TRAJECTORIES

Spring 2008 Newsletter of the ASA Comparative and Historical Sociology section Volume19, No. 2

From the Archives:
Innovative Use of Data in Comparative Historical Research

Editors' Note: For this issue of Trajectories, We invited a series of contributions from comparative-historical sociologists using unusual archival sources. We asked our contributors to reflect on their experiences in archives, commenting on a range of issues including: What are the challenges of archival research? How should one best prepare for archival research? Is there a certain level of knowledge required of the case(s) studied? Of the archive visited? And how should one best approach the period spent in the archives (especially if it's a lengthy one)? We also asked contributors to reflect on the use of classified and/or highly sensitive records. What are strategies for gaining access to these materials? What special burdens do classified or sensitive materials impose on researchers once ferreted out of the archives? Finally we asked contributors to discuss the opportunities and challenges presented by new electronic technologies to archival researchers. In the following essays, Victoria Johnson distills practical lessons from her research in the archives of the Paris Opera, Melissa Wilde describes how she gained access to the Vatican Secret Archive, Simone Polillo's research on financial elites leads him to consider the construction of archival research as a "field," and Amy Kate Bailey, Nathan Cermak, and Stewart Tolnay describe their efforts to use electronic sources to create a new database of lynch victims.

From the Archives

What I Learned, and Loved, in the Archives

Victoria Johnson
University of Michigan

Many people would be surprised to see the words "microfilm" and "racing heart" in the same sentence, but seasoned archival researchers will know what I'm talking about. Some years ago, I found myself seated before a microfilm machine in a sumptuous marble hall in Paris—with a racing heart. The room had been built as the private salon of Napoleon III at the Opéra de Paris; today it is home to the Bibliothèque de l'Opéra, a division of the Bibliothèque Nationale de France. My research goal was to figure out why the royal Paris Opera had survived the French Revolution, and the microfilm in question reproduced a handwritten administrative log kept by the assistant director of the Paris Opera in the 1780s. Turning the crank on the side of the machine, I had arrived at his entries for the chaotic days of July 1789, and it was at this point that my hand slowed down and my heart sped up.

> Tuesday, July 22. Monsieur Foulon...was hanged by the people from a lantern...his head was cut off and his body was paraded and dragged through the streets.
> Wednesday, July 23. Opera closed.
> Thursday, July 24. *Alceste*.

The thrill of suddenly hearing the voices of the dead hold forth on the fate of the Paris Opera never wore off for me during the eighteen months I spent working in five different archives, especially because most of my working days were spent trying to decipher illegible French handwriting that turned out to be telling me nothing of particular interest for my study. Probably a great deal of archival research unfolds in just this rhythm—long periods of hard work punctuated by exhilarating discoveries. With careful preparation, however, one can avoid at least some of the dead ends and squandered time.

Unfortunately, sociologists tend not to get this careful preparation. A graduate student in history who plans to conduct archival research on eighteenth-century France takes courses and exams on the period and receives training in how to work with archival documents and in how to determine the structure and content of the relevant archival collections. By contrast, few sociology departments offer courses devoted to historical methods, and it is challenging for sociology graduate students to take seminars in the relevant historical areas on top of their required sociological coursework. When I headed to France, I had a dim intimation of my audacity (foolhardiness?)—a non-musicologist with little historical training taking on the most thoroughly investigated period in human history, the French Revolution—but this intimation was, fortunately, not strong enough to keep me at home. And I did manage to find my way around, emerging with thousands of pieces of paper, a pile of microfilms, and a good sense of how I wanted to structure my book. In retrospect, though, there are a few things it would have been helpful to learn before I got on the plane. (For all I know, my smart and experienced dissertation advisors did tell me these things before I left, but if they did, I wasn't ready to hear them.)

If at all possible, take a preliminary trip to examine the archives before you go for the long haul. This might seem obvious; it's hard to write a dissertation proposal that corresponds to something you can actually pull off if you don't know at least vaguely what the archives contain. For some archival collections, of course, there are published guides, and these might be available through your library or interlibrary loan, but their terse descriptions of whole mountains of documents—"Administrative Correspondence, 1792"—may not actually help you much. A month or two on site spent learning about the breadth and depth of the collections you hope to study and going through a sampling of cartons and microfilms can do wonders in honing your research plan and preparing you to hit the ground running. For students, the trick is that in order to get a grant at the right time you need to figure out quite early in your graduate school career where you want to go and why.

Resist the urge to plunge in. Once you've arrived at the archives, you face what may feel like infinite possibilities. Should I search through all the newspapers that published reviews of the Opera's performances during the Revolution? Should I read the hundreds of letters about the Opera sent between the director and government officials? Should I type up notes from the Opera's daily box office records? Should I focus on the minutes of its administrative meetings? Should I work through the minutes of the Paris city council meetings at the Hôtel de Ville library? Should I examine the minutes of the National Assembly at the Bibliothèque Nationale? Your impulse may be to order up some cartons of documents or a set of microfilms and start taking notes, ordering photocopies, snapping digital pictures, or transcribing them onto your computer. This kind of work can keep you comfortably busy for weeks, but it's the wrong way to proceed.

Start instead by learning what *kind* of information the archives contain and think hard about how that information is relevant to your project. This might mean spending time with the archival guides (if there are any); ordering up samples of particular types of documents and reading through them to get a sense of what they seem to offer; or in the case of series of documents such as newspapers, reading a sample drawn from every few months, years, or decades (whatever makes sense given the time scale you're working on). If you don't do a fair amount of this archival mapmaking for yourself at the beginning, you may end up investing precious weeks in collecting information you will never touch again. This will happen anyway, of course, because your project will evolve as you go, but you can cut down on the time spent exploring dead ends by doing some big-picture work up front.

Make friends with everyone. Archival research can be lonely and tedious. If you are doing long-term archival work, your early days will be spent dealing with the logistics of moving to a new city or country and learning the conventions of the archives (for me it meant mastering five complex and idiosyncratic document ordering systems, one for each archive I worked in)—possibly in a foreign language. When these challenges are combined with trying to get an overview of the archives and trying to lay down good research plans, the early days can be overwhelming. (It was around this time in my own stay that I wrote an email to Chuck Tilly to say that I was drowning in the archives and would probably never be heard from again. He wrote back with the very sensible advice about sampling from the newspapers that I mentioned above.)

What I didn't realize during these difficult early days was that sitting all around me in the archives were graduate students and professors—lots of Americans, but also local students and students from other countries—who had already learned the ropes. Gradually I saw that I had a lot more help available than I had first thought. For example, a chance encounter with a graduate student in history whom I knew by sight from Columbia led to an invaluable tutorial over coffee on the organization of the surviving minutes of the French Revolutionary assemblies, saving me days and days of work. At another archive, a French professor noticed that I was slowly working my way through the yearly almanac for the 1780s (the shelf was right next to his seat) and asked what I was studying. When I explained, he put me in touch with a student of his who had just done what turned out to be a very informative master's thesis on the structure of the King's Household, under whose jurisdiction the Opera fell.

Beyond the learning opportunities, an additional benefit of striking up the acquaintance of anyone you can is lunch companionship. Some archives have cafeterias; some only have vending machines; some have nothing. But wherever you are working, you are likely to see the same faces day after day, first at their desks, then hunched over sandwiches alone, and then back at their desks. Once I got over my shyness in French (and over my feeling that I shouldn't be hanging out with Americans and squandering my chance to practice French), I found the daily grind in the archives to be much less of a grind.

Remember that archivists are your friends, too. In my experience, archivists range from jolly and helpful through bored and annoyed to forbidding and obfuscatory. Far more were helpful than not, however, and even some of those who were initially unhelpful changed their tune after persistent friendliness and displays of respect on my part. Some archivists know more than others, but you won't figure out who can save you weeks of work unless you ask questions of all of them. And sometimes the archivists themselves can surprise you with their initiative. A man working at the Paris Opera library who himself appeared to suffer from extreme shyness ended up with my undying gratitude: one day, having taking note of the documents I had been ordering, he wordlessly guided me to a distant card catalogue and opened a drawer I might never have found on my own. It contained the call numbers for what turned out to be some of the most important documents in my research.

Archival research can be both deeply rewarding and intensely frustrating, often at the same moment. How exciting it was to finally and so thoroughly master the nearly illegible script of the assistant director of the Opera that I could recognize words at a glance, simply based on the way he shaped his "f" or his "l"—and how utterly useless such knowledge was beyond this particular study. How exciting to handle pages penned in the offices of the Opera during the French Revolution, but how annoying that I sat shivering in my scarf and gloves as I did, because the thirty-foot ceilings in the Opera library made it impossible to keep warm through the winter months. How exciting it was to be in Paris, and how hard it was to truly enjoy it, given the pressures of working my way through the archives before the money and time ran out. Despite these tensions, and despite my initial inexperience, the rewards of intensive archival research have won me over, as they have increasing numbers of sociologists over the last half-century. I'm heading into a whole new set of archives in a few months, and I can't wait.

Just Your Average Full Service Secret Archive

Melissa Wilde
University of Pennsylvania

I have the (mostly) good fortune to be able to refer to the "Vatican Secret Archive" (VSA) whenever I reference the data in my book on the Second Vatican Council in the Roman Catholic Church (Vatican II, 1962-1965) (Wilde 2007). Those three words almost always raise eyebrows and questions (and usually at least one reference to *The DaVinci Code*).

The VSA *is* as mystifying and stodgy as the name suggests (I needed three letters of introduction, my Ph.D. certificate and passport to gain entrance), but is also surprisingly friendly to researchers. The purpose of this piece is to give pointers to others who might have an interest in obtaining sensitive materials – such as those that I obtained, which were the voting records of the bishops who participated at the Council – that tend to be stored in similar hard-to-access archives.

The question that directed my research on the Council was: "What explains why some reforms passed and others failed?" Though a host of other factors came into play before reforms were ever voted on that were certainly relevant to this question, I realized early on that if I wanted to really answer this question, examining the patterns in the votes on reforms themselves would be crucial.

But the Vatican had never released any information about the votes beyond overall totals of how many bishops voted in favor of or against reforms. I needed to know what types of bishops fell on either side, where they were from, and what characteristics of those environments were correlated with their support of an issue. Thus, I needed the votes – the actual reports with bishops' names and how they voted. While the rest of my data was available outside of the Vatican, in archives in Bologna, Italy, Berkeley, CA and Washington D.C. that were very friendly to researchers, only the Vatican would have the votes.

I had some contacts at the Vatican from previous research trips, and in the Spring of 2002, as I was finishing my dissertation, I e-mailed them and asked to be put in touch with the archivist of the

Vatican II archive. It was then that I ran into my first piece of good luck: the archivist who had been in charge of the Vatican II archive since the Council, whose nickname among researchers of the Council was "the Bulldog," had retired – and had been replaced by a non-cleric Ph.D., Dr. Peitro Doria, who, it would turn out, saw his job as helping researchers obtain the information they needed (rather than protecting it from prying eyes).

After spending a few days figuring what votes to ask for (most Council votes happened only after an extensive campaign and revisions that usually resulted in strong consensus – a fact which meant that most votes would not have enough variation to be useful to me), I e-mailed Dr. Doria and requested access to three key, highly contentious votes. While e-mail may seem to be a surprisingly modern way to communicate with the Vatican – it was really my only recourse, in that snail mail across the ocean would take too long, and phone calls were very difficult with the time difference between Italy and California.

Furthermore, while this initial correspondence occurred over e-mail, it was most certainly not informal – in that I decided that the best way to gain access to materials that I knew no researcher had ever seen was to clearly communicate my credentials as well as the ways in which I would use the materials. Thus, I told Dr. Doria that I would be getting my Ph.D. that May, and starting a job at Indiana University the following fall – two pieces of information that I hoped would reassure him that I was "for real" – and I made one important concession (which was actually not much of a concession for me, as a sociologist, at all): I promised that while I needed the actual bishops' names in order to be able to place them in their countries for analysis, I would not report how any individual bishop voted in published materials.

To my utter surprise and complete elation, I heard back from Dr. Doria quite promptly in a message that stated that yes, the votes were there, and would be waiting for me when I arrived in June. Thus, I began making preparations to go to Italy as I finished my dissertation.

A key part of those preparations was figuring out how I would record the information. The bishops at the Council voted on IBM punch card machines that were developed specifically for the Council. I suspected that, like most such machines, a vote tally was printed out by the machine that had the bishops' names and votes listed. And, I suspected (and dearly hoped) that those tallies might still exist – but that if they did not – the punch cards surely still did. Thus, I arrived in Rome with a laptop and an excel spreadsheet, prepared to spend some very long, and hot, days in the VSA in an endeavor that I could only envision as similar to a re-run of Florida 2000.

My first encounter with the Vatican Secret Archive was not a good one: I arrived at about 10:00 in the morning, in a suit (women cannot show their shoulders or knees inside the Vatican), in 95 degree weather with 99% humidity only to be told that library cards are only issued between 8:00 and 9:00 a.m., and that I would need to come back the following day. I arrived back at our apartment and told my husband that I thought that we were going to be there "a very long time."

The next day, however, proved me wrong. This time I arrived at the required hour, was quickly given my card, checked my bags and pens (only pencils are allowed inside), and was ushered into a cool marble hall, through a few rooms where other scholars were silently pouring over what looked like ancient manuscripts, and brought to Dr. Doria, who shook my hand and said, "The votes you requested are waiting for you right here." He showed me over to some large bound volumes, which sure enough, proved to be the tallies of the votes that I had requested, with the bishops' names, titles, dioceses and votes on three central Council issues.

As I stood there looking down at the pages and pages of the more than 2000 bishops who had voted on each of the votes, with my mouth no doubt wide open, I turned around to realize that he had quietly left me alone to do whatever it was that a sociologist would do with such information. Somewhat dumbfounded, I sat down, and leafed through the volumes assuring myself that indeed, there it was, alphabetically arranged, history for the taking. But, the taking was the problem. I couldn't very well create my database myself, in Rome, during the four hours a day that the Vatican Secret Archive was open. I could have started, of course, but would probably have only been able to enter one, if maybe two of the votes over the two months we had planned in Italy – plus, I had other archives elsewhere I needed to go as well. Thus, as my heart slowed down, and I caught my breath, I gathered up my courage and went to find Dr. Doria again. This time I asked a question that is surely not uttered all that often in the VSA, "Could I get photocopies?"

My heart sank with his reply, "No…" only to rise again as he finished his sentence, "Our staff must make the photocopies here, and then we charge you (I believe it was something reasonable like $.10 a page)." I assured him that was fine with me, gave him my business card, thanked him profusely, and left – my time inside the VSA surprisingly brief but successful.

When I arrived in Bloomington a few months later, the photocopies were waiting for me (and my research assistants who would spend the next two years entering and re-entering the bishops' names, votes, dioceses, countries and other biographical information into Microsoft Access).

The votes proved to be essential to the arguments I make in my book and other research on the Council. And, I learned a few things as a result of the process through which I obtained them:

1. Even if you think it isn't likely, it doesn't hurt to ask to see sensitive materials.
2. Before you do so, if at all possible, get your credentials in order and don't hesitate to flash them.
3. It is best to approach such materials and the archives where they are stored only after you have a very clear sense of exactly what data you need: they're not inclined to allow individuals to spend time lazily leafing through their materials.
4. If, like many sociologists, you have no need to actually report what prominent individuals did, explain that up front (in my case, if a bishop was well-known, I knew whether he was liberal or conservative, and could pretty much predict his vote. If he wasn't, there was no need for me to bore my readers listing how the rank and file voted on each of my issues). I suspect that was the most important part to my obtaining access.

In the end, the VSA proved to be even more amenable to research than the fact that they allowed me access to, not to mention photocopies of, vote tallies that no researcher had ever seen before. As my *ASR* article (Wilde 2004) was going to press, I found errors in their tallies (in one vote, for example, they had left off two votes from their official totals that were isolated on the last page, in another, I found out that they had lost an entire page of [about fifteen bishops] votes). Although the errors were small, they were real, and I did not want to report an error on the part of the Vatican without discussing it with them first. Somewhat desperately, I faxed and called the VSA in the middle of the night in an attempt to figure out what they wanted me do. To my great surprise, and in an outcome that only affirmed how wrong popular impressions can be, they begged off completely, saying that they do not infringe upon researchers and that I was allowed to clarify whatever I needed to for the purposes of my research.

Should I ever need to, I'd be happy to obtain more data from the VSA. Besides, I never did find that secret passage to the Sistine Chapel…

References Cited

Wilde, Melissa J. 2004. "How Culture Mattered at Vatican II: Collegiality Trumps Authority in the Council's Social Movement Organizations." *American Sociological Review*. 69(4):576-602.

Wilde, Melissa J. 2007. *Vatican II: A Sociological Analysis of Religious Change.* Princeton University Press.

Archives as fields? A Personal Narrative on Comparative-Historical Research

Simone Polillo
University of Pennsylvania

There is a rather wide gap between doing research and writing about research. It perhaps comes down to a basic finding of ethnomethodology: accounts are post-facto rationalizations, something one constructs to justify strategies, "choices" and moves that come from outside the individual agent – from the interaction, the situation, or as Bourdieu would suggest, the structure of the habitus. Archival research, for example, takes place in an "archival field," with its field-specific "archival capital," its internal struggles and oppositions. Would the analysis of an archival field take us away from the more interesting question of the relationship between archival research, sociology and history? My task here is to show that, in fact, it would not: that the field in which one's archival research takes place, a world of archivists, boxes, folders – affects the ways we produce historical sociology. I don't intend to write an essay about epistemology or methodology – I would like to focus more narrowly on generalizing inductively from my experience, writing something like a brief ethnography of an archive, and suggesting a few generalizations along the way.

My first question in coming into the field of archival research was actually: What's in an archive? Archives can be impossibly large. When I was conducting research at the Historical Collections of the Bank of Italy, there were not only the internal documents produced by the various departments and agencies in which the Bank is divided – there was also the documentary trail of the entire Italian banking system over which the Bank gradually succeeded to exercise its supervision. Obviously the only way not to be completely overwhelmed was to concoct some kind of sampling strategy: but sampling with what purpose in mind, and out of what universe? I was interested in understanding the internal struggles structuring the banking system and thus orienting the politics of Italian financial and political elites in the pre-World War I period. I knew that the Bank of Italy was an ideal site in which such struggles played out because this was where state and capital met: a) the Bank grew out of the Genoese capitalist network of Braudelian fame, and thus included powerful and skilled actors; and b) it acquired a strong institutional, statist identity in the 1900s under the liberal regime to in fact remain the only institution surviving the fascist purge in the 1920s. Hence, if there was any autonomy to any part of the Italian state, it was going to be found here. The theme I was pursuing was organizational persistence and continuity: given my general theoretical orientation, I was looking for persistent conversations, continued encounters that in fact kept the organization going as an interactional achievement.

It was at that point that the advice of a historian of liberal Italy became crucial: read the whole correspondence between the Central Bank Governor and a top private banker to get a sense of their (deteriorating) relationship. 20 years worth of letters. Of course! Why this particular advice was so important was not only because it made perfect theoretical sense – it also helped emotionally, as it came at a moment when I seemed to have lost my bearing in a sea of documents. The historian was an informant, but also the holder of particularized, field-specific cultural capital. This point leads me to a first generalization: acquiring historical background in the topic one is investigating as a sociologist serves as much as background to research as a key to *interact with experts in the field*. This is in turn crucial because it gives the researcher the emotional resources, confidence and patience to undertake specific archival searches and investigations whose payoff is uncertain and certainly distant in the future. (I was really having enough of reading bankers' arcane, early 20th century contrived rhetoric!)

Then came a gestalt switch – those rhetorical flourishes were in fact rather central to the ways the network was held together (see McLean 2007 for a wonderful elaboration of this point). Hence the advice of a historian had led me back to a central sociological concern: the negotiation of social relations. In turn, this was turning me away from my initial effort of getting the events "right," that is understanding the specific referents of the bankers' correspondence. Events remained certainly important to their interactions, but the bankers' institutional location served as a lens through which potential disruptions to their power was filtered into manageable narratives. What this signaled to me is that I had descended into the most "conservative" area of the banking field, where a sense of proper banking was being painstakingly constructed in order to ward off the attacks of the "wildcat" bankers, those willing to make alliances with outsiders and marginal players and thus subvert the hierarchy of the field. This calls for my second generalization: theory not only is an aid in the process of framing one's research, but it remains a *heuristic device throughout the research process as it leads the researcher towards particular paths*. One could sample archives, but also navigate them analytically.

There are radically different kinds of archives, of course. I was fortunate enough to conduct research in some beautifully organized historical collections, which had been plowed deeply by generations of historians before. However, that came with its costs: What could I contribute to the collective research project that had so masterfully organized the archive to start with, and how would I develop a new view of documents which had been dug up before by much more competent and knowledgeable scholars than me? I went for quantity and temporal continuity: the fact that these documents were produced within a formal organization was a distinct advantage, in that the organization itself constituted a focused center of interaction (Glaeser 2005). The archive of the Bank of Italy was thus an archive of the Italian financial elite, which over time was pulled into its sphere of influence and forced to take the Bank of Italy into account. Other archival sites might not possess the same properties. For example, since my dissertation was comparative with the development of the US financial elite as its second case, I spent an equivalent amount of time doing primary research in North American historical collections: but the decentralized structure of the US elites posed new challenges. It was not possible to locate a single archival site that would be in some sense "representative" of the larger dynamics of the banking field. Interestingly enough, academic institutions were often the sites where the papers of important financiers (in their role of benefactors and contributors to university endowments) were collected. My point here leads to a third generalization: *the material location of primary records is itself an institutional outcome of sociological interest*. In my particular case, it indicated that processes of elite distinction based on internally generated claims to prestige and reputation were crucial in the pre-World-War-I United States in ways that they were not in Italy. There, prestige came as much from organizing the banking field as from interacting skillfully and successfully with the state (with the hope of turning a profit in the meanwhile).

These points may seem simplistic and rudimentary to professional historians: but as a sociologist, I found it useful to remind myself of their validity precisely because of the field-specific advantages they would confer on me. Inter-disciplinary dialogue is fraught with difficulties at the epistemological level, but at the practical level too one must take into account the different kinds of professional pressures which motivate one's research and the reception of one's work. Archivists are specialists invested in producing knowledge which is tied to the intricacies of particular archival sites. I have been repeatedly amazed by their ability to navigate the complex structures of their repositories and their professional certainty that making particular claims required a long journey through arcane and sometimes utterly incomprehensible documents. That they were virtually always right validates my point: *historical knowledge developed in different fields is subject to different epistemological criteria*.

This is too static a notion: for example, the historical debate on the economic and political development of liberal Italy has its own historical trajectory which has changed over time the kinds of epistemological criteria used to assess its validity and "truth." The towering figure remains Antonio Gramsci, who from the depths of a fascist prison produced an interpretation of the Italian Risorgimento and subsequent development (or in his view, deterioration) that for obvious reasons was not based on much archival research. Going "back" to the archives within that particular debate was a strategy to debate with, and often critique Gramsci – and recently Italian Marxist historians have begun to complain that nobody is willing to make an argument that is not backed by precise historical evidence. Again as a sociologist, an awareness of such field-specific dynamics became a useful resource. Actually defining myself as a sociologist often served as a source of capital – granting prestige to my strategy of jumping from folder to folder, box to box without really getting into the details of the "event." What must have looked superficial to the professional historian was in some ways a survival strategy for me as a sociologist interested in empirically grounded, yet *abstract* processes.

I want to conclude with a note on the excitement that often characterized my archival experience. The kind of knowledge one acquires first hand can be very much unique: you get a feel of the way the "game" was played, gain a window on the ways historical actors met the demands of daily life – it is certainly a limited window, but it nonetheless "feels" alive with energy and movement. Just as one of the signs that an ethnography is successful is that the investigator begins to act as his or her own informant…

References Cited

Glaeser, Andreas. 2005. "An Ontology for the Ethnographic Analysis of Social Processes. Extending the Extended-Case Method." *Social Analysis* (49)(3): pp. 16-45.

McLean, Paul D. 2007. *The Art of the Network.* Durham: Duke University Press.

Acknowledgements

Many thanks to to Keith Brown and Julia Lynch for their valuable comments.

History Goes High-Tech: Creating a New Data Source Using Online Resources

Amy Kate Bailey, Nathan Cermak, and Stewart E. Tolnay
University of Washington

Matching individual census records across decades is not a novel research method. Our creation of a new database of lynch victims is noteworthy because of our geographic and temporal scope, the variety of documents we incorporate, and the fact that our initial inventory was created using newspaper reports. Perhaps most importantly, our data collection efforts occur online. Rather than scrolling through microfilm or digging through library archives, we use a genealogy website subscription[1] and its searchable web-based interface to access .jpg images of historic census manuscripts. History has gone high-tech.

We begin with the Beck-Tolnay (2004) inventory of lynch victims, constructed two decades ago and including minimal information for 2806 lynch victims: generally their name, race, gender, and the date, state and county of their lynching. By linking these victims with their census records, we glean a broader variety of information about the people who were targeted for these hate crimes. The information in the final data set will include a variety of information for each victim and all members of their household: for example, age, occupation, literacy/education, homeownership and marital status. With these data we can compare lynch victims to random samples of the population that was not lynched and identify whether the groups varied systematically. This new information will help us create a more complete picture of the people targeted for lynching, and to refocus the study of southern mob violence on its victims. We hope to better understand why lynchings occurred, and whether there was patterning in the kinds of individuals selected as its victims.

Our work is conducted by a highly-capable team of undergraduate research assistants, who search for each victim in the census records immediately prior to his or her lynching – a backward search. For example, if someone was lynched in 1902, we search in the 1900 census, beginning within the county of lynching, and expanding outward to contiguous and nearby counties. We also conduct a forward search, looking in the 1910 census for candidate matches we identified in the 1900 census. If someone was lynched during the intervening decade, we should not be able to locate them in the subsequent census. This online research provides high-quality .jpg files of the original census manuscript for each successful match. All U.S. census manuscript records through 1930 can now be searched online using various criteria, including an individual's name, race, age, gender, and their state and county of residence.

While our research primarily relies on census records, we incorporate additional online sources to verify matches and locate information on lynch victims. Chief among these are World War I draft

[1] The genealogy website we use is called Ancestry.com.

registration cards, available for more than 24 million men in 1917 and 1918. The information includes each person's name, race, date of birth, employer, occupation, marital status, and often next-of-kin. WWI draft records had better coverage than the census; however, the draft has limited temporal and age applicability, reducing the usefulness for our project, as lynching prevalence declined over time. Less problematic, since more than 90 percent of people in our inventory were male, is the exclusion of women from compulsory military service. Draft registration records, then, allow us to access confirmatory information on a small subpopulation of individuals.

We also frequently rely on death records to verify facts about particular victims, or to adjudicate between multiple possible matches. Information about death records is often available online, although temporal and geographic coverage is uneven. Within the American South, few records exist before the early 20th century, and implementation of vital records registries varied both within and between states. We have found two main types of death record information available online. The first, death indices, list individuals whose deaths were officially recorded. Electronic images of death indices are available for many geographic areas, and often include basic information such as the individual's name and the year and location of their death. Death indices also frequently include reference numbers that can be used to locate actual death certificates.

In our experience, images of actual death certificates are not available online. However, three sources of online information have been helpful in locating available death records. The first is the Ancestry.com searchable database of death records, which can identify whether a death record exists for an individual, searchable by name and date and location of death. Additionally, many states now have centralized online forms to request death certificate searches, although these requests may be expensive, running between \$10- and \$20- per request, regardless of whether the search produces an actual death certificate. State law varies regarding whether nonrelatives are able to access official death certificates, and also whether all information is made available to members of the general public – for example, Georgia obscures the lines discussing cause of death. Finally, for states without centralized online vital records registries, we have found the Internet to be useful for locating county governmental offices, which often maintain historic records and will forward unofficial copies for a nominal fee – frequently \$1- or less.

We utilize historical newspapers archived on Ancestry.com when we are unable to identify a match by other means. Articles about the lynching in question sometimes include details about the victim – for example, the person's age, occupation, or the names of family members – which can help narrow down our field of match candidates. This information has been useful in a small number of cases; however, we are only able to locate online newspaper articles for a minority of cases, and estimate that only half of those contain information useful in locating the victim. As the number of historical newspapers online increases, and the functionality of search engines used to access them improves, we anticipate that this kind of online resource will become more useful.

Once we have identified likely matches, we enter the characteristics of each victim and all household members into EpiData, a freeware data package translatable into a variety of other software, including statistical packages and programs designed for qualitative data analysis.[2] We will distribute the EpiData file as well as all supporting documentation and our research notes, and anticipate that it will be available in mid-2009.[3]

This effort faces many challenges typical of historical data collection. We rely on data – research notes from staff members of the Beck-Tolnay inventory –collected for a different purpose, namely to identify temporal and spatial variation in lynching prevalence. Those research notes, in turn, were based on historical newspaper reports. In utilizing historic census manuscripts, we are also challenged by illegible or unusual handwriting, enumerators' idiosyncratic methods of recording information, and the deterioration of documents over time. We have found the staff of the Univer-

[2] EpiData is available online at www.epidata.dk.

[3] For a more detailed discussion of the methods we employ, please see Bailey et. al. 2008. For information on the construction of the initial Beck-Tolnay inventory, please see Tolnay and Beck, 1995.

sity of Minnesota's Minnesota Population Center to be exceptionally helpful in surmounting these hurdles. Historical census data also contain an unknown – but likely, a small – degree of error in that all information for a given household was reported by a single member.

The specific focus of our research presents additional challenges. Perhaps most problematic is the underenumeration of African American men – an estimated one in five working-aged men were not enumerated during many decades of our data collection (Coale and Rives 1973). This reduces to zero the likelihood that we will successfully locate a full twenty percent of the black male lynch victims in our inventory. We face an additional restriction due to the destruction of the original 1890 census manuscripts in a fire. The 1890s were the decade in which the greatest number of lynchings occurred, so we lose several hundred cases because we lack primary source documentation to locate these victims. This also reduces our confidence in matches from the 1880s, since we cannot eliminate false positives through forward matching.

A second set of challenges lies in the distribution medium of our data, particularly since the technology is so new it has been under development as we have been using it. Consequently, different search functionalities have evolved over the course of the project, and resources available for the final stages of our searching were not available to us earlier in the project. Some of the functions remain in development. While Soundex searching is available,[4] wildcard searching is limited,[5] and Boolean search terms are not allowed (i.e., one cannot simultaneously search for more than one name or county). Data entry for the online searchable versions of the original census enumerators' manuscripts is being done in non-English-speaking countries, some of which do not use Roman-based writing systems. This poses an additional challenge for the coders (and, therefore, us), who are attempting to decipher enumerator handwriting in order to successfully identify, translate, and enter names common in the historic American South.

This project would not have been possible ten years ago, due to its geographic and temporal scope and the large number of individuals whose data we are collecting. Using traditional historic methods would dramatically increase the labor intensity, since we would need to separately identify – and perhaps request – each source document. This work is not comparable to localized data collection that includes all records from a specific area, or matching records for a single county between two census enumerations. Our project covers ten southern states, and nearly fifty years. Only since the transfer of entire United States census manuscripts from microfilm to an online database has this project become feasible. Despite the uneven availability of online information, and variations in its quality and the level of detail it provides, we believe historical data collection efforts will increasingly rely on the internet. We are excited at the possibilities that expanded technological capacity will offer to historical researchers.

References Cited

Bailey, Amy Kate, Stewart E. Tolnay, E.M. Beck, Alison Renee Roberts, and Nicholas H. Wong. 2008. "Personalizing Lynch Victims: A New Database to Support the Study of Southern Mob Violence." *Historical Methods* 41(1): pp?

Beck, E. M. and Stewart E. Tolnay. 2004. "Confirmed Inventory of Southern Lynch Victims, 1882-1930." Machine-readable data file available from authors.

Coale, Ansley J. And Norfleet W. Rives Jr. 1973. "A statistical reconstruction of the Black population of the United States1880-1970: Estimates of true numbers by age and sex, birth rates, and total fertility." *Population Index* 39:3-36.

Tolnay, Stewart E. and E.M. Beck. 1995. *A Festival of Violence: An Analysis of Southern Lynchings, 1882 – 1930*. Champaign: University of Illinois Press.

[4]Soundex is a phonetic algorithm which allows for similar names to be included – i.e. a search for Johnson would yield hits for Johnsen and Jonnson

[5] A user interface is said to search using "wildcards" when it searches for a string of characters that may appear independently or nested within longer words. For example, a wildcard search for jon* would identify the first names Jon and Jonathan, as well as the last name Jones.

Book Symposium: *The Devil's Handwriting*, by George Steinmetz

Editors' Note: We asked Ralph Austen and Elizabeth Povinelli to submit essays commenting on George Steinmetz's new book: The Devil's Handwriting: Precoloniality and the German Colonial State in Qingdao, Samoa, and Southwest Africa (University of Chicago Press, 2007). *Following Austen's and Povinelli's essays, Steinmetz responds. Both Austen and Povinelli were participants in a Social Science History Association (SSHA) Author Meets Critics Panel organized by Julia Adams.*

The Fourth Wave of Postcolonial Studies

Ralph Austen
University of Chicago

As indicated in his subtitle, George Steinmetz deals with the pre-conditions and implementation of European colonial rule in Asia, the Pacific islands and Africa. Analytically, however, this work helps us define a fourth wave of postcolonial studies, a field developed after colonies achieved formal political independence, but whose subject matter has always centered around colonialism. The first wave of postcolonial scholarship, beginning in the post World War II era of decolonization, focused upon the politics of anti-colonial nationalism and was the domain of political scientists. A second set of studies, undertaken by economists and economic historians, took into account the disillusionments of economic underdevelopment and dependency. The formal label "Postcolonial Studies" is associated with a more recent "cultural turn" in social sciences generally that has privileged anthropologists and literary theorists; it is an immediate reference point for Steinmetz.

As an historical sociologist, Steinmetz combines a concern for much of the theory informing postcolonial studies with what he calls the "specificities" of both the German case and colonialism more generally. His introductory section on "the specificity of the colonial state" removes much of the aura often assigned by postcolonialists to an all-encompassing and ubiquitous colonialism by recognizing both the distinction between a modern colony and other forms of "empire" and the limited, if still authoritarian, political and economic projects of such overseas regimes.

Ethnography (the "Devil's handwriting" of Steinmetz's title) again does specific work. As understood through postcolonial theory, it supports a "hegemonic discourse" by which colonial subjects are constituted as an "other," not entitled to the same political and cultural status as their European rulers. But Steinmetz is again concerned with the historical specificities through which such discourse shapes "native policy," the immediate principles used to govern alien colonies. He also recognizes that this task forces the colonizer to develop some kind of empirical competence about the specific populations he has come to manage. Whatever its political purposes, this understanding is not entirely defined by the colonizers' own subject position. Variations in such competence are thus recognized and can matter.

In his treatment of "the colony as a social field" Steinmetz again moves beyond the dyad of rulers and ruled to stress the specific European metropolitan history that is brought into overseas empires. These domestic conflicts (or at least tensions) and their mapping on to competing competencies for understanding local culture have a major impact on colonial governance and socio-economic development. Germany provides a somewhat limited case for pursing colonial history, since the overseas empire of the Kaiserreich lasted only about thirty years and encompassed only a few territories. Nonetheless, the points made in this book are relevant to understanding the much larger and more durable modern overseas regimes of Britain, France and the Netherlands. One can make this assertion despite (or perhaps because) Steinmetz builds his analysis around the German *Sonderweg* (exceptional [development] path). The key to this exceptionalism is the continued need for the presumed leaders of modernity in Germany, the *Bildungsbürgertum* (educated middle class), to compete for power and stratus with a robust military aristocracy.

The Sonderweg thesis has come under considerable attack over the last decades, not the least in earlier writings of Steinmetz himself.[1] However it does work well for the cases studied here and provides a model which can usefully be extended elsewhere. One aspect of the marginality of colonialism from a European perspective has been the tendency to compare different colonial regimes on the basis of very shallow and cliched "national character" profiles. Thus the Sonderweg, whatever its shortcomings, is preferable to the notion of German colonial rule as some combination of exceptional brutality, applied science and a "*streng aber gerecht"* (strict but just) judicial regime. A parallel to Steinmetz's account of what produced the specificities of German rule might be the understanding of modern French colonialism as driven less by assimilationist Jacobinism and Cartesian logic, than by the conflicts between Church and state of the Third Republic.

Having praised the sociologist Steinmetz, I do feel compelled to assert the historian's prerogative of pointing out that his formulation does not fit all German colonial cases, particularly the two African ones I happen to have studied closely. In German East Africa (now mainland Tanzania) the liberal hero is an aristocratic governor, Baron Albrecht von Rechenberg, and his opponents come mostly from the settler bourgeoisie, albeit a group not noted for its *Bildung*. Steinmetz's model *Bildungsbürger* is the Sanskritist and Samoa governor Wilhelm Solf. In his later role as Colonial Secretary, Solf had to deal with serious "native policy" crisis in Cameroon and comes off as rather racist, while the more liberal political position was defended by another aristocrat, Helmut von Gerlach.

It could also be argued that German East Africa and Cameroon (along with the other German African territory of Togo) were more representative of modern colonial regimes (administrators ruling a largely peasant population on the British Indian model) than are the three cases (Samoa, Namibia, Qingdao) that Steinmetz chooses for this book, perhaps because of their internal variability. Qingdao raises particular problems on two counts. First, as Steinmetz notes, in the course of their political control over this coastal enclave the German respect for their Chinese subjects reached a point where the European rulers became absorbed into the local culture. More problematic for the book's argument is its equation of anthropology, which clearly informed Samoan and Namibian native policy, with philology, the "Devil's handwriting" of German governance in China.[2] Rather than implying the hegemonic conditions of colonial rule, philology is part of European self-understanding, whether applied to the texts of contemporary national cultures or their ancient predecessors. Steinmetz acknowledges the importance of a resurgent "sinophilia" in much German colonial writing about Qingdao and even the quite brutal expedition against the Boxer rebels and he explicitly refutes Edward Said's monolithic understanding of "orientalist" philology. A more fruitful object of comparison for understanding the relationship between philology and colonialism is the prime case of British India, as brilliantly illuminated in the work of Thomas R. Trautmann.[3]

Arguing that this book suggests further avenues of research is less a criticism than further praise of Steinmetz's achievement. *The Devil's Handwriting* takes colonial/postcolonial studies in new directions that will be followed for at least another generation of scholarship.

---

[1] "German Exceptionalism and the Origins of Nazism: The Career of a Concept." Pp. 251-84 in Ian Kershaw and Moshe Lewin, eds., *Stalinism and Nazism: Dictatorships in Comparison* (Cambridge: Cambridge University Press).

[2] This issue was not part of my original SSHA panel presentation but raised instead (and never much discussed) by an audience member whose name I regret not to have recorded.

[3] *Aryans and British India* (Berkeley: University of California Press, 1997) ; *Languages and Nations: the Dravidian Proof in Colonial Madras* (Berkeley: University of California Press, 2006.)

"Theory and the Postcolonial in *The Devil's Handwriting*"

Elizabeth A. Povinelli
Columbia University

I wrote the following words. "*The Devil's Handwriting* is a masterly study of the capacious nature of the colonial form. Comparing three twentieth-century German colonies, Steinmetz demonstrates with great acuity the multiple ways that German administrators and ethnographers deployed the rule of difference in the management of colonial populations. I know of no other study of the colonial state that combines such a breathtaking depth and breadth of archival analysis with such an acute sensibility of the play of difference within the rule of difference. The writing is open, engaging, personable, even as the material is, at times, devastating." And I stand by these words. *The Devil's Handwriting* is a profound piece of scholarly research. And yet (there must always be an "and yet" in critical assignments such as this one) I want to take up a certain provocation in the book—the way *The Devil's Handwriting* stages the relationship between the comparative and historical sociology it proposes and the postcolonial theory it rejects. The following represents, in other words, the impossible task of navigating between the Scylla of my appreciation of this text and the Charybdis of my response to the provocation in this text.

The Devil's Handwriting announces fairly immediately that it opposes dominant forms of postcolonial theory. These dominant forms are characterized as attempting "to identify any singular, general model of colonial rule." (p. 3). In contrast *The Devil's Handwriting* seeks to identify "a limited set of generative social structures or mechanisms and track the ways they interact to produce ongoing policies" (p. 3). The book not only rejects the attempt to produce a "singular, general" model of colonialism, it also rejects attempts to characterize national styles of colonialism (British "indirect," French "direct," US "tutelage") and modal styles of colonialism more generally (settler, extractive). For *The Devil's Handwriting* colonial forms of administration are not only inter-differential they are intra-differential. If they are Orientalist in nature, they are heterogeneously so. Getting at this heterogeneity necessitates carefully tracking three "crucial links in the chain of determinants leading from ethnographic representation to native policy"; namely, "(1) Patterns of resistance and collaboration by the colonized, (2) symbolic competition among the colonizers, and (3) colonizer's imaginary cross-identification with the images of their subjects" (p. 27). These three factors make "the linkages between ethnographic visions and social divisions are contingent and historically variable" (46).

The Devil's Handwriting stages its argument against a set of "postcolonial theorists" clustered under the sign of "Said and Foucault" (p. 25). One is flattered to be included—no press is bad press so the saying goes—in a list with Edward Said, Michel Foucault, Talal Asad, Timothy Mitchell and Susanne Zantop even if this inclusion imbricates one in the accusation of having failed to notice (or acknowledge) that "most formations of ethnographic discourse are multivocal or multiaccentual" and "complexly mediated" (p. 27). One is also offended, crying out—"But what is my book if not an attempt to demonstrate the symbolic competition among the colonizers, and their imaginary cross-identification with the images of their subjects?" And what of all the others who labor within something that could be called "postcolonial theory," striving to demonstrate the complex cross identifications, colorations, and symbolic competitions at stake in the contingent and historical variable terrain of colonialism? One thinks here of Ranajit Guha, Dipesh Chakrabarty, Achille Mbembe, and Partha Chatterjee, some of whom Steinmetz also cites. One might not agree with how they proceed but it would be absurd to suggest they do not understand the complex social relations of identification and their historical variability within the colonial world. But these rounds of accusations about good and bad textual readings, good and bad intellectual typologies, inflated and wounded egos are just part and parcel of academic discourse meant to provoke new thought.

More interesting is the level, or location, where singular, general theory finds an abode in a book whose interests lay in finding "a limited set of generative social structures or mechanisms" that interact historically "to produce ongoing policies." When we look at *The Devil's Handwriting* in this way we see that it has hardly refused to participate in the building of a "singular, general model" for a comparative and historical sociology. In fact *The Devil's Handwriting* presents a robust argument for grounding a comparative and historical sociology in a theory of social action that articulates Bourdieu's practice theory and Lacanian psychoanalysis (pp. 55-65). One can argue whether Steinmetz pulls off this ambitious theoretical program in the actual analysis—how persuasive are certain of his arguments about the psychic interior of German administrators and how one-sided these psychic inscriptions seem to be (not a lot on the psyche of colonized).

Whether successful or not, the social theory *The Devil's Handwriting* proposes is not limited to the colonial spaces that form the content of the book's analysis. "This doubling of symbolic and illegal identifications is not specific to `offstage' or colonial settings but is characteristic of subjectivity in general" (p. 61)? To be sure, these social mechanisms and their psychic investment have a particular German inflection (*illusio*), an inflection refracted across three different colonial settings (p. 49, 61) and the subject of the subsequent chapters. But clearly, the great ambition of *The Devil's Handwriting* is to produce a general theoretical economy that will demonstrate the restricted economy of colonialism, a general theory of subjectivity and sociality that produces a partial theory of colonialism. And this is fascinating: It is as if *The Devil's Handwriting* is stating that the only way we can get a restricted model of colonialism is to have a singular, general model of the social. And it is as if *The Devil's Handwriting* is arguing that the "singular, general" is perfectly suitable for the highest order of social analysis but falls apart at closer range. I am not suggesting that this argument is right or wrong—that would require a more extended thought. Instead I am suggesting that "theory" has become a piece of artillery in the way of position and maneuver, sometimes a decoy, sometimes rolled out in plain view, sometimes smuggled in over a mountain pass. If I do not think too much about these wars of position and maneuver then I can be swept away by *The Devil's Handwriting*'s reinterpretation of German colonialism. Specific historical characters are shown maneuvering within fragmented symbolic fields, refracted through a set of psychic investments, resulting in diverging colonial policies and practices. For instance, these symbolic and psychic fields lead the "abandonment of the rule of difference" in Kiaochow China even as in Southwest Africa German administrators "adhered tenaciously to that rule but abandoned native policy for native massacre" (p. 239).

If I pay attention, however, to these positions and maneuvers of theory then a very different set of critical reflections float to the surface. First, if we accept the argument that the "singular, general" is perfectly suitable for the highest order of social analysis but falls apart at closer range, then we must find someway of explaining this incommensurability within the field of immanent social theory. For sometime we have avoided this problem either by modifying "theory" by "high, mid, low" without bothering too much about the non-passage between the levels—or even bothering to understanding the referent and meaning of "levels" (When we move to mid-range theory are we moving to factual terrain of social immanence?)—or by smuggling in our organizing conceptual frameworks (also called "theory") through the backdoor.

Second, and flowing from this first point, this non-passage between "singular, general" models and mid-level "generative social structures" only amplifies another gap that opens between social theory, whether high or mid, and social life, here understood from the perspective of the people being described. For instance *The Devil's Handwriting* makes a compelling case for thinking about social action through its psychic investments as the general means by which history unfolds in human action. But for the people it discusses—von Trotha, Solf, et cetera—why they in particular made this or that decision, could or couldn't change, when others from their general background made some other decision, or changed, smells more like fate, *fortuna*, than the structuring principles of psychically informed social action. *The Devil's Handwriting* can only tell us, retrospectively, that these decisions were or were not made, that, retrospectively, these people did or did not change. Whether relying on a "singular, general model" or "generative social structure" the mode our analysis touches ground, this person rather than that person, this massacre, rather than that massacre, will remain caught in the fate of *fortuna*, that the gods of social theory fated someone (might) occupy this psychic and social position, and that we are fortunate or unfortunate to be the one so fated.

Response to Austen and Povinelli

George Steinmetz
University of Michigan

I'm happy to have a chance to discuss *The Devil's Handwriting* with leading representatives of German colonial history and postcolonial theory.

Ralph Austen generously assigns my book to something he calls the fourth wave in colonial studies. Austen associates the first wave of colonial studies with political scientists, the second wave with economists and economic historians (including economic Marxist historians), and the third with postcolonial theorists, who are mainly literary scholars and cultural anthropologists. I agree with Austen's characterization of my book as an example of an emerging approach to colonial studies, an attempt to integrate a social theory of the colonial state with the psychic and discursive concerns of postcolonial theory and the political and economic attentions of the first two "waves." I disagree, however, with his summary of my argument as class reductionist, and will devote most of my space to addressing this theoretically challenging point.

The book does indeed attempt to bring together social theory, history, and postcolonial studies. Yet as Beth Povinelli's comments show, one of the most pervasive sources of misunderstanding is the boundary between the "two cultures" of the humanities and the social sciences. As my remarks on Povinelli's insightful comments should make clear, I reject some of the basic epistemic and ontological claims made by postcolonial theorists. First, they conflate positivist notions of general laws and predictions with historical-realist notions of conjunctural explanation. Second, they endorse a theory of unknowable cultural difference. This produces an oscillation between empiricism and metaphysics or grand theory about difference.

Austen first comments on the role of philology, anthropology, and other proto-disciplines in the overall formation I call "ethnographic discourse." I define "ethnographic discourse" here as including any and all representations of the character and culture of the Other (the Other is variously defined by the proto-ethnographers in question as a race, ethnic group, civilization, or culture). I do not equate anthropology with philology or any other proto-discipline, but insofar as members of any discipline commented on China, South Africa, or Polynesia they become part of the formations of discourse I examine. Rather than equating, say, philologists with anthropologists, I emphasize the internal heterogeneity of formations of precolonial ethnographic discourse. European discourse about China in the 19th and early 20th centuries, for example, involved not just philologists and anthropologists, but also philosophers, geographers, historians, amateur travelers, official explorers, merchants, and even some sociologists (e.g. Ross 1911; von Wiese 1922). Sometimes a given professional group or elite class fraction clusters at a specific pole of an ethnographic formation, but in other cases they are divided. For example European philosophers were completely divided in their views of China during the 18th century: Montesquieu and Herder developed theories of Chinese despotism and stagnation while Voltaire, Christian Wolff, and Johann von Justi praising Confucianism and Chinese civilization.

Second, Austen asks about the relationship between social class and positions taken by colonial actors inside the semi-autonomous field of the colonial state. He suggests that social class may not be tightly linked to ethnographic positions, giving the example of Solf and von Gerlach on German Cameroon. This question concerns the relationship between class background and colonial ideology. I argue that the colonial state was structured like a field in Bourdieu's sense (Steinmetz 2008). It was relatively autonomous from the metropolitan state and also from the colonial field of power, meaning that it did not have to obey the demands of European investors, settlers, and planters. Actors competed inside the colonial state field to accumulate a specific type of symbolic capital, ethnographic capital. Making claims to possess this kind of capital entailed exhibiting a sort of acuity in the judgment of native culture and character, that is, making judgments and acting in ways that appeared to exude ethnic acuity. The colonial state field had its own internal history and its own *illusio*, generating adherence to the game among its participants. Colonial officials entered this field with dispositions and cultural capital generated elsewhere, in metropolitan fields. These dispositions had to undergo certain transformations in order to be "legible" in terms of the codes of the colonial state field. The central intra-elite struggle in metropolitan Germany was a triangular one involving aristocrats, capitalists, and *Bildungsbürger* (the university-educated, cultivated middle class). These were the three main groups represented within colonial administration. Their elite class struggle was transported into the colony. But this intra-elite conflict was not carried out in the same terms inside the colonial state field as in the metropole. *Bildungsbürger* could not hope to dominate the colonial state by exhibiting fluency in Greek and Latin or familiarity with lyric poetry, but instead had to demonstrate their perspicacious understanding of the colonies' natives. Native policy, I argue, was largely a result of this conflict among colonial officials to impose their definition of ethnographic acuity.

According to Austen, Solf's racist views of Africans suggest that social class was not closely linked to "ethnographic" postures. But he is comparing Solf's actions in two different fields—an overseas colonial state and the metropolitan government. Displaying ethnographic acuity was not a ticket to social success within the metropolitan state. To understand the meaning of his anti-African racism during his time as Minister of the Colonies one would need to analyze the metropolitan state. As governor of Samoa, Solf consistently argued that Polynesians were culturally superior to Africans in an effort to prevent the metropolitan colonial office from undercutting his independence and imposing a uniform colonial legal code. He framed his opposition to mixed-marriage between Samoans and European by condemning "half-castes" as the spawn of the devil. Had Solf been posted to China he would likely have moved toward a Sinophile position, but in Samoa he was rather Sinophobic, extending the ban to intermarriage between Chinese laborers and Samoans (Shankman 2001: 129). As for Helmut von Gerlach, he was neither a colonial official nor a military aristocrat but a journalist and a politician who had adjusted his inherited dispositions to those fields. My point is that there would be no need to go to the trouble of analyzing the semi-autonomous logics of fields if class background translated directly into perceptions and practices.

Third, Austen asks about the applicability of my model to other overseas regimes. Actually there are both "generalizable" and "particular" points in my analysis. Concerning generalizability: There is no reason to assume that the theoretical mechanisms I discuss—for example the modern colonial state as a field structured by competition for ethnographic capital—are limited to the German empire or to the 1880s-1914 period. This model of the colonial state can indeed be extended to the modern British and American colonial cases (Goh 2008); historians of British and French colonialism have underscored the importance of symbolic class struggles among the colonizers (see Comaroff and Comaroff 1991-1997; Hall 2002; Lardinois 2008). As for particularity: I argue that this model cannot be extended to earlier modern colonies, in which "native policy" was probably less central. Competition inside those colonial states was structured around different principles. In sum, theoretical mechanisms in the social sciences are never completely generalizable across time and space, but some of them are more widely applicable than others.

Finally Austen asks what, if anything, is peculiarly German in this account. The ethnographic formations I analyze were largely pan-European. What is *nationally* specific is the particular constellation of elite class struggle that is transposed into the colony. The absence of militarized aristocracy or a *Bildungsbürgertum* in the United States, for example, introduced an important difference into the configuration of the American as opposed to the German colonial state fields. But even if American, French, and British colonial states had different constellations of groups participating in them, it is entirely plausible that they were all configured around competition for the same general species of symbolic capital.

Beth Povinelli's criticisms relate to my engagement with postcolonial theory and the place of theory in my book. First, she sees me as rejecting postcolonial theory; I understand myself as rejecting, revising, and accepting different parts of that theory. I revise what I call the "devil's handwriting" thesis (whose foundation is Said 's *Orientalism*), arguing that it moves too directly from a homogenized corpus of "travellers' tales" to imperial practice (Said 1978: 117). I do not reject Said's argument that European visions of the Orient were often profoundly immune to counterevidence and that these visions preceded and shaped later imperial interventions. But I show (1) that ethnographic discourse was strikingly multivocal, to the extent that colonial administrators were under hermeneutic pressure to construct their own position on the colonized; and (2) that the passage from Orientalism to colonialism was mediated by several mechanisms, including intra-elite struggle inside the colonial state. The part of postcolonial theory I fully accept is the Lacanian psychoanalytic approach (associated with Homi Bhabha), which illuminates processes of cross-identification across the colonizer-colonized boundary. The only revision I offer is that "mimicry" is situated primarily in the *precolonial*, pre-conquest contact zones (whereas for Bhabha mimicry is mainly a colonial and postcolonial phenomenon). I certainly never suggested that colonial and postcolonial theorists have failed "to understand the complex social relations of identification … within the colonial world"; indeed, I see this as one of their signal contributions, from Maunier (1932) through Bhabha (1994) and beyond.

Where I have problems with postcolonial theory is the following: Postcolonial theory is often opposed to explanation and comparison and in favor of poststructuralist arguments about incommensurability (Steinmetz 2004). From this perspective, the positivist quest for general laws looks identical to my own historicist critical realism (Steinmetz 1998), since both are concerned with explanation and both reject the reduction of theories to Theory. Let me explain what I mean by this. In critical realism there is room for theories of social mechanisms, and these theories are sometimes portable from one context to another. But social processes or events are almost always determined by a contingent conjuncture of multiple mechanisms. For critical realism, social theories are models, pictures, or stories about these mechanisms. Social theories are not sweeping metaphysical arguments about fate or other universal objects. The mechanisms that social theories are about are almost never universally present —if they are, they are almost certainly natural mechanisms, not social ones. Nor does theory take the form of the covering law, which collapses across the levels of mechanism and event and suggests that the same event is everywhere explained by the same mechanism. For the sake of streamlining my account I focused in *The Devil's Handwriting* on just a handful of causal mechanisms and traced their efficacy in each of the colonial contexts. But I showed that native policymaking was shaped by different combinations of these mechanisms and to different degrees in each context, and that additional mechanisms had to be introduced to explain some policymaking events. For example, the switch in native policy halfway through the German colonial period in Kiaochow was co-determined by a change in German geopolitical goals, but these did not play an important role in the other two colonies.

Povinelli suggests that I am trying to move between general and "mid-range" theory. But if theories are models of mechanisms that may or may not be involved in the genesis of a given event, the idea of middle-range theory does not make much sense ontologically (see Steinmetz and Chae 2002). There are no "middle-range" social objects; there are only empirical events and the theoretical or underlying mechanisms that give rise to them. Robert Merton, who popularized the concept of middle-range theories, insisted that they were close to the "observed data" (1968: 39), which suggested a non-stratified social ontology. By acknowledging that a given middle-range theory could be "consistent" with a wide range of different "broad theoretical orientations" such as Marxism or functionalism, Merton pushed his notion of "middle range" theory in an empiricist direction. Indeed, he summarized middle-range theories as "verifiable statements of relationships between specified variables" (Merton 1968: 52). Merton's concept is incoherent. I could only accept the formula "middle-range theory" if it is defined as a theory of a mechanism that exists in a historically delimited geospace, or if it is understood *epistemologically* as a statement of a lawlike regularity that holds only within limited contextual ("scope") conditions.

Finally, Povinelli asks the question of fate. First, if fate means contingency or accident, I have no trouble with this idea. Critical realism recognizes that contingency and accident are ubiquitous and cannot be bracketed out of historical social science. Second, it is difficult to see how a social science of the sort I am adumbrating here could be compromised by the "fateful" sorting of people into social positions. Only if we believe that subjects "fatefully" execute the scripts of the positions into which they were "fatefully" sorted must we resign ourselves to fate. Human action is driven by both conscious, intentional *reasons* (Bhaskar 1979) and unconscious *strategies* (Bourdieu). Human action can therefore be explained retrospectively as the result of an interaction between structure (this is what Povinelli calls "general background") and agency (reasons and strategies). Of course, a subject's "general background" cannot provide an exhaustive explanation, and action certainly cannot be predicted, contrary to the fantasies of positivist science.

References Cited

Bhabha, Homi. 1994. "Of Mimicry and Man: The Ambivalence of Colonial Discourse." In *The Location of Culture*. London: Routledge: 85–92.

Bhaskar, Roy. 1979. 1979. *The Possibility of Naturalism: A Philosophical Critique of the Contemporary Human Sciences*. New York: Humanities Press.

Comaroff, Jean and John Comaroff. 1991-1997. *Of Revelation and Revolution. Christianity, Colonialism, and Consciousness in South Africa*. Chicago: University of Chicago Press.

Goh, Daniel P. S. 2008. "Genèse de l'État colonial. Politiques colonisatrices et résistance indigène (Malasie britannique, Philippines américaines." *Actes de la recherche en sciences sociales,* numbers 171-172 (March) : 56-73.

Hall, Catherine. 2002. *Civilising Subjects: Metropole and Colony in the English Imagination, 1830-1867*. Oxford: Polity.

Lardinois, Roland. 2008. "Entre monopole, marché et religion. L'émergence de l'état colonial en Inde, années 1760-1810. *Actes de la recherche en sciences sociales,* numbers 171-172 (March) : 90-103.

Maunier, René. 1932. *Sociologie coloniale. Introduction à l'étude de contact des races*. Paris: Domat-Montchrestien.

Merton, Robert K. 1968. "On Sociological Theories of the Middle Range." In *Social Theory and Social Structure*. New York: The Free Press: 39-72.

Ross, Edward A. 1911b. *The Changing Chinese: The Conflict of Oriental and Western Cultures in China*. New York: The Century Co.

Said, Edward. 1978. *Orientalism*. New York: Vintage.

Shankman, Paul. 2001. "Interethnic Unions and the Regulation of Sex in Colonial Samoa, 1830–1945." *Journal of the Polynesian* Society 110 (2): 119–47.

Steinmetz, George. 1998. "Critical Realism and Historical Sociology." *Comparative Studies in Society and History* 39(4): 170–86.

Steinmetz, George. 2004. "Odious Comparisons: Incommensurability, the Case Study, and "Small N's in Sociology." *Sociological Theory* 22(3): 371-400.

Steinmetz, George. 2008. "The Colonial State as a Social Field." *American Sociological Review* 73(4) (August).

Steinmetz, George and Ou-Byung Chae. 2002. "Sociology in an Era of Fragmentation: Alvin Gouldner's *Coming Crisis of Western Sociology* after 30 Years." *The Sociological Quarterly* 43(1): 111-137.

Wiese, Leopold von. 1922. *Briefe aus Asien*. Köln: Rheinland-Verlag.

Methods Training

Institute for Qualitative and Multi-Method Research

Malcolm Fairbrother
University of Bristol

"Qualitative methods are currently undergoing a renaissance in political science," two strong advocates of this trend have reported in recent review essays (Bennett and Elman 2007: 111; Bennett and Elman 2006: 455). For several years now, and as part of this trend, leading methodologists in one of sociology's most closely neighboring disciplines have been organizing an annual training Institute, specifically on qualitative research methods and related topics. I attended this event earlier this year, and I believe it could—probably should—be of substantial interest to many sociologists. Given that qualitative research in political science is usually historical or comparative (or both), the event seemed particularly relevant to readers of this newsletter. Consequently, I thought I would provide a brief account of my experience, along with some comments about the broader context of this event. (The views and interpretations expressed here are my own and should not be taken as those of the Institute's organizers or of anybody else.)

The two-week "Institute for Qualitative and Multi-Method Research" (IQMR), organized by the Consortium for Qualitative Research Methods (CQRM), has now been held seven times. Universities (most but not all of which are U.S.-based) buy memberships in the CQRM, and depending on the level of membership they purchase they are entitled to send one to three participants to the Institute. Most participants are graduate students but a minority are junior academics, just as most are from political science while a minority come from cognate disciplines (sociology, legal studies, geography, etc.). This year, nearly 130 people attended. The Institute consists of a mix of plenary sessions; break-out workshops on topics of more specialized interest; organized meals where a small group of participants get a chance to talk informally with presenters; and research design sessions in which participants receive extensive feedback on pre-circulated descriptions of their current or planned research projects (most often their dissertations). The assigned readings, like the opportunities for networking, are extensive.

The impetus behind the Institute—and indeed of many of the methodological developments to which the Institute is connected—is an interesting story in itself. In 1994, the publication of *Designing Social Inquiry: Scientific Inference in Qualitative Research* by Gary King, Robert Keohane, and Sidney Verba, three prominent scholars in political science, presented a major challenge to qualitative researchers in that discipline. The book—which for good reason has been very widely read, cited, and discussed in political science—bluntly argued that qualitative research was being conducted much less well than it could be, and it made a number of suggestions for improvements. Qualitative researchers have been debating "KKV" ever since, and have responded—productively, from my perspective—in at least three ways.

First, a few have accepted KKV's (and others') critiques of previous qualitative research, accepted their suggested changes, and attempted to apply the new methods as effectively as possible (for one good example, see Moravcsik 1998). Second, spurred on by KKV, many researchers have energetically set themselves the tasks of improving the methodological rigor and expanding the repertoires of qualitative research, albeit not necessarily in ways advocated by KKV (see the reviews by Bennett and Elman cited above). Third, others have responded with swinging criticisms of *Designing Social Inquiry*, and have sought to articulate more forcefully the distinctive advantages of qualitative vis-à-vis quantitative research, even just in pre-1994 forms (Brady and Collier 2004 has been key contribution in this regard).

Active participants in this movement established and continue to present regularly at the January Institute, in order to diffuse the results of all these new innovations and arguments, and to foster further innovation in qualitative methodology.

Topics covered in this year's Institute ranged from the very practical to the very philosophical, though the emphasis was on various facets of research design. Among other things, the assigned readings and the presenters covered: various practicalities of research (fieldwork, writing grant applications, writing journal articles, using archives, interviewing elites); the use of case studies; conceptions of causality, including necessary and sufficient conditions; principles of conceptualization in social science; philosophy of science issues; the advantages and limits of experimental research designs (including lab-based, field-based, and natural experiments); linkages between qualitative and quantitative research (such as the use of regression analyses in selecting cases for more in-depth, qualitative analysis); process tracing (or within-case, causal process observations); and qualitative comparative analysis (including with fuzzy sets).

I did not agree with everything I heard or read, of course. (And while there were few open debates, some of the presenters clearly disagreed with each other.) But in my view the overall quality of the material presented was high. In terms of sociological content, Theda Skocpol's *States and Social Revolutions* was probably mentioned more often than any other single piece of research, and two regular presenters at IQMR are based partly in sociology (Jim Mahoney and Charles Ragin). The growing methodological sophistication of qualitative research in political science, I think, represents both an opportunity and a challenge to macro-comparative/historical researchers in sociology. An opportunity, insofar as the new tools of comparative and historical political scientists are freely available and sociologists should be able to put them to good use. A challenge, insofar as increasingly sophisticated and rigorous qualitative work in political science could threaten to outshine similar work by sociologists.

For more details about the Institute, see: http://www.asu.edu/clas/polisci/cqrm/.

References Cited

Bennett, Andrew, and Colin Elman. 2006. "Qualitative Research: Recent Developments in Case Study Methods." *Annual Review of Political Science* 9: 455-76.

Bennett, Andrew, and Colin Elman. 2007. "Qualitative Methods: The View From the Subfields." *Comparative Political Studies* 40[2]: 111-21.

Brady, Henry E., and David Collier (eds.). 2004. *Rethinking Social Inquiry: Diverse Tools, Shared Standards*. Lanham, MD: Rowman & Littlefield.

King, Gary, Robert O. Keohane, and Sidney Verba. 1994. *Designing Social Inquiry: Scientific Inference in Qualitative Research*. Princeton, NJ: Princeton University Press.

Moravcsik, Andrew. 1998. *The Choice for Europe: Social Purpose and State Power from Messina to Maastricht*. Ithaca, NY: Cornell University Press.

Teaching Comparative Historical Sociology

Editors' Note: In the Fall 2007 Issue of Trajectories*, we ran a feature on "Teaching Comparative Historical Sociology," with essays from John Foran, Mounira Maya Charrad, Jeff Haydu, and Mathieu Deflem. The feature prompted this response from historian Christopher Thompson.*

Teaching Comparative and Historical Sociology: A Comment

Christopher Thompson
University of Buckingham

The discussion of the challenges inherent in teaching comparative and historical sociology to undergraduates and graduates (*Trajectories, Volume 19, No.1, Pages 4-17*) was bound to be of interest to historians, political scientists and others because of its intrinsic importance. The distinguished scholars who contributed described the balance each struck between introducing their students to the methodological debates and skills they needed to acquire, to the lessons to be learnt from the evaluation of case studies and of specific books, and to the preparation of research proposals. University teachers everywhere will recognize the range of strategies they employed. Naturally enough, there was a degree of common ground between them but also some important differences – for example, over attitudes to the founding fathers of sociology and on the significance of methodology – to be found.

The contrast with the training of academic historians is striking. Undergraduates in the United Kingdom, where I have taught for several decades, are expected to read widely amongst the secondary sources, i.e. articles in historical journals and academic books, and to be fully familiar with the analysis of documents, the relevance of art, architecture, cultural artifacts, etc., by the time they finish their degrees. All these form part of the course, *Exploring History 1400-1900*, that I am currently teaching. By then, they should be familiar with the debates under way in particular areas of the subject and with the broader explanations current in the discipline. Postgraduates are progressively introduced to the range of archival and bibliographical resources available, to the challenges posed in formulating a research proposal and the problems that inevitably arise in carrying it out as well as to the contingent claims to knowledge made in historians' writings. The and only then are they allowed to undertake research (under supervision) itself. It is a much more structured approach and a far cry from my own experience.

A serious gap thus exists between the two intellectual disciplines. As an historian, it is surprising, even disconcerting, to find a rival claim being made to one's own territory. But we cannot claim absolute property rights here or elsewhere. Most historians *in my experience* tend to be empiricists. They regard the major figures in sociology – Durkheim, Marx and Weber – and its modern practitioners as potential sources of interesting hypotheses to be tested against the surviving records but rarely as authorities themselves. I can only recall one reference in a footnote to Jack A. Goldstone's work, *Revolution and Rebellion in the Early Modern World*, in a serious historical work, and none at all to the books of Rosemary Hopcroft, Richard Lachmann and Edgar Kiser, among other prominent historical sociologists. This is because historical sociologists rely less on archival research than many historians and are also not directly engaged with debates at the frontiers of historical research. By the time such debates surface in the historical journals a year or two later, the focus of discussion has moved on. The mastery of methodological techniques and the modeling of new forms of analysis are much less important to historians than the discovery of new sources or of novel ways of exploiting known ones and the formulation of sound arguments on the basis of the extant evidence. Historians' studies are inevitably less tidy, more cautiously phrased and contingent than those of comparative historical sociologists. Large-scale sociological theories of the kind advanced by the late Barrington Moore and by more recent figures tend to disintegrate under historical examination.

There does, moreover, appear to be some lack of understanding on the part of comparative and historical sociologists about the activities and attitudes of historians. It is certainly untrue to claim that historians are not analytically orientated or that they have left the "lower classes" of earlier societies out of their works. Historians like Bernard Waites have been at least as keen as any sociologist to examine the pasts of parts of the world well beyond Europe and North America. What they are insistent upon is the need to test general explanatory schemes, medium-level ones and micro-historical explanations against the evidence. This is where historians' scepticism about the claims of comparative and historical sociology has its roots: it is also why historians are indifferent to and often ignorant of the work in your field. I suspect that the teaching experiences of John Foran, Mounira Maya Charrad, Jeff Haydu and Mathieu Deflem and the learning experiences of their students would have been significantly different, indeed better, if their colleagues in the History Departments of their universities had been more involved or present.

What, if anything, can be done to open a new dialogue? I do not think it helpful for sociologists to regard the works of historians as a fertile area to be raided for trophies to be carried off to decorate their hypotheses any more than for historians to view comparative and historical sociologists' studies as examples of infertile factual error, intellectual failure and explanatory overreach. In its teaching and training methods, comparative historical sociology certainly needs an infusion of empirical rigour and a much greater contribution from working historians. In return, there are interesting hypotheses and techniques to be offered. The dialogue should start somewhere. Why not here?

Identities

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

Peter Bearman
Columbia University

I was asked to write an essay on how I became a historical sociologist and this is a story organized around the various ways that one could tell a story.

Imprinting: I was born in Virginia in 1956 and Virginia in the 1960s was just breathing history. Everywhere we turned there were revolutionary war monuments and civil war battlegrounds. We dug for Indian relics along the Potomac River where we stood on the rocks and speared herring as they ran the rapids. None of that had any relationship to my becoming an historical sociologist – as far as I can tell. And besides, I am not keen on just-so stories of this particular type.

Fate: We were always already back then historical sociologists. I don't have data to support the following claim (who really cares) but my generation "came of age" in sociology before the domestication of historical sociology and comparative historical sociology into a sub-discipline of sociology, on par with now other forty-one sub-disciplines, like culture, animal-human relationships, and so on. As graduate students, most of us assumed that sociologists had a theory of history (or at least a theory of action); and most of us agreed that sociologists should work on and develop an understanding adequate at the level of meaning of really important substantive phenomena, many of which occurred in the past, and all of which were profoundly shaped by what occurred in the past. So we were always historical sociologists, or wanted to be. Or I always wanted to be anyway.

Overcoming challenges: I got a rude awakening my first summer in graduate school when I read a paper in the *ASR* which looked at all of the current characteristics of the United States – facts like 3% of the population were farmers, and so on – and then looked back to when all these end states started and concluded that all of the things that brought us to where we were (all those "historical processes") had ended. That paper raised a deep question. Was this the end of "the end of _________" movement in sociology, like the end of ideology.

Becoming: Back to the account at hand – how I became a historical sociologist, not. One of the things about becoming processes is that for those who do not become things like alcoholics or Nazis that they involve the accretion of persons onto the self. So they involve networks. Here is the network story. I knew Margaret Somers and Margaret Somers was one of the reasons I went to see George Homans.[6] With Skocpol, Somers was busy laying down the lattice-work that would become the foundation for comparative historical sociology. Somers was close to Skocpol and Skocpol was close to Homans. Maybe I thought I might get some of the magic powder too.

The wrong path: So, one fine day I had a conversation with George Homans. Homans sat behind his desk and looked out the window. I sat in front of his desk and looked at him as he looked out the window.

Bearman: I am writing my dissertation on 16th century elite social structure in Norfolk, and …

Homans: Norfolk didn't exist in the 16th century.

Bearman: (*In some doubt, but trying to hold my own*). Well, you are probably thinking that Norfolk was part of Suffolk, it was -- but by the 16th century it was independent, so …

Homans: Suffolk didn't exist in the 16th century.

Bearman: (*Seriously wondering what I had been doing for the past year but*) Well, I don't know about that, but I have collected data on local gentry

6 One of my students didn't know who George Homans was. I won't name names. But for those of you in the same boat – Homans started the bringing the " _______ in" movement in Sociology.

and I am modeling the structure of kinship networks over time in order to …

Homans: You think you are doing that but there were no people in Suffolk or Norfolk in the 16th century.

Bearman: (*Working hard to convince myself that I existed*) Ok, that's possible if what you mean is that "the individual" is a product of modernity – that is what my dissertation shows – how modernity emerged – but people were still real…..if you know what I mean …..

Homans: (*Turning around to look at me*) I have no idea what you are talking about.

So, we stared at each other for a while. The names of all the dead people from the 16th century I had memorized filled the air; Nathaniel Bacon, Edward Coke, Francis Gawdy, William Paston, Thomas Knyvett, Nicholas L'Estrange, Le Strange Mordaunt, the other Knyvett's, Gawdy's, Peytons, Farmers, Lovells; the Protestant saints and Catholic recusants. Turning back to the window, Homans said: "Oh you mean Norfolk England; I thought you meant Norfolk, Massachusetts." We talked a minute longer. I described the paper that would become *Generalized Exchange* a decade later. Homans concluded the conversation by stating "evidently, you don't know anything about kinship or history". I never bothered to try to talk to Homans again. That was my loss. For the record, he was dead right about history.

Moving forwards: Back to Somers: Somers was a couple of years ahead of me so I could stand on her shoulders a bit. She was working on the 19th century English labor movements and about when I was thinking about writing an historical thesis she was at the tail end of the 15th century, about to enter the 14th. Her historical descent rate was just over 27 hours a day.[7] What I discovered talking to Somers was that things happened and then other things happened and then other things happened and all of those things got piled up on one another in a giant temporal stack and that trying to find the cause of why things happened led irretrievably to a great descent into the murky past without a stopping rule. I got interested in the 18th century after reading *Internal Colonialism* and watching Somers, and on my own rapid descent into the past, I made the firm decision to stop in 1540 and move forward. So my first (and only?) decision as a historical sociologist was to stop being historical. I have never turned back.

The future: Sometime in the mid 1980s Skocpol said that "the future of historical sociology lay in social networks". I was just starting out on the job market and I would start my talks with that pronouncement – and then in a quite remarkable exhibition of arrogance announce that I was pleased to say that that the future had arrived. That was way off the mark. We are not yet there. And we can hardly argue that we are closer. It seemed possible to get to that future faster back in that past. Just then we were beginning to challenge two dominant views. The first was that people acted on the basis of interests that we [sociologists] would impute to them on the basis of their categorical positions. The early network revolution revelation that identity -- shaped by embeddedness in complex interweavings of social relations -- mattered as a foundation for action opened up all sorts of new possibilities. The second revelation that sequence mattered for meaning – a revolution of thought led by Andy Abbott – was equally huge in challenging the mindlessness of the single pursuit of cause (over meaning) as constitutive of our sociological practice. But most of the work on sequences and networks and on events and history presumes that there is some sequence and some network, that events have some meaning, and that the work of historical sociology is to uncover the reality that was then. I think I am late to the party, but the disassembling of this strangely static view and the creation of a totally new way to conceptualize, represent, and measure the ways in which the things that happen in the past carry multiple meanings simultaneously by virtue of their embedding in multiple interwoven event sequences, the project of historical sociology today, is enormously enticing.

[7] Unfortunately, I have lost the super fancy algorithm I developed to precisely quantify historical descent rates and so this is a rough approximation based on fading memory.

Karen Barkey
Columbia University

It is said that often social scientists study the subjects that help explain the influences that shaped their lives, the contradictions and tensions inherent in the settings in which they lived, letting some of the personal guide the research questions of their academic lives.

In many ways my encounter and choice of historical sociology as an academic field to pursue was determined by the setting in which I grew up as well as the trajectory of my education. Whether I understood it as such or not, in hindsight, it is clear to me that the source of significant tension in my upbringing was the dramatic top down transformation of the society in which I lived and this in many ways determined much of my curiosity.

I grew up in Istanbul, Turkey at a time of strong nationalist and secular Kemalist (after the leader of the War of Independence and the Republic of Turkey Mustafa Kemal Ataturk) ideology, born to a small westernized Jewish community, in which the generational gap between the lives of my grandparents and my parents was deep and reflected the dramatic transformation of Turkish society. My initial interest was aroused by a statement my grandfather made when I was just starting to be politically conscious explaining to me that "as a minority" he preferred the imperial Ottoman system of rule to the new Turkish Republic. In the latter, everyone was equal in principle, but discrimination was widespread in public offices. He compared this to his previous existence in the Ottoman empire, where as a Jew, he knew what the rules and regulations were, and kept within known boundaries. At the same time he also told stories of the easy diversity he grew up with, living in the midst of communities of different religions and worship. He very much continued his life of imperial subject where he could. We lived every summer in his house at the Princes Islands, on one of the most multireligious and multiethnic islands, Buyukada or Prinkipo as Greeks called it. I went with him many Saturdays on his long march to the top of the hill, to the Greek Orthodox Monastery of Saint George, where he enjoyed both the scenery and the quietude of a sacred place. Our neighbors had a very small Orthodox Church in their extensive garden, another religious space visited by all, Greeks, Jews and Armenians who lit candles while making detailed wishes! Languages buzzed around me, Armenian, Greek, Ladino, the languages of each community and French and English, languages that any self-respecting minority knew and boasted. I adopted the Greek word "papu" for my grandfather, thinking their word was as familiar as ours. This old-world diversity was marked by the easy fluidity of identities, a sharing of cultural markers, a crossing of boundaries.

In the winters we returned to the city where a much more uniform, nationalist and secular educational culture took over, even though I attended a French Catholic Nun's school throughout middle and high school years. The French school followed the Turkish Ministry's educational curriculum, adding on a parallel French curriculum. In the Turkish curriculum, courses of "citizenship," geography and history instilled the Turkish national ideology, Kemalist secular thought and the modernist identity of the west. The French curriculum was infused with French secularism, while the Catholic nuns taught courses in Judaism and Christianity. As such the secular and uniform ideals of the modern state hardly fit with the reality I experienced on the ground. Despite that, between the indoctrination of schools and the intense modernity, western secularity of my father I adopted a much more Kemalist outlook to life. I also understood the manner in which the modern Jewish Turkish citizen could find a place in society, hide his/her insecurities through his/her modernity, industry and appeal to western values of the Enlightenment.

The tensions between these different world-views, the Ottoman style that permeated the older generation and the Kemalist vision of uniformity and modernity did not always marry well. I worked hard during my high school years to understand these two worlds and bring them together, as I became increasingly aware of their incompatibility. What I was to understand later was the contradictions between different political formations, the imperial and the nation-state. By the time I graduated from high school and decided to go abroad to study, violence ripped apart at the fabric of Turkish society in a typical developing world reproduction of the Cold War, localized struggles between Marxist-Leninist youth movements and the conservative right as well as an ultra right nationalist-Turanist movements. They battled in the political arena, but also at the university.

When I went to college in the US I had been already warned by my high school philosophy teacher that the field that would bring all these issues together for me was more likely to be sociology than philosophy. I therefore came to Bryn Mawr intent on focusing in sociology to at least find if it was compatible with my questions. By the time I was writing an Honors thesis in sociology I had started to work on issues minority-majority relations in divided societies and used the case of Ottoman Jews as the case of a loyal, apolitical community that underwent the transition from empire to nation-state without too much upheaval, compared to other communities that became nationalist and aspired to separation from the empire.

For graduate school I promised myself to understand better the minority/majority question in two different political formations, the empire and the nation-state. Beginning graduate school at the University of Washington under the mentorship of Daniel Chirot was crucial to the foundations of my becoming a historical sociologist, to learning from him the value of a comparative political analysis that insists on asking questions about large-scale outcomes that are substantively and normatively important and have an impact on the world in which we live. In Seattle, I read feel like I read continuously, social history, political history, the Annales School, and listened to continued analyses of the historical continuities and discontinuities in political struggles. As Dan had written on Romania as well, our interests coincided in Balkan history and I delved into the history of Balkan nationalism, as apart of the larger puzzle of imperial decline and transformation. As I studied the Balkans, my fascination with "empire" grew. My interest was focused on how different ethnic and religious groups lived together, how empires ruled different peoples, and the conditions under which relatively peaceful coexistence could become contentious, violent and destructive.
All of this seemed to turn for me around the question of the type and the nature of political formations, in this case the empire as a form of rule. I focused on the actual workings of empire, to uncover the manner in which empires became such powerful political formations, ruled differentiated groups, and maintained cohesion in times of upheaval. In such a moment of upheaval – a period of widespread banditry – I discovered an important key to empire: that empire was a "negotiated enterprise," and regardless of its strength an empire has to work with the peripheries in order to maintain a mix of compliance and tribute and military cooperation, as well as ensure political coherence and durability. This became the theme of *Bandits and Bureaucrats*.

This theme is further developed in my new book *Empire of Difference: The Ottomans in Comparative Perspective*, where my main interest is to understand the longevity of empire as a political form. I carry out an analysis of the Ottoman Empire's social organization and mechanisms of rule at carefully selected moments of its history: emergence, imperial institutionalization, imperial remodeling, and transition to nation-state, comparing it to similar processes in other empires. I think it is in this book that my identity as an analytically inclined historical sociologist is clearest. I am trying to understand how institutional and organizational structures enable or hinder the action of agents and networks of agents whom I consider crucial to my analysis. Developing an explanation for the longevity of empire for me means reconstructing a relatively faithful representation of a social process, identifying the typical actions, interests, and meanings of agents, and networks of agents, relating to each other through webs of association.

In many ways I feel like I tied some of the loose ends of my past experience in this book, in ways that might have led to other questions, but still give me a sense of intellectual satisfaction and further curiosity. For example, the question of coexistence, dissent and violence in societies divided along religious and ethnic lines is at the center of my experience and of this book. I argue for an organizational understanding of Ottoman toleration, a modus vivendi that came from top down and bottom up (community leaders) interest in maintaining inter-ethnic peace. I show that this easy diversity and fluidity of imperial co-existence was actually the product of much organizational coordination, legibility and boundary maintenance, which was also fragile and liable to break down. I also try to understand why the Ottomans tolerated

Christians and Jews, but persecuted their own Muslim heterodox brethren. In many ways, these questions were originally relevant to my past experience, but as I became more infused in my trade, I also saw how the issues of diversity, multiculturalism and the relationship between religion and politics are still at the core of our intellectual queries. Having analytic historical comparisons only enhances our tool-box and provides us with a distinctive understanding of our often beleaguered present.

Jason Kaufman
Harvard University

I never intended to become an academic. Classical music was my first love. But after a few months at a highly competitive conservatory — the Curtis Institute, in Philadelphia — I realized that there was more to life than etudes and orchestral excerpts. Fortunately, my mother, had made me apply to college at the same I was auditioning at conservatories. Mom must have known (or at least hoped) I would be a music-school drop-out…

That year in Philadelphia taught me a lot. I started reading the newspaper every day — as a classical trumpeter, one spends a lot of rehearsal time waiting for something to do — I walked the length and breadth of downtown Philadelphia; and I went to the Philly Museum of Art every Sunday morning, when it opens its doors to the public for free. At some point, I'm not sure exactly when, I came across a copy of David Reisman's classic, *The Lonely Crowd*, and it changed my life.

Reisman's book struck a chord with me. I'll never forget the sensation. *The Lonely Crowd* discussed topics of direct relevance to my paltry 18-year-old existence while mustering evidence from centuries of history. I decided then and there that what I wanted most from life was to feel that way every day: stimulated, engaged, confused, and amused, all at the same time. Macro-historical inquiry seemed to be the ticket.

The next fall, I started as a freshman at Harvard. I had no idea what I wanted to study, except that I knew I *did not* want to study music. (I am a musically active person today, but I sorely needed a break at that point in my life.) I had a vague sense that I was interested in history and philosophy, but I only had bad experiences with those kinds of courses as a freshman: history courses were too detailed, too a-theoretical for my ken; philosophy courses seemed too theoretical, not grounded enough in real life. Feeling lost and hopeless, I did what many other future-sociologists do: I majored in Social Studies.

Despite its infantile name, Harvard's Social Studies program has produced an astonishing array of outstanding sociologists, particularly in the comparative-historical vein. A year-long sophomore social theory course is the anvil on which Social Studies types are forged. Tocqueville, in particular, surfaces frequently in my scholarly work. Social Studies also encouraged in me, for better *and* worse, a desire to think like a 'fox' instead of a 'hedgehog': we had to take courses across the disciplines and were encouraged to mix-and-match topics and approaches. Methodology took a backseat to theoretical scope and ambitious. (In the spirit of full disclosure, I later served as Harvard Sociology's Director of Undergraduate Studies. There are many subtle differences between the Sociology and Social Studies programs, but epistemology is the most striking.)

My college years also introduced me to a strange but powerful mix of mentors. John Skrentny was at the time a graduate student in sociology and a "non-resident tutor" in my residential college. We became acquainted in the computer terminal room at William James Hall (this being before the age of laptop computers and plug-and-play statistical software), started having lunch regularly, and have been close friends ever since. When I asked John what is was like to be in graduate school, he told me, "It's wonderful. It's like everyday is Saturday...."

Saturday, indeed — if you normally spend your weekends in the library…

A less-likely pair of mentors were Aage and Mette Sorensen. I chanced into a research assistant job with Mette, who needed someone to do SPSS programming for her. I was almost totally incompetent in SPSS and even worse in statistics, but Mette was nice to me, and the computer staff in the sociology department helped me along. After a successful year, Mette passed me on to her husband, Aage, who was one of the great early quantitative sociologists. He regaled me with stories about working for James Coleman and doing regression analysis by hand. I only later learned that Aage had an abiding interest in comparative-historical sociology. I worked for him for two years and we remained friends thereafter. He was later instrumental in bringing me back to Harvard as a junior professor.

My Social Studies sophomore tutorial instructor was Liah Greenfeld, an eclectic, razor-sharp, and extremely well-read sociologist. When I submitted a paper using *The Beastie Boys* to assess the sociological relevance of Weber's "Class, Status, and Party," Professor Greenfeld called me into her office. I was sure she was about to fail me out of the course. Instead, she praised the paper and urged me to consider graduate school.

So graduate school it was. In keeping with my hip-hop themed research — I'd written my senior thesis on the political potential of rap music — I entered Princeton thinking I was going to keep studying popular culture. Several outstanding historical sociologists there convinced me otherwise. Miguel Centeno led a scintillating, year-long seminar on macro-historical classics. Viviana Zelizer showed me how to handle complicated ideas and complex historical data in ways that are accessible, engaging, and enlightening. Bob Wuthnow, Paul Starr, and Paul DiMaggio all excelled at historical analysis. Disciplinary boundaries were also quite weak at Princeton, and I was free to take courses in classics and history. A course on "Democracy and Politics in Ancient Athens" convinced me of my passion for history — for the longest time, I wanted to be the world's authority on plumbing and sewerage in antiquity. Courses on legal and medical history also got me interested in some of the less-well-traveled avenues of American history — my interest in legal history resurfaced recently while trying to make sense of the contrasting political trajectories of the United States and Canada.

While still at Princeton, Chuck Tilly allowed me to join his "Contentious Politics" seminar at the New School, and later Columbia. He was truly an exemplary scholar — attentive, open-minded, encouraging, energetic, and knowledgeable about seemingly every topic in the universe. I started doing research on AIDS/HIV and social movements, and under Chuck's tutelage, I ended up learning more than a little about earlier efforts to combat sexually transmitted disease. I was hooked.

Whereas Chuck took a soft-shoe approach to mentoring students, my experience with my dissertation adviser, Frank Dobbin, was a bit more hands-on. Frank took me on as a cocky young upstart and molded me into a somewhat-more-serious sociologist. He has had a huge impact on my work, my career, and my intellect. We fought a lot about the marriage between theory and evidence, but fighting is not always a bad way to learn. I would write fanciful, ambling papers and he would cut them down to size. Eventually, I produced a colorful dissertation on the history of municipal institutions in 19^{th} century America and a hard-core empirical analysis of the impact of various kinds of civic organization on 19^{th} century American municipal spending. The latter was published in AJS, much to my surprise, "and the rest" — *has someone already called dibs on this?* — "is history…" The history of fraternal lodges and labor unions, the history of cricket and baseball, the history of Canada and the United States, the history of Vermont and New Hampshire…

I do several kinds of sociological research today, but comparative-historical sociology remains my bread and butter. Now if only every day felt like Saturday.

Recent Dissertations

Julie Collins-Dogrul
University of California, Davis
December 2007

Managing Transnational Problems for the National Good: U.S.-Mexico "Border Health," 1942-2002

Abstract
This dissertation elaborates a broker-centered theory of transnational organizational fields to explain how U.S. and Mexican non-profit and government organizations coalesce around shared public health problems. Using historical comparative methods I analyze the border health sector's period of emergence from 1942 to 1952, and period of change from 1992 to 2002. I explain how systemic brokers foster transnational networks, governance systems, and discourse, but also how they enable the U.S. to pursue national interests in Mexico – interests that shape a particular process of border health social problem construction that privileges some patients, public health problems, and programs over others. Thus, while this dissertation shows how organizations can work together on transnational social problems it is also a cautionary tale that illustrates how power inequalities operate within transnational fields ultimately helping dominant actors shape agendas and construct social problems that reflect their interests.

Elizabeth Helen Essary
Duke University
2008

Latent Destinies: Separatism and the State in Hawai'i, Alaska, and Puerto Rico

Abstract
This dissertation offers a comparative historical analysis of the role of the state in shaping nationalist sentiment in Hawai‘i, Alaska, and Puerto Rico during the twentieth century. There are active separatist movements within the three territories, yet there is important variation among them in terms of rationale, organization, and goals. More specifically, the Hawaiian Sovereignty Movement draws on ethnic dimensions of nationhood to pursue greater self-determination. By contrast, the Alaskan Independence Party evokes civic rhetoric to justify secession. The case of Puerto Rico *independistas* is characterized by a hybrid of ethnic and civic sentiment, where being *Puertorriqueño* is simultaneously a heritage and a political orientation. I explain such differences as a result of United States federal legislation throughout the process of incorporation. From the moment a territory is annexed, various policies are directed at organizing and integrating the new populations. As a latent effect, these policies both reinforce the status of the territory and its residents (vis-à-vis the American populace) and grant disparate privileges within the territory's population. In turn, these dynamics shape a collective identity that coalesces around separatism. Using archival materials, I focus on status legislation, land rights, and language, illustrating how policy in these arenas created the rationale and rhetoric for subsequent separatist movements. My research contributes to understanding how identity and legislation, nation and state, are mutually constituted.

New Publications of Section Members

Almeida, Paul D. 2008. *Waves of Protest: Popular Struggle in El Salvador, 1925-2005.* Minneapolis: University of Minnesota Press (Social Movements, Protest, and Contention Series).

Arjomand, Said A., ed. 2008. *Constitutional Politics in the Middle East with special reference to Turkey, Iraq, Iran and Afghanistan.* Oxford and Portland: Hart Publishing.

Arjomand, Said A., ed. 2007. *Constitutionalism and Political Reconstruction*. Leiden: E.J. Brill

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

Bandelj, Nina. 2008. *From Communists to Foreign Capitalists: The Social Foundations of Foreign Direct Investment in Socialist Europe*. Princeton: Princeton University Press.

Bergesen, Albert J, 2007. "Three Step Model of Terrorist Violence." *Mobilization.* 12 (2): 111-118.

Bergesen, Albert J., ed. 2008. *The Sayyid Qutb Reader: Selected Writings on Politics, Religion, and Society.* London: Routledge.

Breznitz, Dan. 2007. *Innovation and the State: Political Choice and Strategies for Growth in Israel, Taiwan, and Ireland*. New Haven: Yale University Press.

Bryant, Joseph M. 2006. "The West and the Rest Revisited: Debating Capitalist Origins, European Colonialism, and the Advent of Modernity." *Canadian Journal of Sociology* 31(4): 403-44. The article is now the focus of a debate exchange on the "Rise of the West / Great Divergence" problematic, in the *Canadian Journal of Sociology,* vol. 33, issue 1, with contributions from Jack Goldstone, Rosaire Langlois, Joseph M. Bryant, and Mark Elvin.

Calvo, Esteban and John B. Williamson. 2008. "Old-Age Pension Reform and Modernization Pathways: Lessons for China from Latin America." *Journal of Aging Studies* 22(1): 74-87.

Chung, Chi-Nien, and Xiaowei Luo. 2008. "Human Agents, Contexts, and Institutional Change: The Decline of Family in the Leadership of Taiwanese Business Groups." *Organization Science* 19: 124-142.

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. Springer Publishers.

Ermakoff, Ivan. 2008. *Ruling Oneself Out: A Theory of Collective Abdications*. North Carolina; Duke University Press.

Fischer, Claude S. and Michael Hout. 2006. *Century of Difference: How America Changed in the Last Hundred Years*. New York: Russell Sage.

Gotham, Kevin Fox. 2007. *Authentic New Orleans: Race, Culture, and Tourism in the Big Easy*. New York: New York University Press.

Hall, Thomas D. and James V. Fenelon. 2008. "Indigenous Movements and Globalization: What is Different? What is the Same?" *Globalizations* 5(1): 1-11.

Haydu, Jeffrey. 2008. *Citizen Employers: Business Communities and Labor in Cincinnati and San Francisco, 1870–1916*. Ithaca: Cornell University Press.

Ho-fung Hung. 2008. "Rise of China and the Global Overaccumulation Crisis." *Review of International Political Economy* 15 (2).

Kardulias, P. Nick and Thomas D. Hall. 2007. "A World-Systems View of Human Migration Past and Present: Providing a General Model for Understanding the Movement of People." *Forum on Public Policy*, on-line:

http://www.forumonpublicpolicy.com/archivesum07/kardulias.pdf

Koenig, Matthias and De Guchteneire, Paul (Hg.). 2007. *Human Rights and Democracy in Multicultural Societies*. Aldershot: Ashgate.

Koenig, Matthias. 2007. "Europeanising the Governance of Religious Diversity? An Institutionalist Account of Muslim Struggles for Public Recognition." *Journal of Ethnic and Migration Studies* 33(6): 911-932.

Koenig, Matthias. 2008. "Institutional Change in the World Polity - International Human Rights and the Construction Of Collective Identities." *International Sociology* 23(1): 97-116.

Larson, Erik and Ronald Aminzade. 2007. "Nation-States Confront the Global: Discourses of Indigenous Rights in Fiji in Fiji and Tanzania." *Sociological Quarterly* 48(4): 801-831.

Larson, Erik and Ronald Aminzade. 2007. "Neo-Liberalism and Racial Redress: Indigenization and Politics in Tanzania and Fiji." *Research in Political Sociology*, special issue on "Politics, Neo-Liberalism: Structure, Process, Outcome," vol. 16, edited by Harland Prechel.

Martin, Isaac. 2007. *The Permanent Tax Revolt: How the Property Tax Transformed American Politics*. Palo Alto: Stanford University Press.

Patil, Vrushali. 2008. *Negotiating Decolonization in the United Nations: Politics of Space, Identity and International Community*. London: Routledge.

"Politiques Impérialistes. Genèses et structures de l'état colonial." *Actes de la recherche en sciences sociales*. Special issue (171-172), with original articles (in French) by Michael Mann, Yves Dezaley and Brian Garth, Daniel P.S. Goh, Roland Lardinois, Romain Bertrand, and George Steinmetz.

Ragin, Charles C. 2006. "How to Lure Analytic Social Science Out of the Doldrums: Some Lessons From Comparative Research." *International Sociology* 21(5): 633-646.

Ragin, Charles C. 2006. "Set Relations in Social Research: Evaluating Their Consistency and Coverage." *Political Analysis* 14(3): 291-310.

Ragin, Charles C. 2007. "Comparative Methods." In *Handbook of Social Science Methodology*, edited by Stephen Turner and William Outhwaite. Thousand Oaks: Sage.

Ragin, Charles C. and Claude Rubinson. 2007. "New Methods for Comparative Research?" *Comparative Social Research* 24: 373-390.

Steinmetz, George. 2007. "The Historical Sociology of Historical Sociology: Germany and the United States in the 20th century." *Sociologica* (*Italian Journal of Sociology online*) 3.

Thomas D. Hall. 2007. "Incorporating North America into the Eurasian World-System, in World System History," in *World System History*, edited by George Modelski , and Robert A. Denemark, in *Encyclopedia of Life Support Systems (EOLSS),* Developed under the Auspices of the UNESCO, Eolss Publishers.

Vogel, Mary E. 2007. *Coercion to Compromise: Plea Bargaining, the Courts and the Making of Political Authority*. New York and Oxford: Oxford University Press.

Vogel, Mary E. *Crime, Inequality and the State*. London: Routledge.

In the next issue of *Trajectories*:

Special Feature on:

Remembering Charles Tilly

…plus a review of Giovanni Arrighi's *Adam Smith in Beijing* by Janet Abu-Lughod and more!

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