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A Sociologist Among Economists

Some Thoughts on Methods, Positionality, and Subjectivity

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I arrived in Mexico City in the summer of 1995 with a vague area of interest rather than a clear hypothesis. I was generally interested in how U.S.-trained economists had come to be so influential in the Mexican government, and how changing economic ideas had contributed to Mexico's move to free-market economic policies. The topic was timely. For more than a decade, technocrats with advanced degrees from American universities had been liberalizing the Mexican economy. Universally praised by the international business press, these technocrats had suddenly fallen out of favor, with the sudden, sharp devaluation of the peso at the beginning of that year, which was followed by a tremendous economic crisis.

Early on, I decided that my original "take" on these issues would be to study the historical evolution of the Mexican economics profession over the course of the 20th century. My doctoral dissertation, which was the basis for what later became the book *Managing Mexico: Economists from Nationalism to Neoliberalism* (Princeton University Press, 2001) ended up drawing on a quirky smorgasbord of methods, including primary and secondary historical sources, interviews with key insiders, an analysis of the career trajectories of graduates from Mexico's top economics program

(ITAM), an analysis of trends in public and private higher education in Mexico based on government data, and a content analysis of 287 undergraduate economics theses. In this essay, I re-create the story of how I arrived at these methods, and my experiences in deploying them. In the first section, I discuss how I arrived at my particular combination of historical methods and sources. In the second section, I discuss how I negotiated the dilemmas of my own position vis-à-vis my subjects.

Negotiating Historical Methods

Historical sociology is not a method in itself, but rather encompasses the “study of the past to find out how societies work and change” (Smith, 1991, p. 3). It therefore encompasses a myriad of topics and a variety of methodological approaches. Sociologists looking to make broad historical generalizations about more than one national case tend to rely heavily on the synthesis of secondary sources, because it is usually not feasible to find and analyze primary sources for long spans of time and multiple national contexts. Perhaps the most famous example of this sort of research is Barrington Moore’s *Social Origins of Dictatorship and Democracy*, which examines historical origins of political regimes in the United States, France, England, Germany, China, and Japan (Moore, 1966).

Because I was only looking at a single case, I did not wish to confine myself to synthesizing secondary literature, particularly as the existing literature on the topic in which I was interested was extremely sparse. However, analyzing primary documents presents historical sociologists with a dilemma: when one chooses to adopt the methods of historians, it becomes much more difficult to draw the larger theoretical conclusions favored by sociologists. None of my dissertation committee members was a Latin Americanist, and none of them was interested in Mexican economists as a topic of study per se; they wanted me to tell a story that had implications that could be of relevance to nonspecialists—about the relationship between the state and professions, how policies change, how external pressures shape organizations, and so on. This meant that I could not afford to lose sight of the forest for the trees. I vividly recall conversations that I had with a historian friend in Mexico City, who failed to understand how I could possibly be doing serious research, because I was only willing to spend a week or two looking for archival documents on Mexican technocrats in the 1930s, and subsequently moved on to other decades, sources, and subjects!

Of course, I was interested in many other decades, and I wanted to link a story about economists over these multiple decades to a larger story about the

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evolution of the Mexican economy and society. In other words, I wanted to synthesize multiple levels of theoretical analysis—I wanted to be “close to” original, micro-level data, while preserving my ability to draw macro-level conclusions. Historical sociologists interested in striking this balance arrive at different mixtures of primary and secondary materials. On the one hand, John Markoff’s magnum opus on the French Revolution, *The Abolition of Feudalism*, relies very heavily on coded *cahiers de doléance*, lists of grievances that were presented to the Estates General upon the disintegration of the old regime (Markoff, 1996). Viviana Zelizer’s *Pricing the Priceless Child* is a cultural analysis of changing attitudes toward childhood in the 19th and 20th centuries in the United States; although it makes significant use of secondary sources, the core of the book is made up of excerpts from the popular press, the reports of government and public service organizations, and other miscellaneous cultural artifacts from the periods covered by the study (Zelizer, 1985). Toward the other end of the spectrum, my dissertation advisor’s book *City of Capital* draws on share-trading records of the East India Company to show how political party affiliation influenced economic behavior in England in the 1700s, but sets these findings within a context of many chapters synthesizing secondary literature (Carruthers, 1996). Vivek Chibber’s recent study of postwar economic policy in India and Korea is mostly a synthesis of secondary sources; however, Chibber is able to offer a new and heterodox account of why the two national cases turned out so differently through skillful mining of archival resources, including Indian government documents and collections of private papers (Chibber, 2003).

A significant problem for historical sociologists interested in working with primary materials is finding documents that allow for analysis of trends over significant spans of time. To take a contrasting example, my historian friend in Mexico was writing his doctoral dissertation on the life and ideas of a famous figure of the Mexican revolution. His job was to locate the archives where information on this individual’s life was located, and to systematically mine these sources. My task, in contrast, was to find information that was at once shallower and wider, spanning from the 1920s through the 1990s. Ideally, I needed documents spanning the entire period that were both consistent and comparable. They also needed to be analyzable in a way that did not bog me down in a swamp of details about any single period or historical figure.

Thus, I needed to find historical documents that were consistent enough to be comparable over time. Institutions that endure over time, such as newspapers, governments, and universities, tend to produce this sort of document, and are thus a boon to historical sociologists. For example, Tilly, Tilly, & Tilly’s (1975) famous work on cycles of protest from 1830 to 1930 relies on the sampling of newspaper articles to document how many, where, when,

and what kinds of protests occurred. Unlike these authors, however, I was particularly interested in trends in *ideas* over time. In particular, I wanted to look at the ideas emanating from the two most important economics programs in Mexico: Mexico's first economics program at the Autonomous National University (UNAM), and its currently most influential economics program at the Autonomous Technological Institute of Mexico (ITAM). Whereas the UNAM has the reputation for being a hotbed of leftist ideology, the ITAM is known for being a center for the training of American-style economics, where students go on to receive American Ph.D.s in economics and to achieve top positions within the Mexican government. Three of Mexico's finance ministers over the past 20 years have been ITAM graduates.

It occurred to me that the most direct way to look at how these economics programs changed over time was to examine the syllabi of required courses and to use the works listed on the syllabi as a way of gauging where the programs "stood" on matters of economic policy. However, it soon became apparent that the syllabi that would have allowed for such an analysis did not exist. In hindsight, I suppose I should not have been surprised by this; after all, universities in the United States do not generally hold on to syllabi from past undergraduate courses, and there was no reason to expect Mexican universities to be any different. A sociologist at the Colegio de México, Francisco Zapata, offered me an extremely useful tip that was to extricate me from this dead end. "Why don't you go look at the economics theses?" he offered. "They're all just sitting there at the university libraries."

The suggestion that I look at undergraduate economics theses requires further explanation. Unlike American undergraduate programs, which allow students to explore courses outside their major, and which last only four years, Mexican programs specialize in a single discipline, last five years, and typically result in the writing of an undergraduate thesis; thus, it is something like an undergraduate and graduate education combined. When I went to the library of the Faculty of Economics at the National University, I discovered that the theses were a treasure trove of information just waiting to be mined: they spanned from the late 1920s through the present, their rhetoric was rich, they exhibited fascinating differences over time, and the authors they cited and methods they used were evocative of what it meant to be "an economist" in Mexico at different historical periods. I became even more excited when I ventured into the economics library at the ITAM, where the more recent theses were perfect replications (in Spanish) of mainstream American economics.

I was to spend many hours in the libraries of these two universities with my laptop, taking notes and developing a coding system for content analysis. Content analysis is a method for analyzing the message characteristics of documents systematically—or, to put it somewhat differently, any method in

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which the meaning of documents is analyzed in a way that attempts to be duplicable and comparable (see Gamson, 1992; Neuendorf, 2002). Conventional historians are known for synthetic interpretations of primary sources that are often unique—personal letters, the Declaration of Independence, and so on. In contrast, content analysts *count* message elements in documents belonging to the same category (e.g., newspaper articles, history textbooks), according to a predetermined coding scheme. Content analysis allows researchers to make generalizations about the overall content of a set of documents in ways that are easily comparable across groups of documents.

Content analysis can be particularly powerful for analyzing systems of meaning comparatively and historically, because it theoretically allows the researcher to track more subtle differences or changes over time than might come to light using more impressionistic methods. For example, content analysis might allow us to state that a random sample of 1000 issues from three major newspapers from 1965 through 1975 show a steady increase in the mention of the issue of women's rights; it might also allow us to say that the *New York Times* was consistently more sympathetic to the cause than the *Washington Post*. Because I was interested in changes in economic thought over time and across economics programs, content analysis seemed like the ideal method to use. The content-analytic approach I ultimately adopted was to thoroughly read the introduction and conclusion of each thesis, where main theoretical points and citations were made, and skim the middle for methodological approaches. I coded each thesis for theoretical citations, methodology, and various rhetorical features—most important, position on government intervention in the economy. I also copied juicy quotes verbatim to use as illustrations of general trends.

However, the theses did not speak for themselves. As I continued to code and analyze them, I was plagued by multiple unanswered questions. Why, for example, was there a sudden escalation in the citation of University of Chicago authors in the ITAM theses during the 1970s? Did this indicate a deliberate institutional alliance between the ITAM and the University of Chicago economics program, as had occurred with Chile's Catholic University in earlier decades? How, then, could I explain the subsequent decline in Chicago citations in the 1980s and 1990s—when neoliberalism in Mexico came into its own? At a more macro level, how did the differences that I observed over time and across programs fit into a larger story about Mexican economics and the role of economists in policymaking?

These were questions that could not be answered by any amount of content analysis: the micro-level data could not tell me about the macro-level context. Thus, as my research progressed, I found myself drawing on many other sources. Some of these sources were secondary—histories of the

Mexican political system, economic histories, Roderic Camp's invaluable and exhaustive historical studies of Mexican political elites, and official institutional histories of the National University. In addition to secondary sources, informants were a crucial resource that allowed me to tell a larger story about the data contained in the theses. People directly involved in the institutions and processes that I was studying answered my questions both directly, in interviews, and indirectly, through facilitating access to other sources of data. In retrospect, I believe that I did the research for this book just in time: in just the past 5 years, a number of my key informants have passed away.

Fortunately, I had an abundance of connections, because a year prior to coming with a Fulbright to do my dissertation research, I had spent a year studying at the Colegio de México with a grant from the Social Science Research Council's Pre-Dissertation program (which unfortunately no longer exists). This enabled me both to become comfortably fluent in Spanish and to make many friends and contacts in Mexico City, who provided both wonderful social support and a link to sources of information.

Sometimes the help my informants provided was unexpected. Because I was a personal friend of several UNAM economics graduates, I was given access to an unpublicized collection of political documents from the radical student movement that had so heavily influenced the UNAM economics program in the late 1960s and 1970s. Through another UNAM personal contact, I was introduced to the (then) rector of the UNAM economics program, who gave me access to a recent study on the career trajectories of UNAM economics graduates, which I immediately photocopied and incorporated into my study. Because I knew an ITAM professor personally, I was able to establish a series of connections that led me to the ITAM's Alumni Society, which maintained a database of graduates that included information about current employment and graduate training. Coding these thousand-odd cases (with names removed to protect confidentiality) was extremely tedious, but it helped me to answer a number of important questions. Most important, it showed me that although ITAM graduates with American Ph.D.s had high-profile positions within the Mexican government, most ITAM graduates had neither foreign graduate degrees nor fancy government jobs: The modal ITAM graduate held a BA and was working for the private sector.

My study benefited tremendously from the fact that it was, for the most part, relatively *contemporary* history, which enabled me to talk to people who had lived through and made the history that I was investigating. Over time, my initial group of informants expanded, as connections spawned further connections. The most important of these informants was Víctor Urquidi, a Mexican economist who began working at the Mexican Central Bank as a very young man in the 1940s, and who remained an active

participant in debates about Mexican economic policy until his recent death in 2004. Of all my informants, Víctor was the one who provided the most key pieces of information, as well as the most connections to other informants. Víctor was simultaneously a subject, a lunch companion and friend, a provider of personal contacts, and an academic advisor who read my dissertation from cover to cover with extraordinary speed.

Over the course of my research, I conducted 53 interviews with informants, who provided me with two different kinds of information. The first was a sort of “thick description” of the periods and institutional contexts with which they were familiar, which gave me glimpses into the larger meanings of the events I was documenting. What did it feel like to be in the Mexican Central Bank in the 1940s? What were some of the internal disputes and divisions over the reform of the ITAM’s economics curriculum in the 1960s? The second kind of information was key pieces of data that enabled me to “fill in the blanks,” answering questions on which my documents were silent. For example, from interviewing ITAM professors, I was able to discover that the sudden increase in Chicago citations at the ITAM was not due to a larger institutional commitment to Chicago thinking, but rather because the director of the economics program at the time happened to be a Chicago graduate; as soon as the directorship changed, the disproportionate weight of Chicago citations disappeared. From interviewing an official of the government scholarship agency, I was able to discover that the reason there seemed to be hardly any Mexican Ph.D.s from more leftist American economics programs (e.g., the University of Massachusetts, the New School) was that these programs were not on the “List of Excellence” that qualified them for government scholarship assistance.

Because interviews with informants were so important to making sense of my topic, I found myself confronting some issues more typically faced by ethnographers than historians. My own characteristics—as an American, as a student, as a young woman, and so on—were at risk of influencing the way my informants responded to me, and impeding my access to information. In the following section, I discuss how these dilemmas of “positionality” played out, as well as how I managed the issues of my own subjectivity and bias.

Negotiating Positionality and Subjectivity

Although interviews were indispensable to my research, I cannot claim that this study represented *ethnography* in the serious sense of the term. Rather, my interviews mostly assumed the form of incomplete oral histories, in which individuals recounted parts of their lives and careers. Nevertheless, some of

the issues that I encountered in gathering information through interviews were common to ethnographers. A researcher's own ascribed and achieved characteristics influence the way their subjects respond to them in the field, as well as the way they analyze the social language and behavior they encounter. As Reinharz (1997) points out, researchers in the field do not simply play roles; they bring multiple "selves" to the process of investigation. These multiple selves should not be viewed merely as hindrances to "objective" research, to be shed whenever possible in favor of a more scientific role; they are tools that can be mobilized as a way of making connections to the people whom we investigate.

The most obvious characteristics of mine that might have influenced my subjects' responses were my culture and nationality. Over the centuries, Mexico has been invaded by the U.S. many times; this history, combined with ongoing American influence over Mexican policies, has contributed to a strong sense of nationalism that often merges into anti-Americanism. My overwhelming sense, however, was that my being a U.S. national was more of an advantage than a burden. When dealing with U.S.-trained economists, my nationality was, of course, an advantage. More generally, however, I think that my respectful attitude, combined with the fact that I was showing a flattering interest in people's lives and careers, as sufficient to overcome any anti-American prejudice that I might have encountered.

I also believe that my gender was a tremendous asset in helping to get my informants, the vast majority of whom were men, to open up to me. As a woman in her late twenties, I appeared nonthreatening, and often found myself the recipient of "gentlemanly" treatment—being taken out to lunch, having doors held open for me, and so on. In Mexican culture, as in American culture, there is an established gender dynamic in conversation: The female role is to ask eager questions about a man's life, and the man, flattered by the focus of female attention, holds forth at great length. Thus, what I was doing was following the steps of a well-understood cultural dance. Had I been conducting equivalent research in China, for example, I wonder whether I would have had the same experience.

Perhaps the most significant obstacle I faced in my research was that I was, in many cases, "studying up"—interviewing elites, who were either formerly or currently in positions of power. How would these individuals react to a humble and impoverished graduate student's pleas for information? Here, I found that social networks were essential. This was one of the areas in which Víctor Urquidi's assistance was absolutely crucial, because he had a tremendous network of personal connections to economists, businessmen, and government officials. I recall that in some cases, he would even have his secretary call on my behalf to make appointments for me. Once launched in this way,

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I was able to establish contacts with more individuals sympathetic to my research, who were then able to help connect me to more interviewees, in snowball fashion. At the time, I owned a single suit, bought for me by my mother, which received considerable use, and which still hangs in my closet—for sentimental reasons, as it is now out of fashion!

Moreover, there was an important characteristic that I shared in common with many of my informants, which undoubtedly helped enhance communication: namely, that I was a Ph.D. student studying academics. This not only made it easier to converse, but also, I believe, served as a hermeneutic bridge—I did not feel like an outsider studying “the Other.” This was particularly the case with informants who had studied in the United States, but with almost all my informants, the points of common understanding were manifold: theories of social change and development, course requirements, scholarships, examinations, grading, and so on.

Where the interpretation of my findings was concerned, my most significant problem was not overcoming my own subjectivity, but finding an appropriate subjectivity to begin with. In other words, what I needed was a “voice” through which to make sense of my findings and tell a story about them. Finding a voice is particularly fraught with difficulty in situations where researchers are examining topics around which there is great political controversy (see Michalowski, 1997). “Going native” was described by European colonialists as the process of conforming to the uniform social and cultural pattern of those among whom one lives. But the “natives” among whom I was living were far from uniform in their points of view. My topic lay directly on top of a deep political fault line dividing Mexicans who endorsed the market-liberalizing model favored by Washington, and those who thought that the model was the most recent incarnation of Yankee imperialism. The former camp had won control of the Mexican government, and economists belonging to this tendency were in a dominant position within the profession; meanwhile, economists associated with the latter position were marginalized from policy debates. As I was interpreting and telling a story about my findings, I could simply not fail to take some kind of position on these issues—but what would that position be?

On the one hand, I was skeptical of the neoliberal claim that removing government interference with markets was the holy grail of development success. On the other hand, I did not feel particularly aligned with the economists who had come out of the student movements of the 1960s and 1970s—now mostly teaching at public universities like the UNAM. Many remained committed to the tenets of orthodox Marxism and dependency theory. Although I was sympathetic to these theoretical tendencies (as a professor, I teach both in my classes), I could also understand the criticism

leveled against the UNAM: that it failed to prepare its graduates for jobs in the world; during the late 1960s and 1970s, practical topics such as mathematics had been de-emphasized, and the program had adopted a new program of studies in which students were required to take seven semesters of Marxist theory. It seemed to me that for all their virtues as critical tools, both orthodox Marxism and dependency theory were deficient in their ability to create a positive policy program—short of socialist revolution, which seemed a rather remote possibility. In large measure, it seemed to me that this tendency within Mexican economics was marginal to policy by definition.

I did ultimately end up “going native.” However, the group of natives to whom I became identified belonged neither to the former group nor to the latter group described above. Through getting to know Víctor better, I became aware that there had been an older generation of economists and policymakers who Raymond Vernon (1963) described as *técnicos*—hard-headed pragmatists who presided over the so-called Mexican Miracle of the 1950s and 1960s, when the economy grew at an average rate of 6% per year. Within this group, there were some political differences, with some falling to the right and others farther to the left wing of the spectrum. Some of them were self-taught; a handful of them had foreign graduate degrees. What they shared in common, however, was a lack of theoretical dogmatism of any sort—including the dogma of market liberalization.

While I was aware of the faults of Mexico’s postwar development model, it seemed to me that the point of view of Víctor and others belonging to this group provided a strong position from which to evaluate and criticize the reigning neoliberal model. Thus, not only did I bring multiple “selves” to my research, but I also found myself actively constructing a new “self” as my research progressed (see Reinharz, 1997). I emerged from this experience permanently changed, and I imagine that my “Mexican developmentalist self” will influence my work throughout the rest of my career.

Some Final Thoughts

As I conclude this piece for a book on emergent methods, I find myself asking whether there is a name for the kind of research that I did. I ultimately think the best way to describe my research was that I used everything I could get my hands on. I am not the first historical sociologist to proceed in this way. I was fortunate to have been in an ideal position to triangulate data from historical documents with oral-historical accounts from individuals who had participated in that history. Because I was in a land of strong institutions—universities, state agencies, archives, and so on—I was able to glue my story together with rich historical accounts and descriptive statistics. A similar

study would doubtless be possible in the United States or Korea; it would be far more difficult to put together in a place like El Salvador or Mozambique.

I also believe that researchers engaged in this study need either to have exceptionally high self-esteem or to have exceptionally strong external support. As one slowly collects the patches that are to be assembled into the quilt, it seems impossible to imagine how it will all fit together. The task seems doubly impossible when one compares one's own activities to those of researchers engaged in methods that are more straightforward—to the historian in his archives or the demographer at her computer terminal. I had many moments of despair, when it seemed arrogant to have attempted such an ambitious and unorthodox task. Evidence of my hubris, it seemed to me, was that I was constantly cutting, pasting, and rewriting—tearing apart what I had built, rebuilding it, and then tearing it down again once more. I was fortunate to have had extremely supportive and responsive committee members, who continually told me that I was doing something worthwhile and interesting.

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