessary to keep loans current by adjusting their terms.

Regardless of the policy measures (TARP, the stimulus package, new regulations, etc) taken to nurse the economy back to life, on Wall Street rocket scientists are already recalibrating their models and re-estimating their risks. Doubtless, the new models will be an improvement over the old. One lesson that may be harder to learn, however, is the lesson of overconfidence. Financial institutions believed they could turn uncertainty into manageable risk by hiring the best and the brightest to do rocket science in a highly quantified and mathematically rigorous fashion. The fact that so many of their methods diffused throughout the entire financial community (ranging from Black-Scholes option pricing and VaR to more complex methods) bolstered their confidence with social consensus. Thanks to such efforts, people expected not to be surprised. So when the unexpected occurred, they were truly surprised. Despite their best efforts, markets could still blindside them with uncertainty.

Lemon Socialism and Securitization

Sarah Quinn
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From his column in the *New York Times*, Paul Krugman is leading the charge against Treasury Secretary Timothy Geithner's plan to solve the securitization crisis. The plan calls on the U.S. government to buy $1 trillion of toxic securitized bonds currently stagnating in banks. If these bonds turn out to be valued below the government's price, then the government will absorb the loss. In this way taxpayers carry the substantial risks that come with the inflated prices the government is likely to pay for the troubled assets. Not mincing words, Krugman has branded these kinds of plans "lemon socialism" because they socialize losses but hand over profits to bankers (Krugman 2009). Krugman's critique only gets stronger if we consider that lemon socialism is not just a consequence of securitization, but also a cause. When it comes to securitization, private profits and socialized risks are nothing new. At the close of the 1960s the US government used federal guarantees *in order to create the securitization market in the first place*. That is, the securitization market was built on a foundation of socialized risks.

The government has long been involved in the secondary mortgage market (the market where existing mortgages are bought and sold) because mortgages pose a problem for investment. Each is pegged to a unique location, building, and owner, so a fair amount of local knowledge is required to understand the value and risks associated with a given mortgage (Carruthers and Stinchcombe 1999; Maisel 1967). That the standard mortgage is thirty years long exacerbates this problem, as it leaves plenty of time for a borrower to undercut an investors' expected profits by paying off the mortgage early, or worse, by defaulting on the loan (Sellon and VanNahmen 1988). Before securitization, most investors on Wall Street simply dismissed mortgages as more trouble than they were worth. This created a shortage of funds in housing finance. The local savings and loans and banks that issued mortgages were funded through deposits. Once they lent out all of their money, the housing market would grind to a halt.

In the absence of additional private investors for mortgages, the government stepped in to fill the role. Since the depression, the federal agency Fannie Mae injected capital into the housing market during downturns by buying existing mortgages. This provided lenders with new funds, helping money flow into housing. Buying mortgages was expensive for the government in the short term, and these purchases were booked as expenditures on the budget. In the long-run, however, these costs were largely offset by the money they brought in. Between selling individual loans, issuing bonds, and collecting fees, Fannie Mae in this era could make enough to finance its operations (Federal National Mortgage Association 1966).

Everything changed in the 1960s when the distressed housing market collided with President Johnson's wartime budget. In 1966, rising inflation caused a credit crunch in housing, and the biggest dip in home building in 20 years spurred Fannie Mae to purchase $2 billion in mortgages (Fish 1979; Green and Wachter 2005). Experts predicted that problems with housing would persist and even worsen. Fannie would soon face even larger outlays (Sellon and VanNahmen 1988). The problem was that the Vietnam War had already combined with Johnson's Great Society programs to push the budget towards the debt ceiling. The Administration could not politically afford additional expenditures that would add to the deficit, even if the outlays would eventually bring in offsetting funds.

Johnson's solution was to remove Fannie Mae from the federal budget completely. To do this without devastating the economy, he had to create a private secondary mortgage market big enough to take the government's place. The Housing and Urban Development (HUD) Act of 1968 set out to create such a market, taking two giant steps that would revolutionize American housing finance: the Act privatized Fannie Mae, and it laid the foundation for a market for *Mortgage-Backed Securities* (MBS). To make a MBS, a company takes a group of mortgages, pools them, and then uses a bond to sell off parts of the pool. The process is called securitization, and these securitized bonds eventually evolved into the toxic assets covered under Geithner's plan. It is important to remember that back in 1968 no one was sure whether investors would accept MBS or the newly privatized Fannie Mae. So officials used a small arsenal of government guarantees and support to lure Wall Street investors into the secondary mortgage market.

Privatizing Fannie Mae removed it from the government's budget, but not from the government's care. Fannie agreed to support the housing market, and in return received privileges that included conditional access to a $2.25 billion line of credit from the Treasury. This was an economically and politically efficient solution for Johnson because a guarantee did not add to the deficit. With Fannie, these explicit forms of government support were underscored by a giant implicit guarantee. Many people believed that Fannie Mae was too big and too important for the government to ever let it fail. Debated for years, the issue was definitively settled in 2008 when the troubled company was reabsorbed by the government, rendering the veiled promises explicit.

How did the HUD Act of 1968 set the foundation for the MBS market? It authorized Fannie to issue securitized bonds, and encouraged private companies to do the same. MBS held a great deal of promise, and a Senate committee boasted that "if such securities become well enough established so that many private issuers are issuing them, they could constitute a significant factor in attracting investment funds to the field of mortgage investment" (U.S. Senate 1968). Officials knew that risks had heretofore paralyzed the market. To counter this, they offered a "double guarantee" to create "a virtually riskless security with broad market acceptability" (OBR (Reeve) to Director; Nov 9, 1967. Califano Office Files; Aides; LBJ). First, the MBS pools would contain mortgages already insured by the Federal Housing Authority or Veterans Administration. This offered protection if homeowners defaulted (Black, Garbade, and Silber 1981). A second guarantee of the pool itself protected investors if a bank that issued the securitized bonds defaulted (Ibid).[1] The latter was

[1] These guarantees were offered through the new federal housing agency Ginnie Mae (Government National Mortgage Association), which was created to take over key government functions in the secondary housing market. In 1970 Freddie Mac was created in the same model as Fannie Mae. It would ac-

added because bankers had insisted that without it, MBS would be passed over in favor of safer and more familiar Treasury securities (Minutes ACFA, Feb 15, 1966, E37, RG 51, NARA II; Minutes HCCM, May 1967, Maisel Papers). Eventually investors became comfortable enough with MBS that they no longer required such strong support. Still, these guarantees played a crucial role in normalizing MBS and establishing the market in the first place.

MBS helped the Johnson administration remove the costs of housing finance from the federal budget, thereby relieving a great deal of political pressure. In forging a flexible tool with broad applications – one that effectively decoupled risks and profits – the administration set the stage for the contemporary crisis. The legislation created a system that encouraged people to take on more risks, and to feel more comfortable holding risks that they did not fully understand. Of course, these policies alone did not cause the credit and housing bubbles that have so deeply wounded our economy. The 1970s and 1980s brought waves of innovations that made MBS much more flexible, complicated, and popular. Aided by credit rating agencies and new computer technologies, entrepreneurs figured out how to design bonds that they could sell without government guarantees, even when the underlying mortgages included big loans to people with bad credit. A series of deregulations stripped away important government controls in housing and financial markets, and then low interest rates encouraged investors to pour money into housing finance. Still, before there was a race to the bottom of the subprime market, Wall Street first had to learn to stop worrying and love MBS. The U.S. government privatized profits and socialized risks to make the latter happen.

Mortgages carry special risks that have stymied private industry under the best of circumstances. In a crisis, socialized risks are virtually assured. What is really at issue, then, is whether privatizing profits at the same time is fair and wise. Critics of Geithner's plans are right to pause for thought at this juncture. If the current crisis has taught us anything, it is that freeing profits from risks is relatively easy, but doing that *responsibly* over a long period of time is a good deal harder. Today as the US government rushes to bail out an economy nearly drowned under a sea of credit, it resembles nothing so much as the Sorcerers' Apprentice who failed to control the magic he invoked to do his bidding. It is fair to ask whether a return to lemon socialism amounts to little more than an attempt to re-enchant renegade brooms. In light of this history, Krugman's reservations are well placed.

tually be Freddie that took the lead in the issuance of MBS throughout the 1970s and 1980s.

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Credit Crunches, Then & Now

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The financial crisis has held no shortage of surprises for students of U.S. political economy. The short list would include the sudden extinction of the investment bank, the near nationalization of the U.S. financial system, and the rapid transmission of what at first appeared a financial panic confined to U.S. mortgage markets to the global economic system. But more surprising, I think, than the remarkable events that have occurred since U.S. mortgage markets began to implode over a year ago is an event that has *not* occurred. Given the scope of the financial crisis, and its devastating implications for American households facing foreclosure, job losses, restricted credit access, and other hardships, we might expect widespread social protest. While there have been a few instances of protest activity in the United States in response to the financial crisis, these episodes have been sporadic and limited in nature. In the wake of the recent outrage over the payment of corporate bonuses at AIG, it appeared that America would finally have its populist moment, but as of this writing the new populism appears stillborn.

The puzzle deepens when we put contemporary financial politics in longer-term historical perspective. Our own era of financial exuberance, manias and crashes, resembles no other period in our history so much as the late nineteenth- and early twentieth-century era of finance capitalism. That period, notably, was characterized by several vigorous social movements that politicized issues of money, credit, and finance. But paradoxically, such issues are not the stuff of politics in our own time. Rather, with few exceptions, money, credit, and finance appear as technical questions, not political issues at all. In this regard, the relatively muted response to the ongoing financial crisis fits into a broader pattern which has characterized U.S. political economy since at least the early 1980s.

How can we explain the curious absence of a vigorous financial politics in our own era of financialization? As with so many puzzles of contemporary political economy, I am going to suggest that the answer lies not in recent developments but several decades back, in the tumultuous 1970s. In that decade, there *was* an incipient financial politics, with political mobilization occurring in particular around access to credit (Greider 1987). What happened to this mobilization? Why were financial questions subsequently removed from politics, even as financial activities increasingly became the axis on which the U.S. economy turned? At one level, the answer to this question is obvious, and does not merit much elaboration: U.S. financial markets were deregulated over the course of the 1970s, with the result that credit was no longer restricted, but widely available. In sharp distinction to the experience of the late nineteenth century, an era of acute credit shortages, financial politics in the contemporary era have been washed away by abundant credit.

This is a familiar story, and it helps to make sense of contemporary politics generally, where the role of credit in easing the social and political tensions that might have been expected to accompany stagnant and declining real incomes has been noted. But to return to our opening gambit, this still leaves the problem of why the current credit crunch has not reinvigorated oppositional politics in the economy – or has done so only to a very limited extent. Here I would like to suggest that there is a more nuanced story than the standard account of widened access to credit that can help us to make sense of contemporary financial politics. I hasten to add that, given the still unfolding nature of the crisis, this alternative account should really be treated more as a hypothesis to be tested by events rather than a definitive or final statement.

In brief, my argument is that the muted nature of contemporary financial politics reflects not only widened access to credit as a result of financial deregulation, but also the manner in which deregulation has changed how episodes of credit restraint are shared across social groups. In order to lay this argument out, it is necessary to explain how credit restraint operated in the U.S. economy prior to the deregulation of financial markets. In the era preceding the passage of financial reform legislation in 1980, the key mechanism imposing restraint on the flow of credit was a device called Regulation Q, a regulation that imposed a ceiling on the rate of interest banks and thrifts could pay for deposits. The express purpose of Regulation Q, which had been legislated as part of the Banking Act of 1933, was to prevent ruinous competition between depository institutions. In the wake of spreading bank failures in the 1930s, it was widely believed that a bidding war for deposits in the 1920s had caused financial institutions to pay too much for deposits, drawing bankers into reckless lending.

But in addition to suppressing competition between financial institutions, Regulation Q also served as a convenient tool for stabilizing the economy over the course of the business cycle. When inflationary pressures in the economy stirred, market interest rates would rise above the regulated ceilings on savings deposits, prompting households and corporations alike to pull their funds out of depository institutions and invest in Treasury bills and other instruments offering a market rate of return. The predictable result was that, in periods of high market interest rates, capital would hemorrhage from banks and thrifts, and lending from these institutions would come to an abrupt halt. These credit crunches would sharply and quickly curtail economic activity. As the economy plummeted, the mechanism would quickly go into reverse: market interest rates would fall back below Regulation Q ceilings, causing funds to flow back into depository institutions, restarting lending and economic expansion.

This system had the significant advantage of imposing restraint on the economy at relatively low rates of interest (Kaufman 1986; Wojnilower 1980). Market interest rates merely had to inch above regulated ceilings and the flow of credit to the economy was quite literally shut off. Unlike what occurs in a deregulated economic environment, in which credit becomes more expensive during periods of economic exuberance, it was simply *unavailable* in the pre-deregulation era. During such episodes of credit restraint, would-be borrowers with credentials that make today's mortgage brokers swoon (e.g., a down payment equivalent to 25 percent of the purchase price of a home) were routinely turned away by lenders. Cities were unable to raise bids in municipal bond markets in order to finance infrastructure projects or build public housing. Otherwise profitable business ventures went undeveloped for lack of credit. And this shared rationing experience produced a vigorous politics around credit as numerous, intersecting social movements sought to define access to credit as a basic entitlement of citizenship. As Gilbert Stewart, President of the National Small Business Association warned Congress in 1973, unless suburban homeowners, inner-city residents, small business owners, and farmers denied access to credit received relief from tight credit, legislators would see a surge of popular anger so potent they would wonder whether they had time-traveled back to the nineteenth century (United States House of Representatives, 1973, p. 92).

It was precisely this fate that legislators sought to avoid when they deregulated financial markets by removing interest rate ceilings from savings deposits in 1980. In a context in which it appeared that credit would always exist in short supply, legislators were increasingly under pressure to devise schemes to allocate credit, directly determining which sectors – households or small business, municipalities or farmers – would receive preferential access. This was a task that legislators were quite reluctant to take up – for the reasons that Gilbert Stewart made clear. Deregulation offered an enticing alternative. Without interest rate ceilings on savings deposits causing banks and thrifts to periodically hemorrhage funds, credit would be free to flow to the highest bidder. Willingness to pay, rather than rickety interest rate controls, would determine access to funds. In short, removing interest rate controls meant that the *market* rather than state officials could distribute scarce credit among competing social sectors.

Central to legislators' support of deregulation, of course, was the notion that the price mechanism would ration credit in much the same way as had been formerly achieved through interest rate ceilings. As the economy accelerated, the cost of credit would be bid up, discouraging would-be borrowers from seeking access to loans and thereby imposing restraint on the economy. But one of the great surprises of deregulation was that prices largely *failed* to ration (Greider 1987; Wojnilower 1985). As it turned out, Americans were insensitive to the cost of credit in their borrowing decisions – they would continue borrowing except at *very* high levels of interest. In order to impose restraint on the economy, then, policymakers would have to push interest rates to very high levels indeed. In short, the result of the deregulation of interest rate controls was free-flowing, but *expensive* credit.

In this context, credit politics in the U.S. economy were dramatically reconfigured. Free-flowing credit disorganized the broad-based coalition of surburbanites, inner-city residents, small business owners, and farmers that had politicized credit in the 1970s. No longer would credit markets periodically seize up, shutting out borrowers of the most varied financial circumstances. Now credit would always be available – *at a price*. In this manner, financial deregulation divided individuals into those who, with proper credit histories and formalized relationships to financial institutions had virtually unrestricted access to credit, and those, euphemistically referred to as the "unbanked," who did not. Credit activism moved from the town hall and the labor union to the soup kitchen, developing from a preoccupation of middle-class homeowners into a movement directed primarily at issues of urban poverty.

We can discern the traces of this transformation in the current credit crisis, which differs in important ways from the episodes of credit restraint in 1970s. Importantly, credit rationing in a regulated environment cut *across* social classes; credit was unavailable regardless of the creditworthiness of the individual or project. This is quite distinct from the form taken by credit restraint in a *deregulated* environment, where rationing operates through the price of credit or through the application of more stringent lending criteria rather than through availability per se. To be sure, there has been of lot of discussion of the availability of credit in the recent crisis, but this is a bit misleading. With the exception of a relatively brief period immediately following the failure of Lehman Brothers, credit has been available through the current "crunch" to borrowers who meet lenders' strict qualifications, and who are willing to pay a premium (sometimes a significant one) for access to capital. In this respect, credit restraint in a deregulated environment does *not* affect all individuals equally but is stratified in its impact by the economic position of the borrower. This shift, I argue, offers important insights into why contemporary financial politics differ so greatly from otherwise comparable periods when credit rationing was shared much more widely than it is in the present context. As the financial trauma spreads, we may yet witness a repoliticization of finance as access to credit becomes newly restrictive for ever broader segments of American borrowers. But the analysis presented here suggests that this is likely to be a very different – and more limited – kind of politics than has characterized previous historical experience.

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Dialogues: Author Meets Author

In this issue of Trajectories, we continue a new feature in which we invite the authors of two recent books on closely related themes to interview each other. In this column, Julian Go and Karen Barkey, both authors of important new books exploring aspects of empire, discuss their work and the development of the field of comparative and historical sociology. Each author begins with a synopsis of the other's book.

Julian Go. *American Empire and the Politics of Meaning: Elite Political Cultures in the Philippines and Puerto Rico during U.S Colonialism* (Duke University Press, 2008)

Review by Karen Barkey

Julian Go's remarkable comparison of the elite political cultures during the years of American rule in Puerto Rico and the Philippines comes at the time when discussions of American imperialism are heightened and where empire has become an important political trope for the international order as we experience it. The book takes the notion of an American empire seriously, in the sense that America by extending its rule in many parts of the world is both expanding its sphere of influence in political, structural and cultural ways, but also countries under United States domination respond gravely to the nature of imposed rule. In these responses, Go identifies both similarities in the political culture of initial responses and differences in the reproduction of colonial-colonized relations that stem from patterns of cultural transformation. We learn more about the subtle ways in which cultural understandings, schemas and signifiers work to influence these relational outcomes. Julian Go provides us with a sustained cultural analysis of colonialism as experienced by the elites of Puerto Rico and the Philippines, a task that helps us recognize how the cultures of the colonized and colonizer have an impact on the process of colonialism. But he does more than that since he also demonstrates the manner in which the cultural approach to colonialism is not just about an encounter and the production of a symbiotic outcome, but the production and reproduction of colonial relations under changing conditions and cultural formats. Thus, even though Go studies a short period of time, there is tremendous longevity and depth to the unfolding of the colonial process as studied in cultural terms. *His interest is in cultural trajectories and how these unfold over time as processes of meaning making over time*.

This book provides a lucid and methodical argument that unfolds in two parts. First, Go presents the cultural schemas of the different players in the colonial setting. The American and the cultural systems of the significant elites of the two colonies are examined to treat the encounter of their schemas, meanings and representations. Go emphasizes the degree to which the American imperialism that spread across the two countries was similar in its goals, intentions and management styles and understandings of how to build legitimacy. American colonial rulers were keen on paying attention to the perceived needs and demands of the local elites, to build a legitimate colonialism. The projects diverged at the local levels given how the elites domesticated American tutelage. This divergence emerged from the manner in which elites understood their own pre-existing meanings and their particular position vis-à-vis the relations they wanted with the United States. What is really important here is the manner in which Go gives agency to the local actors, to their relations and their historical and socio-political context which is the basis of how they constructed their understanding of what they wanted out of American imperialism. This is new and innovative for most of the scholars of empire who ignore the agency of the colonized until they resist, let alone actively shape the manner in which colonialism is established. Go richly depicts the ways in which the local elites turned around and imbued the American discourse and openings with their own cultural meanings, making them available and acceptable to the people, domesticating tutelage.

Yet if both elites domesticated American tutelage, and American colonial intentions were similar, what explains varying outcomes, a structural transformation for Puerto Rico, and a revaluation of the existing system in the Philippines? Here, Go becomes really innovative. By focusing on *culture as a semiotic system-in-practice*, Go takes seriously the constraints that the institutionalized culture presented for the elites and whether they provided validation of their predictions with regard to the impact of American tutelage. While the Puerto Rican case demonstrates that the elites' assumptions were disrupted by the practice, leading to questioning, refashioning and change, the Philippines remained more stable in their reproduction of the forms of American tutelage, maintaining their own meanings and evaluations.

The merits of this book are numerous and multilayered. The comparative approach, increasingly under fire in historical sociology, Go demonstrates, is still a powerful tool in our hands. The comparison between these two cases highlights a totally different set of questions and strategies than the cases engender on their own. Also, a simple structural functional approach might identify a variety of cleavages and inconsistencies in the structural system of society. Yet, it is unable to get at the reason why individuals and groups act to relieve these contradictions or why some cleavages are highlighted rather than others. Here Go provides the layers of meaning that make action possible. While many scholars espouse a cultural approach, Go actually theorizes his own particular brand and demonstrates its advantages at the empirical level. This work also goes a long way towards confusing the simplistic arguments for or against empire. With great nuance Go shows that the American colonial project is neither entirely good or bad; that the outcome of the colonial encounter is much more contingent, and in many ways the unintended outcome of the various aims and meanings attributed by actors on each side of the divide. This is an elegant study of comparative historical studies, cultural theory and American imperialism.

Questions: Karen Barkey to Julian Go

1. BARKEY: One can argue that in many ways the project of American imperialism has changed very little in substance from the cases that you analyze to the more contemporary example with Iraq being the prime example. The discourse of democracy and self-government remain the key markers of both eras. However, the relation you describe between the official American colonial discourse and its implementation seems different in your cases than the recent ones. Can we compare these two enterprises?

GO: Yes, one similarity between America's occupation of Puerto Rico and the Philippines in the early 20th century and the current US military occupation in Iraq is the American occupiers' democratizing discourse. One big difference, however, is that the US was more willing to take Puerto Rico and the Philippines as colonies in the traditional sense of the word. The US has always insisted it would only temporarily occupy Iraq; but with Puerto Rico and the Philippines there was left open the possibility that the two places might remain under direct US control forever (there was even some consideration of making either of them fully-fledged states in the American Union, though this was dismissed later on numerous grounds). Another big difference is that I think at least initially, American colonialists in Puerto Rico and the Philippines were much more serious about transferring American-type political institutions and making the political system look very "American" both in form and in practice. With Iraq, while there has been a lot of American involvement in, say, writing the constitution and in creating the political system, we hear less talk of directly "Americanizing" the system and more a rhetoric of localizing it to meet the peculiarities of a presumed Iraqi "culture."

2. BARKEY: Your book helps bring back comparative analysis to historical sociology, demonstrating that after all the various critiques it is still a tool we cannot dispense with. I was wondering how you would respond to critiques leveled at this approach?

GO: I actually agree with the main thrust of the general critique: that is, I agree with Bill Sewell's critique of Skocpol's Millsian approach: it does run the risk of freezing social processes in time and overlooking the processual and sequential character of social life. However, I don't think that comparison is inherently antithetical to rigorous temporally-structured analyses. What I try to do in the book is wed a more Millsian type of comparison to a more temporally-sensitive approach. I compare trajectories of meaning-making rather than simply comparing cases and outcomes; I look for initially common paths and then track divergences in the hope of explaining them (by, for instance, comparing possible turning points). In a sense, I try to marry Sewell and Skocpol (hopefully neither will be offended by this metaphor!).

3. BARKEY: How could one extend your analysis to understand the waning years of American colonialism in Puerto Rico and the Philippines and the differences in their political positions today? Can you speculate on this? In what ways can your analysis be helpful for us to understand the continuing, albeit different, US-Philippines relations of today?

GO: One of the most fascinating aspects of the comparison between Puerto Rico and the Philippines is that they ended up so differently: the Philippines ended up as an independent nation-state while Puerto Rico ended up as a "Commonwealth" (with more direct and formal political controls exercised by the US federal government). I have yet to see a good explanation for this difference, and since my book is concerned only with the incipient years of US colonialism it does not explain it either. My book does offer some clues, however, and I am currently trying to work out a more definite answer (for now I'd say it has a lot to do with the relative economic autonomy of the Philippines vs. Puerto Rico since the period of late Spanish rule through the early years of American occupation). I'm also trying to work out another related puzzle: why the US gave formal independence to Cuba just at the same time that it rejected independence for both Puerto Rico and the Philippines. But I hope that these are the sorts of questions that I do not have to answer on my own in the future. One goal of my book after all is to invite other comparative-historical sociologists to think harder about American empire and about America's dealings with weaker countries more broadly. The study of American empire is far too important to be left to only historians, economists or political scientists.

Karen Barkey. *Empire of Difference: The Ottomans in Comparative Perspective* (Cambridge University Press, 2008)

Review by Julian Go

Karen Barkey's *Empire of Difference* is an exemplary work in historical sociology. Rather than telling a tired tale of the rise and fall of empires, it illuminates how and why empires persist over time, tackling the too often neglected issue of imperial resiliency and robustness. The empire in question is the Ottoman Empire, which lasted over 600 years and which covered a diverse array of peoples and spaces over three continents. Barkey explores the social organization of this long-lasting and remarkably diverse empire and its transformations over time. Along the way Barkey makes comparative nods to other empires, not least the Roman, Hapsburg, and Russian empires.

If the task of explaining persistence is what makes *Empire of Difference* palpably historical, what makes it sociological is the explanation itself. The key to understanding longevity, argues Barkey (building upon her previous work), is the "negotiated" character of imperial rule. From inception, Ottoman rulers were able to create extended networks across religious, ethnic, and cultural lines through key cooperating elites who mediated between center and periphery. The empire was successful in maintaining stable and beneficial relations with these "intermediary elites" not by coercing them or their local populations but by collaborating with them. And it was successful in maintaining rule over radically diverse populations not by suppressing or erasing difference but by, at times, cultivating it. In short empire was built and sustained not by the sword but by the bricolage of social actors and the layering of social fields and institutions.

It is hard to underestimate this novel intervention. From world-systems theory, we have economic accounts of empires. From political scientists and international relations scholars we have strictly political accounts. From Barkey we finally receive a truly sociological account. Barkey deftly enlists network theory and shows how the empire followed a "hub and spoke" pattern whereby localities were attached to the center but kept at distance from each other. Even then, this hub-and-spoke model is arguably too formal, for it belies the fact that the Ottoman empire was ultimately composed of multiple networks of interaction contingently conjoining diverse groups to the center through a variety of localized and flexible compacts. This complex arrangement minimized outright resistance to rule, allowed for the continued absorption of new subjects, and entailed the construction of "mobile markers of difference" (as Barkey astutely phrases it).

The sociological dimensions of the Ottoman Empire thereby disclosed are all the more brightened by the book's ability to track dialectics of structure and agency. Barkey looks at the long durée, carrying us through centuries. But she is able to construct a compelling narrative by organizing the book around four key moments: imperial emergence, imperial institutionalization, imperial remodeling, and transition to nation-state. And at each part of the story we see actors in contexts: local rulers strategizing and negotiating, religious groups marking differences and declaring loyalties, and sultans and peasants alike struggling with structural dilemmas. *Empire of Difference* never loses sight of the people producing processes. Yet, it also does well to maintain a cohesive theoretical framework. The networked, negotiated, hub-and-spoke model of empire developed in the book illuminates not only the emergence and sustenance of the Ottoman imperial formation but also its later demise. As *Empire of Difference* convincingly shows, the very social structures that sustained the empire also ultimately contributed to its own demise, marking the passage from empire to modern nation-state.

As more and more scholars become interested in empire (or, for some like Barkey herself, continue their long-standing interests in empire), *Empire of Difference* will be a necessary reference point. How does a sociologist "do" empires, when empires are such big things? *Empire of Difference* offers a lead. What sociological theories might we enlist in our task? *Empire of Difference* offers us a compelling framework to further fill and extend.

QUESTIONS: Julian Go to Karen Barkey

1. GO: Unlike my book, which covers a span of less than twenty years or so, your book covers centuries. Not many of us can do that, and even your previous work (*Bandits and Bureaucrats*) did not cover such a long period of time. What would you say was the biggest pitfall in studying 600 years and how did you overcome it?

BARKEY: I think the manner in which this project clicked in my mind somehow helped me avoid the issues related to scope. I had started a project on the end of the empire, on the nationalist movements and their impact on the core. Yet, at every turn, I felt like I had to explain how the empire worked, how it managed diversity, how rule was organized to tackle other issues. It then became clear to me that I was much more interested in a sociological study of imperial organization. This helped me make choices about moments in imperial history that remained key to organization and then work in a fashion to aggregate data and narratives on these moments before I even began to construct an analytic argument. I think my approach that likes to spend a lot of time embedded in history, and then work back and forth between history and my analytic argument paid off in the sense that it took time, but I did not lose my bearings. I think for me historical sociology is about the constant conversation between history and theory, and the slow shaping and reshaping of narratives along analytic lines that make sense at the levels of the macro historical events as well as the local micro level where individuals and groups interact and construct networks that are the middle range order of relations.

2. GO: Your book has a potentially wider import than just the study of empire. Could not your "network-negotiation" model (as I would call it) also apply to, say, modern nation-states that persist without serious internal weakening or rebellion?

BARKEY: Initially my interest in empire was comparative with the nation-state in the sense that we live in a world organized by the latter and not empires. However, the longevity of the nation-state is still a question; we are living the nation-state experiment. And other forms of organization like the European Union are also becoming important. So, my interest in empire is closely associated with the other side of the coin, the nation-state. In many ways they are such opposites that we have to think of both in order to make sense of each. Also up to very recently sociology was very focused on the nation-state, so one had to think about the theories developed in this context and see how they worked in other political formations. So once again, I think we have to go back and forth between cases and theories, between types of political formations and the analytic tools at our disposal to creatively study differences in rule and state society compacts. Empires are about negotiations between centers and different peripheries where the negotiation with one periphery is not necessarily replicated in another. In fact, the more diversity, the better. The nation-state strives for more homogeneity and uniformity of compact between state and society. Therefore, negotiation as I use it in empire is not exactly the same as in the nation-state. Yet, in both cases negotiations are embedded in the networks that are constructed at the interface of state and society.

3. GO: One of your later chapters addresses the transformation of the Ottoman empire to the modern nation-state. You highlight some international factors that contributed to this, but what about internal logics or built-in features of the empire that might have immanently led to imperial demise?

BARKEY: Alex Motyl argues that empires have a basic internal structural weakness built into them so that after a while intermediary elites who collect information and taxes for the state will not be willing to pass on their resources and will keep them locally instead, weakening the center and leading to the demise of the empire. This is according to him an internal dynamic that leads to the end of empire. This, to me is not entirely helpful since we have to understand why and how local intermediary elites who presumably made good deals with the center will change their mind. This happens if they have an incentive to send goods elsewhere, to deal with others and therefore, such changes occur when new opportunities for trade arise. Usually this is related to an exogenous process of change. I am not saying that this system was internally very tight and that unless something happened from the outside, it would not have changed. Rather, these were very connected spaces, empires and networks of trade and politics and they adapted all the time; yet many things had to happen together for the basic structure of empire—the hub-and-spoke—arrangement to start altering. And this happened very slowly, over time, with many different parts of structure registering some change and others remaining the same. To me this is due to both exogenous and endogenous processes of transformation.

Identities

In this section, we invite comparative-historical scholars to reflect on why they entered the subfield.

Sarah Babb

Boston College

I became a historical sociologist by accident. I was admitted at the very last minute to the Ph.D. program at Northwestern, and arrived in Evanston a month later with a very vague and eclectic array of sociological interests. Northwestern happened to have a program that was strong in historical sociology, and there I happened to take a course with Bruce Carruthers, who subsequently invited me to work with him on a research project concerning 19th century debates over monetary policy. I spent my first Chicago summer between the beach on Lake Michigan and a pile of political pamphlets from around the time of the Civil War. The rest, as they say, is history.

Although I arrived at historical sociology by accident, I was temperamentally inclined to stay. In part, this was because my experience with late-Cold War campus activism had made me interested in revolutions. As an undergraduate at the University of Michigan, I became involved in the issue of U.S. policy toward Central America, and spent two months in revolutionary Nicaragua working on a construction brigade, mixing concrete (badly) and learning about the devastating impact of the Contra war and the less inspirational aspects of the Sandinista regime. I became fascinated with historical debates within the sectarian left—often founded in different interpretations of the Russian revolution that had shaped the revolutions of the 20th century. Had the Russian revolution "gone wrong" with Stalin, as the Trotskyists contended and the Maoists denied? Or with Lenin or Marx, as many anarchists or Democratic Socialists argued? How implicated were ideas—in particular, the ideas of Marx and Lenin—in the Russian revolution's trajectory?

Among many young American leftists of the late 1980s, these sorts of arcane questions were still debated with a level of seriousness and passion that seems quaint today. Although the Soviet bloc collapsed soon after I graduated from college, my interest in the debates of the sectarian left prepared me well for social theory in graduate school.

I had not studied sociology as an undergraduate, but I came to graduate school well-versed in the writings of Marx (and, less usefully, the ideas of Lenin and his contemporary critic, Rosa Luxemberg). A friend in Ann Arbor, who called himself a Maoist at the time (and who is now a historical sociologist), suggested that I apply to sociology Ph.D. programs, since, in his words "you can study anything you want."

Once at Northwestern, I soon realized that comparative and historical sociology was a subfield filled with scholars who shared my youthful obsession with the causes, consequences, and pitfalls of revolutions. Cutting my intellectual teeth on the work of Barrington Moore and Theda Skocpol transformed my understanding of what revolutions were about, and introduced me to an intriguing new set of methods. I never became an accomplished macro comparativist—I was better suited to immersing myself in primary documents on specific times, places, and issues—although I like to think that a comparative spirit infuses all my work. Nor did I become a scholar of revolutions, although I teach an undergraduate course in the sociology of revolutions that I enjoy very much. Nevertheless, once I became exposed to the methods and topics of comparative historical sociology, the subfield acquired a hold on me that I was unable to shake off.

Unlike many historical sociologists, I have mostly gravitated toward contemporary issues rather than topics of more specifically historical interest. I tend to find research topics in current events and the work of economists and political scientists, not in the writings of historians. For example, Mexico is a historian's paradise, filled with well-organized, government-financed archives. But my doctoral dissertation (and later book, *Managing Mexico*) traced the history of the economics profession from its early-20th century origins to the present. It was inspired by the flamboyant U.S.-trained technocrats who were running the Mexican government at that time. Obviously, focusing on contemporary topics has no intellectual merits. Yet I've come to think that it has certain advantages in the competitive world of academic publishing. On the one hand, topics of contemporary interest are open to a wider audience, and hence of interest to a wider range of book editors. On the other hand, framing such topics historically often gives one's work a longer "shelf-life" than other works in sociology, which take so long to review and get published that they are often dated by the time they appear.

As I near the middle of my career, I can see that most sociologists tend to stick with the methods they learned in graduate school, and I am no exception. I think it's possible for old dogs to learn new tricks, but when those dogs are busy with teaching, service, and family obligations, it is hard to find time to learn a whole new way of doing things. Under the tutelage of Bruce Carruthers and Art Stinchcombe, I learned to combine a range of historical methods and sources. Since then, my usual modus operandi has been to identify a topic (e.g., economics in Mexico, the lending programs of the IMF, American policy toward the World Bank), and search for a collection of relatively homogeneous historical documents that allow for a longitudinal analysis; examples of documents I have analyzed in this way include articles from 19th century newspapers, undergraduate economics theses, IMF letters of intent, and Congressional appropriations hearings. To provide a backdrop and fill in the blanks, I supplement with secondary literature, additional archival documents, and personal interviews (for historical topics of relatively recent vintage). And then I shuffle the resulting voluminous piles of material around (previously on hand-written notecards, now organized in Atlas.ti) to "tell a story," as my professors at Northwestern always exhorted us to do. I hope I have some stories left to tell.

Chad Alan Goldberg

University of Wisconsin

When I was invited to contribute this essay to *Trajectories*, I was honored. But then I began to worry because, in the same way that Jason Kaufman "never intended to become an academic" (*Trajectories*, Spring 2008), I never set out to become a comparative-historical sociologist. Looking back, there seems to be little in the successive stages of my professional socialization to indicate the comparative-historical turn that I would eventually take; it is an identity that I assumed relatively late. Yet, as any good comparative-historical sociologist knows, the past is the material out of which the present is made. So how did the comparative-historical sociologist that I am now emerge out of such apparently unpromising material?

My introduction to sociology came when I was an undergraduate at New College of Florida. New College was founded in 1960 as an experimental liberal arts college with few course requirements and no grades, where every student was encouraged to take responsibility for his or her own education. In 1975, Florida's public university system bailed it out of debt and turned it into the system's honors college, though it retained its quirky character. I ended up there in 1989 because my parents, who lacked college degrees of their own, also lacked money to pay for mine, and because my good grades and status as a Florida resident ensured that I could go there for free. (I suppose that makes me an "oblate"—Pierre Bourdieu's term for teachers from modest backgrounds who owe their success to the educational system and thus become very loyal to it). I loved New College and flourished there. The course offerings made me feel like a kid in a candy store, and I relished the freedom to learn and explore whatever I was interested in, from English and Russian literature to anarchism, medieval philosophy to psychoanalysis, deep ecology to Hegel. Eventually I came under the influence of sociologist David Brain, a veteran of Harvard's famed Social Studies program, who introduced me to classical and contemporary social theory. I found social theory exciting and endlessly fascinating, because it grappled with big and important questions about freedom, rationality, individuality, inequality, and solidarity. In my fourth year, I declared a "split major" in philosophy and sociology, and I wrote a senior thesis that compared Emile Durkheim and Karl Marx's conceptions of freedom. At this point, a trajectory toward professional sociology might be discernable, but hardly a path to comparative-historical sociology. I was primarily interested in theory, and astonishingly and regrettably I didn't take a single history course when I was an undergraduate! Only later would I come to see a connection between theory and history.

After a brief post-college stint working for the Public Interest Research Groups, I decided I was a terrible organizer and that I missed the intellectual stimulation of my undergraduate days. I applied to several graduate programs in sociology, including the University of Wisconsin-Madison, which rejected me (only to hire me later as an assistant professor in a minor historical irony). Instead, I ended up going to the New School for Social Research in New York City. The New School was appealing for many reasons, not least because of its location at the center of the universe, and because it was the ideal place to study social theory. I developed a substantive interest in the welfare state there, both practically (those were the days of Newt Gingrich's Contract with America and "ending welfare as we know it") and theoretically (the "crisis of the welfare state" that Jürgen Habermas and Claus Offe sought to analyze). But there was still no sign of comparative-historical sociology on my horizon. In fact, I wrote my field statements in social theory and political sociology, not comparative-historical sociology, though that was an option.

It was not until I began searching for a dissertation topic that I really started to develop an interest in comparative-historical sociology. That interest was partly a result of social networks. To be sure, my advisor—Mustafa Emirbayer, whose guidance benefited me enormously—was another Harvard Social Studies veteran and better known as a theorist than a comparative-historical sociologist. But I also acquired a job at the New School's Center for Studies of Social Change, which Charles Tilly had founded before he left for Columbia University, and later a job with the journal *International Labor and Working-Class History*, both of which put me in contact with historically-minded faculty and graduate students (eventually including Tilly himself). No doubt these ties influenced my decision to write a dissertation that compared struggles over the status and rights of New York City workfare workers in the 1990s to similar struggles involving Works Progress Administration workers sixty years earlier.

But was my comparative-historical turn merely a "network story," to use Peter Bearman's expression (*Trajectories*, Spring 2008)? I think not. There must have been something in my previous development that made me receptive to those network influences. In my case, I think it was my strong interest in and engagement with social theory. Rather than being an impediment to comparative-historical work, it helped to sensitize me to the importance of historical legacies in shaping the present, whether it was the vicious circle described in Tocqueville's *Old Regime and the French Revolution* in which centralized despotism and revolutionary "anarchy" repeatedly engendered each other or the historical traditions that Marx saw weighing "like a nightmare on the brain of the living" and shaping the political loyalties of the French peasantry in *The Eighteenth Brumaire*. Of course, social theory can be ahistorical—perhaps too much of it is—but the best social theory, from the classical period to the present, is deeply informed by historical analysis and insight. That is why I find theory and history difficult to separate today, both in my teaching and in my research. Whether I am teaching classical sociological theory or "Capitalism, Socialism, and Democracy in America since 1890," whether I am writing about ideas (as in my recent article on Emile Durkheim's contributions to the sociology of anti-semitism) or about the history of the U.S. welfare state (as in my book *Citizens and Paupers*), I always strive to see what social theory and history have to say to each other. Viewed in this way, my own comparative-historical turn is neither accidental nor a turn away from theory, but rather an outgrowth of it, for there is no sociological theory worthy of the name that lacks an historical character.

Recent Dissertations

Gabriel Abend
Northwestern University
2008

A Genealogy of Business Ethics

This dissertation is a genealogy of business ethics, focusing mainly on the United States from the 1880s to the 1930s. Looking back from the 1930s, it asks: Where does business ethics come from? What is it made out of? What are its social, cultural, and institutional lineages? Chapter 1 clears the ground for the historical account. My genealogy of business ethics is a genealogy of public moral normativity. By "public moral normativity" I mean that which is publicly regarded as morally acceptable and/or desirable in a society *S* at time *t*. By "genealogy" I mean a narrative about where a particular social thing comes from. Chapter 2 explores how business ethics figured in the establishment and early activities of university-based business schools. I argue that the establishment of business schools was represented and justified partly in moral terms. From a public moral normativity viewpoint, business ethics provided good answers to the questions of why universities ought to offer business education, and what business the business schools were in. Chapter 3 explores the relationships between business ethics and business associations. In the early 20th century "American business" became a distinct entity, social actor, and moral subject. This new moral subject was accused of bad business ethics, most famously by the muckrakers. As a defense against the (perceived and/or real) effects of these accusations—public indignation and regulatory action—business associations resorted to business ethics. Chapter 4 considers the scholarly and practical/political value of my genealogy of business ethics. It concludes by making a case for a sociological account of societal metaethics.

Victor P. Corona
Columbia University
2009

Career Rhythms of United States Army Officers, 1870-1960

Why do certain personnel in organizations rise to the top of a career ladder while others exit at lower rungs? To explore this question in a large organizational labor market, this dissertation examines structures of careers in the United States Army officer corps over nine decades of massive deployments and organizational reform. The analysis is based on an original dataset of 5,114 careers originating in the years 1870-1922, collected from official Army records. Variation in these data is shown to be reducible to a small number of career rhythms, which I define as clusters of careers with similar speeds in their attainment of promotions and credentials. Optimal matching sequence analysis is used to uncover distinct rhythms associated with early exiters, wartime casualties, mid-risers, remedial staffers, late-exiting veterans, lockstep risers, and stars, among others. Three interacting factors are consistently associated with the careers of stars, those who climb to the top of the Army organizational ladder: early promotion to the rank of captain, early awarding of a temporary wartime command, called a brevet, and career entry shortly before a war. These findings imply that career trajectories are driven by a career's temporal proximity to major exogenous events like wars as well as cumulative advantage processes in which benefits accrue to early achievements. Since job performance and satisfaction are related to the possible careers that individuals perceive, the dissertation develops one way to parse the interwoven effects that produce distinct career outcomes.

Robert S. Jansen
University of California, Los Angeles
2009

Populist Mobilization: Peru in Historical and Comparative Perspective

While populism has long been a prominent feature of the Latin American political landscape, it remains poorly understood. This dissertation argues that populism is most productively treated as a particular type of political mobilization—as a *means* that challengers and incumbents alike can employ in pursuit of a wide range of social, political, and economic agendas. The first part of the dissertation explains the historical emergence of populist mobilization in Latin America. Through a comparative analysis of Latin American countries during the first half of the twentieth century, it identifies populist mobilization's historical preconditions. It then draws on archival data from a strategically selected case—Peru's 1931 presidential election—to identify the paths by which two politicians of very different ideological orientations came to undertake this particular line of political practice. The second part of the dissertation illuminates the practical organization of populist mobilization, demonstrating that it is far from the disorganized, demagogical sleight of hand implied by the existing literature. It does this by analyzing two common populist tactics—grassroots incorporative organizing and the staging of mass rallies—as they were undertaken by the competing populists of Peru's 1931 election. The dissertation concludes by suggesting that the practice of populist mobilization might itself have important consequences—social polarization in the short term and political instability in the long term—that are, at least to a certain extent, independent of the ends toward which it is directed.

Luisa Farah Schwartzman
University of Wisconsin-Madison
2009

Breaking the Wall, Drawing The Line: Racial Categories, Social Boundaries and Affirmative Action in Brazil

This dissertation examines the relationship between racial classification, racialized stratification and race-based affirmative action policies in Brazil. My aim is to show how approaches that do not take bounded and neatly distinguishable "black" and "white" groups for granted can be useful for both policymakers and stratification researchers. Chapter 1, the introduction, reviews both the Brazilian and the U.S.-based literatures on racial/ethnic "group-making" and categorization, and discusses how they can complement stratification research in informing our attempts to diagnose and address persistent forms of racialized inequality. Chapter 2 examines empirically if "money whitens" across generations in Brazil, investigating the extent to which parents' education affects how they racially classify their children in a national household survey. Chapter 3 shows how the particular race-class intersection from which students are recruited, and the different criteria for racial classification that different Brazilian universities use, result in different racial composition of student bodies in several Brazilian universities that have implemented affirmative action. Chapter 4 describes how students at one university justify their decisions about whether or not take advantage of affirmative action policies, and discuss why their choices often diverged from what policymakers have intended. In Chapter 5, the conclusion, I summarize my findings regarding the dynamics of social and symbolic racial boundaries in everyday and bureaucratic contexts in Brazil and discuss the implications for social scientists, activists and policymakers interested in diagnosing and diminishing racialized inequality.

Paul V. Stock
Colorado State University
2009

The Original Green Revolution: The Catholic Worker Farms and Environmental Morality

The following dissertation examines the history of the Catholic Worker farms. The Catholic Worker farms have printed a newspaper, run houses of hospitality and farms in the hope of treating people with dignity and working toward a common good. Founders Dorothy Day and Peter Maurin encouraged a Green Revolution predicated upon education, care for those in need and an agrarian tradition. Drawing on Jacques Ellul's work on the effects of a technological society, I offer a history of the Catholic Worker farms and its theoretical foundations as one way to mitigate those same effects. The Catholic Worker farms provide one illustration of an environmental morality that is counter to the ethics and theoretical morality common to the discourse of environmentalism.

Christopher S. Swader
Bremen International Graduate School of Social Sciences, Bremen, Germany
2008

Transformation to a Market Economy and Changing Social Values in China, Russia, and Eastern Germany

This thesis investigates the mechanisms driving changes in social values, or those values emphasizing relationships, intimate bonds, and families, in the new market economies of Russia, China, and Eastern Germany. It is hypothesized that tensions between social values and individualism, materialism, and calculative rationality have arisen as a result of the transformation to a free-market economy. Methods used are both contrasted semi-structured qualitative interviews with new-rich businessmen and their fathers in Moscow, Shanghai, and Leipzig and the secondary quantitative analysis of World Values Survey data. Findings illustrate the roles of cognitive adaptation, cognitive dissonance, ideological conflict, and intergenerational changeover as mechanisms through which individuals' values tend toward de-intimization as a latent effect of their adoption of the following 'tools of success' critical to the core of capitalist market culture: profit calculation, commodified time, instrumentalization of relationships, image cultivation, personal ambition and independence, enhanced work focus, tolerance of failure, and moral flexibility.

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Conference Announcements

XVII World Congress of Sociology, July 11-17, 2010, in Gothenburg, Sweden, is being organized by the International Sociological Association (ISA). ISA Research Committee Futures Research (RC07) invites proposals for papers and sessions. Comparative and historical work is particularly welcome. Contact: Markus S. Schulz, email isarc07@gmail.com, visit http://www.isa-sociology.org/congress2010/rc/rc07.htm.

Please remember to register by July 1!!

CHS Section Mini-Conference:
Comparing Past and Present

Location: Berkeley, CA
Date: August 12, 2009, 8am – 7pm

Featuring:

Opening Plenary: Past and Present: Using Theory

Panels: economic systems, immigration, collective action, religion, citizenship, empires, gender, states, and technologies of power.

Closing Plenary: Past and Present: Methods and Models

For a detailed schedule see: http://www2.asanet.org/sectionchs/chsprogram.pdf