

TRAJECTORIES

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From the Chair: Comparing Past and Present

Rebecca Emigh

University of California-Los Angeles

As comparative and historical sociologists, many of us are sensitive about justifying our choice of topics to audiences who may perceive them as obscure. Past CHS chairs, such as Gorski, Lachmann, and Goodwin, have argued for the relevance and importance of comparative and historical sociology in this column. In a recent issue, essayist Prasad considered ways to combat the “Prada Bag” problem that comparative/historical positions are only found in top-ranked sociology departments. This year, I hope our section can tackle these issues head on by addressing the question of how past and present can be compared. I will begin the discussion with some comments in this column, but more importantly, this year, after the ASA Meetings, from August 11th to August 12th, the Comparative and Historical Section will be sponsoring a mini-conference with this theme of “Comparing Past and Present.” It will have opening and closing plenaries on theory and method, respectively, and breakout sessions on states, empires, economic systems, gender, religion, immigration, class, and collective action. Please plan on staying one extra day after ASA to enjoy San Francisco and some stimulating discussion courtesy of your CHS section!

Some Current Social Problems:

A quick glance at the daily newspaper or a perfunctory listen to National Public Radio provides a long list of issues that suggest past and present comparisons. In these past months of September

and October, the stock market set new records for daily point gains and losses, while economists began to warn of a recession, or perhaps even, a depression. Explicit references to the Great Depression abound. Will the entire economy collapse as it did during that period of time? Is the current volatility in the stock market similar to past episodes? Does the extent of current regulation and financialization of the economy mean that the current economic crisis is more or less dangerous than in the past?

Like the economy, immigration policy has been another flash point in this year's presidential election. In this case, the press coverage focuses mostly on contemporary questions of whether immigration should be, or even can be, curtailed and whether immigrants assimilate into American society. Yet, the large-scale immigration to the United States in the late nineteenth and early twentieth centuries provides an obvious historical comparison. How does the scale of immigration compare in past and present? How do different laws in the two periods affect immigration? Does the experience of "assimilation" characterize immigrants, either past or present?

International current events also imply past and present comparisons. In the past several decades, tumultuous ethnic and racial relations have sometimes escalated into large-scale genocide. The latest upheavals along the Russian and Georgian border are a recent example of such tensions. More generally, what are the bases of social division and social stratification in past and present? What is the relative importance of class, race, and ethnicity? Though some theories suggest that ethnic and racial divisions should decline in contemporary societies, this outcome is clearly not the only possibility. Is class of primary importance only during industrial capitalism, while race and ethnicity are important before and after?

Finally, while research on transitions to capitalism were confined to historical studies for much of the twentieth century, the fall of communism in 1989 ushered in a whole new era of comparing past and present. Do the same basic mechanisms bring

about capitalist social institutions in the past and present? History must also make a difference. Historical actors rarely understood that they were in the midst of an epochal change of economic form, while contemporary actors are often trying to introduce explicit policies that bring about just such changes. Furthermore, forms of technology have changed dramatically over time. How do such historical changes affect how capitalism develops?

Types of Past and Present Comparisons:

While current events provide ample raw material for thinking about past and present, what sorts of intellectual models exist for explicit comparisons? Four possibilities seem obvious to me, though there may be others.

The Past as a Model for the Present: The most famous examples of this were found in the mid twentieth-century literature that focused on the "stages of development" through which all countries supposedly passed. Developed western countries provided models that undeveloped countries could follow, mostly in the same sequence, to achieve economic growth. Marxist variants of this model argued that all countries had to be capitalist before they

became socialist, so that they followed the same overall trajectory implied by Marx and Engels's *German Ideology*. Though this model seems to negate the importance of historical change, the idea of "history repeating itself" is still common.

The Past as a Difference from the Present: Foucault's method of genealogy (from Nietzsche) considers history as difference. To apply this method, the researcher begins with a present day social structure, and then steps backwards in time, looking for the same social structure in the past. This process continues until the researcher locates some past social structure that is strikingly different from the current one. Then, the researcher traces the social structure forward in time to discover how the difference arose historically. This method problematizes the present by showing how what is taken for granted in the present is not "natural" but is instead a social and historical construct. The historical practice seemed just as natu-

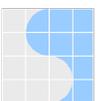
Explicit references to the Great Depression abound. Will the entire economy collapse as it did during that period of time? Is the current volatility in the stock market similar to past episodes?

ral to its practitioners in the past, yet was also a social construction. As used by Foucault, it fits with the overall Marxist agenda of uncovering taken-for-granted cultural understandings (hegemony or doxa) that veil power relations.

The Past as a Referent for the Present: This use of history outlines the similarities and differences between the past and present—almost like applying Mill’s methods of similarity and difference—to understand what is general and specific to any set of social processes across time. This method looks at cases in which the outcome both occurs and does not occur. I have argued for this use of history to compare transitions to capitalism, past and present. Some research uses England, as the first industrializer, to try to understand factors that produce transitions to capitalism, and thus deploys the “past as a model.” Instead, the “past as referent” approach considers a range of cases, where the transition to capitalism both occurred and did not occur in past and present, to try to understand which dimensions of these changes are historically specific and which ones are universal.

The Past as a Path to the Present: This model uses the past to trace a lineage to the present, by showing how the past is constraining or enabling. Thus, this approach stresses how the past sets overall paths or trajectories that lead to the present. This is often used to show why particular countries go in some directions, while other go in different ones, even when faced with the same national, international, or global conditions. Some researchers explicitly draw on the methods and heuristics of “path dependency” to conduct such comparisons, while others make use of narrative analysis.

I hope these comments stimulate ideas for our CHS mini-conference next August—see you there!



Remembering Charles Tilly

Editor's Note: On August 29, 2008 Charles Tilly passed away following a long illness. Tilly's contributions to the fields of comparative and historical sociology and social history are legendary. With the permission of the Social Science Research Council, we are reprinting some of the tributes to Tilly published on the SSRC's website. (More essays reflecting on Tilly's life and work can be found at <http://www.ssrc.org/essays/tilly/>).

A Voice We Will Miss

Craig Calhoun

New York University and the Social Science Research Council

Charles Tilly passed away last week. He was among the most distinguished of contemporary social scientists. Indeed, the SSRC had just awarded Tilly its highest honor, the Albert O. Hirschman Prize. Like Hirschman, however, Tilly was not only distinguished but distinctive. He had a voice of his own.

Chuck's voice animated his numerous books. From his remarkable study of counterrevolution in The Vendée through major historical studies in France and Britain, to an exploration of *Durable Inequality*, efforts to theorize contentious politics, and an investigation of the very act of giving explanations, Chuck's written work always had style. The style was often slightly impish, with a wink to knowing readers, and always elegant. He was witty without telling many jokes. But as wonderful as Chuck's writing was, he was even better in person. His talks were nearly always written out in advance, clear and precise, and timed to the minute. I heard him give dozens of speeches, but he is the only academic I have ever known who never went over the allotted time limit. And you could see him smile slightly to himself when he finished on the dot.

Chuck was also astonishingly prompt as well as detailed and effective on the range of collegial chores that demand the time and attention of contemporary academics. Universities are full of professors who teach less, write less, and have less

research basis for what they teach and write yet claim to be terribly busy when contacted to undertake a review. Chuck was an editor's dream, sending cogent comments almost by return mail. He was also an impressively effective editor, running for decades an Academic Press series that shaped the interdisciplinary field of historical social science. He didn't like everything he read, of course, and I have to admit he rejected my first book. But he was unfailingly and extraordinarily generous in comments on the draft manuscripts of his students.

And Chuck's students formed a network that stretched beyond the universities where he taught. He served as a mentor to SSRC dissertation fellows as far back as the 1960s. With Sid Tarrow and Doug McAdam he assembled summer workshops of young social movement scholars at the Center for Advanced Study in the Behavioral Sciences. He helped organize a legendary interdisciplinary network at Michigan and in his last years ran a Workshop on Contentious Politics that drew participants from all around New York, including many visiting international scholars.

Over the years Chuck served on several SSRC committees. They reflected some of the diversity of his interests and the different fields in which he was a recognized leader. He was co-chair of the History Panel in a major Survey of the Behavioral and Social Sciences for which the SSRC joined forces with National Academy of Sciences in the late 1960s. He was chair of the Committee on Mathematics in the Social Sciences in the late 1970s. He was a member of the Committee on States and Social Structures in the late 1980s.

Perhaps Chuck's most influential SSRC role was an early one. In 1969, he was asked by the Committee on Comparative Politics, chaired by Gabriel Almond, to bring historians and history into their social science conversation. The CCP had been running for 15 years at that point, and had exercised an enormous influence on the development of comparative research and especially on the challenges of new and newly independent states. It had spurred the development of generalizations, even theory of "nation-building" and the challenges faced by developing states. Chuck's charge

was to lead a group looking at European history to see whether the generalizations held there and whether history might even yield an improvement or two. The result was a truly path-breaking book, *The Formation of National States in Western Europe* (1975). This challenged the reigning developmentalism, pointing out how many states disappeared in European history, how constant were the conflicts and challenges, how central the processes of war. It upended a number of the CCP's previous generalizations, but not simply in the direction of particularism. Rather it offered new explanations. These centered substantively on the extent to which a struggle for survival amid conflict shaped European states more than domestic nation-building efforts. But the impact was not just substantive. The project helped to create a field of historical social science—or social science history. And for some thirty-five years, Charles Tilly would be one of its handful of leading practitioners.

One of the most distinguished of all contemporary social scientists, Tilly was an influential analyst of social movements and contentious politics, a path-breaker in the historical sociology of the state, a pivotal theorist of social inequality. When I spoke to him a couple of months ago about his having been awarded the Hirschman Prize, he told me he had long been an admirer of Albert Hirschman and it is easy to see why. Both men remade fields. Both wrote clear books that made complicated and nuanced analyses seem almost obvious—but only after their lucid formulations. Both men combined a passion for social science with a determination not to let this be owned by narrow disciplinary agendas or internal academic debates that lost purchase on the big issues in the larger world.

Early in his career, Tilly did historical sociology in a discipline where that hadn't yet become a recognized subfield. He studied conflict in a field dominated by Parsonsian functionalism and even initially at Harvard where Parsons held center stage and figures like George Homans and Barrington Moore were pushed a bit to the wings. Tilly might have chosen to exit. He might have decided he would get a better job as a loyalist. But he chose instead what Albert Hirschman clarified for us was always the third option: voice. Tilly's voice changed several fields, remaining impressively clear despite major contention and more

than a little conflict. He both studied how voice could matter and exemplified it

Chuck was startled when younger people thought of him as one of the older authorities and offended when they thought he wanted to encourage conformity. His own voice did command authority; sometimes there was conformity; and often he had the best arguments in the room. Moreover, his very eloquence meant sometimes that his positions were so completely stated that it was hard for others to see where to intervene. But though Chuck enjoyed using his voice to shape intellectual work, he never stopped nurturing others in developing theirs.

The Political Ethnographer's Compagnon

Javier Auyero

The University of Texas-Austin

“[R]ight now I want to be the mournful friend, *compañero del alma*, who tends the ground you fertilize and lie in, gave too soon,” writes Spanish poet Miguel Hernandez about the death of his close friend Ramón Sijé. Those words came to my mind, like lightning, when I learned about the passing of our Chuck Tilly on Tuesday last week. Ever since, I have been grieving the loss of my mentor, colleague, and *compagnon de route*. But I am not alone. Dozens of us who belong to Chuck's vast “contentious politics” virtual network, which he created and nurtured over the years, have been filling each other's inboxes with expressions of mourning. We have been sharing stories about the person, the teacher, the friend, the advisor, the jazz lover, the volleyball player, the wizard who has just left us.

I won't attempt in this brief tribute to summarize all the wonderful memories and anecdotes that have been circulating in that forum about his days at the University of Michigan, the New School, and Columbia. They all speak of his immense generosity, curiosity, humility, and openness. He is invariably described as a brilliant intellectual and scholar, as a unique advisor with an unparalleled kindness and egalitarianism, and as a noble man who was always there for us in the form of long and always insightful conversations, penetrating remarks in our workshops, encouraging e-mails in

response to our queries, and reactions to our papers, which always came sooner than expected. There has been a phenomenal outpouring of recollections of our shared times with Chuck—in seminar rooms, in his office, strolling down the streets of Ann Arbor or New York—to which no words can do justice.

Like many others, some of my most precious “Chuck moments” are related to the times when I left his office feeling smarter than I really was, knowing where to go with my research (or, at least, the three potential routes to take—his legendary advice almost always came in a set of three options), and with a fresh sense of possibility. (“With this project, you can take over where Wright Mills left off,” he once told me. “Really, Chuck?” I remember thinking, “You have more confidence in me than I have in myself.”) But it was his gentleness, his smile—and the way in which his sparkling eyes were half-closed when he was thinking—that I will never ever forget.

In this tribute I would like to speak of a Tilly that quite likely scholars in the future will not remember: the political ethnographer’s compaignon. Call him my own private Chuck if you will. He is the Tilly of big and small structures, large and micro processes, huge comparisons and tiny variations within a case. True, Chuck was not an ethnographer. But he was always there—for me and for others—when he was needed. Those of us who chose ethnography as our way of understanding (and, yes, explaining) the manifold and complex ways political actors act, think and feel the way they do, could count on him not to tell us what to think (never!) but how to go about collecting evidence and making our arguments. We could lean on him to sharpen our analytical perspectives. The principle he inevitably invoked—which I heard so many times in various incarnations because (silly me!) I kept forgetting—was quite simple: “Besides the case, what is your study about?” In his straightforward way, Chuck kept reminding us to locate our theoretical concerns at the beginning and at the end of the ethnographic enterprise: “What can those who are not interested in (say) Argentine poor people’s politics learn from your study?”

But that was hardly all. I cherish this paragraph he wrote to me after reading one of my drafts, which

sums up his (as far as I know unpublished) view of the craft of political ethnography:

Political ethnography is a risky business, at once intensely sociable and deeply isolating. On one side, its effective pursuit requires close involvement with political actors and therefore [implies] the danger of becoming their dupes, their representatives, their brokers, or their accomplices. On the other, bringing out the news so others can understand depends on multiple translations: from the stories that political participants tell to stories that audiences will understand, from local circumstances to issues that will be recognizable outside the locality, from concrete explanations for particular actions to accounts in which outsiders will at least recognize analogies to classes of actions with which they are familiar. The most widely read reporters on political conflict personalize their accounts brilliantly—but thereby neglect the social processes they are observing. Very few achieve balance between the personal and the systematic. Those few include James Scott, Adam Ashforth, Elisabeth Wood, and Pierre Bourdieu.

That assertion, which for me became a stimulating invitation, summarizes the challenges that lie ahead: a chase to find the right equilibrium between involvement and detachment, the personal and the systematic, being there and being here, stories told in the field and stories told to the public. And we know, because Chuck never let us forget, that we might fail in the attempt, but we should never cease to enjoy the search. Ultimately, then, I think the best tribute we can pay him as a scholar and as a mentor is to have fun in our intellectual pursuits and, as he repeatedly said to many of us, to do for our students and colleagues all the things he did for us.

The last stanza of Miguel Hernandez’s “Elegy” reads like this: “And I call you to come to the milky almond blossoms who are souls flying./That we still have so many things to talk about, compañero del alma, compañero.” With those words, I’d like to end my short tribute: thanking Chuck for all the things we learned both with and from him. Gracias, Chuck; eternal thanks for making us all not just better scholars but what is more important, better persons.

Dear Joan, Call Me "Chuck"

Joan Scott
Princeton University

In 1966, I was awarded a research training fellowship by the Social Science Research Council. The point of the fellowship was to encourage interdisciplinary training; the recipient was assigned to a mentor who would help accomplish that over the course of several years. I was incredibly fortunate to have Chuck Tilly as my mentor. My own dissertation adviser was less than helpful, so absorbed was he by his own narcissism. He did suggest the town that became the focus of my research, but that was about all he contributed to my academic development. Chuck was the opposite: a caring, patient teacher who gave consistent feedback, who tolerated what now seem (as I reread the letters we exchanged between 1966 and 1970) brash arguments from a feisty history graduate student, and who shepherded me along as I found my voice as a social historian and my place among people who would become colleagues and friends.

Early in September 1966, Chuck invited me to visit him at Harvard. I was still nursing my son, Tony, who had been born two months before, but went anyway. I was extremely anxious about how I could juggle motherhood and a professional career (feminism hadn't yet taught me that mine was not an individual problem), but much of that anxiety vanished the moment I met Chuck. He introduced me to his group of students (women and men treated equally) and to Louise; and we planned out the relationship that subsequently would be conducted mostly through letters, with occasional visits to Toronto (where he was then headed) and later to Ann Arbor.

I had a long reading list, mostly of sociologists. Chuck had me write papers about groups of readings I'd done. In response to one of these, which I called "Little Boxes," citing the Malvina Reynolds song from the 1960s ("little boxes made of ticky-tacky"), he taught me a good lesson about analytic perspective:

Now I'm NOT urging you to set up your work as a hokum test of an antiseptically-formulated general proposition; you've already told me how much you distrust boxy-blocky formulations of the process of industrialization, and how concrete you

want to be. I'm saying that in doing your relatively concrete historical job you still have to adopt some version of one or more of these analytic devices, and you might as well do it deliberately.

As I formulated my dissertation topic (on the glassworkers of Carmaux), Chuck not only provided direction and more reading (in my file, there are many hastily handwritten notes on small bits of paper from him with yet another suggestion of a book or article to read), but also contacts he thought would help me build a network of the kind he was so good at creating. In January of 1976, he alerted me to the existence of Bill Sewell this way:

William Sewell, Jr., a graduate student of Neil Smelser's at Berkeley, is undertaking a dissertation on "the response of various occupational groups in the city to the problems of urban and industrial life from 1850-1875"... His long letter makes him seem an intelligent man, worth keeping in touch with. I'm going to tell him you exist. Maybe you'd like to drop him a line [the address followed]. That's up to you.

Needless to say, I followed his suggestion and Bill and I have been friends ever since.

A few years later, when I was back from France and writing my thesis in Chicago, he wrote to advise me about how to present the first academic paper I would do for a serious conference—Steve Thernstrom's meeting at Yale on cities in the nineteenth century—which would become my first published article. And, of course, it was through Chuck that I met Louise, who would become my collaborator and co-author. Chuck encouraged our work, read drafts critically, and was the one who told us, when we'd written three articles on women's work and the family, that we now had to write a book.

That kind of advising and networking came naturally to Chuck. The famous Sunday evening seminar at the Tilly's in Ann Arbor provided an extraordinary set of connections for the growing world of social history/historical sociology all through the 1970s and 80s. In fact, wherever Chuck went, he created places in which intellectual exchange happened—comfortably, critically, profoundly. His role in the famous Round Tables at the Maison des Sciences de l'Homme in Paris

during the 1970s helped form an international network of social historians who, long after the conferences themselves, corresponded, cited one another, and formulated their work in terms of theoretical debates he had helped launch. In the New York Times obituary, Adam Ashforth called Chuck a sociologist for the 21st century. That may be true, but I think he will hold a more prominent place as one of the founders of social history in the 20th century and as one of the architects of that field—through his own publications, the students he trained, the seminars he held, and the networks he nurtured.

One of the most impressive aspects of Chuck was the friendly egalitarianism that characterized his interactions with colleagues and students: in fact, we were all treated pretty much the same if he thought our ideas were worth engaging. I'll never forget the letter I got from him in June 1967, as I was about to take off for dissertation research in France. In it he praised my prospectus, warned me that I'd never do everything I said I wanted to do (and that that was okay), suggested how to go about doing a first reconnaissance in the libraries and archives, and even recommended a place for me and my family to stay in Paris. But what I cherish most about that letter was how it began. For months, he had addressed me as "Joan"; I replied to "Mr. Tilly." This letter signaled the end of the formality: "Dear Joan," he wrote, "call me Chuck; it's more like equality."

I did call him Chuck after that, and I'm terribly sad that I won't be able to do it ever again.

I Went Up To Amiens Today

John Merriman
Yale University

I went up to Amiens today, out of nostalgia. Chuck and I had gone there long ago for some research in the Archives Départementales de la Somme. I write from Rouen, where I teach in May, and although I am often around here, I had not been up to Amiens for a long time. Thus, after hearing the sad news that Chuck had died, I wanted to go back. Of course, going on the road with Chuck was not the typical road trip of my college days. For one thing, he got up terribly early—it seemed like 3:00 a.m. but was probably

closer to 4:30, and began to do sit-ups. Then he read and thought for what seemed like hours. I, too, had thoughts at that time in the morning, since I suddenly found myself awake, but my thoughts, such as they were, concerned what I would eat for lunch that day. Chuck and I were very different.

When Chuck first went off to Angers to begin his research on the counter-revolution in the West of France that has come to be called the Vendée, after one of the key départements in the conflict, he knew little French. Back in the late 1950s, he was one of the very first generation of U.S. scholars to work in French departmental archives. Entering, he encountered the classically grumpy, blue-clad gardien. Slightly intimidated, Chuck froze when the man asked him what he wanted to see. He could not say anything. Finally, when the irritation of the gardien had become anger, Chuck was able to blurt out, "Montrez-moi un document (show me a document)!" The archival employee did just that, and then, when Chuck had conveyed the fact that he was interested in the counter-revolution during the French Revolution, hundreds and hundreds of documents followed.

In 1968, I started graduate school at Michigan in history, for not very compelling reasons. I was playing baseball in Ann Arbor in the summer and needed an excuse to stay around, so I took a history course. Still clueless about what I wanted to do, I enrolled in a seminar in the fall that seemed to focus principally on what French generals such as Marshall Soult thought about Louis-Philippe's July Monarchy. Someone—I cannot remember who—told me that I should read Charles Tilly, *The Vendée*. I did, one Saturday afternoon. Suddenly, it seemed that one could explain important events by looking at the bigger picture. Rebelions, indeed revolutions, had causes, and were part of change. Moreover, Chuck had just arrived at Michigan. I took his seminar the second semester of my second year. He was so nice and encouraging. I had become interested in the Revolution of 1848, and Chuck had on microfilm much of the relevant archival series, BB18 and BB30. He suggested that interesting things were going on in Limoges and the region of the Limousin during the Second Republic. I wrote a seminar paper on the dynamics of police repression there. Several months later, following my oral exams (I had met Chuck so recently that he was not even on committee for my orals), I was off in France for re-

search on my dissertation, with Chuck as the director.

Chez Tilly on Hill Street in Ann Arbor was the setting for now legendary Sunday evening seminars. Natalie Davis and Maurice Agulhon were among the speakers, but one could also hear graduate students discussing their dissertations. This was a perfect kind of apprenticeship, and the most important thing about it was that Chuck tolerated no kind of hierarchy, and everyone's ideas (even not so good ones) were equal. Wayne Te Brake, Bob Schwartz, Mike Hanagan, Miriam Cohen, Ron Aminzade, M.J. Maynes, and Bill Roy were among the participants in those days. And of course from the very beginning, Chuck was always just plain Chuck, not professor or doctor or some other pompous title, and most of the rest of us have kept that tradition alive.

Chuck worked almost all the time. He was so busy—and yet so generous, always, with his time. (This made it very difficult to imagine ever turning in a paper late; if he could work like that, we should be able to do so as well.) But he did not like to waste time. He always received so many letters, phone calls, and visits. (He once explained to me his “neutral corner” strategy for dealing with visitors, which would be to suggest not to meet in his office, but in some other place, so that he could decide when the talk was over after a reasonable amount of time and return to work.) Then with the computer age he always had hundreds of messages. He had almost no patience with small talk, and certainly not by e-mail. To get a response, one had to pose a specific question, “What is good on Albanian collective violence?” or “How are you?”, and then he always wrote back, immediately.

He was so loyal to his students, colleagues, and friends. He claimed that the only time he ever wore a tie was when he and Louise flew out to Carol's and my wedding in New Haven in 1980. (That was about my case, too.) Their presence obviously meant a great deal. When we took up permanent residence in France, in Ardèche (a challenge as I teach at Yale and go back and forth), he came to see what our village was like. Though he

seemed to enjoy himself, when he left I could tell he was thinking, would I really be able to get enough work done there. For about nine or ten years, Chuck had the lease for a tiny apartment in the second oldest building still standing in Paris on Rue François Miron in the Marais. I served as something of the agent for the apartment, and was there much more often than Chuck. We had very different standards of maintaining the apartment. Once, when Chuck arrived after my departure, he described it as looking like the last days of Pompeii. He could be a bit compulsive: on one occasion, he spent the last 20 minutes of his visit tying pieces of string together so that they could be added to the apartment's ball of string. After France became much less of a focus for his research, he really did not return here very often.

Chuck remained committed to virtually same day service when it came to reading manuscripts—thousands and thousands of them over his career—especially those of his colleagues, former students, and students. Within the past six years, my editors at Oxford and Norton contacted him to see if he would have time to write a blurb. In both cases, they each called me in astonishment to say that he had read the book and provided a wonderful blurb the next day. He

was like that.

Last year, Chuck agreed to give the keynote talk at a conference in Washington. He was not feeling well and was very tired, but was there all the same. I had been invited to introduce him, and he told me that he was very tired, and would speak only 35 minutes, so I introduced him for 10 or 12. He was fantastic, brilliant, cogent, compelling. It was vintage Chuck. It was the last time I ever had the chance to hear him speak.

Chuck's 51 books, by latest count, and hundreds of articles have of course had enormous influence on history, as they did on sociology, political science, and one should probably add anthropology and economics as well. He once described himself as working in the no-man's land between sociology and history, but what he did basically was to create historical sociology. In my case, as an historian, he has influenced all my books on France

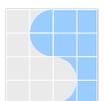
[Tilly] once described himself as working in the no-man's land between sociology and history, but what he did basically was to create historical sociology.

in important ways (including the one I dedicated to him in 1992). Examples of what I learned from Chuck include: how to think about the revolutionary process; the need to keep the dynamics of economic, social, political, and cultural change up front—to “put this in neon,” as he once said—within the context of the narrative history I sometimes do; the need to appreciate the complexity of cities and towns and relations between city and country; and much more.

Chuck once wrote, “It is bitter hard to write the history of remainders,” and that has always stuck with me. For *The Red City*, I first got the idea of pulling the comparison between the corporation of butchers based in the center of Limoges and the porcelain workers in the faubourgs from something once said. When I wrote *A History of Modern Europe from the Renaissance to the Present* and the subsequent edition, indeed the new one I am now concluding, I have always kept in mind something that he told me long ago—that history should not be seen as a series of bins that one opens up and then closes, moving on to another, that such big themes as statemaking and capitalism provide a way of understanding and presenting the past.

Whenever as a student (and beyond) I came to a snafu in what I was working on or writing about, I would go to Chuck to seek his advice. He invariably said, “Look, there are three aspects to this,” while holding his hands somewhat off to the side, oddly enough, in what seemed to be the shape of a box, one that suggested four aspects—but with Chuck, three or four was a simplification for the rest of us, because he could imagine about a hundred at any one time. (There is a photo of him on the inside jacket of one of his books, if I remember correctly, of him in that pose.)

Well, there are (at least) three ways of thinking about Chuck Tilly. He was the most brilliant person any of us will almost certainly ever know. His great influence on the social sciences will continue, through his own work and hopefully through his colleagues and former students. And he was a wonderful human being, someone of great good will and good humor, who cared about people and the human condition. And he was the perfect mentor and colleague, a wonderful friend. How we will miss him.



Dialogues: Author Meets Author

Editors' Note: In this issue of Trajectories, we introduce a new feature, Dialogues, in which we invite the authors of two recent books on closely related themes to interview each other. In this inaugural column, Monica Prasad and Isaac Martin, both authors of important new books exploring aspects of neoliberal politics, discuss their work and the development of the field of comparative and historical sociology.

Prasad Interviews Martin

Isaac Martin's The Permanent Tax Revolt examines one of the central political events of the last three decades, the California property tax revolt that (Martin argues) triggered the era of neoliberalism by transforming the Republican Party. Martin shows that the tax revolt was not at its origins a phenomenon of the right, but became such for contingent reasons. He also makes the argument that the American tax revolt is the equivalent of movements that argue for greater welfare states, because the tax revolt was at its core a demand for protection from the market. The book won the Social Science History Association's President's Book Award for 2007.

Prasad: You argue that the reasons for the tax revolt's turn to the right were largely contingent—the most right-wing policy happened to be “the first proposal that was good enough.” But one of the surprises of the recent literature on neoliberalism is the role that the left played in the 1970s at the origin of several policies that we now call neoliberal—this is something we see in Greta Krippner's work, for example, as well as in mine. Could there be a pattern to the contingencies?

Martin: The surprising finding in these studies is not that the right ultimately captured popular movements for tax relief and deregulation, but that the left seriously contested for leadership of these movements—even, for a time, led them.

The left could have won this particular battle over California's tax policy in June 1978. But this is one of the differences between our books—you emphasize structural forces pushing American politics rightward across several policy domains.

I think you are correct to suggest that the right was bound to win some battles in this period. There were many forces pushing American politics and public policy to the right after the 1960s—the economic polarization of the electorate and the racial realignment of the parties, for example. So without Proposition 13, I think we would still have had a neoliberal moment. I just think it would have looked more like Thatcherism than Reaganism—which is to say, less debt-ridden, less prone to magical thinking in fiscal matters, and perhaps ultimately less destructive.

Prasad: The word “culture” doesn't appear in your book. What is your take on the cultural turn, and what is your take on what we have learned from it, or not learned from it?

Martin: What we now describe as the cultural turn began when you and I were in grade school; it was already the subject of retrospective review essays around the time we arrived in college. By the time we came of intellectual age, many of the ideas we associate with the cultural turn were the unquestioned common sense of the intellectuals I most admired.

For me, one consequence of this generational experience is that I see cultural sociology as a rich storehouse of intellectual tools and explanatory strategies that I raid from time to time. But another consequence is that I have never really felt it necessary to invoke the word “culture” as a banner or slogan for what I do—because I never felt that cultural sociology was embattled, and I never felt the need to break with a non-cultural approach, whatever that is.

Two chapters of the book are devoted to tracing how a particular policy came to be interpreted, and how that interpretation itself became a social fact that had quite far-reaching consequences—a classic move in cultural sociology, one that has affinities with a famous thesis of W. I. Thomas and with recent work by Bill Sewell. The book also depicts policy-makers as culturally embedded actors who select their appeals from a limited repertoire of learned strategies—a picture very much in keep-

ing with Ann Swidler's conception of culture as a tool kit.

But other conceptions of culture turned out not to be so useful for my purposes. The book argues specifically against two other varieties of cultural explanation.

The first is the hypothesis that people need to share an ideology or a set of causal beliefs about the ultimate source of their grievances in order to act collectively. Many of us who work on social movements flirt with this assumption, perhaps because we are in the business of selling ideas ourselves, and we find it flattering to think that the right ideas might some day unite people into a movement. But that was not how it worked in the property tax revolt. The movement included people who had radically different ideas about why the property tax was illegitimate, but who nonetheless joined the same groups and protested the same targets and made the same policy demands.

The second is the "national values" explanation that you also criticize: the idea that a society bounded by national borders has a fixed set of values or preferences—for example, "Americans hate taxes"—and that appealing to those values is sufficient to explain any political outcome that appears to conform to them. This is a style of explanation that never seems to go away because it is so easy—one simply has to have a few national stereotypes ready to go.

Prasad: Taxation has not been a central issue of concern in sociology, and over the last few years the study of the welfare state has migrated into political science. How did you find your way to taxation?

Martin: I was tangentially involved with several progressive grassroots lobbying campaigns at the state and local levels in California, and I wanted to understand why it was so hard to get egalitarian policies passed. One of the reasons it turned out to

be so hard was the climate of fiscal austerity. That, then, became the intellectual problem—why can we not raise money for schools, for housing, for health care?

So I suppose my path illustrates one possible way people may arrive at studies of political economy: we may be led there by some level of sympathetic engagement with a world of political organizing outside of the academy. Maybe this is the silver lining for sociology in the current economic crisis. The scale of the crisis will make it hard to ignore the importance of institutions like financial markets and, say, unemployment insurance for all of the other things we study.

One lesson I learned from this project is that there is no shortcut from theoretical sociology to policy advice....If you are interested in doing public sociology, it is important to commit to a serious intellectual engagement with details of public policy.

Prasad: I know you started this project with political intentions. At the other end of the project, what is the status of those intentions? Have you come to agree with those who think that academic analysis should not be concerned with real world relevance? If not, what do we learn from your research about how to make a better world?

Martin: One lesson I learned from this project is that there is no shortcut from theoretical sociology to policy advice. The intellectuals who helped steer the property tax revolt in a progressive direction in the early 1970s—and the intellectuals who

helped capture it for the right in the late 1970s—were the ones who spent their time thinking about the technical details of obscure state tax policies. I guess the positive way to frame this point is that if you are interested in doing public sociology, it is important to commit to a serious intellectual engagement with details of public policy.

Prasad: So what are the open questions or the research leads that you might give a dissertation writer looking for a good project in political economy to take on? And what are you working on now?

Martin: Joseph Schumpeter advised sociologists to approach our fundamental question of social

solidarity by following the money. Under what conditions are people willing to share economic resources—on what terms, for what purposes, with whom? I think this is still excellent advice. I am trying to carve out a small piece of this puzzle by examining the conditions under which people with lots of resources refuse to share—specifically, episodes of political mobilization by the rich for policies that will reserve resources categorically for the rich. I call these episodes rich people’s movements—because they are in some ways the mirror image of the poor people’s movements described in Frances Fox Piven and Richard Cloward’s classic book. And, like poor people’s movements, they turn out to matter rather a lot for the history of inequality in the twentieth century U.S.

Martin Interviews Prasad

Monica Prasad’s The Politics of Free Markets argues that neoliberalism in the US and Britain in the 1970s and 1980s can be traced to policies that punished capital in the post-war period, as well as to particularly adversarial political institutions (as compared to France and Germany). The book won the Barrington Moore award from the Comparative Historical Sociology section of the ASA in 2007.

Martin: One of the refreshing things about your work is that you are clearly committed to a policy-relevant social science. Your recent work on the carbon tax, for example (forthcoming in Balleisen and Moss, *Toward a New Theory of Regulation*, Cambridge University Press), uses comparative historical sociology to draw lessons for environmental policy.

Yet your book argues that the ideas produced by academic social scientists had virtually no independent impact on policy, at least in the episodes you study. Given your skepticism toward the hypothesis that academic ideas shape public policy, how do you understand the place of your own policy-oriented scholarship?

Prasad: It’s interesting that you bring up the carbon tax paper. I wrote it for a conference at which several policymakers were present, and one of them made it a point to find me and tell me how interesting he thought the paper was. I thought, great, maybe my work will get out into the policy

world. But it turned out he wanted to mine my paper for ammunition against the argument the paper was making. In the paper I give various reasons why carbon taxes might not work, and then I try to show how to overcome those obstacles, and the paper comes out in favor of carbon tax. But this guy was opposed to carbon tax, and he had zeroed in on the reasons why the tax might not work and ignored the rest of the paper. He was going to use those reasons to try to defeat carbon tax!

Given my study of how economists’ ideas were abused by neoliberals, I should have anticipated that. But this instrumental use of ideas isn’t an inevitable feature of politics: it wasn’t the case before Congressional committees were defanged in the 1960s—turning national politics into a survival of the most populist—and it isn’t the case in other countries. For example, I argue in the book that part of the reason why French neoliberalism never became extreme is that it was constrained by economists. (That’s one of the ironies of the book, by the way: where economists were powerful, they used that power to constrain neoliberalism.)

And I can’t help thinking that the incoming administration in the U.S. is giving signals of being more open to learning from the insights of researchers than some previous administrations have been, so perhaps the populist period of the last few decades is at an end.

Martin: Your book seems to me to hearken back to an earlier wave of comparative historical sociology—the so-called second wave that posed Marxian questions about inequality and class conflict, and offered Tocquevillian answers about state structures. But your book is also clearly informed by the more culturalist third wave. This is clearest in your chapter on France, in which you employ some of the theoretical apparatus of cultural sociology—such as Bill Sewell’s conception of culture as a set of contested and mutable symbols—precisely to argue against the idea that we can explain public policy choices by appealing to anything like a unified “national culture.” So—a very open-ended question here—how do you see your work in relation to the cultural turn?

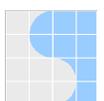
Prasad: The cultural turn is many things. As you say, my book is very much in the tradition of post-structuralist understandings of culture as contested and mutable—Sewell’s quote is “contradictory, loosely integrated, contested, mutable, and highly permeable”—and I take that argument to its logical conclusion, namely that if culture is contested and mutable and permeable etc. then it’s hard to see why culture should be particularly constraining. Cultural traditions can lead to and justify all sorts of different actions, precisely because cultural traditions are so contradictory and mutable.

But of course there is a newer strand of argument that sees culture as stable, because embodied, and therefore much more of a constraint on behavior. All I can say about this is that at least in the world of policymaking, there are so many cross-pressures that there doesn’t seem to be a clear translation of culture onto policymaking or political behavior. For example, I have a paper with some colleagues coming out soon that shows how a cultural commitment to egalitarianism among some voters translated into voting for Republicans. So my work is more at odds with that part of the cultural turn.

Martin: Your book shows that you read widely in economics, political science, and history (in several languages!). But I also know that you find something of particular value in the sociological tradition. So I would like to ask a question about your book as a way to ask a more general question about your views on what comparative historical sociology can contribute to the study of political economy. What if anything about your book makes it comparative historical *sociology*—and why does that matter?

Prasad: When I was starting my dissertation, I took my prospectus to a member of a different department for advice, a non-sociologist who was an expert on some of the issues. When he saw that I was planning to study three different policy domains in each of three different countries (the dissertation was only on the US, Britain, and France), he nearly had a heart attack. He tried very hard to convince me to narrow it down. For comparison, here is what my sociology department committee members said about the same prospectus, and I quote: “Why don’t you add Germany as well?”

Sociology has kept open this little corner of academia where it’s possible to do big-picture projects, to stitch together the work of the experts to try to come to some larger conclusions. The kind of work that Marx did, or Weber—those great attempts to try to figure out how to collectively rule ourselves, how to avoid driving the whole system in the ditch, how to make things better—and that Barrington Moore resurrected, doesn’t seem to me possible in any other discipline. And I think it’s extraordinarily important that we have at least a few people doing this kind of work. When I read the tax scholarship for my book, I was surprised to see the tax scholars concluding that American tax policies were actually more punitive to capital than the European countries’ tax policies. They would always say “surprisingly, tax policy does not follow the pattern of American *laissez-faire* that we know is true in other policy domains.” And then when I read the specialized literature on regulation, the scholars there were also concluding that American regulation is more punitive to business than European regulation. They didn’t know the tax scholarship, so they would say “surprisingly, regulatory policy does not follow the pattern of American *laissez-faire* that we know is true in other policy domains.” The same thing is in other policy domains. But very few scholars look across policy domains, because our system of producing specialized scholarship doesn’t set you up to do so, so no one discusses the overarching pattern. In history there is the practice of writing a few synthetic essays late in your career, but that work often seems to me disappointing because the scholars attempting it are not very practiced at doing it, and it isn’t easy to do good big picture scholarship. Comparative historical sociology is an exception to all this, and for that reason it seems to me worth defending.



Book Review: *Adam Smith in Beijing*

Giovanni Arrighi, *Adam Smith in Beijing: Lineages of the Twenty-First Century* (London and New York: Verso, 2007).

Reviewed by Janet Abu-Lughod
New School of Social Research

This is a must read, a deeply thought out argument by a master of historical narrative and economic theory tackling the great contemporary issue: the prospect that the world-system is being radically reorganized as hegemony has begun to shift eastward in the 21st century. It is also a demandingly complex book to absorb. Sometimes one feels as though one were viewing the transparent operation of a formidable brain as it processes too much data drawn from a wide range of sources (happily cited in footnotes at the bottom of each page). But most of all it is a provocative book. Indeed, Arrighi throws down a gauntlet in the midst of a hornet's nest of war-mongering neo-liberalists,¹ and in return has been viciously attacked by threatened spokesmen on the right as being merely "ideological" (i.e., as communist, new left, Chinese apologist).² To some extent, the hype with which the book has been advertised, "How China Will Rule the World," has both frightened and called forth a

dismissive oversimplification of his carefully documented but also carefully hedged conclusions.

Arrighi seems to recognize that his ambitious agenda has too many goals, leaving readers to pick and choose. He confesses in his preface (p. xi). "Friends, students, and colleagues who have read and commented on the manuscript...have given unusually discrepant assessments of its components. Chapters that some readers enjoyed most, others enjoyed least.... [S]elections that some readers thought central to the argument..., others found superfluous." I confess that I am among these "discrepant" readers. I skimmed the in-house theoretical dialogues with Marx, Adam Smith, Robert Brenner, Schumpeter et al. that introduce the problematic. The parts I found most highly developed were the middle chapters chronicling the causes and consequences of the "decline" of American hegemony from its post-World War II "altruistic" peak to its blatant and unsuccessful militaristic attempts to "dominate without hegemony." Intriguing but less convincing, probably because the author is still exploring newer ground, were the final chapters on recent Chinese developments. These left me hungry for more details, albeit intrigued by Arrighi's hypotheses concerning why China has been so successful in recent decades. Knowing how each of Arrighi's successive books sets the stage for its sequel, however, I am willing to wait.

It is impossible to summarize the entire argument, and I shall instead try to lay out the structure of his argument, unjustly simplifying it. The major thesis is not in doubt: the balance of economic power between the "west" (i.e. the U.S.) and the "east" (notably China) has been radically shifting in recent decades as the world system has undergone reorganization. However, In contrast to much of the superficial causal analysis attributing this to "abstract globalization," Arrighi carefully distinguishes between policies adopted independently by each region and the interactions between them.

In Part I (chapters 1-3, about 100 pp.) Arrighi searches for a useful set of theoretical propositions to explain the anomaly, contra Marx, of how a communist nation became "capitalist." China's

¹The substitution of the Chinese threat for the Cold War was already on play before the "fall" of the Soviet Union. "The Project for the New American Century" (PNAC) was founded in Washington, D.C. in 1997 as a neo-conservative think tank, housed in the same building as the American Enterprise Institute with overlapping membership; its statement principles, signed inter alia by Rumsfeld, Cheney, Wolfowitz, called for unilateral military intervention to ensure America's status as the sole global superpower. By 2000, PNAC had issued a stronger position paper explicitly identifying China as the major threat to American world dominance. The parallels with the official "National Security Strategy of the United States" issued by Wolfowitz's division of the Pentagon are obvious.

² See, for example, Gregory Clark's "China as the Antidote to Oppression and Exploitation? So envisions a new book, never mind the facts," in the *Chronicle of Higher Education Review* (March 14, 2008); or George Walden's dismissive review (posted on www.bloomberg.com on April 4, 2008) that begins "Purveyors of leftist orthodoxies wear heavy boots, and you can hear them approaching...Though Marxists have struggled to explain the eclipse of communism, religions never die.... Arrighi pins his hope on the rise of Asia. His book amounts to a prayer that China will displace the global dominance of the U.S. economy and way of life."

rapid growth is being created by mechanisms of state initiation and oversight that diverge from the history of capitalism described and critiqued by Marx. Hence, his happy “rediscovery” of a distinction made a century earlier by Adam Smith between “unnatural” (European) development based upon mini-states in ruthless competition for imperial expansion (and often at war with one another), and the more “natural” (i.e. Chinese) development based upon agriculture, a large land mass, a large internal market, and guided by rulers of a hegemonic state powerful enough to offer benefits to its neighbors in return for respect. Arrighi contends that far from defending *laissez faire*, Smith favored an active role for statesman-like controls to curb rapid and brutal capitalist exploitation. Arrighi even quotes Hobbes³ when he compliments China’s generosity regarding its tribute trade which, in earlier centuries, enabled “the Middle Kingdom to ‘buy’ the allegiance of vassal states, and at the same time to control flows of people and commodities across its far flung frontiers.” (p. 273). Arrighi thus uses Adam Smith to defend his assertion that there are at least two alternate roads to hegemony and development, and to at least hope that China’s “natural” path may lead eventually to a more felicitous outcome for the [Third] world than the “shock therapy” of western attempts to substitute military force for respect.⁴

The bulk of the book then takes an important and lengthy detour (of some 170 pages) away from China, whose recent gains (becoming the workshop of the Walmart and the creditor to American unregulated financial institutions and the ballooning national debt) are systemically entangled with western decline. Part II (Chapters 4-6) and Part III (Chapters 7-9) are devoted to reviewing the cycle of the consolidation, geographic transfer and decline of western hegemony during the “Long Twentieth Century.”

After pointing to the parallels between the rise, and subsequent collapse of the golden Edwardian

³ “[T]o have servants, is Power; To have friends, is Power, for they are strengths united... Also Riches joined with liberality is Power, because it procureth friends, and servants.”

⁴ To my mind, this is the most convoluted and contentious section of the book, as Arrighi engages in dialectical debates with Marx and his friends and opponents, chiefly on the left, when his own historical knowledge and world system perspective could have assured him of multiple possibilities.

Age at the turn of the twentieth century, and its resuscitation in post-World War II America, Arrighi diagnoses the sequential crises of economic and political turbulence that eventually led to the ultimate collapse of American hegemony after the turn of the twenty-first. Arrighi carefully distinguishes between a prolonged “signal crisis”⁵ in American hegemony that was already evident by the closing decades of the twentieth century (covered in Part II), and the “terminal crisis” of American hegemony,⁶ begun when the U.S., unable to use the United Nations as its imperial agent for world government, decisively forfeited hegemony in favor of brute military force (domination).

The kernel of causation is captured in the title of Chapter 7, “Dominance without Hegemony,” which introduces Part III, chronicling the failure of the United States to translate its unparalleled and promising advantages into a synergistic and equitable world order. Here I must quote: “Just as the United States emerged as the real winner of the Second World War after the USSR had broken the back of the Wehrmacht in 1942-43, so now all the evidence points to China as the real winner of the War on Terror...” (p. 261). How the U.S. frittered away its proud heritage of generosity and moral standing is traced in chapters 8 and 9. The major thesis asserts that the balance of power between the “West” (i.e. now more specifically the U.S.) and the “East” (notably China, but also India and the Pacific Rim) is being reversed as American financial and industrial dominance has declined and China has experienced rapid growth. The turning point came “in the wake of September 11 [which unleashed forces that] have precipitated the terminal crisis of US hegemony and consolidated China’s leadership of the East Asian economic renaissance” (p. 151). Part III carries the story up to “tomorrow,” offering prescient insights into American economic straits (the collapse of credit and housing) that would be more fully revealed after the book went to press.

These six chapters best display Arrighi’s masterly ability to synthesize history, politics and technical economics to yield fresh and dramatic insights into “how the world works.” They are my favorite

⁵ Defined as a potentially “resolvable” challenge.

⁶ Defined as a doomed attempt to dominate without true hegemony.

chapters in the book, in which Arrighi's sophisticated technical knowledge of economics combines with his insights into (and criticisms of) wrong-headed political decisions, spell out the mechanisms that have changed the balance of global dominance and turned the U.S. into a debtor nation.

The final three chapters comprising Part IV (titled "Lineages of the New Asian Age") and an Epilogue return us to the promised book on China. These are the most original and controversial of the book. Occupying little more than 100 pages of text, they compress the most important issues that I, for one, had hoped this book would have addressed. Chapter 10 covers too much ground: it lays out the fear-driven inchoate policies the U.S. might be tempted to follow to meet the Chinese challenge, but finds, in Chapter 11, some reassurance (in a quick summary of 500 years of Chinese history in the Asian system!) that China is unlikely to recapitulate the militaristic methods of its European/American forebears in relation to its region and beyond. These chapters cry out for fuller and deeper treatment, as does Chapter 12, whose 27 pages race through the "origins and dynamics of the Chinese ascent" and its advantages and contradictions. Certainly, we remain at the mercy of our sources and deserve better than ex cathedra pronouncements.⁷

To help me evaluate these chapters I asked for reactions from a mature former student of mine, Dr. Haishan Liu, who is intimately knowledgeable about post-Deng Chinese policy decisions. His dissertation, dealing with the first Special Economic Zones, emphasized the extreme caution with which Deng first introduced capitalism and foreign investments from the Chinese diaspora in experimental ways and into geographically peripheral and cordoned areas.⁸ Only later, and in

light of what was learned from them, were the Export Processing Zones that Arrighi describes allowed to multiply. Similarly, an experiment is currently being undertaken in political administrative reform in Shanghai in the hope that it will begin to match the more decentralized industrial ventures by greater decentralized democratization. Both of these illustrate state power as advocated by Adam Smith. But Dr. Liu also criticized Arrighi's views for being those of a foreigner (too "external" to the culture) and based upon limited sources written by dissidents whose work has been banned by the government.

In short, these chapters and the epilogue remain temptingly provocative but filled with hypothetical conjectures and unproven reasoning. For example, further studies might support Arrighi's claim that one of the compensatory gains from Mao's painful cultural revolution, by favoring peasants and disciplining labor (pp. 376-378), actually allowed Deng's reforms to inherit a more independent educated and disciplined labor force that could substitute for heavy capital investment. More research and future developments might also support his hypothesis that this "independence of spirit" will be responsible for increasing labor strife/strikes that could, in turn, result in greater democracy. Whether Chinese competition for scarce resources (from Africa for instance) can be won by Hobbes' "benign" method, and whether conflict over Middle Eastern/Central Asian petroleum resources can be ultimately avoided remains unpredictable, as does whether global warming and the proliferation of atmospheric and land pollution by both the declining and rising world powers can be stopped in time to save the planet!

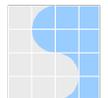
These questions all lie beyond my competence and probably beyond our current knowledge. We can only hope they will be the core topic of Book 3 in Arrighi's ambitious trilogy-to-be.⁹

⁷ Some of the allegations (probably unprovable) that caught my eye was Arrighi's view that Mao's cultural revolution strengthened appreciation for the ingenuity of peasants that counteracted the preferences for exclusive urban development in "new" China, or the emphasis of revolutionary China on expanding minimal literacy led to the self-confidence with which the industrial labor force has organized protests. These broad hypotheses may or may not be true, but it will require much more than opinion (or wish) before we can evaluate them.

⁸ Haishan Liu, "China's Special Economic Zones from Shenzhen to Shanghai. A new Path to Industrialization,

Globalization and Modernization (Ph.D. Dissertation, New School, 2005).

⁹ Taking *The Long Twentieth Century and Adam Smith in Beijing* as volumes 1 and 2. Am I being too greedy?



Identities

Editors' Note: In this section, comparative-historical scholars reflect on why they entered the subfield. We invite contributions to this section for future issues of the newsletter.

Dietrich Rueschemeyer

Brown University

Born in 1930 in Berlin, I grew up in a family that combined professional work with strong political interests. My mother became part of the tiny first generation of women physicians after initially studying social science. My father was a physician as well. Their political interests had historical roots. The families of the previous generation—one farming, the other entrepreneurial—had dissented from Bismarck's unification policies because of regional and religious loyalties. Between the World Wars, my parents took a left position in the Catholic Center Party. My father was then a deputy in one of the borough parliaments of Berlin. During the Nazi time, they were known to friends, patients, and party monitors as critics of the regime. After the Second World War, my father was inducted into politics by the British military government and appointed to one of the first regional parliaments. He was a regional co-founder of the Christian Democratic Union, but throughout his subsequent political work he maintained close relations with other members of the new political elite who were not compromised by the previous twelve years, whether they were conservatives, socialists, or communists.

My interest in sociology and comparative social analysis is rooted in experiencing the Nazi regime as an adolescent and then participating in the democratic reconstruction of Germany. In the 1940s, I often accompanied my father in political travel, and I followed with intense interest the discussions, at home and in journals of opinion, on Germany's disastrous history and its future. When I graduated from high school and left home, I considered political and social adult education as my professional goal. That made economics, social

policy, and sociology central to my studies at the universities of Munich and Cologne. Until my doctoral work, sociology was a supplement—fascinating but extremely complex as a field, yet at the same time promising of a deeper understanding of Germany's past and prospects. Throughout the 1950s, I regularly taught political economy and sociology in extra-mural extension courses and in workers' educational programs. Comparative social and political essays of Lipset and Parsons—together with classic works such as those of Robert Michels and Karl Mannheim—were important guides in this work.

At the same time, my interest in sociology as a field grew. And it grew at the expense of economics. Economic theory seemed too unconcerned with confronting its central hypotheses with empirical evidence. The philosophy of science of the Vienna Circle and of Karl Popper harmonized better with the practices of sociology than with those of economics. These ideas were introduced by

Hans Albert, who was then a critical voice at Cologne University's advanced seminars in economic theory and later became a major figure in discussions with Theodor W. Adorno on the methodology of the social sciences.

When my written comprehensives surprised my later doctoral advisor, René Koenig, with informed discussions of random sampling as well as the outlook

of the young Marx, he offered me a part-time research job and suggested a dissertation about the use of certain forms of registration for random sampling. I accepted the job, but opted for a dissertation on the sociology of knowledge. This decision drew me closer to the theoretical core of sociology. Here Homans, Merton, and Parsons became the main guides. The dissertation turned into a critical assessment of the work of Karl Mannheim and Max Scheler, an attempt to place the study of knowledge at the center of sociological analysis, and a meta-analysis of research results on small groups relevant for the understanding of knowledge processes.

Comparative sociology, earlier mostly linked to my concerns with Germany's history and recon-

Until my doctoral work, sociology was a supplement—fascinating but extremely complex as a field, yet at the same time promising a deeper understanding of Germany's past and prospects.

struction, became a natural preoccupation when I married Marilyn Schattner, who had grown up in New York, and we decided to live and work in the United States. A study of the legal profession I had begun in Germany turned into a comparison of lawyers in the US and Germany. In a broader sense, *Lawyers and Their Society* (1973) became a vehicle for finding my way in a new home base. The book argued that the relative timing of capitalist market extension and the rationalization of state structures and operations in the two countries explained the long-term differences and a limited convergence between the two legal professions and their countries. Later, Marilyn and I treasured the many semesters we spent at the Universities of Oxford, Toronto, Berlin, Rostock, Uppsala, and Bergen and—of special emotional and intellectual significance—at the Hebrew University of Jerusalem. We learned about the distinctive perspectives of colleagues in very different circumstances and acquired both knowledge and an intuitive feel about the social and political life in countries we cared about.

Marilyn ventured into comparative research about professional work, marriage, and personal life in the different political systems of East and West. This work later led her into exploring the place of women in the politics of East European countries after the collapse of Communism and to study the relations of art and artists to the state in the Soviet Union, Scandinavia, and East Germany before and after German unification. Our work differed in style and content—hers more concerned to recover the experience of real people, mine more oriented to theoretical generalization—but we have never ceased talking to each other, encouraging, commenting critically, and underlining the importance of shared concerns.

Beginning with my first year of working in the United States, problems of social and economic development have always been part of my teaching. Confronted with vast new problems, I turned to the ideas of the classics about how Europe was transformed by the joint rise of capitalism and the modern state in order to gain some purchase on the prospects of decolonized “new states”. If this was the tack of modernization theory, I sought to do justice to historical complexities by advancing and detailing such conceptions as “partial modernization.” (At some later point, a Sudanese doc-

toral candidate, Majoub Mahmoud, and I agreed that this concept reflected similar autobiographical experiences of both of us.)

I found marvelously congenial colleagues at Brown. Peter Evans’ qualified conception of dependency theory, condensed in the title of his first book *Dependent Development*, clashed with my attempts to differentiate modernization theory historically; but there was enough commonality to inspire long and fruitful discussions. One common concern—to explore states and state-society relations in comparative and historical perspectives—we shared with Theda Skocpol, who had just publish *States and Social Revolutions*. The Social Science Research Council supported us in creating a working group on States and Social Structures, which led to the publication of *Bringing the State Back In* (1985) as well as other edited volumes. In *Power and the Division of Labor* (1986) I reflected on broader but closely related issues of macro- and meso-sociology.

John Stephens came to Brown with the results of his first comparative work on rich countries’ welfare states in hand. His findings gave strong support to the power resources view of social policy. Our shared interests, both specific and broadly theoretic ones, made for regular and most enjoyable discussions. While we both taught for years parallel courses on democracy (John on breakdowns of democracy in Europe, I on the chances of democracy in the Third World), he researched with his wife Evelyne Huber attempts at social democratic policies in Jamaica. Our triple cooperation on *Capitalist Development and Democracy* (1992) could build therefore on long standing interests that fortuitously came together -- on states, on social welfare policies in rich and poor countries, on the conditions of democratic and authoritarian regimes, and on the underlying sources and balances of power.

In more recent years, Brown’s Watson Institute for International Studies supported comparative working groups on economic globalization and welfare states and on “effective and defective states” in a historical perspective. At the same time, I had intense and satisfying discussions with Jim Mahoney—on the methodology of comparative historical work, on analytic tools specially suited to assess time- and sequence-bound causation, and

on the evaluation of cumulative results of comparative historical research in particular fields. This not only made us friends but found intellectual expression in *Comparative Historical Analysis in the Social Sciences* (2003), which we edited together.

I have had the good fortune to find intellectual stimulation, real advances of understanding as well as friendship and love in the pursuit of comparative work on matters which we—close colleagues, my wife, and I—consider important. If I turn in the forthcoming *Usable Theory: Analytic Tools for Social and Political Research* to broad issues of social and political theory, this seemingly solitary undertaking relies in all its pages on what I learned from the company I have kept in years past.

Edwin Amenta

University of California-Irvine

How I became a comparative and historical sociologist, or, really, a historical sociologist with comparative tendencies, is mainly a story about how I became a sociologist. Looking back, I made crucial choices for whimsical reasons.

My parents were from lower middle class urban immigrant backgrounds and grew up during the Great Depression. My father was steered into dental school by his father, who came to Chicago from Sicily as a boy, worked as a dental technician, and eventually owned a lab. My father married a young woman whose grandparents were from County Cork and the Rhineland and who was soon to become an ex-nurse. They caught the wave of the post-World War II economic resurgence, my father's practice buoyed by the fact that Chicago's drinking water was only recently fluoridated. Being well off beyond their imaginings and Catholic before the Second Vatican Council, my parents had six children in rapid succession, of which I was the third. Before I was a teenager my parents had abandoned the bungalow belt for the suburbs—and Catholicism for public schools and church-free Sundays.

I accompanied fellow upper middle class white kids to a high school that offered a course in sociology. It would simplify my task here if taking it hurtled me down a path, but it did not, and its ex-

istence mainly indicates the public school system's capacious resources. If I stood out in any way it was for being physically underdeveloped and, probably not unrelated, a math whiz/nerd. Although I tested well and was at the top of my class and the postman kept delivering National Merit-induced brochures, my parents possessed the meager sort of cultural capital that discounted dreams of the Ivy League. I was not certain I even wanted to go to college, being run down by the AP math-science track on which I was set. Also, I felt I was on the cusp of achieving popularity and was reluctant to leave town.

The late 1970s were years of anything goes and even for the era I felt less pressure than most young people to do or be anything in particular. Although I was my family's best bet for pre-med/pre-dent I was not pushed in that direction because my father felt as though he had been railroaded into dentistry, and had regrets. At the beginning of the decade he had sought to transcend his station by launching an advocacy organization, the American Society for Preventive Dentistry, as well as the allied March publishing company, an amalgam of my parents' first names, which had devoured our family's savings. Still, I think he was hoping that I would come around to his profession on my own, because once I had enrolled in a graduate program in sociology he would address and introduce me, dismissively, as a "professional student," accent on the student. For my part I thought I was engaged in a vocation that was noble, if poorly remunerative. But I am getting ahead of myself.

I attended Indiana University largely because my eldest brother was studying music there, it accepted late applications, and it was cheap even for out-of-state students. I did well in my classes, but as best I can remember my freshman year I was concerned chiefly with getting stoned. I took some math classes, but soured on the subject—thanks to a mathematician teaching Calculus III as topology—and I feared taking the chemistry class I had tested my way into. I tentatively decided to go pre-law, for which there were no prerequisites. I enjoyed a sociology class that assigned Becker's article on becoming a marijuana smoker and various works by Goffman, whose lessons I sought to apply to my life as trying to define situations as ones in which I was really cool and after which

coeds might plausibly follow me back to my dorm room. Two results of this experience were that I had gained credits toward a major and that my sociological perspective turned sharply macro.

As an upper classman, burned out on being a burn-out, I became serious. One key impetus, due mainly to the fact that my friends were doing likewise, was to move out of the dormitory, which, once outside, I realized was less academic incubator than drug den. I also talked my way onto the prestigious student newspaper—to set the time, I reviewed both Richard Nixon's memoirs and the locally shot "Breaking Away"—and by senior year was considering journalism as a career. Law now seemed just boring and in the groovy lingo of the day, not "relevant." I was also taken under the wing of some younger members of the sociology faculty, notably Larry Griffin and Charles Ragin, who enrolled me in their graduate seminars. When starkly juxtaposed, sociology seemed so much deeper a mode of analysis than journalism. Also, I could imagine myself becoming someone like my youthful and brilliant sociologist mentors—Ragin was 23 and looked 18—rather than the Indiana Daily Student advisor, a washed-up alcoholic ex-journalist. Ragin strongly suggested that I apply elsewhere, and I gained admission in several fine departments, choosing Chicago largely because my journalist girlfriend took a job downtown with the Crain's chain.

Like many fellowship students I immediately found myself adrift, with little connection to the sociology department, and like a lot of new sociology students I styled myself as being "political." At the time Ronald Reagan was seeking to dismantle the welfare state and in my studies I groped toward making sense of that. At Chicago study across disciplines was encouraged and I did well in my potpourri of self-selected classes, though was intimidated in Adam Przeworski's seminar, which had a groupie- and disciple-like following, by an extremely knowledgeable and voluble older classmate already with an advanced research agenda. I thought that if students like this were typical I was never going to make it in academics, but I was relieved to learn that he was a Northwestern assistant professor named Alex Hicks.

After exhausting my fellowship I was forced to seek employment and benefited from the fact that there was much funded research at Chicago and no opportunities to teach, undergrads there being quarantined from the pedagogical ministrations of grad students. I interviewed with Ed Laumann, who was engaged in a fascinating and massive study of DC interest groups, and Theda Skocpol, who was investigating the making of the U.S. welfare state. She was offering seminars on historical sociology and the New Deal, where the programs Reagan was seeking to dismantle had been constructed. Possibly influenced by Laumann's chalkboard sporting a massive two-by-two with Parsons's AGIL, I chose Skocpol.

In the mid-1980s Chicago was extremely hospitable to historical sociology. A ramshackle A-frame on campus had been converted into a research home for students from sociology and political science, including Margaret Weir, John Ikenberry, Elisabeth Clemens, and Bruce Carruthers, with weekly workshops led by Skocpol and regularly attended by faculty such as John Padgett, David Laitin, and Ira Katznelson. Under the circumstances being a historical sociologist working at the border of sociology and political science seemed not unnatural. Nor did it seem like career suicide.

After collaborating on articles with Skocpol, who as a mentor was as supportive as she was demanding, I wrote a dissertation on the development of U.S. social policy in the New Deal in comparative perspective. Addressing why the United States created the late and odd welfare state that it did in comparison with similar countries required historical inquiry; related questions could be addressed with historically sensitive data sets, using standard multivariate techniques or Boolean analyses of the sort Ragin was developing. I began my research thinking that New Deal social policy battles were going to be about social security and found that they were mainly about work programs unprecedented in size and importance.

Since then, I benefited from the political and historical turns in sociology and several unrelated developments. Always pushing back in time, Skocpol ended up writing a book about the U.S. social policy in the Gilded Age and Progressive Era, leaving the New Deal, in a sense, available.

In the meantime, as sociologists jumped into historical inquiry, with increasingly sophisticated path-dependent and configurational approaches, other disciplines abandoned the field, historians slouching toward the humanities and political scientists careering at economics. My recent book on the Townsend Plan and Social Security was motivated in part by the impression gained during my previous research that scholars had got that particular story way wrong, also that social movement scholars did not understand how extreme political pressure could be exerted without physical threat. And no one else had written about it.

All this seemingly has worked out for me. Who knew that sociology would flourish while fluoride would turn dentistry into cosmetics? And once I secured regular employment—and my brother quit voice in favor of otolaryngology—even my father came around. Still, I do not think that I will press my own four-year-old twins also into becoming professional students. But because the expectations of parents are very much different nowadays I expect they will get far more direction than I did.

Recent Dissertations

Fumiko Fukase Indergaard
Columbia University
2007

Communities of Discourse and Networks of Action: Protestantism and the Making of Democratic Movement Leaders in Modern Japan, 1868-1930

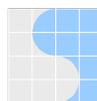
My dissertation examines why and how: 1) Protestantism—an alien religion - was able to take root in the hostile environment of post-feudal Japan; 2) Protestants became involved in democratic reforms, activism and movements; and 3) disproportionately became activist leaders. It shows that the making of religio-cultural policies involved more than elite manipulation. Contingencies of geopolitical competition and state formation also came into play: state elites had to balance international affairs with domestic social control agendas. Protestant development was, in large part, an unintended consequence of state politics.

The essentialized understanding of Protestantism often claims that it is innately radical and supports democratic activism. However, I compare three early Protestant bands that produced most leaders, showing that member actions varied from contentiousness to conformity. The different, but mostly conformist tendencies helped Protestantism avoid targeting for state repression, abetting its survival.

Protestantism's role in recruitment and mobilization was more complex than cultural determinists suggest. Although discourses of the bands diverged, they shared a broad Christian faith/identity and a similar form of nationalism. This shared culture aided the formation of a multi-organizational field across band lines. I use two movement cases (freedom of religion and anti-public prostitution movements) to show that some Protestants participated not only because of ideals but also because they could access networks emerging in the Protestant field to support specific democratic causes.

Finally, I show that: 1) the ex-samurai status of most early Protestant leaders; and 2) bridge mechanisms linking young elites in the state's elite educational system to Protestant leaders and activists, contributed to the influential roles of Protestants in democratic activism.

My study affirms synthetic trends in the social movement literature treating both structure and



culture as causal mechanisms. It contributes to this literature by explaining: 1) how *informal* and *emergent* networks based on religion aided mobilization; 2) how non-instrumental aspects of religion facilitated movement emergence; and 3) how overlapping of the movement, Protestant and state higher education fields abetted leader production.

Michael A. Elliott
Emory University
2008

**A Cult of the Individual for a Global Society:
 The Development and Worldwide Expansion of
 Human Rights Ideology**

One of the most dramatic developments of the 20th century has been an increasing concern for human rights as a global problem and the proliferation of both governmental and non-governmental efforts to articulate and protect such rights. While much has been written on the subject, few have studied the actual development and elaboration of human rights as a sociological phenomenon. Indeed, much of the literature relies on certain explanatory mechanisms (e.g., hegemonic coercion, interest maximization, or social movement activity) that do not adequately explain the ongoing and tremendously expansive nature of this phenomenon. Based on a comprehensive coding of 779 human rights instruments, I present an unprecedented portrayal of the global institutionalization of human rights since the mid-19th century. Using this quantitative data as well as historical research on the medieval foundations of this ideology, I critically evaluate prevailing theories of human rights expansion, highlighting both their strengths and weaknesses as explanatory frameworks. In the process, I develop an alternative account that stresses the rationalized construction of human rights as an element of the long-term rise of the universal, egalitarian individual as the primary entity of social organization, value, and meaning in world society. Overall, this perspective proves to be more fruitful in making sense of the data and provides numerous avenues for future research in this area.

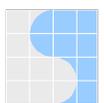
Dan Lainer-Vos
Columbia University
2008

**Nationalism in Action: The Construction of
 Irish and Zionist Transatlantic National Networks**

This dissertation treats nations as an organizational accomplishment. It examines encounters between Irish Americans and Jewish Americans and their respective homelands to understand how national movements establish cooperation between the groups that constitute the nation. Part One examines Irish and Israeli attempts to sell national bonds in the U.S. in 1920 and 1951 respectively. Sold as a mixture of a gift and an investment, the Irish bonds generated conflicts between Irish and Irish Americans. In contrast, the Israeli bonds helped establishing cooperation. they functioned as a boundary object allowing American Jews to gift Israel while Israelis treated it as an investment.

Part Two examines the construction of national attachments in a Jewish American summer camp, and an Irish American Gaelic Athletic Association. Attempting to endow diasporic subjects with a sense of belonging, national entrepreneurs constructed these sites as liminal places. The Jewish camp functioned as a simulation of Zionism. This simulation allowed campers to believe that others, in Israel, experience wholesome national belonging. In the Irish case, the regulation of matches generated a sense of friendly rivalry among teams representing different counties thereby fostering a sense of Irishness.

Studying nationalism as an organizational accomplishment clarifies that diversity within the nation is not necessarily an obstacle to nation building. The crux of nation building is the orchestration of difference. Nation building processes center on the establishment of cooperation without consensus. The concept of simulation shows how subjects to make sense of their difference from within the nation.



New Publications of Section Members

- Dill, Brian and Ronald Aminzade. 2007. "Historians and the Study of Protest," in *Social Movements Across the Disciplines*, edited by Conny Roggeband and Bert Klandermans, pp. 267-311. Springer Publishers.
- Drahokoupil, Jan. 2008. *Globalization and the State in Central and Eastern Europe: The Politics of Foreign Direct Investment*. London: Routledge.
- Ermakoff, Ivan. 2008. *Ruling Oneself Out. A Theory of Collective Abdications*. Duke University Press.
- Fitzgerald, David. 2008. *A Nation of Emigrants: How Mexico Manages Its Migration*. Berkeley: University of California Press.
- Fukase-Indergaard, Fumiko and Michael Indergaard. 2008. "Religious Nationalism and the Making of the Modern Japanese State." *Theory and Society* 37(4): 343-374.
- Goldstone, Jack A. 2008. *Why Europe? The Rise of the West in World History 1500-1850*. McGraw-Hill.
- Heller, Jacob. 2008. *The Vaccine Narrative*. Vanderbilt University Press.
- Ho-fung Hung. 2008. "Agricultural Revolution and Elite Reproduction in Qing China: The Transition to Capitalism Debate Revisited." *American Sociological Review* 73(4): 569-88.
- Johnson, Victoria. 2008. *Backstage at the Revolution: How the Royal Paris Opera Survived the End of the Old Regime*. Chicago: University of Chicago Press
- Larson, Erik and Ronald Aminzade. 2008. "Nation-States Confront the Global: Discourses of Indigenous Rights in Fiji and Tanzania." *Sociological Quarterly* 48(4): 801-831.
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- Larson, Erik and Ronald Aminzade. 2008. "Dilemmas of Nation Building in Tanzania and Fiji." *International Social Science Journal*, special issue on Dilemmas of Nation-Building.
- Loveman, Mara. 2007. "The U.S. Census and the Contested Rules of Racial Classification in Early Twentieth-Century Puerto Rico" *Caribbean Studies* 35(2): 79-114.
- Mallard, Grégoire, Catherine Paradeise and Ashveen Peerbaye, eds. 2008. *Global Science and National Sovereignty: Studies in Historical Sociology of Science*. New York: Routledge.
- Mallard, Grégoire. 2008. "Can the Euratom Treaty Inspire the Middle East? The Promises of Nuclear Regional Authorities." *The Nonproliferation Review* 15(3): 459-477.
- Mielants, Eric. 2008. "Le développement historique de la xénophobie et de l'islamophobie dans le contexte sociopolitique belge" in Mohamed Mestiri et. al. *Islamophobie dans le monde moderne*. Paris: International Institute of Islamic Thought.
- Mielants, Eric and Fouad Kalouche. 2008. "1968 and After: Transformations of the World-System and Antisystemic Movements" in *Making Waves: Worldwide Social Movements, 1760-2005*, edited by William G. Martin. Boulder, CO: Paradigm Press.
- Patil, Vrushali. 2008. *Negotiating Decolonization in the United Nations: The Politics of Space, Identity, and International Community*. New York: Routledge.
- Raffin, Anne. 2008. "Postcolonial Vietnam: hybrid modernity." *Postcolonial Studies* 11(3), Special issue on Southeast Asia, 329-344.
- Raffin, Anne. 2008. "Tours of Duty, Cross-Identification and Introjection: The Colonial Administrative Mind in Wartime Indochina." *Journal of Historical Sociology* 21(2/3): 183-212.
- Ragin, Charles C. 2008. "Measurement versus Calibration: A Set-theoretic Approach," in *The Ox-*

ford *Handbook of Political Methodology*, edited by Janet Box-Steffensmeier, Henry Brady, and David Collier. New York: Oxford University Press.

Ragin, Charles C. 2008. *Redesigning Social Inquiry: Fuzzy Sets and Beyond*. Chicago: University of Chicago Press.

Ragin, Charles C. and Sarah Strand. 2008. "Using Qualitative Comparative Analysis to Study Causal Order." *Sociological Methods and Research* 36(4): 431-441.

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van Apeldoorn, Bastiaan, Jan Drahokoupil and Laura Horn, editors. 2008. *Contradictions and Limits of Neoliberal European Governance: From Lisbon to Lisbon*. Basingstoke: Palgrave Macmillan.

vom Hau, Matthias. 2008. "State Infrastructural Power and Nationalism: Comparative Lessons from Mexico and Argentina." *Studies in Comparative International Development* 43 (3-4): 334-354.

vom Hau, Matthias and Hillel Soifer. 2008, "Unpacking the 'Strength' of the State: The Utility of State Infrastructural Power." *Studies in Comparative International Development* 43(3-4): 219-230.

Call for Awards

Barrington Moore Book Award

The section awards the Barrington Moore Award every year to the best book in the area of comparative and historical sociology. Nominated publications should have appeared within two years prior to the year in which they are nominated (i.e. for the 2009 award only books published in 2007, 2008, or 2009 will be considered). Books may be nominated by authors or by other section members.

Non-authors may nominate a book by sending a letter or email to the chair of the Moore prize committee. Non-authors should ask authors to arrange to have the book sent to each member of the committee. Authors may nominate their book by sending a letter of nomination to the Moore prize committee and making arrangements for each member of the Moore prize committee to receive a copy. Nominations must be received by February 27, 2009 to be considered.

The committee members and their email and mailing addresses are:

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Comparative Historical Best Article Award

The section awards this prize every year to the best article in the area of comparative and historical sociology. Nominated publications should have appeared within two years prior to the year in which they are nominated (i.e. for the 2009 award only articles published in 2007, 2008 or 2009 will be considered).

Authors or other members of the section may nominate an article by sending a letter or email to each member of this prize committee along with a paper copy of the article. The letter and copy of the article must be received by each member of the committee by February 27, 2009 to be considered.

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Reinhard Bendix Student Paper Award

Every year the section presents the Reinhard Bendix Award for the best graduate student paper in the area of comparative and historical sociology. Submissions are solicited for papers written by students enrolled in graduate programs at the time the paper was written.

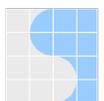
Students may self-nominate their finest work or it may be nominated by their mentors. Authors and mentors may nominate a paper by sending a letter or email to each member of this prize committee along with a paper copy of the article. The letter and copy of the article must be received by each member of the committee by February 27, 2009 to be considered.

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In the next issue of *Trajectories*:

Special Feature on:

*Comparative Historical Sociologists
on the Financial Crisis*

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...And much more!!

*Contributions welcome: please contact the Editors at krippner@umich.edu
and nistan_chorev@brown.edu*

