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Preface

Josh DeWind, Director and Jennifer Holdaway, Associate Director
SSRC Migration Program

In this web-publication, fellows of the Social Science Research Council’s International Migration Program reflect upon their experience conducting research on international migration to the United States. Although their essays describe the substantive findings of their research, their main focus is on the multiple methods employed in producing those findings. They discuss the process of focusing research questions, redefining concepts, selecting apt investigative procedures, carrying out research in the face of unanticipated challenges, and analyzing and interpreting the significance of their findings. We believe that their insights into these processes will be useful to social scientists who are about to embark on a new research project.

These “stories from the field” provide practical research lessons. They also help to clarify the methods of research and analysis through which social science knowledge is created and can claim authority in informing public understandings and debates about social life. Each sector of society, such as religion, government, business, the military, or the media, produces social knowledge based on quite distinct experiential perspectives. Unlike understandings derived from religious revelation, building a democratic consensus, making profits, securing national defense or journalistic reporting, the validity of social science knowledge derives from making manifest, and exposing to critique, the process by which meaning is derived from research. Establishing the validity of new social science knowledge usually depends less on the apparent “truth” of research findings, which may or may not be popularly agreeable or easily accepted, but more on the credibility of the procedures by which factual information and interpretations of its significance have been acquired and produced. Equally, the success of an effort to discredit an established social science opinion usually depends on attacking its methodological underpinnings. Ultimately, the persuasive power of the social sciences in public life derives from its distinctive methods of arriving at knowledge through explicit and self-conscious methods of
questioning, investigation, analysis, and interpretation.

Thus, while the public may be most concerned with the findings of research and their implications for public policies and programs – indeed policy makers and administrators frequently demand “executive summaries” free of qualifying footnotes, sources, and methodological explanations – social scientists must be equally preoccupied with their methods of producing such findings in order to determine and defend their validity. Debates among social scientists within and between different disciplines about when certain methods are appropriate and how accurately they have been employed are of course far reaching and at times quite heated, and have resulted in a quasi-hierarchy of disciplines that are recognized, if not universally accepted, to be most authoritative with regard to particular social issues or sectors of society. The saying, “economics is too important to be left to the economists,” reflects both the strength and weakness of the topical hegemony of a discipline and applies equally to other fields.

A guiding principle informing the creation of the Social Science Research Council in 1923 was that all disciplines of the social sciences have much to learn from one another’s methodologies for carrying out research and arriving at theoretical understandings of complex and important issues in public life. This principle continues to guide the Council today. In seeking to build U.S. immigration studies as an interdisciplinary sub-field within the social sciences, for example, the Council organized a multidisciplinary Committee on International Migration that guided the organization of a variety of activities designed to foster international migration studies as an interdisciplinary field of study, which included a fellowship program that, for seven years, supported predoctoral and postdoctoral research about immigration to the United States.¹

¹ Funding for research fellowships was provided by the Andrew W. Mellon Foundation from 1996-2002 and by The Pew Charitable Trusts for 2000-2001.
Following the final fellows’ conference in 2004, three selection committee members, Sherrie Kossoudji, Louis DeSipio, and Manuel Garcia y Griego, realized that the collections of fellows’ research essays that had been published thus far failed to reflect an important part of the intensive exchanges that had engaged the fellows during the conferences and had excited them about the contributions of different disciplines. These essays, which had been published in books and special issues of journals, emphasized the insights attained and their contributions in advancing social science and public understandings.2 While these publications made clear the methodological underpinnings of their findings, they generally did so retrospectively and without much discussion of how research questions were identified, or how methods were chosen, tested, and adapted in the process of research, analysis, and interpretation.

In other words, the focus on research findings detracted attention from the equally important process of inquiry and doubt through which scholars considered, chose, and employed the research methods on which their findings were based.

Particularly missing were the stories that the fellows had shared with one another, particularly through informal conversations, about dilemmas they confronted in choosing between and deploying various qualitative and quantitative methods, including participant ethnography, surveys, archive retrieval, data set development, or other investigative and analytical techniques. In many cases, fellows had pursued multiple methods of research and had then to figure out how best to combine the results into a coherent combined analysis. While all social science researchers confront similar issues in planning and carrying out their investigations, rarely are these experiences, the choices they entail, or their implications for successful outcomes assessed in ways that can benefit others or aid in future research planning. For this reason the three editors decided to develop this web-publication of research stories.

The narratives of methodological practices presented here have been selected in part because they address central themes and questions of international migration studies and will be substantively relevant to the research findings of other scholars in the field. More significantly, the experiences of these researchers have broader relevance and can be useful to all social scientists who are wondering how to cope with the methodological issues that will ultimately determine the validity of their findings, both within the social sciences and for the public debates that they hope to inform.

In addition to the persevering efforts of the editors and authors, the publication of this web book has relied upon the collaboration and support of many people beginning with Harriet Zuckerman of the Andrew W. Mellon Foundation. Dr. Zuckerman was the first to imagine the contributions that the Social Science Research Council could make to the study

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2 See http://www.ssrc.org/program_areas/migration/ for a description of Migration Program activities and publications.
of international migration and the social sciences more broadly by organizing annual predoctoral and postdoctoral fellowship competitions, which took place between 1996 and 2002 (http://fellowships.ssrc.org/intmigration/). At the Council, we have relied upon and benefited from the gentle editing of Paul Price, publication design of Debra Yoo, communications expertise of Mary-Lea Cox, and web site architecture of Ravi Rajakumar.
**Introduction**

Sherrie Kossoudji, Louis DeSipio, and Manuel Garcia y Griego

Immigration is in the news every day as people in the United States grapple with questions of undocumented immigration, political incorporation, and whether economics, politics, or historical associations should dictate visa categories. But until relatively recently, there were few scholars working on immigration issues. While there are several classic studies of immigration and the immigrant experience, the number of contemporary studies had been limited by the fact that any research on immigration almost certainly flows over disciplinary boundaries. Such studies were difficult to pursue and notoriously difficult to fund because the questions didn’t fit into any discipline’s traditional boundaries, and no single discipline had an overall stake in the results of the study. Researchers also struggled with methods as they attempted to fit the unwieldy questions of immigration into patterned disciplinary methodological techniques.

The Social Science Research Council (SSRC) noted this lack and encouraged such studies financially and intellectually through its International Migration Program, which operated from 1996 to 2002 with funds from the Andrew W. Mellon Foundation (1996-2002) and the Pew Charitable Trusts (2001-2002). This program funded competitive fellowships for both dissertation and postdoctoral research on immigration. One goal of the SSRC enterprise was to bring forth a new generation of immigration scholars. It did. Overall, there were seven fellowship competitions and 108 researchers whose work was funded by the SSRC.

When reading the fellowship applications, the selection committee was impressed by proposals that contained a sound interdisciplinary question, with its potential theoretical implications about an immigration problem. We also looked favorably on applications that engaged in a comprehensive methodological discussion, realizing that good ideas without good methods are difficult to turn into a complete work. Out of the myriad applications for funding, we chose those that outlined well thought-out projects through a comprehensive plan, where problems were anticipated and solved before turning in the proposal.
But, of course, the proposal and the final product are two different things. Writing a dissertation or a publishable article is not simply a matter of choosing a good question, hypothesis, and method, going through the research paces and checking off each step. Listening to the funding recipients present their work in annual conferences and seminars, we learned that the funded projects did not unfold in the smooth ways predicted by the proposals. Scholars grappled with the very issues of interdisciplinarity and methodology that had made their projects appealing in the first place. They faced adverse research situations and had to question their decisions or make new ones that would move the project along. Almost none of these scholars’ projects proceeded in the ways so confidently asserted in their SSRC proposals.

Curious, we asked ten scholars in the SSRC International Migration Program to describe the process of their research rather than to write a traditional paper about the results of their research. The chapters in this volume reflect these efforts. They feature not the fruits of their labor, but the labor itself. In their essays, these ten funded recipients weave together a discussion of their research with an examination of their decisions as they encountered problems, setbacks, and challenges during the project. Our hope is that the readers of this publication who are at the beginning of a research project will be able to use the lessons these authors learned, find support in their successful problem resolution, and draw inspiration from their tales of serendipity, frustration, challenge and adaptation. We believe these essays provide a sense of the adventures associated with interdisciplinary work that often uses mixed or multiple methodologies.

This insight into the research process is critical for new researchers. When we read a scholarly article we see the successful resolution of a thousand different research problems. It looks so easy when it’s done. Readers never know how many problems had to be solved to generate a particular sentence in an article or how a particular decision led to a completely different sentence than the one the researcher expected. Readers get no sense of the frustration experienced when a research plan goes awry or when well considered methods fail to achieve the expected results. The chapters in this volume tell the tale of the challenges of doing research on immigration, choosing and changing methodologies, and juggling the disciplinary and interdisciplinary requirements of the work’s audience.

Research in general, and research on immigration in particular, never proceeds in a straightforward manner. These scholars confronted issues like suspicious interviewees (Osili, Chapter 6), frightened subjects (Oxford, Chapter 7), and canvassing for participants on street corners (Chin, Chapter 3). They encountered interviewees who breached professional boundaries (Talwar, Chapter 10) and advisors who forced the researcher into lengthy additions or detours (Law, Chapter 4). Bartram (Chapter 1) frankly notes the gap between
the certainties expressed in the proposal and the eventual outcome: “I could write a convincing account of my research on international labor migration in which I made sensible decisions at every stage, so that the results flowed like clear water. It would read like a good grant proposal, and it would describe the past just as well (or, rather, just as poorly) as a typical grant proposal describes the future.” Good researchers learn how to respond, adapt, and change course during a research crisis.

Methodology takes a front seat in these authors’ discussions of their work. Sometimes methodological decisions are obvious from the nature of the project. Several authors talk about how certain they were at the beginning of the project that a particular methodological choice was appropriate. Takenaka (Chapter 9) writes, “I chose an ethnographic method, because I believed it was the best—indeed the only—method to study such a small and geographically dispersed population.” For some, disciplinary requirements drove the data and methodology. Newbold (Chapter 5), for example, felt he could only use a traditional quantitative methodology using large-scale data sets. He writes, “In addition, the reliance upon these data files from a geographer’s perspective is somewhat pragmatic, owing to our interest in the role of space in immigration, migration, and assimilation (or other phenomena that occur over space)…The only way to really grasp the role of geography is by using large public data files, with such sources enabling analysis at a variety of different geographic scales including the nation, region, state, county, metropolitan area, and within metropolitan areas.” Talwar (Chapter 10) felt secure in her methodological choice given her questions. “The nature of my questions, being open-ended, rather than hypothesis-oriented, were best treated through a kind of grounded theory approach that would allow me to move back and forth between empirical findings and theoretical framework to continually redirect the orientation of my research questions. I thought that a survey method, usually more associated with a positivist methodology, would not give me this kind of flexibility.”

While it is easy to assume that a well-defined question automatically suggests an appropriate methodology, it is not to be expected. Immigration researchers are often working across the boundaries of disciplines with different methodological approaches. They often have novel questions and interests and so the appropriate methodology may not be obvious because there is little previous work to use as a model. Some authors write of being confronted with the dilemmas of appropriate methodology and challenged by the differences between disciplinary and interdisciplinary studies. Law (Chapter 4) grappled with the problems of a dissertation subject that did not fit in: “All in all, the search for a methodology involved about two years of false starts, dead ends, hair pulling, and extreme frustration that stemmed in part from trying to do research that straddled two distinct academic disciplines, political
science and law, while residing in a home discipline (political science) that was rather conservative and oftentimes none too receptive to interdisciplinary work that was regarded at best as ‘soft’ or ‘not rigorous’ and at worst as ‘illegitimate’ and ‘not real political science.’” Osili (Chapter 6) found that traditional economic methodology wasn’t going to help answer her research question. To find the answers she would have to undertake activities considered radical for an economist: “But very few graduate students in economics conduct field research. Unlike anthropology and sociology, graduate students in economics are seldom trained in survey methods and field research. Instead, most graduate programs in economics are geared to equip students with sophisticated econometric techniques and modeling tools to allow them to answer questions by using existing data sources.” These authors found that the guidelines of discipline and discipline-based methodology didn’t work when conducting research on immigration. To successfully answer their questions, they had to breach the boundaries of their own field.

Others felt it was important to let their own skills be a guide. Law (Chapter 4) admits that it took time and that there were roadblocks to some methodological choices: “This task proved by far the most difficult and frustrating part of the project and I spun my wheels on this task for at least a year and a half…. I simply was not a strong quantitative scholar nor did I get excited by or even agree with the epistemological assumptions in rational choice/game theory or in quantitative behavioral approaches. The choices of methodologies were quickly narrowed down to the qualitative ones.” Shih (Chapter 8) similarly found that the beginning of the project took substantial time and that a successful move for her was to fall back on past experience. She says she spent “a (painful) year to identify a suitable site” and that her prior experience drove her methodological choice: “Indeed, in retrospect, I cannot honestly recollect which came first—whether I chose my method after deciding upon my research site, or whether I chose Silicon Valley partly because it suited my choice of method. I had some experience in analyzing semistructured interview data because I had the opportunity to work on my mentors’ datasets, and frankly, I suspect that most scholars in their first project use the methodologies with which they are most familiar.”

It is not just the overall methodological choice that can be time consuming, but also the little choices within a particular methodology that are required because of the nature of the project. Chin (Chapter 3) found that she had to be sensitive to the lives of the workers and employers she interviewed, making her own work more difficult as a result. “Because of the nature of my population—mostly new and undocumented immigrants—and the fact that all the owners were participating in some kind of off-the-books unregulated activity, I did not think it was appropriate to tape-record their interviews. I felt that introducing a recording device would have discouraged many of the
informants from discussing sensitive matters that were crucial to my study...Because of this limitation, I could not do more than two interviews at a time.”

Indeed, many authors found that flexibility was required for successful completion of the project. Oxford (Chapter 7) discovered that her respondents weren’t answering her questions in the way she expected. When faced with a consistent reaction to basic questions during interviews, she responded by changing her research question: “Thinking of the interview guide as flexible instead of immutable allowed me to develop an argument about how refugees experience and perceive downward mobility when they come to the United States. I changed the research question from ‘How do Somali refugee women experience changes in gender status during resettlement?’ to ‘How do Somali refugee women experience downward mobility during resettlement?’”

Though it is difficult to anticipate the consequences of changing a research question midstream, Oxford notes, it can be more dangerous to ignore answers that suggest you’re not interested in the right issues.

Even though many of the scholars felt they had used a good methodological approach, the end of the project led to reflections about alternatives. For some, their methodological choices came to be reexamined; for some they were reconsidered; for at least one, a new approach was unsuccessfully attempted. Even when authors did not feel a need to alter their methodological choice, they considered the limitations of the method they used. Takenaka (Chapter 9) says, “In retrospect, however, I should have combined ethnographic research with a survey, even a small-scale one, that would have allowed me to compare across regions more systematically. That way, I might not have struggled so much in writing it up as I did.”

Talwar (Chapter 10) ruminates, “My qualitative approach had prioritized giving voice to my respondents’ lives and stories, hoping the text that emerged would provide meaningful understandings about the immigrant experience in the context of global economic transformation and the growth of the giant corporate chains. But I had not anticipated how difficult it would be to analyze, interpret, and present the data.” Shih (Chapter 8) describes that ‘uh oh’ moment when she realized that her methodology came with costs: “When I began the process of writing the data up, I was faced with a challenge that I had not foreseen. I realized then that in using interview data (with a relatively limited number of cases) as a primary method, one runs the risk of being ‘stuck in the middle of nowhere’ between a true ethnographic method and a survey method. This is a problematic place to be, because interview data have neither the breadth of data garnered by surveys, nor the depth of data garnered by ethnography.” One researcher tried to amend or add to his methodological decisions, and found it difficult. Newbold (Chapter 5) wondered if he was now consigned to a particular methodological
identity: “Part of my proposed research agenda included a qualitative analysis of individuals’ intentions to migrate based upon surveys and interviews, to which one reviewer noted something to the extent that (and I paraphrase) ‘the researcher proposes to continue positivist-oriented, quantitative research methods and theories.’”

The funding applications chosen by the SSRC were deemed fundable because they had a well defined research question. Only in reading these chapters did we find that these research questions were hard won. Bloemraad (Chapter 2) says, “As a new graduate student, I viewed social science research as a quest for answers. I had not realized that an equally difficult task was finding and asking the right question. Before I could develop an argument about Canadian and American societal differences, I needed to establish that there was in fact some U.S.-Canada difference worth explaining. In the language of hypothesis testing, I needed a dependent variable. This sounds obvious now, but specifying the research question became a project in itself.” Sometimes, choosing the question was driven by the needs of the discipline. Newbold (Chapter 5), as a quantitative researcher on geographic issues, acknowledged a reality of quantitative research: “In a perfect world, our data (regardless of whether it is primary or secondary) would capture everything that we need. The reality, of course, is far different, such that public data will potentially disallow some questions to be answered, meaning research questions must be framed relative to the data source.”

In all, these chapters should be required reading for anyone setting out on a new research project in immigration. Methodology and interdisciplinarity are fraught issues for immigration researchers. But these authors give the reader profound insights into the research process. They articulate the role of serendipity and adversity (always present in research), the incidents that can change the research midstream, and the many ways that a researcher’s day-to-day choices affect the study’s outcome. When we chose the recipients of the SSRC fellowships, we did not realize that what we were in fact choosing were researchers who could successfully cope with the many challenges brought forth by research in immigration.
Chapter One  Igloos in Borneo: Variation and Conceptualization in Research on Foreign Workers

From
Researching Migration: Stories from the Field

By David Bartram
Senior Lecturer in Sociology
University of Leicester

http://www.ssrc.org/pubs/researching_migration.pdf
People who have completed a big research project know that success requires not only hard work and good decisions but also a bit of serendipity. I could write a convincing account of my research on international labor migration in which I made sensible decisions at every stage, so that the results flowed like clear water. It would read like a good grant proposal, and it would describe the past just as well (or, rather, just as poorly) as a typical grant proposal describes the future.

Even if serendipity accounts for much of my research trajectory, however, it is nonetheless true that key decisions regarding certain aspects of research methods were influential in leading me to the findings and arguments I have made. The most important of these decisions had to do with research design and, in particular, the decision to study Japan as a “negative case” of labor migration. This strategy was reinforced by a second: to consider closely the conceptualization of “foreign workers,” instead of being content with the definition implicit in the data commonly used to measure their presence in receiving countries. The research design, in which I resolved to pay explicit attention to variation in wealthy countries’ use of foreign labor, was novel (as far as I am aware), and in retrospect it is no great surprise that it led me to an innovative explanation for labor migration. The experience has been salutary: I now routinely look for opportunities for similar twists in relation to existing research, and I also make such considerations a central feature of my teaching.

Variation and Negative Cases
Scholars have primarily studied migration by investigating cases in which migration has, in fact, occurred. At first glance, this is an entirely sensible approach. Why should migration scholars spend valuable time and resources looking at, say, Finland when Finland has few immigrants? Surely there are more pressing tasks? The problem is that this approach overlooks a core principle of research methods, that is, the importance of making variation the main focus of one’s explanations (King, Keohane, and Verba 1994). To the extent that migration scholars study only immigration countries, they overlook an important dimension of variation in relation to their central concern.

My primary research interest was to explain the initiation of foreign labor flows to Israel in the mid-1990s. The evolution of this interest was almost entirely serendipitous. I went to Israel in 1994 to conduct research on a completely different topic. This project fell apart almost immediately after I arrived: Despite having received a generous grant from an institution that will remain nameless, I had a fundamentally flawed understanding of the situation I wanted to study. I floundered for months, until another sociologist mentioned to me in passing that Israel had begun importing tens of thousands of workers from
Romania and Thailand, among other places. I had written a master’s thesis on Palestinian workers employed in Israel (who, it seemed, were being replaced), and so I instantly had a new project. First lesson: Being among the first to write about an emerging issue of significant public concern can lead to a fair amount of visibility. Second lesson: The collapse of a project is a good time to digest the notion that the same Chinese character is used for both “crisis” and “opportunity.”

Learning about foreign workers in Israel was easy; the newspapers were full of stories and interviewees were willing to talk. But my sociology methods courses at the University of Wisconsin had prepared me to ask: How was I going to have a foundation that enabled me to say something about labor migration more generally? At the very least, I needed another case.

This is where the counsel I had received about paying attention to variation became important. To account for the initiation of labor migration flows to Israel, I would have to offer an argument that did two things. Obviously, it would have to work not only for Israel but also for other countries with similar experiences. But perhaps less obviously, it would also have to explain what made Israel (and other positive cases) different from countries that had refrained from importing significant numbers of foreign workers. I decided, then, to include Japan as a contrasting case and to search for an explanation that emphasized factors that distinguished between Israel and Japan (and, I would hope, between positive and negative cases more generally).

Migration researchers had of course written quite a lot about migration to Japan, but they usually set for themselves the task of explaining why there were increasing numbers of foreign workers in Japan. This question made sense in its own terms: Japan had refrained from importing workers when most other wealthy capitalist countries were employing foreigners on a significant scale, but in the 1990s there was indeed an increase. Researchers are not unreasonable in wanting to understand an emerging trend.

With a proper basis of comparison, however, I believe this was not the most important question. Although there are something like one million employed foreigners in Japan, this number represents less than 1.5 percent of the Japanese labor force. In comparison with other countries, where the figures have approached 10 percent, what matters is not the similarity but rather the contrast between Japan and other countries. If the numbers continue to increase, then perhaps one day it will make sense to put Japan in the same category as, say, Germany. But even if that happened, it would still be necessary to account for Japan’s divergent history.

On this basis, it seemed to me that the most productive questions were: Why are the numbers of foreign workers in Japan so
low? Given that the numbers were starting to increase, why have they nonetheless remained so low for so long, in contrast to the experience of other countries?

In general terms, a negative case is a country that has few immigrants despite embodying some of the factors that migration scholars believe generally lead to immigration.4 The point is not simply that there are few immigrants. Guyana probably has few immigrants—but then, given theoretical understandings of migration, no one would expect that it would have many.5 In other words, negative cases are anomalies with respect to existing theory. The value of studying such cases would seem obvious.

It was apparent that research on labor migration had not worked systematically with questions focused on variation in the dependent variable (why some countries have many foreign workers while others have few). In my judgment, the most popular schools of thought suffered accordingly: They offered explanations for migration that invoked factors present in both positive and negative cases and thus failed to explain variation.

The most obvious challenge arises in relation to a standard neoclassical economic perspective, that is, the basic notion that migration is driven by differences in levels of economic development. The experience of countries such as Japan and Finland, among others, shows in a straightforward manner that it is quite possible for a country to be much wealthier than potential sending countries without developing an actual migration flow. The point should not be surprising when the question is transferred to the actions of individuals (rather than aggregate country-level outcomes). We may start with the impression that most people who migrate do so with the intention of improving their economic circumstances. I do not dispute that contention in its own terms, that is, when stated in that form. But I do question whether it amounts to a coherent explanation of migration flows. The key issue is that people who do not migrate are also typically motivated to improve their economic circumstances. This basic motivation, then, is not in itself sufficient to explain why some people attempt to achieve this goal through migration while others try to achieve it in other ways.

This point becomes especially compelling when we remember that the vast majority of the world’s population does not move across international borders: Migration is the exception, immobility is the rule (see, e.g., Brubaker 1995). This point is well recognized in the literature on migrant networks (e.g., Massey et al. 1987), but it has apparently not penetrated to the more general notions that many laypeople and some scholars hold concerning the causes of migration.
This point was useful in rebutting a commonly held notion concerning why Israel had imported foreign workers. According to some Israelis, the answer was that Israel had “finally” become a “developed” country—so it obviously was going to have foreign workers just like all other developed countries. The trouble with this notion is simply that there are several developed (or wealthy) countries that do not have significant numbers of foreign workers. Talk of structural necessity for foreign labor would be unhelpful here.

A similar analysis can be applied to other schools of thought concerning the causes of migration. Japan, for example, poses a challenge to Piore’s (1979) dual labor market theory of migration: Japan has a highly dualistic economy and labor market (Pempel 1978), the key factor that in this view leads to demand for immigrant workers. Indeed, Piore’s analysis seems to rest on the notion that dualism of this sort is a universal feature of advanced capitalist societies; it is not clear why Japan, Finland, and other negative cases would be exempt from this dynamic. There is some discussion of the role that institutional differences concerning job security might play in accounting for why different sectors attract immigrants in different countries, but this is an underdeveloped addendum to the main conclusion concerning dualism as a cause of migration. The key point here—one suggested by a consideration of negative cases—is that dualism might well lead to employer demand for immigrant workers, but there is no guarantee that that demand will actually be satisfied: Employers sometimes go hungry (cf. Weiner 1995; Lim 1992) and have to adjust in other ways. In other words, economic dualism leads to migration perhaps only in combination with another factor of some sort.

World-systems theory raises some similar issues. The key factor leading to migration in this view is incorporation into the capitalist world system, that is, through the formation of ties of trade, investment, colonization, and so forth, between core and peripheral countries (Sassen 1988). These ties have two significant consequences; they disrupt local “survival strategies,” and the ties themselves constitute “bridges” that people exploit to gain access to core countries (where demand for cheap immigrant labor arises in a way not terribly different from the process described by dual labor market theory). Thus Britain’s colonial legacy creates opportunities for people to move from these (former) colonies to Britain; the same holds for more recent relationships between the United States and, say, Vietnam.

The difficulty is of the same type we encountered previously: The explanation for migration invokes factors that also characterize countries that have few immigrants. Japan’s experience is the most relevant here: As a regional (not to mention global) economic superpower, Japan has long had ties of the type described by this perspective. These ties are undoubtedly a key factor in accounting for the immigration of those people (e.g., Koreans) who have
in fact moved to Japan. But the problem is that, with a world-systems understanding of migration, we would have to wonder why the level of immigration to Japan is not much higher. World-systems theory helps us understand why immigrants to a certain place come from some countries and not from others, but it seems not to clarify why some countries have considerably more immigrants than others.

Another theoretical approach involves attempts to explain features of migration policy, that is, to bring politics back in (e.g., Hollifield 2000). This is a promising school of thought and the one most likely to benefit from consideration of negative cases. As matters stand, however, scholars concerned with policy have constructed questions in a way that obscures the existence of negative cases. Typically, the questions asked resemble the following: How do we explain the significant increase in international migration in the postwar period? In other words, why do advanced industrial countries maintain relatively open immigration policies? We can accept the premise of these questions in its own terms—there has been an increase in international migration in the postwar period—while still understanding how that premise presents an incomplete picture of the relevant reality: This increase has been uneven, that is, it has occurred in some countries but not in others.

The difficulty is apparent in the otherwise insightful work of Gary Freeman (1995). Freeman has argued that liberal democracies maintain open immigration policies because of a key feature of interest politics. The interests that benefit from immigration (mainly employers) are small and more easily organized, while the interests that oppose immigration (mainly labor) are diffuse and more difficult to coordinate. This argument is challenged by the need to explain why some liberal democracies do not maintain open immigration policies. Freeman tells us what many immigration countries have in common (liberal democracy, with opportunity structures that benefit certain kinds of interests over others), but the problem is that this feature is also shared by some countries that have few immigrants.

These considerations relating to theory reinforced for me the value of looking closely at countries with few foreign workers. It is true in a basic formal sense that migration scholars have not done this systematically—but the point is only important if it has substantive implications. In other words, before embarking on a new strategy we would have to have some confidence that it would lead us to new ideas. In my case that confidence was strengthened by the reflections on theory summarized above.

Conceptualization of “Foreign Workers”

The strategy of investigating negative cases (such as Japan) is reinforced by another consideration: Some people counted as foreign workers in standard data sources should not be included in that category. When we pay close attention to the conceptualization of “foreign workers,” the
extent of variation in (potential) receiving countries’ experiences is even greater,
because the bottom end of the range is lower than it otherwise appears. The premise of
this analysis is that the notion of “foreign worker” is quite complex. While the concept
of foreign labor might seem straightforward for any individual case, the subtleties and
variations become apparent when one turns to a comparative analysis of diverse cases.

To begin to elaborate principles for categorizing different types in different
(or similar) ways, it is helpful to make explicit a general, intuitive sense of what
we mean when we invoke the notion of a “foreign worker.” The key point here
is that scholars and others are typically interested in the foreign worker phenomenon
because it constitutes a particular kind of social problem for the receiving country.
(I hasten to add that the problem may not be the presence of the workers themselves
but the way they are or are not accepted by the receiving country.) To anticipate:
The problem is rooted in the notion that
the workers are viewed as too different in
too many ways from the receiving country
population to be considered anything other
than workers; they are not considered
acceptable as potential members of the
society. Foreign workers—to use the term
for the moment in a less precise sense—who
do not constitute such a problem, perhaps
because they “fit in” more easily, are in this
approach something different. To support
this statement I suggest simply that if all
foreign workers were accepted by receiving
countries with no difficulty or friction, there
would likely be much less scholarly interest
in the phenomenon.

The paradigmatic case of foreign
workers is Turkish “guestworkers” in West
Germany. The problem—and I choose the
word deliberately—is well known: Turkish
workers (and others) were brought to West
Germany in the 1960s with the assumption
that their presence would be a temporary
solution for cyclical labor shortages. The
prospect that they would become permanent
members of German society was both
unanticipated and unwelcome. This is
something that German government and
society have been attempting to address for
several decades now, with little success in
the eyes of many observers.

There are several simple, person-in-the-street understandings of what makes
someone a foreign worker, but I believe it is unwise to rely on them. The reason
is that these “rules” lead to decisions that seem counterintuitive with respect to the
discussion in the previous paragraphs. For example, many laypersons (and perhaps
scholars as well) would probably assert that
an employed person is a foreign worker
if he or she does not hold citizenship in
the country of employment. Alternatively,
some people might believe that a person is a
foreign worker if he or she was born outside
the country of employment and thus perhaps
remains a foreigner even after naturalization.
As we will see, however, these views lead to seemingly strange results: Certain types
of people are categorized as foreign workers
despite being part of a group that does not
constitute the type of problem identified above. To foreshadow this particular argument, it probably does not make sense to think of Norwegians working in Sweden as the same type of foreign worker as Turks in Germany or Thais in Israel.

The main claim here, then, is that foreign workers, as understood in much of the research and popular literatures, are international migrants (and perhaps their offspring born in receiving countries that do not adhere to \textit{jus solis} citizenship traditions) whose presence increases the supply of low-wage labor in the host country, under conditions of restricted political or civil rights that impair their ability to compete in the labor market. It is a truism that foreign workers are viewed solely as workers and not as people. They are not viewed as full members of the receiving country’s society and polity; if they were full members, they would not be useful as foreign workers. This incomplete membership—which typically does not even amount to what Tomas Hammar (1990) refers to as denizenship—encapsulates the “problem” as defined above: foreign workers are often segregated into ghettos, their children receive inferior education, and they feel aggrieved about their diminished and restricted status and rights. Foreign workers, then, to be counted as such, must be significantly \textit{disadvantaged} by their foreignness, relative to citizens. Foreign birth and noncitizen status are typical markers that signal and maintain that disadvantage, but they mean different things in different contexts, and our use of these markers must be flexible and sensitive to those contexts.

These considerations lead to decisions to exclude certain types from the category of foreign workers. First, however, the easy cases. There is no difficulty including guestworkers on temporary restricted contracts. Contract workers are often not permitted free mobility in the labor market; the visa often ties the worker to a specific job with a specific employer, or at least to a certain region and/or occupation. Likewise, it is easy to conclude that illegal immigrants are foreign workers as that term is being used here. Their presence as workers is desired by employers and perhaps implicitly by others who benefit from their inexpensive labor, but they are clearly unwelcome as members of the society. The stereotype—which contains more than a kernel of truth—is that such workers do the most undesirable jobs for low wages. Are there illegal immigrants who would not be considered foreign workers as per the approach here? I can think only of a time when, while conducting research abroad, I myself violated the terms of my student visa by editing a manuscript for a sociologist whose native language was not English—an exception that seems to prove the rule.

There are several types, however, that might constitute \textit{prima facie} cases of foreign workers, but on further consideration there are convincing reasons to exclude them from this category. I contend we should exclude workers that move within a common labor market arrangement such as the European Union or the Nordic Common Labor Market.
Again, Norwegians working in Sweden probably have more in common with Swedish workers/citizens than they do with Turks in Germany or even Turks in Sweden (i.e., those who have not gained Swedish citizenship). The most obvious reason is that Norway itself is quite similar to Sweden in wage levels, labor rights, political development, and so forth. While it would go too far to assert that Sweden and Norway are really one country in every respect, in regard to the labor market the relationship between them shares notable similarities to that between, say, Illinois and Michigan. Common labor markets are typically formed among countries that do not worry about being swamped with one another’s “cheap” labor; new members are usually admitted when their economies have advanced to the point that membership will not stimulate a mass exodus to the richer countries, as with the accession of Portugal, Spain, and Greece to the EEC.

The category “foreign workers” should also not include professional and managerial workers, such as the large number of managers of international corporations in Britain or foreign teachers in Ireland. The fact that there is no common labor market arrangement between the United States and Britain does not mean that U.S. managerial staff of multinational corporations are properly conceived as foreign workers. Integration in receiving countries of professionals and managers receiving high salaries has apparently been much less problematic; in many respects such individuals remain “invisible” (Böhning 1991; see also Castles and Miller 2003), in part because they are less likely to create close ethnic communities (Portes and Rumbaut 1996). Another personal note is appropriate here: As an American sociologist employed by a British university, I am confident I do not face anything approaching the degree of exclusion encountered even by some nonwhite U.K. citizens, despite the fact that I have precisely the kind of visa and work permit typically held by guestworkers as described above.

The ideas discussed in the previous two paragraphs lead on to the notion that workers from wealthy countries should not be considered foreign workers even when they are not professionals or managers and even when there is no common labor market arrangement. Even a semiskilled worker from Germany who seeks work in Switzerland would seem to have more in common with Swiss citizens than with (to invoke another paradigmatic case) Moroccans working in France. The numbers of such persons is probably not large, but the exception is necessary to deal properly with the fact that some countries, Switzerland in particular, employ many workers from other wealthy countries despite not being part of the EU or some other similar arrangement. Canadians working in the United States and vice-versa constitute another important example.

There are other types, including more troublesome ones in which the decisions are not as easy. But the arguments presented so far are sufficient to make the central point. Excluding the workers in the three
categories identified here (workers covered by a common labor market arrangement, people employed at a high level such as professionals and managers, and workers from other wealthy countries) reduces the number of people counted as foreign workers, and the result is that there are a number of countries that can reasonably be designated as negative cases. In Japan the effect is quite modest; with approximately 1,000,000 foreign workers in 2000 (including undocumented workers), the foreign share of the labor force is 1.5 percent, and if we subtract 50,000 people from other wealthy countries such as the United States nothing much happens to that percentage. There is a slightly stronger effect for Finland, where the gross figure in 2001 was 1.5 percent of the labor force: subtracting those in the exclusion categories identified above reduces that figure to less than one percent (data from SOPEMI).

But the effect for other countries is more dramatic. Ireland has received a great deal of attention recently as a new destination. Gross figures are indeed remarkable: In 2000 there were approximately 60,000 foreign workers, amounting to 3.4 percent of the labor force. But more than 47,000 of these were from elsewhere in the European Union (mainly the United Kingdom), and another 3,000 were from other wealthy countries and/or employed at a relatively high level (see Bartram 2005 for further details and sources). The reasonable conclusion is that there are very few foreign workers of the paradigmatic type—in fact less than 0.6 percent of the labor force. For some countries long considered to be standard receivers of foreign labor migrants, the picture changes significantly with this adjustment. In 1999 Belgium had a foreign labor force of almost nine percent, but removing intra-EU workers (many of whom are no doubt associated directly or indirectly with EU institutions in Brussels) reduces that figure to less than three percent. Similarly, the percentage for Luxembourg decreases from 57.3 to 4.1.

This analysis reinforces the point that there are some countries where the presence of foreign workers is quite limited (although I do not mean to imply that Belgium and Luxembourg belong in that category). This presence would be considered quite low in some cases even without the modifications just applied to the rough figures, but sometimes the effect of those modifications is a significant reduction. These countries constitute significant anomalies with respect to extant theories of labor migration and thus ought to be of great interest to migration scholars. The point can be expressed also in terms of variation. If we compare Japan, Finland, and Ireland to Germany, Austria, and Switzerland (where the adjusted figures are approximately six percent), we see that the foreign share of the labor force in the latter group is approximately six times that in the former. Claims in the globalization literature to the effect that migration has become a universal feature of wealthy societies might bear reconsideration in light of this way of seeing the data.
Findings and Arguments

The arguments above point to a research question different from the one normally invoked when trying to account for international labor migration. The question is generally some variant of the following: Why do wealthy countries import workers? Having gained a clear understanding of the extent of variation in stocks of foreign workers, the question is better phrased: Why do some wealthy countries import workers while others do not? I have approached this question via my research on Israel and Japan: Why is the foreign share of Israel’s labor force almost ten percent while in Japan the foreign share has remained below 1.5 percent?

When the question is specified in terms of variation, the explanation must also point to variation. We may invoke the well-known methodological axiom that you cannot explain a variable with a constant. Thus it is no surprise that the argument I have constructed is different from others that conceive of labor migration as a constant.

The approach I take is rooted in comparative political economy. I begin with a presumption that using foreign workers to address labor shortages is a policy governments would turn to only with reluctance in recent decades. The experience of labor importers in the 1960s and 1970s showed that “guestworkers” inevitably turned into permanent settlers in a way receiving countries neither anticipated nor wanted. Importing workers was profitable for employers, but these programs resulted in substantial externalized costs for the receiving society more generally. Policy makers that have faced labor shortages since roughly 1970 onwards have generally been well aware of the disadvantages of foreign workers. The question then becomes: When labor shortages arise, why do some countries resolve those shortages by importing workers while others are able to achieve an alternative solution?

The argument I offer draws on well-developed ideas concerning variations in state and institutional structures and the capacity for economic governance (e.g., Evans 1995). The pursuit of profits via employment of guestworkers can be construed as rent-seeking. States can be described as developmentalist or clientelist; opportunities for successful rent-seeking are more constrained in the former than in the latter. In a developmentalist state, authority for policy making rests mainly in a bureaucracy largely insulated from the political pressures employers would naturally bring to bear on politicians. The ability of policy makers to pursue coherent objectives (when those objectives diverge from employers’ fundamental interest in maximizing profits) is further enhanced by long-term single-party dominance.

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6 This is a specific statement, and it does not address the issue of whether there are disadvantages to immigration more generally. The point is that when countries try to import workers without offering them the prospect of full social membership, there are serious negative repercussions not just for the workers themselves but for the society as a whole.

7 This is a modification to Evans’s typology suggested by Wright (1996).
These states are able to pursue a long-term objective of structural economic transformation that increases labor productivity and thus reduces the “need” for cheap labor. In contrast, authority for policy making in a clientelist state rests with politicians, not bureaucrats; the former are “reachable” by private interests and are constrained in their vision of economic policy by a short-term electoral cycle. A clientelist state therefore perceives that it has “no alternative” to importing workers as employers demand.

This perspective has close affinities with Freeman’s model of immigration policy making in liberal democratic countries (1995). That model, however, does not exploit the fact that some democratic countries embody quite restrictive immigration policies. I contend that the reason those countries succeed in curtailing (potential) immigration that is normally driven by employers’ desires is that they are able to implement a demanding economic policy that helps overcome the need for low-level foreign labor.

The notion of a developmentalist state was elaborated early in relation to Japan (Johnson 1982), and it is no surprise that Japan has largely succeeded in attempts to reduce the need for cheap labor. Avoiding immigrant labor was not the primary purpose of its economic policy, but its overall policy mode was certainly conducive to its success in addressing the labor shortages that arose in the early 1970s and again in the late 1980s. Employers lobbied hard for access to foreign labor, but the government was able to avoid succumbing to this pressure because of its success in facilitating structural transformation in various sectors. Increasing mechanization of the construction sector (including a turn to prefabrication) was a key element of this policy: Construction workers in Japan are mainly Japanese, in part because construction wages in Japan have risen to a level above the average Japanese wage, in marked contrast to countries where many construction workers are foreigners.

Such an approach was not possible in Israel, where the bureaucracies are much weaker in relation to the political parties (and where employers are quite powerful in the parties themselves). There was rhetoric (and feeble attempts) toward the kind of economic policies Japan pursued, but these were bound to fail, in no small measure because employers succeeded in pressing their demands for access to foreign workers, undermining incentives for investment that might have resulted in greater labor productivity and thus the ability to pay wages sufficient to attract Israeli workers. Construction employers, in particular, have enjoyed very significant profits as a result of importing workers, although many believe the society as a whole pays a high price.

I have published these arguments in a book (2005) and several articles: two in International Migration Review (1998, 2000) and one in Politics and Society (2004). Having considered the notion of negative cases in relation to potential receiving countries, I am currently moving beyond Japan to investigate more intensively Finland and Ireland, the other two wealthy countries...
where foreign workers (properly conceived) constitute only a very small portion of the labor force.

**Conclusion**

When I began to research the question of why the Israeli government was allowing employers to import workers from low-wage countries, I struggled to formulate ideas that transcended the common-sense claims voiced by politicians and other public figures. While common sense often gets it right (sociology sometimes deserves the epithet often leveled at economics: the painful elaboration of the obvious), in this case it was unsatisfying. Migration theory didn’t seem to take me much further—or rather, Israel didn’t seem to take me very far in relation to migration theory.

The decision to study Japan as a contrasting case was the key turning point in the project. I was no longer bound by the terms of the obvious question (why did Israel do what it did?). I suddenly had access to a more powerful question: why did Israel not pursue a different strategy for resolving its problems with labor shortages? This question seemed particularly appropriate to Israel, where the idea of large-scale non-Jewish immigration was extremely discordant with dominant ideas about the nature of the society and its politics.

Stated in the abstract, the claim that there are benefits from studying variation in the dependent variable is not terribly remarkable. Consider the parallel in another type of research: someone investigating the effectiveness of a new drug would never dream of looking only at cases where the treatment outcome is successful (or, for that matter, of studying only patients who actually receive the drug). Nevertheless, it seems likely that some fields of social science do not always take full advantage of it. Scholars with particular interests often focus their efforts on cases in which those interests are embodied; it is perhaps not immediately apparent why one should conduct research on igloos in Borneo. Fortunately, there are many fields of research where that notion, generalized, is by no means absurd.
Works Cited


Chapter 1: Igloos in Borneo: Visualization and Conceptualization in Research on Foreign Workers


CHAPTER TWO  Of Puzzles and Serendipity: Doing Cross-National, Mixed Method Immigration Research

From
Researching Migration: Stories from the Field

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http://www.ssrc.org/pubs/researching_migration.pdf
I sat in my advisor’s office, feeling more and more foolish. I had been talking for about five minutes, trying to outline a dissertation project, while she tapped a stack of yellow “While you were out” message slips against her chair. With my vast knowledge of U.S. society—I had now lived in the United States for six months—I was convinced that the process of immigrant integration in the United States differed significantly from that in Canada. I was having trouble, however, explaining what the difference was, much less how I was going to study it.

I had lived in Canada for fourteen years before moving to Massachusetts for graduate school, and I believed the Canadian cliché contrasting Canada’s multicultural mosaic with the melting pot to the south. According to Canadian conventional wisdom, immigrants in Canada could be themselves—a unique tile in a vast mosaic—and still be Canadian; in the United States, assimilatory pressures forced immigrants to pledge exclusive loyalty to an American identity and way of life. I suspected that the Canadian government’s support for official multiculturalism affected immigrants’ integration, especially their incorporation into the political system. I thought it would make them more likely to feel included, and thus participate politically, although I was well aware that the opposite argument could be made: By promoting diversity and pluralism, official multiculturalism might divide Canadian residents and ghettoize newcomers, thereby marginalizing immigrants from politics.

According to the literature, my assumption about a significant U.S.-Canada difference was wrong. As a political science undergraduate I had reviewed research on naturalization, the process by which immigrants acquire citizenship. While variation in citizenship acquisition in Europe was explained by different state structures and national ideologies, research on the United States and Canada suggested that the two countries were interchangeable: Both are traditional immigrant-receiving societies with liberal welfare states and low barriers to political participation. Given few structural barriers, differences in immigrants’ acquisition of citizenship must stem from immigrants’ attributes—differences in skills, resources and interests—not from differences in the context of reception. As one long-time observer of American immigration puts it, “the settlement, adaptation, and progress, or lack of it, of immigrants is largely, in the U.S. context, up to them” (Glazer 1998, 60).

Most North American naturalization research consequently replicates standard voting models in political science, which are overwhelmingly statistical. Variables such as immigrants’ length of residence, income, and level of education are regressed on an individual’s propensity to acquire citizenship. These studies are helpful in identifying individual-level variation in naturalization, but I found the exclusive focus on newcomers’ attributes problematic. This approach invites the seductive conclusion that if some immigrants, or some immigrant groups, do not integrate
into the political system, there must be something wrong with them, rather than with the reception they receive. I turned to sociology for my graduate training, drawn to sociologists’ attention to structure and institutions. I thought that interpersonal ties, immigrant organizations, and the symbolism of public policies such as official multiculturalism must surely affect political incorporation.

My advisor had the uncanny ability of getting to the crux of a research problem within seconds. I wanted her as a mentor precisely for this talent, but she did not suffer fools gladly. I became increasingly nervous as I ended my little monologue. She put the message slips down and, having listened to my description of the mosaic/melting pot distinction and my thoughts on the naturalization literature’s shortcomings, she asked a single question: “What is the puzzle?” I didn’t have an answer. I left her office as soon as I could, convinced that my career as a political sociologist was over before it had begun.

While not particularly good for my self-esteem, this meeting was critical to the success of my dissertation. It forced me to think about what, exactly, I wanted to study. What was the outcome that I wanted to explain? What were the hypothesized dynamics, the mechanisms, by which macro-level differences in Canadian and American society and public policy, as epitomized in the mosaic/melting pot distinction, influenced individual immigrants’ political behaviors? The creation of a solid research design, and the answers I found, depended on the integration of quantitative analysis and qualitative interview data, as well as the careful use of multiple comparisons.

Finding a Puzzle
My advisor’s challenge—What is the puzzle?—demanded a clear statement of the research problem. As a new graduate student, I viewed social science research as a quest for answers. I had not realized that an equally difficult task was finding and asking the right question. Before I could develop an argument about Canadian and American societal differences, I needed to establish that there was in fact some U.S.-Canada difference worth explaining. In the language of hypothesis testing, I needed a dependent variable. This sounds obvious now, but specifying the research question became a project in itself.

Was there any difference in political incorporation in the two countries? A recent book had questioned the mosaic/melting pot duality by showing little difference in Canadians’ and Americans’ attitudes on diversity and cultural retention (Reitz and Breton 1994). The authors were cautious in their conclusions since no sustained U.S.-Canada comparison had yet been completed, but by cobbling together results from a variety of surveys and opinion polls, they suggested that U.S.-Canada distinctions were overblown. Their thesis did not auger well for my project.

My first step was to define “political incorporation.” I developed a rich conceptual understanding of political incorporation...
by delving into a variety of theoretical literatures, but I kept getting stuck when it came to identifying observable, empirical indicators of my phenomenon. What could I measure to look for a U.S.-Canada difference in political incorporation? More problematic: What could I measure that was comparable in the two countries? I started with naturalization. Immigrants acquire citizenship for myriad reasons, including instrumental concerns such as wanting to sponsor a relative to the United States or wanting a Canadian passport for travel. At the same time, citizenship is a prerequisite for political acts such as voting and running for office, and it serves as a symbol of political membership. I assumed that measuring and comparing naturalization would be simple: A person either was or was not an American or Canadian citizen. I soon learned that gathering and comparing statistical data was much messier than a neat column of numbers lets on.

Working from the assumption that the agencies in charge of naturalization, the (then) U.S. Immigration and Naturalization Service (INS) and Citizenship and Immigration Canada (CIC), would have good data on immigrants’ acquisition of citizenship, I pored over their publications and asked about public use datasets. I could measure naturalization as an absolute number per year, as a proportion of the total immigrant population (a level), or as a rate capturing the time elapsed between migration and naturalization. The INS regularly published the number of naturalizations each year, but it did not put this number in the context of the number of immigrants eligible for naturalization. The INS figures consequently had limited value: If the number of naturalizations in one decade exceeds that of a previous decade, but the number of immigrants increases more rapidly, political incorporation slowed, despite the greater number of new citizens. It made more sense to talk about the level of naturalization—the total number of naturalized immigrants divided by all immigrants eligible for naturalization—but neither INS nor CIC could furnish this information.

Luckily, census enumerations in both countries ask residents where they were born, whether they are citizens, and how they acquired citizenship—by birth or by naturalization. Using these three pieces of information, I could calculate the total foreign-born population and the population of naturalized citizens, producing an estimate of each country’s level of naturalization. Unfortunately, it is hard to separate those

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1 I also considered voting as an outcome measure, but I found that voting surveys included too few immigrants to allow for any sustained analysis, especially when the category of “immigrant” was broken down by country of origin. In addition, most surveys are conducted in a single receiving society. It is rare to find a survey that spans political borders or contains questions with wording similar to surveys done in another country. I had more success with a second outcome measure, immigrants’ election to national office. I found a pattern similar to the naturalization data.

2 The INS and CIC compile data on inflows of legal migration, but they do not keep track of those who leave or pass away and consequently they do not publish figures for the stock of legal immigrants in the country at any one time.
Researching Migration
http://www.ssrc.org/pubs/researching_migrations.pdf

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eligible for citizenship from all the foreign-born enumerated. In the United States, census forms do not ask about legal status, so the category of foreign-born includes undocumented and temporary migrants. This causes comparability problems since the United States has a bigger undocumented immigrant population than Canada. Calculating a rate of naturalization offered an alternative measure, but the INS and CIC rarely published these data and when they did, the calculation was done differently.

And so it went. What I thought would be a simple exercise in gathering some readily available numbers turned into a research project by itself. I kept confronting comparability issues. How do you standardize level of education across two countries (and multiple states and provinces)? How do you compare immigrants’ ability to speak English when the Canadian and U.S. Census questions have slightly different wording? I eventually opted to use census data despite their limitations because they were the most reliable and extensive and they also included information on important sociodemographic characteristics such as level of education and length of residence.

Resolving problems of comparability as best as I could, I was thrilled—and relieved—when my final table of citizenship levels magically transformed into a striking bar graph. The level of naturalization in the United States and Canada rose and fell in tandem throughout most of the twentieth century, but after 1970, the patterns diverged. In 1970, 64 percent of the foreign born in the United States were Americans, a figure close to the 60 percent of naturalized immigrants in Canada. By the 2000 U.S. Census, the level of naturalization had fallen to forty percent, but north of the border, 72 percent of the foreign born living in Canada held Canadian citizenship. I had a puzzle!

Or so I thought.

**Constructing a Quasi-Experiment**

Happily sharing my puzzle with all and sundry, I was quickly confronted by doubters. Sure, maybe aggregate citizenship levels differed, but maybe getting citizenship was just easier in Canada. Were the benefits of Canadian citizenship more attractive? Perhaps the naturalization gap was merely a function of differences in the migrant streams to the two countries. I did not just need to find a puzzle, but I also had to convince people that it was a true research problem, a surprising difference that could not be easily explained by common sense.

Those who questioned the significance of the North American naturalization gap frequently pointed out that immigration to Canada and the United States differ in important ways. In the United States about two-thirds to three-quarters of legal newcomers arrive through family sponsorship. In Canada, the percentage is smaller, about a third to a half, while a substantial proportion of migrants instead enter as “independent immigrants,” selected on factors such as education, language skills, and age. The origins of immigrants also vary. Most migrants to the United States
come from Mexico and Spanish-speaking countries in Latin America, South America, and the Caribbean. In contrast, Asia is the source of a majority of contemporary migration to Canada. Skeptics objected that the divergence of American and Canadian naturalization stemmed from differences in immigration, not from the two societies’ reception of immigrants. I responded by identifying a “quasi-experiment,” choosing an immigrant group whose origins and characteristics were nearly identical in the United States and Canada.

Many introductory research methods courses, including one I took as a master’s candidate, introduce students to social science research by holding up experimental design as the golden yardstick. Students are told that a well-designed experiment can isolate causal forces in a way that observational data cannot. Most observational data suffer from selection bias: If you compare the educational outcomes of children in public and private schools, you cannot necessarily conclude that one type of school is better than another. An important difference exists between families that send their children to private rather than public schools, and this difference cannot be completely captured through statistical controls of income, religious background, and parents’ education. Thus if you do find a statistically significant difference in public and private students’ SAT scores, you cannot be sure whether this is because of the school, or because of the factors that led parents to enroll the children in one system over another. In contrast, experimenters randomly assign participants to a “treatment” or a “control” group. Since placement in one or the other group occurs by chance and is not related to any particular trait, differences in outcome can be attributed to the treatment, not selection biases.3

It is usually impractical or unethical to do random assignment in social science. We cannot arbitrarily place people in schools regardless of their wishes. Students of immigration face a similar problem. Ideally, if we want to know whether the context of reception in one immigrant-receiving society facilitates naturalization more than in another, we should randomly place foreigners in one country or another and compare outcomes. But we cannot travel the world sending some people to certain countries and forcing others to stay where they are. We can, however, try to minimize selection biases by comparing immigrant groups with very similar origins and patterns of migration in two different countries.

Serendipity led me to the Portuguese. A summer research job early in my doctoral program introduced me to the glories of salted cod, Holy Ghost festivals, and the teacher-student ratios produce better test scores), but it would not necessarily tell us how this happened (e.g., by providing each student with more time with the teacher and more personalized instruction, or by creating fewer distractions, allowing students to better concentrate on the material).

3 I leave aside the question of whether experiments actually help determine the mechanisms of causality. Even if we could conduct an experiment on public versus private school education, random assignment would only tell us that the absence or presence of a certain factor leads to a specific outcome (e.g., low

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spirit of migration that many Portuguese trace back to Henry the Navigator and Vasco da Gama. I knew little about Portugal prior to researching my dissertation and embarrassingly had never heard of the Azores, Portuguese islands home to the majority of Portuguese immigrants in North America. In Massachusetts, I lived in an area with a heavy concentration of Portuguese Americans, so I struck up conversations at the corner grocery store that sold linguica, Portuguese sausage, and at a local tailor shop where I would get a skirt hemmed or a zipper repaired. When I said that I was from Canada, people invariably mentioned that a Portuguese-born cousin, niece, or brother lived in the Toronto area.

Using my new familiarity with census data, I created a statistical portrait of Portuguese-born individuals in Ontario and Massachusetts. The two groups appeared strikingly similar. The Portuguese became my quasi-experiment. Indeed, when I later did interviews in Toronto, one Portuguese Canadian man spoke of being selected for agricultural work by Canadian immigration officials the same week that his brother stepped on a plane destined for a job in New England.

Given the substantial similarities between these Portuguese communities, we would expect little variation in citizenship levels if the U.S.-Canada naturalization gap is purely a function of immigration differences. I used the power of statistics to model the probability that a Portuguese immigrant was a naturalized citizen in Ontario and Massachusetts. I included in my model variables identified by prior research as consequential to explaining naturalization, such as length of residence, English ability, and amount of schooling. Even after introducing these statistical controls, the odds that the average Portuguese immigrant in Ontario was a naturalized citizen were significantly higher, a three out of five chance, than a similarly situated compatriot in Massachusetts, whose odds were just two out of five. The puzzle remained.

Dealing with the skeptics took a significant amount of time, but it paid off in an article published in *International Migration Review* (Bloemraad 2002). The article shows that citizenship regulations in Canada and the United States are remarkably similar, so European research that identifies legal differences as a source of citizenship variation does not apply in North America. Further, the benefits of citizenship are higher in the United States than in Canada. American citizens enjoy broader opportunities to sponsor relatives into the country than permanent residents; Canadian citizenship provides no sponsorship benefits. Higher citizenship levels in Canada therefore cannot be attributed to the benefits received. Finally, the article breaks down aggregate naturalization data by country of origin, revealing that in every case proportionally more immigrants hold citizenship in Canada than in the United States. I had a solid, intriguing puzzle. Resolving it would prompt a series of comparisons and take me from statistics to qualitative analysis.
Using Comparative Logic to Deal with the “Small N” Problem

During my time at Harvard, the sociology faculty included Theda Skocpol and Stanley Lieberson, two leading scholars of social science methodology who hold radically different approaches to comparative research. Skocpol helped instigate a revival in comparative-historical studies by insisting that a small number of case studies, carefully compared for their differences and similarities, can produce causal theories (Skocpol 1979, 1984; Skocpol & Somers 1980). Critics such as Lieberson question these “small N” studies as requiring deterministic theories in a world which, according to him, can be better understood with probabilistic causality (Lieberson 1991). Further, given myriad possible explanations—or independent variables—at play, a researcher cannot dismiss all alternative hypotheses if the number of cases is smaller than the number of potential explanations. Studies with large numbers of cases—that is, with a “big N”—should be preferred. Case-oriented researchers respond that by following sequences of behaviors and events through process tracing, comparative-historical research gets much closer to a causal story than the correlation analysis conducted in “big N” comparative studies.

In the spirit of true open-mindedness, or indecisiveness, I saw merit in both sets of arguments. My overarching project was a small N comparison of just two countries: the United States and Canada. I could have increased the number of cases and made my project a traditional statistical analysis, but the data requirements were insurmountable—countries just did not have similar data on immigrants and their political behaviors. More fundamentally, I agreed with the critics of variable-oriented comparisons that causal mechanisms could be better uncovered and described in richer detail through in-depth comparison than with statistical correlation. If differences in the social and political contexts of Canada and the United States influenced immigrants, the effect would occur through a complex conjunction of causal dynamics, not due to the additive effects of variables understood to be independent of each other.

At the same time, I kept seeing one glaring weakness in my U.S.-Canada comparison, one regularly identified by the critics of small N studies. While the United States and Canada are quite similar relative to most countries in the world, they differ in a variety of ways. The United States is founded on a republican presidential system; Canada has a parliamentary constitutional monarchy. The United States must contend with a legacy of slavery, while Canada has repeatedly overcome secession threats by its French-speaking minority. The United States is a country almost ten times more populous than Canada, and it is a world superpower. The list could go on. If I identified a reason for the divergent pattern of political incorporation over the past thirty years, how could I be sure that it was the right one, rather than a product of one of the other numerous U.S.-Canada differences?
The short answer was that I could not be sure, but as I audited a course on research methods with Lieberson and read more about research design, I began to consider the power of multiple comparisons. Could I extend the logic of my argument to another comparison, within the overarching U.S.-Canada study? By this time I had begun to develop an explanation centered on the importance of government assistance in fostering immigrant communities’ political participation. Many of the local advocacy organizations and social service providers, which often spoke up in the media on behalf of immigrants and which occasionally organized naturalization drives or voter registration campaigns, relied heavily on government grants and contracts to stay alive. In Canada, governments provide more money to immigrants through settlement assistance and multiculturalism programs than newcomers receive in the United States. Was there a way to test this argument using another comparison?

I found that there was, thanks to an inspired idea from a fellow graduate student. Discussing my “small N” problem in the research methods seminar, a classmate noted that refugees in the United States also receive significant government assistance, unlike migrants who come as workers or through family reunification. According to the logic of my argument, I should see less variation between refugee populations in Canada and the United States, more variation between non-refugee immigrants in the two countries, and significant differences in political incorporation between refugees and non-refugees, holding everything else constant, within the United States. This suggestion led me to expand the U.S.-Canada comparison beyond the Portuguese to include Vietnamese communities in the Boston and Toronto areas. The Vietnamese also constituted something of a quasi-experiment. Vietnamese populations in the two metropolitan areas differ more than the Portuguese, but the resettlement decisions made for many refugees in Thai, Indonesian, or Filipino refugee camps still felt like the random assignment of a lab experiment. Using multiple comparisons, I would leverage my observations to convince skeptics of my story… if it held up during fieldwork.

**Mixed Methods: Combining Statistics and In-Depth Interviews**

Various mainstream research method textbooks, if they mention mixed methods at all, outline a division of labor between quantitative and qualitative-oriented social science. In-depth interviews and ethnography, we are told, help generate ideas and provide fertile ground for the germination of new theories. For these ideas and theories to gain more credibility, however, they must be tested using rigorous statistical methods that evaluate their generalizability.

My research did not follow this conventional wisdom. Quantitative data and statistical modeling laid the groundwork for the project. I needed numbers to establish
that I had a puzzle: that citizenship levels varied on either side of the 49th parallel and later, that representation by the foreign-born in national legislative office is more prevalent today in Canada than in the United States. I employed sophisticated statistical modeling to eliminate alternative hypotheses, such as the notion that U.S.-Canada citizenship differences stem from immigrants’ attributes rather than features of the receiving societies. Quantification set the stage. However, it was ill-equipped to explain why the players did what they did.

I consequently turned to in-depth interviewing to uncover the mechanisms structuring political incorporation. In all, I conducted 151 interviews with ordinary immigrants and refugees, community leaders, government officials, and others involved in newcomer settlement. I speak neither Portuguese nor Vietnamese, so at times I turned to interpreters to help me understand migrants’ narratives of political activity. This was not ideal—I literally lost some of the richness of their stories in translation—but the loss was similar in the United States and Canada, thereby avoiding bias in my overall comparison of the two countries.

I would start my interviews by asking how my respondent came to North America. This open-ended question usually led to a story, their migration story, which encouraged people to talk freely. Many of those I interviewed were nervous, never having been asked questions for a research project before, and some were intimidated, uncomfortable with my status as a university student when they themselves had not completed elementary school in their homeland. More than once, after the interview was finished, a person would ask worriedly, “Did I pass?”

Since everyone is an expert on their own journey to the United States or Canada, asking about their experiences usually broke the ice. I would follow with questions about their early experiences finding work or going to school in North America, experiences with discrimination, and their sense of identity and awareness of multiculturalism. I then would ask a series of questions about political incorporation: whether they had naturalized, whether they voted, what type of civic groups they belonged to, and so on.

Beyond my linguistic limitations, I faced two additional challenges. One was emotional: For a number of respondents, recounting their past lives, their trip to North America, and their sense of what they gained—and lost—in migrating evoked tears. Ilda told of how a trip to the blackboard in eighth grade, where she did long division as she had been taught in Portugal rather than the “American way,” led to her humiliation by the teacher and her decision to leave school. Thus ended her dream to become a nurse. The first time a man cried during an interview, he told of leaving Vietnam and his family one night during a dash to a boat on a dark beach. I felt helpless. My cultural background left me ill prepared to see a man cry. Although I only listened to the stories, and I could not hope to understand them fully in an experiential...
sense, I would come home exhausted from
my interviews. Asking questions, and
listening carefully with empathy, is much
more difficult than textbooks let on.

The second challenge was trying to
link individuals’ personal stories to the
larger institutional factors that I suspected
could explain societal differences in political
incorporation. I first had to move away
from survey-style interviewing. When I
asked whether a person was a citizen or had
voted, I would usually get a monosyllabic
“yes” or “no” answer. Since my sample
was far from a random probability sample,
these answers did not get me far. I could
not use my respondents’ answers to report
descriptive statistics, such as “40 percent of
the Vietnamese vote,” since my interviewees
were not representative of all Vietnamese-
or all Portuguese-origin individuals in the
Toronto and Boston areas. What I could
do, and which emulated the process-tracing
technique I found so powerful, was to ask
for a chronology of the naturalization and
voting process. When did you first hear
about citizenship? From whom? Where?
When did you, personally, first get interested
in voting? What happened? Did anyone
help you file for citizenship? Who? Was this
person affiliated with any organization? Did
someone else help? In what way?

Using these types of questions, I had
respondents reconstruct the thoughts and
events that led up to a successful citizenship
application, or their first experience voting,
or the respondent’s most recent electoral
campaign. Most striking was the extent
to which these narratives of political
incorporation were social processes:
Immigrants received assistance from friends
and family, from employers and coworkers,
from teachers at school and from fellow
students. Community organizations played
a significant role. Immigrants with limited
English language skills often received
help from a local social service agency
with co-ethnic staff, or from the agency
that first helped them resettle, even though
naturalization came many years later.
Political incorporation was clearly not the
atomized, individual process implicit in
many statistical models of naturalization
and voting.4

I then took process tracing to the next
level. While personal ties clearly facilitated
political incorporation, the institutional
location of various “helpers” was
noteworthy. A number of these individuals
worked for nonprofit organizations or
government agencies. I visited most of
the major community organizations and
agencies, interviewed key informants in
these organizations, and, where possible,
collected copies of annual reports and
financial statements. The financial statements
allowed me to trace funding streams and
identify the key financial backers. In almost
all cases, government played a significant
role. Given what I knew about greater

4 Of course, not all the literature takes this
tack. The qualitative naturalization study by
Alvarez (1987) first alerted me to the role of
nonprofit organizations in citizenship acquisition.
I also found useful the social and institutional
approaches of Rosenstone and Hansen (1993)
and Verba and colleagues (Verba, Schlozman and
Brady 1995), both of which are statistical.
government funding for immigrants in Canada, and relatively more support for refugees in the United States as compared to other newcomers, I speculated that the organizational capacity of a migrant community—that is, the number and diversity of its community organizations—should vary with public financial support. This was indeed the case (Bloemraad 2005).

By tracing immigrants’ stories of their political incorporation upward, to the assistance provided by community organizations, and government funding downward, to the financial backing given to these organizations, I could link micro-level dynamics with the larger structural argument about institutional differences. I call this process of political incorporation “structured mobilization”: Immigrants acquire citizenship, learn about politics and, in numerous cases, participate due to localized social relations and personal mobilization efforts. These efforts lie nested in, and are structured by, prevailing governmental attitudes and the level of public intervention afforded to the newcomer community.

In-depth interviewing also offered an advantage over standard survey questions by allowing me to probe respondents’ feelings about their new home and their sense of citizenship. To incorporate feelings and beliefs in quantitative studies, a researcher must classify responses into a relatively small number of mutually exclusive categories, thereby losing much of the richness, and contradictions, of people’s emotions. Ann, for example, repeatedly said that she loved Canada and that she felt at home in her new country. Asked why she had applied for Canadian citizenship only three years after arriving, she told me, “Because I love my country! This I look at like my country. I feel it’s my country.” She had arrived in Canada as an adult from Vietnam with few job skills, but she took courses at a local community college and eventually became the owner of a successful beauty salon. She claimed to have experienced no discrimination in Toronto, be it at school, work, or in public places. Yet when I asked how she would identify herself, whether she felt Canadian, she looked surprised and answered, “I still Vietnamese. . . . I never think I’m Canadian, right? Because I live here, I from Vietnam, I still Vietnamese. Maybe my son will think differently . . . because he born here. But for me, I think I still Vietnamese.” Ann was not the only one who claimed strong attachment to her new country, but who found it incomprehensible to say that she was just Canadian, or even Vietnamese Canadian.

These responses forced me to rethink my simplistic assumptions about the Canadian mosaic versus the American melting pot. Immigrants and refugees in Canada usually felt accepted in their new 5

5 Since I am of European origin, it is quite likely that my respondents underreported instances of racial or ethnic discrimination. Beyond such interviewer effects, however, the Vietnamese appear to report far fewer experiences with discrimination than other Asian groups (Lien et al. 2001). It is unclear whether this is because the Vietnamese experience fewer problems or, more likely, because they are more reluctant to report problems.
home, but this did not necessarily translate into a clear preference for a hyphenated Canadian identity. Some could not imagine themselves as Canadian while others bristled at being anything other than “only” Canadian; they believed that hyphenation ghettoizes minorities by underscoring their otherness. In the United States, some immigrants who had migrated decades earlier, like Ilda, recounted stories of unforgiving Americanization, but many newcomers experienced American society as tolerant and even welcoming of diversity and hyphenation. Through the eyes of many of my respondents, Americans accepted multiculturalism. As Reitz and Breton (1994) had argued, the mosaic/melting pot distinction was clearly overblown.

Yet official multiculturalism in Canada does matter. I found that the political expression of multiculturalism, especially as a discourse that legitimizes immigrants’ place in the country, sends a strong message to immigrants that they are rightful citizens. Participation in the political system—both a right and a responsibility—is normalized. Government programs that include or explicitly serve immigrants reinforce this sentiment. Ann, for example, took part in a new mothers program hosted in a municipal community center soon after arriving in Toronto. Sensitive to local demographics, the program was offered in a variety of languages, including Vietnamese. The more universal nature of social welfare programs in Canada also fosters a sense of engagement with government. Government programs matter, thus participating in the selection of government matters. In the United States, social benefits are more prone to be stigmatized, and access to government largesse is often overlaid with the politics of race (Lieberman 1998; Quadagno 1994). Multiculturalism also revolves more around race, largely defined as constituted by native-born minorities rather than immigrant newcomers. Migrants in the United States are grateful for the rule of law and economic opportunity, but they do not feel the same sense of engagement or invitation to participate in a common political space (Bloemraad 2006). I could not have reached these conclusions without having conducted in-depth interviews.

**Lessons Learned**

I regularly show the graph of divergent citizenship levels when I give talks about my research, now a book called *Becoming a Citizen*. It is a striking visual representation of my research question and it immediately invites others to speculate as to what is going on. Having others puzzle with you engages your audience in your research enterprise. Not everyone will agree with your conclusions, but most will be sufficiently curious to listen and become absorbed in your work. Not all research requires a neat puzzle, but a crisply worded question certainly helps the researcher, and her audience.

My dissertation research also taught me not to see research design as a dry methodological enterprise, but rather as a creative venture. We are all limited in
what we can do—how many countries we can study, how many groups we can include, whether we can find the right data for our topic. But every project contains multiple observations, as ethnographic field notes, interview responses, or cases considered. Creative comparisons can leverage the available data by testing the logical implications of an emerging or hypothesized relationship. Maximizing such comparisons increases your confidence in your conclusions.

I also found mixing methods to be particularly helpful in building my argument. Some are suspicious of mixed methods—I was told by one professor while on the job market that those who mix quantitative and qualitative research tend to do neither very well—but I find my results much more convincing after I triangulate data sources and data types. In my case, statistics described the generalized nature of the problem and helped cast doubt on alternative hypotheses. Qualitative interviews and documentary data uncovered the mechanisms linking the structuring forces of governmental policy to the individual actions and decisions of immigrants and refugees.

Without one or the other, the story would have been incomplete.

Finally, I learned to be thankful for serendipity, such as the well-placed suggestion of a colleague and the discovery of an immigrant group about which I knew little. And, ego considerations aside, I even learned to be thankful for the hard questions of a dissertation advisor that forced me to rethink my entire project and to get serious about research design.
Works Cited


CHAPTER THREE

From the Field: Asian and Latino Immigrants in the New York City Garment Industry

From Researching Migration: Stories from the Field

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http://www.ssrc.org/pubs/researching_migration.pdf
This chapter examines the research process and questions of trust between respondents and researcher in a study of two types of sewing shops: Chinese who hire co-ethnics, and Koreans who hire Mexicans, Ecuadorians, and Dominicans. The research explored immigrant garment shop owners’ and workers’ views on who they worked with and why they worked in certain industries. The study also examined how immigrants fared in employment and how ethnicity was invoked and served as a resource. I found that immigrant workers looked for jobs that complemented their household roles as parents, providers, or supporters of relatives overseas rather than just jobs with the best wage. Ethnic relations mattered, although not all of the time.

Defining the Study

Although the New York City garment industry itself has been studied extensively, no comparisons exist of the Chinese, Korean, and Latino groups within the industry. Methodologically, by choosing different immigrant groups in the same industry, I was able to draw inferences about economic adaptation, the use of social ties to attain jobs, and the benefit of those jobs to the workers.

My initial premise was to study the garment industry in New York City’s Chinatown. I was very interested in ethnicity and how ethnic groups and ethnic enclaves seemed to support one another. Studying one group, the Chinese, and one industry in depth would surely have lead me to interesting findings about the Chinese, but would the study help me understand ethnicity any better? I was intrigued and for awhile tried to understand the Chinese community in the Chinatowns of Manhattan and Brooklyn. I did fieldwork and a number of interviews in both communities and found that the two had many links to the garment industry: for example, Chinese employers often ran shops in both neighborhoods simultaneously; however, the shops in Brooklyn were nonunionized and paid lower wages. This framework allowed me to understand how businesses expand and how an ethnic “enclave” can expand across noncontiguous sites. These visits also allowed me to see the different kinds of workers attracted to these shops. The neighborhood affected the wages, the benefits, and the workers’ expectations.

At the same time, I was also exploring shops in Manhattan’s garment district a few miles north of Chinatown. Surprisingly, I found few Chinese shops; the majority were run by Korean employers who hired a mix of Mexicans, Ecuadorians, and Dominicans. In fact, there were many more of these Korean enterprises than there were Chinese-run shops in Brooklyn.

Shortly after my preliminary fieldwork, I realized that a comparative study of different immigrant ethnic groups in the same industry would yield much more information about how ethnicity is used and defined. One of the most important decisions that I made was to conduct this research as a comparative study of different ethnic and immigrant groups.
In doing a comparison, I thought I would be able to show that the ethnicity of the owner or employer in a garment shop mattered little. Initially, I thought both the Chinese who hired co-ethnics and the Koreans who hired Latinos used similar methods in recruiting workers. Chinese co-ethnics shared information about jobs in the industry and sent friends and family to the industry. This is exactly what Latinos told me in preliminary interviews. They sent other co-ethnic Latinos to look for jobs with the Korean employers. If both sectors worked similarly, then what previous researchers thought of as “ethnicity” in examining a single group would in fact just be immigrant ways of finding jobs, regardless of ethnicity.

In comparing the two groups, I discovered that the hiring and work practices were different and much more nuanced than I had expected. Ethnicity was invoked at different times and under different conditions. Ethnicity was appealed to differently in both these sites such that there was a very positive effect on hiring for the Chinese, while the use of ethnicity was limited among the Latino workers. For example, ethnicity and relationships were hidden from the Korean employers because the Korean owners did not want workers who knew of the conditions in the garment factories. Koreans preferred Latinos who could sew but had little knowledge of the acceptable wages. Latino workers did not or could not reveal any prior knowledge of a friend or family member in the New York City industry—revealing that information would hurt their chances of getting a job. Ethnicity and the information shared between co-ethnics was optional, and, in fact, workers or employers learned to use or hide that information for their own benefit.

In deciding against a study only on Chinese garment workers, I was able to gather information that would develop a richer, more intricate description of the two cases. My current research has been shaped by this experience. Whether I am writing or doing fieldwork on one or more groups, I go back to this first study in order to inform and develop my ideas by using a comparative perspective.

The Sites
Although these groups work in the same industry, the sites are entirely different. The Chinese work on the northern edge of Chinatown, surrounded by the ethnic shops and services in the neighborhood. The Latinos work in midtown, surrounded by other ethnic groups. No one ethnic group dominates the midtown area.

At the time of my research, from 1994 to 1999, there were more than 400 Chinese shops in Chinatown, with an average of 40 employees each. Thus, more than 16,000 garment workers populated Chinatown. In midtown, Mexicans, Ecuadorians, and Dominicans come pouring out from the subways to work in the shops of the garment district. The majority of these shops are on the side streets between 35th and 41st Streets between 7th and 9th Avenues. Within this neighborhood, more than 300 shops clustered
with 12,000 mostly Latino workers. The only visible hint of a garment industry were the trucks double-parked to load and unload clothing and other materials. There was no specific ethnic feel to this neighborhood, while Chinatown is clearly an ethnic enclave.

**The Topic**

The comparison of the sites intrigued me. The subject matter of my book, *Sewing Women* (2005), and much of my subsequent work on the garment workers has been a reflection of personal concerns. The research topic, the research questions, the way I collected and interpreted the data, and how I interpreted data were all deeply connected to who I am. I am the daughter of a Chinese garment worker and a restaurant worker. As a child, I used to spend much time in Chinatown garment factories with my mother. I remember very clearly playing with dolls and putting them to sleep in boxes filled with fabric. I grew up listening to my mother and her friends discussing their lives as garment workers and I followed their search for better jobs that eventually took them out of the factories. As my mother left to pursue other jobs, her brothers’ wives, all recent immigrants, entered the industry. My aunts worked in the garment industry until just after September 11, 2001.

In addition, as a sociologist, I always had questions about immigrant ethnic enclaves and how immigrant workers and entrepreneurs were portrayed in the literature. I often wondered how the findings would differ if two different immigrant groups were contrasted. I felt that a comparison would yield an excellent study since it could reveal nuances between the different immigrant groups. Thus, the research questions grew out of my personal interest in immigrant groups and the working poor, and in my sociological belief that there was much more operating in these industries than economic laws of supply and demand or ethnic collaboration.

The sectors described above are just two of many in the garment manufacturing industry in New York City located just three miles apart in the borough of Manhattan. They are very different from each other, but there were enough similarities that they could be compared. Both the Korean- and Chinese-owned shops produced moderately priced women’s clothing with a “Made in the USA” label and were able to produce short runs of fashionable items. They differed in that the Chinese shops were most often unionized and hired documented co-ethnic workers, whereas the Korean shops were not unionized and often hired undocumented Latino workers.

The comparison was extremely important. If I had looked at each of the cases individually, I would have found that the Chinese followed the patron–client ethnic enclave paradigm in which the Chinese do well enough by taking care of their own. If I had looked at the Korean factories only, I would have found that they followed a strict Fordist model, always looking for the cheapest employees and exploiting undocumented workers as much as possible. This would have been the easiest and
simplest characterization of the two sectors. My fieldwork indicated that more was going on.

Underlying these two types of industries was a whole host of social factors that challenged the images I mentioned above. Although it seemed as if the Chinese workers had a patron–client relationship within the shop, such that existing employees often trained newer workers, had somewhat flexible hours, and had health benefits, the workers were more often exploited. These workers rarely earned more than four dollars per hour on average, and they often worked late and extra hours just to get the extra pieces finished. At the same time, these Chinese women went into this industry because they were young mothers who wanted the flexible hours and family health benefits. So while they knew they were being exploited, this industry also offered what they needed to maintain their life in the United States. The Chinese women workers were able to combine their household and childcare needs with the hours required in the garment industry and took what little wages they were offered.

In the Korean/Latino sites, while these workers were in very structured assembly-line workplaces, they were often paid more than the Chinese and were able to gain even higher wages, especially if they were willing to change jobs. This was wholly unexpected, because these workers were undocumented. In the end, the experienced undocumented workers could command higher wages. The Latino workers did not feel any loyalty to their employer. If given the chance, they would leave for the highest paid job.

A major difference between these two groups was how their ethnic social networks operated. I found that one way to understand this was to examine how ethnic groups might be utilized in getting jobs. I had already observed Chinese garment workers in their factories and was more familiar with them. I had read research on the Mexicans in New York City and their working relationship with Korean grocery owners and was fascinated by the Latinos’ kin-like relationship with their employers.¹ This finding in the literature indicated to me that the kinds of information shared in these relationships are not limited to ethnic networks. Therefore, ethnicity may not be the defining factor of networks that share job information. There were structural factors as opposed to cultural factors that led these groups to share job information. To show this, one cannot just look at the inner workings in the Chinese sector; the process also had to be compared to the Latinos who worked for Koreans.

After looking closer at the two sites together and doing some preliminary comparisons, the most “obvious” was no longer obvious. Deciding to do a comparative study was simple. It was apparent that the only way to answer questions of how people use ethnic networks, that is, whether ethnic network usage was a cultural or structural phenomenon, was to compare them. The literature pointed the

¹ Rob Smith and DaeYoung Kim had worked on this topic.
way to this study. Moreover, a comparative study offered a system and logic in how I constructed questions. From the broader questions of whether ethnicity mattered at work to looking at the particular situations of why Chinese and Latino garment workers immigrated, the comparison helped frame the study. The Chinese worker exploited every single ethnic connection they had among friends and family to gain a seat in the factory. Moreover, the ethnic relationships between workers who helped others get jobs constrained what they could advocate for. On the other hand, the ethnic relationship that the Mexicans, Ecuadorians, or Dominicans had with their co-ethnics was hidden from the Korean employers. If those relationships were revealed, workers had a more difficult time in getting hired.

**Recruitment**

**Owners**
The Chinese owners I interviewed were recruited from the two shops I visited, through contacts via the business association, and garment owners’ friends. Gaining interviews through the Chinese contractors’ business association seemed by far the simplest, but it really was not. After meeting with officials from the organization and explaining my project, they agreed to give me a few names of owners for me to interview. After I called and mentioned that the association gave me their names, they seemed more than happy to oblige. In the end, I only interviewed one Chinese owner through the association. The second person and I could never find an appropriate time to meet; I believe that he really did not want to be interviewed, and avoided setting up a meeting. After I already found my fifth interviewee, I told this owner that it was all right not to meet and I would not contact him again.

Although I gained entry to two Chinese garment shops via worker contacts, it was hard for me to initially speak with the owners. Even though they allowed me to witness illicit activity at their shops, they were not ready to be interviewed. In the beginning, they were somewhat skeptical of what I was doing, even though they trusted the workers who brought me in. After I had become a semiregular figure in their shop—I often offered services such as advice on high schools for their children, help with their children’s homework, translation services, and occasional errands—they began to trust me. The other two owners I interviewed were friends of these two owners.

The Chinese owners of the shops I visited did not complain to me about my visits. I think my being Chinese, and therefore an insider, helped allay any distrust they had of me. The owners assumed that I had seen or knew everything that happened in a garment shop because my mother had been a garment worker. I tried not to interfere with any of their regular operations. I was just an observer and the slow approach I took to gain their confidence helped me attain interviews that were more thorough.

I had a somewhat different experience with the Korean owners. The owners I met while observing shops proved to be reluctant interviewees. Although I did interview them, both were hesitant to introduce me to others.
They told me that they did not want others to know that they had an outside visitor in their shops observing their work, and they especially did not want others to know that they were interviewed for a study. First, I believe that they felt this way because they each did not give permission to me to observe the shop; instead, it was their foreman who granted me access. Because of the excellent relationship the Ecuadorian foreman had with the owners and the extra favors he had given to them, he convinced them that it was all right for me to observe. Second, I witnessed many “violations” that they thought were completely new to me. The Korean owners of shops I visited felt uncomfortable with my presence. They did not ask me to leave, but they felt like they had lost control. They told me that my visits had to be confidential, and that no one should know about my presence. I believe they did not want to appear to their peers as violating secrets of their trade to an outsider, or a Chinese, or someone who could report their labor violations. All of these issues were conflated.

On the other hand, the owners who were introduced to me via the Korean Contractors’ Association (KCA) were very friendly and forthcoming. The president of the KCA was impressed with my background, mostly my educational background, and how I started as the daughter of a garment worker. He was also very helpful in securing Korean owners for my interviews. The owners he introduced me to were more willing to speak and to find others for me to interview. I interviewed three owners who were friends of officers of the association. Although I only visited these owners’ shops briefly, it seemed that they were not only doing this interview for me, but for the association. I believe these owners were more open because the KCA had validated my study. Thus, when I approached these owners, they were not fearful or apprehensive.

During the first study, I interviewed only ten owners (five Chinese and five Korean). I had remained friendly with both the Chinese and Korean owners I interviewed before. They also kept in contact with me because many of them wanted me to advise them on their children’s schooling. I told them that I needed to interview them again and wanted them to help me find more employers to interview for the follow-up study. I asked additional questions: What did they think was going on?
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...to happen to their workers? Will there be another group that comes to replace them? Will the Chinese women work shorter hours because their parents will no longer be able to get Supplemental Security Income to support themselves while babysitting for them? Will the Dominican women leave to get other jobs by retraining, or will they compete for jobs with Mexican and Ecuadorian workers because there are few jobs for which they are qualified?

I convinced six of the ten original owners (eight of them agreed to be interviewed for the follow-up) to agree to contact other owner friends for me to interview. Basically, I asked if they could each come up with three others. I figured that if they could get at least one person for me I could continue with a “snowball” sample by asking each of the new participants to recruit as well.

I was convinced that the owners would be my best recruiters. They had known me for at least three to four years and they understood the study. However, the owners really had to be pushed to call other interviewees, and even to give me names. Like everyone, they were reluctant because they were wary about revealing too many illegal practices in the industry. I did not think it was because it was a closed community, but was, in fact, a real decision that they thought could have an impact on their livelihood. They knew many owners who were willing, but they were reluctant to make that call for me. They thought it was fine that they themselves were involved in these discussions with me, but they did not want to get others involved. I had to reaffirm my study once again, convincing them that the information I was gathering concerned how these women lived and how they needed more money to live on. The owners saw other support allowed these women to work for them. I was not interested in a study on illegal activities. These owners each found at least one other recruit for me, and I followed up with snowball sampling.

The owners were quite open and many of them did tell me that they knew that their workers were receiving other types of government support. The Chinese owners were quite accurate in describing the benefits that their workers received. However, the Korean owners were less likely to know that many of their Dominican workers were actually permanent residents or citizens who received welfare, food stamps, or Medicaid. Most of the owners assumed they were undocumented like the Mexicans and Ecuadorians. For a few, it was a revelation that many of their workers were hard-working welfare mothers. However, most of the Korean owners were not surprised to hear that women worked while getting benefits on the side. They admitted that their wages were not enough to support a family.

Workers
To locate workers for the study, I used a variety of techniques at various sites. For the Chinese workers, I recruited at garment shops and at ESL classes. At most places, I was able to make announcements about my study and I would just arrange a time...
and place for an interview. Another source of interviewees came from the Chinese workers’ referrals. The referral interviewees were always eager and ready to arrange an interview appointment immediately. They expressed their interest and relayed information about the positive experiences that others had. They were much easier to talk to than the interviewees that I contacted via the classes or the shops. Their friends had a huge influence on them. The social networks that brought the Chinese women to the shops were also useful in recruiting interviewees.

In the case of the Latino workers, I also recruited using a variety of techniques. For the most part, a Spanish-speaking graduate student and I stood on what I call the “for hire” corner in midtown Manhattan and canvassed for participants. We were able to take our time in explaining the study to many of the folks at the street corner. We visited on a regular basis two to three times each week for over six months. Many of the workers recognized us as the graduate students. Some overheard our interview questions; others knew we offered a fee for participating. It was difficult at the start of the interviewing process, but recruitment became much easier over time. For the follow up, we relied more on worker referrals. As with the Chinese, worker referrals provided much better interviewees. They were more relaxed and understood our study better. In general, I had more luck in recontacting the Chinese workers than the Latino workers. Nevertheless, the follow-up interviews were much more thorough.

### Collecting the Data

#### Insider/ Outsider Status

Most studies depend on access to data. In a comparative study, one needs to gain entry to at least two sites. For this study, I had to find four groups of people willing to be interviewed. I needed to understand what the Korean and Chinese owners thought about their employees, how they hired them, what favors, if any, were offered on the job floor, and what they thought about workers receiving outside funds (I discuss interviewee compensation in a later section). At the same time, I wanted to understand the hiring process from the viewpoint of the Chinese workers and various Latino workers who were employed by the Koreans. I also wanted to understand the family lives of the individuals in the garment industry, including those who had children and spouses and those who were alone.

As you can imagine, my identity was deeply intertwined with the subject matter. As a researcher, I drew upon the most obvious identity—a Chinese immigrant garment worker’s daughter. There are many works that discuss the impact that a researcher’s social identity can have on subjects and their projects (Reich 2003; Warren and Hackney 2000; Rosaldo 1989; Stanfield 1994). These studies state that researchers need to learn how the relationships between the subject and researcher can affect, both positively and negatively, the ability to gather information and to interpret the data. My main concern at
the start was whether I could access the data. I did preliminary fieldwork in a graduate research class, and I thought I would be able to gain access to at least one of the communities—that of the Chinese who hired Chinese workers. While it would be a challenge to enter the Korean shops that employed Latinos, I thought I would be able to overcome any obstacles. I had some reasonable success visiting the Korean garment shops in the midtown area. In the early stages, I realized that I needed a translator. Since I had reasonable success myself in access to the Chinese garment shops, I searched for someone like me, that is, who might have had parents who worked in the industry. During the course of the research, I learned that there were other social identities that I needed to draw upon or to distance myself from. I could gain access to the Korean owners if I stressed that I was an Ivy League college graduate. They were impressed with my social mobility from being a garment worker’s daughter and wanted advice for their children. Latinos trusted me more if I made sure to explain that I was not Korean, even though I was also Asian. I had to explain that my mother was a worker, not an owner, and that I could relate to their work conditions.

After getting into the sites, I started to wonder if my data were trustworthy. I found that having access to many subjects and the ability to do long-term field work allayed many of my concerns regarding the validity of the data. In each of the two sites, I was able to interview more than 100 individual workers and more than 30 owners. I was also able to observe the workers’ and owners’ interactions in the factories on a long-term basis. I was able to use both sources of data to cross-check the information. When what I observed did not agree with what people told me, I could follow up and make an inquiry.

In collecting data from two separate sites, I found myself inquiring more deeply. Some of the information I took for granted initially was questioned by the data I found in the other site. I inquired more. I interviewed more. I did more background research. I kept resorting the data as I analyzed the information. I had confidence in my findings when the various data from the different sites started to repeat and to make sense as a whole.

The Process
For Sewing Women (2005b) and subsequent follow-up work, I used the following multifaceted qualitative approach in collecting data: (1) nonparticipant observation at four garment shops; (2) informal, in-depth interviews with 72 Chinese workers and 68 Latino (Mexican, Ecuadorian, Dominican) workers, 15 Chinese garment shop owners, and 15 Korean garment shop owners; and (3) various interviews with union officials, Department of Labor employees, and ethnic business association officers and support staff.

Because of the nature of my population—mostly new and undocumented immigrants—and the fact that all the owners were participating in some kind of off-the-books unregulated activity, I did not
think it was appropriate to tape-record their interviews. I felt that introducing a recording device would have discouraged many of the informants from discussing sensitive matters that were crucial to my study. Thus only handwritten notes were used to aid my memory for the later reconstruction and transcription of the interviews. Because of this limitation, I could not do more than two interviews at a time. In fact, I preferred to do one interview at a time, type up the field notes, and then start another, even if the interviews were on the same day.\(^5\)

Although garment factories were too noisy and workers in them were too busy to take time away from their work for extensive interviews, I chose to observe in garment shops because I was interested in the interactions between workers and owners and wanted to be able to discuss on-the-job interactions readily. The workplace was the only setting where I could observe this. Thus observation was necessary for my data gathering.

For *Sewing Women* (2005b), I interviewed everyone personally.\(^6\) For the follow-up study, I hired interviewers who were fluent in Chinese and Spanish. One of the Dominican translators who worked with me on the first project joined me as both a translator and an interviewer on the follow-up. She was the daughter of a Dominican garment worker and was at ease with the workers. The Chinese interviewer I hired was the son of a garment worker. We teamed up at first, and I also had them do mock interviews in English. I taught them to take notes and both interviewers typed their interviews in English for me.

Interviews with the owners usually took place in their garment shops, in their offices or office space. Interviews with workers took place in various locations. The majority of the Chinese workers were interviewed during lunch hours in the various eateries in Chinatown. Some were also interviewed right before or after their English as a Second Language (ESL) classes. I would frequently follow up on our conversations with a telephone interview.

Latino workers were interviewed in the same way. The major difference was that only about a quarter of them received a follow-up telephone interview. This proved difficult because these individuals were hard to keep track of by phone. Frequently, phones would be disconnected, or the individual would not be home. Since I had become spoiled by the ease with which I could obtain information from the Chinese workers, I felt frustrated that I could not ask follow-up questions that would come to mind after the interview.

In my initial study, I had received money from the National Science Foundation to pay the workers a small fee of $10. I did not compensate the employers at all. For my SSRC postdoctoral research, I paid the employers and workers $25 each for being interviewed. Signed consent forms were difficult to collect from the workers.

\(^5\) I used my laptop to type up interviews as soon as I possibly could. Coffee shops in Midtown were great locations to do this in between or right after interviews.

\(^6\) I interviewed 110 workers and 10 owners.
because they wanted the interviews to remain confidential and anonymous. Since it was agreed that the workers did not have to use full or real names, they signed the consent forms with whatever names they wanted. I believe that more than 50 percent of the names were correct. Immigration lawyers also assured the Human Subjects Review Board that I had no obligation to report workers who entered the United States without documentation since I could not determine whether they were covered under any other legal proceedings or were awaiting a change in status.

I increased the amount of money given to the participants between the first and the follow-up study because I felt that the information I was trying to gather the second time around was more sensitive. The hiring process, the check and wage splitting, the wages, and even how undocumented workers got hired were generally known throughout the Chinese, Korean, Mexican, and Ecuadorian immigrant garment sectors. It was not difficult to get that kind of information from the participants. However, for the follow-up interviews, I needed to ask more in-depth and sensitive questions on budgets and spending. I felt the $10 offer really helped the first time because it gave me some breathing room to explain the study to them before they outright refused to be interviewed. Likewise, the $25 offer was a nice amount; it was equivalent payment for four or more hours of garment work. Moreover, I also needed the workers’ participation in helping me locate other interviewees. I felt the increased amount would give them an incentive to help me locate other interviewees and it could help them convince others to come forward. Moreover, it was not so much that it would make a huge difference in their family budget but it did give them extra money for purchases like groceries or a nice toy or a treat for their children. I paid the employers for the follow-up as well because I felt that is was necessary and professional. Since I was planning to enlist the employers themselves to help me recruit, I wanted to ensure that they had an easier time as well.

Knowing When to Stop
All during the interviewing period, I refined the questions while doing preliminary analysis. I generally interviewed participants until I heard answers being repeated. Sometimes I probed deeper to get clearer answers before I stopped, but the general rule I used was that I was confident when the answers were being repeated by a number of the subjects.

As an insider, I can have easier access, but it is just as difficult or sometimes more so to get full answers. Some interviewees were ashamed to share their answers for fear of being looked down upon. Sometimes, they trusted me less, because there were too many people with whom I could share their answers and one of those people could be someone they were trying to hide from. Probing questions or comments were ideal for this situation. When I had a sense of clear differences or nuanced similarities between the two groups, I was able to draw
conclusions between the groups and the processes they used. I was able to understand how culture or the ethnic social network or the work organization played a role in how each of the groups got their jobs.

**Conclusion**

In any type of ethnographic work, preliminary exploration in the field is useful in refining the research questions. In many cases, the situation that is described in the literature no longer reflects the reality. The circumstances in various communities often change depending on the economic situation at the time of the study, and in immigration work, new groups and new laws often affect the communities.

Moreover, a comparative methodology requires additional work at the data collection stage; however, the analysis is often straightforward. For example, in *Sewing Women* (2005b), my analysis indicated that documented Chinese garment workers were not able to get greater pay than undocumented Latino workers, but the women cherished their flexible work hours so that they could combine household responsibilities with work responsibilities. They wanted this because most of them had their children living with them. The Latino workers, on the other hand, received higher wages, but had very strict work hours. Unlike the Chinese women, most of the Latino workers did not have children living with them in the United States and did not need the flexible hours to accommodate the daily family routine.

The techniques that I developed and refined while in the field were all used again in many other subsequent projects that looked at the working poor in various communities (Newman and Chin 2003) and Chinese garment workers after 9/11 (Chin 2005a). While analysis is difficult when there is plenty of data, getting into the field and refining your subject can also be cumbersome. Gathering the data can only be completed by trying different techniques and those that I have shared here have helped me the most.
Works Cited


Chapter Four  In Search of a Methodology & Other Tales from the Academic Crypt

From
Researching Migration: Stories from the Field

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http://www.ssrc.org/pubs/researching_migration.pdf
This chapter is a reflection on the research design and methodological decisions I faced in what began as my dissertation project in graduate school. The original dissertation has now evolved into a more ambitious project in terms of the expanded time period covered and, I hope, into a richer book manuscript. The research project focused on how Supreme Court and U.S. Courts of Appeals judges decide immigration cases across two time periods. A remark made by a former employer led me to that question. Before returning to graduate school, I worked as a program analyst at the U.S. Commission on Immigration Reform, a bipartisan congressional commission charged with making policy recommendations to Congress and the president. At the commission one day, my boss remarked, “Boy, if you’re an immigrant, you better hope your case never makes it to the Supreme Court.” He explained that the Supreme Court was not very friendly toward immigrants and that they were very unlikely to win their cases there. “Why? Is the Supreme Court xenophobic?” I asked. How could the highest court in the land be xenophobic? He never gave me an adequate answer. Years later, when I entered graduate school, the question of why immigrants were not winning in the Supreme Court still nagged at me, and this is an account of how that question eventually turned into a dissertation project and full-scale book project. In this chapter, I trace the evolution of this research project, including the roadblocks, unexpected curve balls, and triumphs I encountered along the way. I begin with a discussion of the framing of the research question, move to the (sometimes desperate) search for an appropriate methodology, and then move to the research design of the project.

Defining and Framing the Research Question
How does one take an interest in a subject area—in my case, U.S. immigration policy and law—and turn it into a dissertation? “You have to find a hook for your research,” a way to add and contribute to the field of political science, one advisor told me. My professional experience before entering graduate school consisted of three years of work in the U.S. immigration law and policy fields. All I knew for certain upon entering graduate school was that I was interested in immigration—but an interest does not a dissertation make. I started graduate training in political science and eventually found a home in a subfield of the discipline called public law, which is the study of judicial institutions and processes. Unlike law professors who tend to focus on the development of legal doctrine (i.e., precedent created through case law), political science public law scholars focus on the effect of institutional rules and arrangements on judges’ behavior and other motivations (other than precedent) that influence judicial behavior such as judges’ personal background characteristics. More important, public law scholars study the courts as part of a larger political system in which the judiciary is one of three “separate institutions sharing power,” as presidential
scholar Richard Neustadt has described the American political system (Neustadt 1990, 29). The subfield of public law especially appealed to me because I had seriously considered going to law school before finally opting for graduate studies in political science. In my search for a dissertation topic, it seemed natural to pursue a question about immigration through law. The next step was to create a research question on immigration by couching it in public law literature and other political science literature.

Framing the issue was especially important because I began the project with an interest in the obscure field of immigration law. In addition, the Immigration and Naturalization Act is second in complexity only to the Internal Revenue Service tax code—yet I found this area of law endlessly fascinating. But why would it interest others in my discipline of political science, in the field of migration studies, and beyond the literally ten or so immigration law professors who write on the subject? The answer to how to bridge my interest in immigration law with the broader discipline of political science came to me only after reading the dissertation of a friend who also focused on immigration and political science (Tichenor 1996). His dissertation, which was later published as a book, used U.S. immigration policy as a case study to illuminate the effect of parties and interest groups on congressional behavior (Tichenor 2002). I realized that the way to pursue my interest in immigration law was to use it to say something new about broader issues in political science and the public law subfield in particular. The narrow subject of immigration law would not be the focus of study in itself but instead would be used to shed light on a larger public law question that would be of interest to political scientists as well as migration scholars.

But what immigration law question and what public law literature or other political science literature was I to frame it around? The remark my boss at the immigration commission made to me years ago came back to me: “Why is the Supreme Court unsympathetic to aliens’ claims?” I understood from background research that the explanation was largely doctrinal: Aliens were not likely to win at the Supreme Court level because the high court has consistently cited two ideas that greatly favor the federal government’s position and leave the aliens at a disadvantage. These ideas, which have now become legal precedent, snowballed and simply precluded challenging legal reasoning that would have increased an alien’s chance of legal victory. The precedent argument sounded like a path development argument that is familiar to political scientists, yet the unique contribution of political science research on law is that judges are political actors and courts are political institutions; there are negotiations, bargaining, and power struggles among judges, the judiciary, and other political institutions. I was skeptical of the precedent argument, given my political science background. The precedent explanation begged the question of why the Supreme Court would behave this way in the first place. Then I discovered
a more interesting wrinkle. As my boss at the commission had noted, an alien should hope that his/her case did not reach the Supreme Court—he did not say anything about the lower courts. Further investigation of secondary literature suggested that lower courts seemed more sympathetic to aliens’ legal claims and that, on rare occasions, the Supreme Court would come up with a pro-alien ruling. If precedent was the explanation, would not the same precedent bind all levels of the courts? It appeared that aliens ran into a brick wall only at the highest level of the federal judiciary.

The realization that there seemed to be something qualitatively different between the Supreme Court and the lower courts led me to the American political development (APD) literature on institutional norms. I was interested in the subfield of public law in political science as a field of study but in my graduate career I was also drawn to APD as an epistemological approach to studying and understanding political phenomena. APD is an approach in political science that takes a long view of political phenomena by focusing on the timing and sequencing of key transformative events in the development in U.S. politics and political institutions. Within the APD approach a subset of scholars called historical institutionalists focus particularly on the evolution of institutional rules and norms of political institutions in the belief that these rules and norms shape, circumscribe, and construe the behavior of these institutions and their actors. Using a historical–institutionalist approach, the purpose of the project is not merely to study the past by incorporating historical data but also to emphasize “change over time” and “movement in politics” (Orren and Skowronek 2004, 13). Within the subfield of public law, there are many methodological approaches in the study of judicial institutions and law, but I knew there was also a band of scholars who employed APD methodologies, so there was an overlap of my preferred subfield and a particular methodology.

The final research question that guided the dissertation research asked how institutional norms affected judicial decision making. In retrospect, the framing of the question was a search on my part for the intersection of a subfield that would be “home” to the research topic and an epistemological approach with which I was most comfortable. The implications of the research, specifically that we need to understand the process by which judges make these decisions because they influence case outcomes, was clearly spelled out for immigration practitioners, migration studies scholars, and political scientists who focused on judicial decision making and the effect of institutional rules and structures on its occupants. The end product addressed issues in the political development of one area of law and the two levels of the judiciary. Ultimately the research question investigated how institutional norms—which I argued were actually two distinct sets of norms specific to the Supreme Court and U.S. Courts of Appeals, respectively—affected
the way judges decided immigration cases. Broadly speaking, the study focused on how institutional norms, consisting of rules and structures, influenced judicial decision making. The project did not argue that judges behave the same way when deciding cases in every area of law, but that the process by which institutional norms shaped judicial behavior could indeed be transferred and applied to help scholars understand the process of judicial decision making in other areas of law.

**Methodology and Determining the Sample Size**

Having nailed down the research question, I began to search for an appropriate methodology that would help me answer it. This task proved by far the most difficult and frustrating part of the project and I spun my wheels on this task for at least a year and a half. Early on, I had ruled out a doctrinal approach preferred by legal scholars that focused on the development of precedent through case law to the exclusion of institutional and political variables. But within both the subfield of public law and the epistemological approach of APD, there was a wide array of methodologies that scholars employed, ranging from quantitative statistical analysis to rational choice/game theory approaches, ethnographic approaches like those employed by anthropologists, and close reading interpretative analysis like those employed by literature scholars. The range of available methodologies was quite large, and there was no obvious approach that immediately leaped out at me.

One advisor had told me, “Pick the methodology that will best allow you to answer your research question—do not let the tail wag the dog by becoming attached to a favored methodology and then letting that constrain the choice of your research questions.” That was easier said than done, as I realized that the range of methodologies was realistically not as large as I thought for the simple fact that I was constrained by my own limitations. I simply was not a strong quantitative scholar nor did I get excited by or even agree with the epistemological assumptions in a rational choice/game theory or in quantitative behavioral approaches. The choices of methodologies were quickly narrowed down to the qualitative ones. I knew that the data I would be looking at would be legal opinions on immigration from the Supreme Court and U.S. Courts of Appeals. But how does one determine institutional norms from reading legal opinions? This was the most difficult question I was faced with in choosing a methodology. So I put that question aside for a few months and tackled another issue first: How many legal opinions should I include in the study? How many cases would be enough to produce a rigorous study? I knew that I would have to include cases from across time. APD approaches examine cases across time with the belief that overarching structures such as legal principles and institutional norms can only be uncovered by analysis of data across a broad time period as opposed to a single snapshot moment of analysis. Additionally, extending the time frame of the study also
allows for a larger number and range of cases that in turn provides for more variation in outcomes of social experience in the data (Skocpol and Pierson 2002, 9). The fact that I would have to look at legal opinions across time and in two levels of the courts was fairly obvious, given the questions I was asking.

I was also mindful of a common positivist critique of APD work that such studies were “selecting on the dependent variable” by allegedly cherry picking a small amount of data that would support the researcher’s hypothesis. It is true that many works in APD concentrate on a close reading and explication of political phenomena or a small number of cases, but I did not agree with the critics of APD that this meant the study was illegitimate or not rigorous. I would not have been overly concerned about these common critiques of APD if I were already an established scholar. But as a fledging scholar just breaking into the business and about to go on the job market, I did not have the luxury of completely ignoring these critiques. If I could avoid the small N problem and the problem of selecting on the dependent variable, I could avoid two holes that could be poked in my project when I went on the job talks.

I decided to compare the Supreme Court and U.S. Courts of Appeals cases in two ten-year periods selected to vary the level of national hostility or welcome toward immigrants. The first period, 1883–1893, reflected one of the most restrictionist eras in U.S. immigration history; the contemporary period, 1990–2000, encompassed both a time of openness beginning in 1990 and one of restriction beginning in 1996. The number of Supreme Court cases to select was easy; there were not that many, so I read all of them from both time periods.

The number of U.S. Courts of Appeals cases was a little trickier to determine. There are 12 U.S. Courts of Appeals across the country and each of these covers a specific geographical area defined by state boundaries. The numbers were manageable enough in the historical time period but they had grown exponentially by 1990; in one circuit alone, the cases numbered between 600 to more than 1,000 in a year. Reading all the immigration cases generated by all these circuits would not have been possible, given that I had no research assistants and I did eventually want to graduate from the PhD program. I narrowed the number of cases by narrowing the circuits to three: the Fifth Circuit, Ninth Circuit, and Eleventh Circuit. In so doing, I chose the two circuits that adjudicated the largest number of immigration cases, the Ninth and the Fifth. I also threw in the Eleventh Circuit at my dissertation advisor’s suggestion to vary the race and nationality of the immigrant flow.

Because the Ninth Circuit alone adjudicated a little more than half of all the immigration cases at that level, I had to random sample that circuit to keep the number of cases manageable. I conducted the random sample by printing out a list of the Ninth Circuit cases from 1990 to 2000 and reading every other case. By virtue of the number of immigration cases in the U.S. Courts of Appeals, I did not have to worry
about the small N problem because when all was said and done, I had a total of 1,801 cases in my study. Although I had enough cases to produce a convincing study of judicial trends in immigration law, the choice of these particular three circuits and the two ten-year periods would have implications for my subsequent book project. But at this point, none of the implications of my choices in time period and circuit selection were yet clear to me.

The selection of a time period and the circuits was the easy part. The reason the search for a methodology was so difficult was because the subject of immigration law is almost exclusively the province of lawyers who take a very specific doctrinal approach to their research. This kind of approach, which would focus on the internal logic of case law and precedent, was not of particular interest to social scientists because of its limited utility in explaining social and political phenomena. The largest hurdle to overcome in the search for a methodology was how to analyze the data to “measure” and assess what I was actually trying to evaluate: the effect of institutional contexts on the decision making of the judges in the Supreme Court and U.S. Courts of Appeals. How does one uncover institutional context and institutional setting through reading legal opinions? My own discipline of political science was not very helpful with methodology. Usually, political science studies that analyze texts do so by coding these texts for variables and then running these coded variables through statistical analysis, such as noting how often a term appears or noting any correlation between terms and variables. In addition to my own limitations in quantitative research, I rejected a purely quantitative statistical approach because of its inability to capture the context in which legal themes are used and the nuance and tone of legal opinions. Also, correlation and frequency tests would only get me so far in answering my research question. I found traditional political science methodologies too constraining and, more important, inappropriate for what I was trying to do in my project.

Still in search of a methodology, I turned to some of my favorite books. Many of the scholars I admired were law professors who wrote not traditional legal studies but works that crossed over into social science research. I tried to learn as much as I could about methodology by reading books like law professor Ian Haney-Lopez’s *White by Law: The Legal Construction of Race* (1996) and Kenneth Karst’s *Belonging to America: Equal Citizenship and the Constitution* (1989) to see how they carried out their research. I had returned to the legal field, but this time to its margins. I turned to the two best-known interdisciplinary journals: *Law and Social Inquity* and *Law and Society Review*; the latter is the flagship interdisciplinary journal in law and is well respected by legal scholars as well as social scientists. I combed through several years of issues of these journals to get ideas about how scholars working at the edges of multiple disciplines did their work. I focused on articles that used legal decisions as data.
I eventually came across an article by legal scholar, Robert Gordon with the following quote:

[Case law and treatise literature] are among the richest artifacts of a society’s legal consciousness. Because they are the most rationalized and elaborated legal products, you’ll find in them an exceptionally refined and concentrated version of legal consciousness. Moreover, if you can crack the codes of these mandarin texts, you’ll often have tapped into a structure that isn’t at all peculiar to lawyers but that is the prototype speech behind many different dialect discourses in society. (Gordon 1984, 120)

Eureka! Here was the first and most cogent justification that I had come across for using legal texts as data. Further plowing through the interdisciplinary journals reviewed a methodology I had previously not considered, one that focused on modes of legal reasoning that are the recurring themes (not just words and terms) that comprise the justification and rationales offered by the judges for their decisions; these are the “distinctive legal reasoning” or “legal logic” employed in legal opinions. In most instances, since this project involves the study of judicial institutions, these themes or modes of legal reasoning derive from institutionally based rules such as rules of precedent (e.g., doctrine or stare decisis) and rules governing due process. In some instances, the modes of legal reasoning are terms and logics that can be understood independently of legal rules and indeed are terms and concepts that are comprehensible in different kinds of discourses outside of legal reasoning. But in most cases, the words that constitute modes of legal reasoning are best understood in proper context and in relation to the legal institution. As legal theorist Joseph Raz writes:

[I]f law, morality, physics and medicine, e.g., are each subject to different rules of logic then either they employ distinct terminology or use the same terms in different meanings. In fact while some terminology is special to different domains (for example, “quarks,” “resulting trust”) for the most part we use one and the same language in all domains, and it would be preposterous to suggest that the same words bear different meanings when used by doctors, lawyers, bus conductors, accountants, etc.¹

So while modes of legal reasoning may take the form of commonly understood words and terminology, for the most part these themes and modes of legal reasoning that appear in legal opinions will be terminology and terms that derive from, and therefore should be understood, in their legal and judicial contexts.

After discovering the existence of modes of legal reasoning, I realized that I did not have to abandon political science methodologies altogether, but instead of looking for patterns in particular and

individual words, I could look for patterns in the modes of legal reasoning in the cases. Recalling the APD literature that contended that institutional rules and norms played a large part in defining and constraining the proper professional behavior of judges, a hypothesis arose in my mind about the connection between how institutional settings define and construe the behavior of judges and the modes of legal analysis they might prefer. I proposed that, in order to gain analytical leverage on the manner in which institutional norms and institutional settings alter the way judges perceive their missions and purposes, the project would analyze modes of reasoning found in legal opinions. These modes of legal reasoning also offer insight into how judges think about the immigration issue over time vis-à-vis their perceptions of their own mission and purpose that flow from their specific institutional contexts. Modes of legal reasoning also serve as proxies to assess the nature of the cases brought before the courts (e.g., substantive vs. procedural issues) as well as indicators of what values (jurisprudential, cultural, or otherwise) the judges find compelling.

Finding the modes of legal reasoning concept was the big breakthrough in my search for a methodology. All in all, that search involved about two years of false starts, dead ends, hair pulling, and extreme frustration that stemmed in part from trying to do research that straddled two distinct academic disciplines, political science and law, while residing in a home discipline (political science) that was rather conservative and oftentimes none too receptive to interdisciplinary work that was regarded at best as “soft” or “not rigorous” and at worst as “illegitimate” and “not real political science.” I learned to frame descriptions of my methodology in grant proposals, conference proposals, and potential journal articles not as textual analysis or discourse analysis, which would elicit hoots and giggles from “mainstream” political scientists, but rather as interpretative content analysis, which were terms used in the mainstream political science journals. The fact that the project focused in part on the U.S. Courts of Appeals meant that without going out of my way to find them, I had a large number of cases to work with. Simply plugging in some relevant keywords into the Lexis/Nexis with the proper time constraints returned thousands of U.S. Courts of Appeals cases. The small N critique of many APD projects was immediately overcome. But the question of how to analyze and make sense of the data presented by all these cases still remained. My quantitative friends and colleagues salivated over the number of cases I had collected. They advised coding for certain variables and then running multivariate regression analysis because, with the number of cases numbering 1,801, the results would surely be statistically significant. My own aforementioned weakness in statistical analysis aside, I had misgivings about coding for variables and running the regression analysis my friends recommended for the simple reason that I would rather trade the alleged statistical rigor and certainty for a
more nuanced understanding of the judicial opinions. Simply scanning the opinions for keywords and discrete variables such as the judges’ party identification may very well have yielded interesting statistical correlations, but coding for such isolated, discrete variables would divorce them from the tone and context of the opinion. Instead, at my quantitative friends’ suggestions, I chose to “code” for some of these types of variables, including the kind of case (asylum, deportation, exclusion), the names of the judges who sat on each three-person panel, and the outcome in the case (vacated, reversed, remanded, affirmed), and the nationality of the alien when available. These variables could be used to generate basic descriptive statistics to supplement my qualitative analysis and would be useful in the event that I wanted to do more extensive statistical analysis in the future.

The data collection method for my project consisted of my reading each of the 1,801 cases specifically for the mode of legal reasoning used by the judges and for the outcome of the case by deciding whether the alien or the federal agency or government won. In order to identify the primary mode of legal reasoning, I could not simply scan for keywords because in many instances judges would use one type of reasoning and never use a certain keyword in doing so. For example, a common mode of legal reasoning cited in Supreme Court cases was congressional plenary power over immigration and a corresponding judicial deference. Without ever writing the words “plenary power,” numerous opinions referred to immigration being “the province of the political branches” and the “proper branch” to adjudicate immigration issues was Congress or a federal administrative agency. The use of software such as NUDIS that would scan for certain keywords would have missed many of the plenary power cases. Also, in many cases, judges ran through a list of possible legal reasoning and dismissed them one by one before actually settling on one to justify the outcome in the case. In such a case, a coder would have to read the case in its entirety to understand and identify the preferred mode of legal reasoning. While far more tedious and time consuming, this kind of close reading and interpretative analysis was better suited to understanding how judges form preferences among competing modes of legal reasoning, issues that statistical analysis would have missed entirely.

One important insight that came to me while I was collecting the data was that by focusing on the modes of legal reasoning, I was also getting a picture of what ideas in general are in the arena to begin with, that is, what legal ideas were viable in this area of law (Lieberman 2002). Another insight gained during data collection was that only by reading the entire opinion could a coder understand not only the preference of judges for one mode of legal reasoning over another but also the intensity of their preferences as evidenced by their choice of forceful, bombastic, deferential, or neutral language in the opinions. Interpretative textual analysis yielded not only valence but also intensity.
that in the end allowed me to tell a more nuanced story of the development of the Supreme Court and its evolution on the immigration issue. In a nutshell, over time, the Supreme Court had become less intense and militant about national sovereignty than it had been in the early foundational immigration cases. I speculate in the book manuscript that the strong language on national sovereignty was tied to the uncertainty of the United States as a power on the world stage in the 1880s and the gradual receding of the national sovereignty idea as a foregone conclusion by the 1960s.

My choice to conduct a close, interpretative analysis of the data instead of a quantitative statistical analysis produced a distinct sort of project. It meant that the dissertation would be a study of the process of judicial decision making, specifically of how judges formed preferences over time (and the intensity of those preferences) among available modes of legal reasoning. The project would not be an authoritative account of the correlation of certain variables, as a behavioralist study would generate. Nor would the project produce a model with predictive value for future cases, as a game theory study would produce.

In retrospect, my methods and research design choices probably also determined the type of audience that would be reading my work. If one asks academics and editors of academic presses, it is clear that there is not a lot of cross-pollenization going on between the disciplines of law and social science. Law professors do not read much social science research and social scientists for the most part dip only lightly into legal research. This situation is unfortunate for both disciplines and stems from the preference for particular methodologies and research questions by scholars of law and those of political science. By eschewing a doctrinal approach, I may be losing some legal scholars who are uninterested in my approach, which focuses on institutional norms.

My choice of methodology also meant that I spent an entire year reading a steady diet of Supreme Court and U.S. Courts of Appeals cases in order to construct this original dataset; it was my “fieldwork” year, I told my comparative politics colleagues. As a graduate student, I did not have access to research assistants to help me code the cases, nor did I expect such assistance. What I needed most was release from my TA duties. This assistance came in the form of two very timely external fellowships from the Social Science Research Council and the Center for Comparative Immigration Studies at the University of California, San Diego, which gave me the luxury of a year and a half free of teaching duties so that I could finish collecting and writing up the data. Without these fellowships, these two tasks would have taken at least another year.

Data Analysis

After collecting the cases and noting the variables in each case, including the primary “variable” of the modes of legal reasoning, I began looking for patterns in the legal reasoning. The strategy I adopted...
for analyzing the data was not all that different from the strategies employed by my quantitative colleagues. I looked at the cases for any patterns to the modes of legal reasoning. My rationale was that judges advance their arguments and justify outcomes through the use of modes of legal reasoning. In cases where multiple modes of legal reasoning were cited, I noted whether these reasonings appeared in the majority, concurring, or dissenting opinions. I also noted the frequency with which certain legal reasoning would appear, in which time periods, and also at which level of the judiciary. The data showed a correlation between certain modes of legal reasoning and the level of the judiciary: the Supreme Court consistently preferred plenary power and national sovereignty, while the U.S. Courts of Appeal consistently preferred procedural due process. The preference for certain modes of legal reasoning over others in turn has profound consequences over legal outcomes in these cases.

One of the arguments advanced in this project is that, although judges must have regard for precedent, they still have room to behave strategically by selectively applying precedent and engaging in creative noncompliance to sidestep Supreme Court doctrine and congressional intent and that institutional settings provide incentives for them to behave in certain ways. Further, the selection of the mode of legal reasoning will skew the outcomes in the cases. The fixation of the Supreme Court on plenary power and national security has meant that alien victories are very rare at that level. The frequent pro-government outcomes (at the aliens’ expense) at the Supreme Court are not difficult to understand, given that the choice of national sovereignty and plenary power are the preferred modes of legal reasoning in immigration cases. These two modes of legal reasoning encourage court deference toward the other branches of government and encourage a broad focus that entails grand jurisprudential pronouncements about policy issues that will reach far beyond the individual litigants in the case. This latter goal is achieved by shifting the legal focus to sweeping issues of national significance instead of focusing on the individual claims of the alien in the case. Equity and individual justice for the alien in these cases are not likely to be a high priority for the Supreme Court, as the high court seeks to use one case to make a policy and political pronouncement about a large class of similar cases.

Just as the choice of legal reasoning has consequences for the outcomes of the cases in the Supreme Court, the U.S. Courts of Appeals’ preference is for procedural due process, as their reasoning is likely to create a legal outcome favorable to the alien, not the government. The reason that the use of procedural due process is likely to produce a pro-alien outcome is that the U.S. Courts of Appeals are looking for procedural errors committed by the administrative agencies or by the U.S. District Court or Board of Immigration Appeal’s decisions from which the case is being appealed and are therefore
examining the individual factors in each case very carefully. In contrast, if a plenary power or national sovereignty reasoning is used, the individual facts of the case are less important and become trumped by larger national concerns; the alien and his/her facts of the case are no longer the primary focus. The use of procedural due process as a theme of legal reasoning does not guarantee an alien a victory, but it does vastly increase the chances.

Ultimately what I described in my project was a multistage process of judicial decision making in which judges had to choose among a set of viable modes of legal reasoning derived from the nature of that specific area of law as well as deep-rooted jurisprudential and political cultural traditions. As stated above, the choice of modes of legal reasoning has consequences for the legal outcomes, but what influenced judges at different levels of the judiciary to favor one mode of legal reasoning over another was not precedent alone—it was their institutional setting and context. Here I gleaned from the APD literature that implicit in historical institutionalism is the assumption that institutions serve as active political actors because they “shape interests and motives, configure social and economic relationships, promote as well as inhibit political change” (Orren and Skowronek 2004, 78). Put differently, institutions are a “source of distinctive political purposes, goals, and preferences” because they “structure one’s ability to act on a set of beliefs that are external to the institution” (Kahn 1999, 176). My project focused on several interconnected institutional attributes such as the position of the court in the judicial hierarchy, the jurisdiction of the court, the ability or inability of the court to control its own docket, the two courts’ caseloads, and the size of the immigration administrative bureaucratic apparatus. In turn, the institutional occupants’ perceptions of their goals, missions, and purposes will affect legal outcomes in cases by leading judges to favor some modes of legal reasoning over others. The point of departure of my study was that the Supreme Court and U.S. Courts of Appeals are governed by two distinct sets of institutional settings and that the two courts operate in decidedly different institutional contexts, thereby leading these judges of each court to favor dissimilar modes of legal reasoning.

Post–9/11 Implications
As I was happily reading and “coding” cases, the unanticipated and unimaginable happened: The 9/11 terror attacks occurred just as I was about to finish reading my cases. The implications of the attacks on my study were not immediately obvious as I remained glued to the television like the rest of the country. Soon enough, news reports revealed that the hijackers had legally entered the United States with validly issued nonimmigrant visas and that they had simply overstayed their temporary visas or violated the terms of their visa status. It was revealed in the following weeks that along with the massive failure in the U.S. intelligence apparatus, U.S. immigration policy had also failed
Although I wanted to scream bloody and uncontrollable murder, I knew my advisor was right and that it would preempt questions that would most certainly arise at job talks about the impact of the 9/11 attacks on judicial decision making in immigration cases. While reading the additional cases, which took three months, I was holding my breath to see if the data would show significantly different results from the pre-9/11 cases and whether the most recent data would completely destroy my hypothesis. The post-9/11 cases I added to the study were from January, February, and March of 2002. Given the lag time after the actual attacks, any effects would presumably have manifested themselves in these cases. No significant difference emerged from the post-9/11 cases.

From Dissertation to Book Manuscript

Upon my completion of the dissertation and graduation with the PhD, I breathed a sigh of relief. But as someone told me early on in my graduate career, “You had better love your dissertation topic. It will be with you for many years because it will likely lead to your first book.” That person was right: I have been with this project now for six years and counting. I contemplated turning the dissertation into a book manuscript as I was juggling my first year of teaching duties in my first real academic job. After consulting with senior colleagues in the field, I decided to extend the time frame so as to produce a true longitudinal study. The original time frame was two ten-year periods, 1883–1893 and 1990–2000, but the inevitable question arose about what happened in those intervening years. What also became clear to me only at the end of the dissertation process was that the institutional norms and settings of the Supreme Court and U.S. Courts of Appeals were not static, but were constantly, albeit slowly, changing over time. The book project moves the focus of the study away from comparing the legal outcomes in both courts to the evolving relationship between the Supreme Court and U.S. Courts of Appeals vis-à-vis their changing roles,
missions and institutional settings.

I returned to the book manuscript after a year of teaching by way of writing a grant proposal for the National Science Foundation (NSF). The proposal and the NSF’s exacting standards forced me to revisit and rethink many of the premises of my research design and methodology. In preparation for submission of the proposal, I forwarded a draft to a colleague who happened to be a former program officer at the NSF. He unceremoniously tore the draft to shreds and bluntly told me, “Your project was good enough as a dissertation, but it is not good enough as a book manuscript or as a project NSF would fund.” After I crawled out from under a rock and recovered from depression, in the subsequent revisions of the NSF proposal draft, my thinking on the project evolved and in the end the overall project was, as celebrity chef Emeril Lagasse would say, “kicked up a notch.” What my colleague caught in his review of my draft was that in an effort to select the original time period of 1883–1893, I had only immigration history in mind and had ignored Circuit Courts history. He pointed out that the U.S. Courts of Appeals as we know it today was not conceived until 1890, so to compare the pre-1890 Circuit Courts to the contemporary U.S. Courts of Appeals was comparing apples and oranges. I remedied that problem by extending the time frame of the study to cover the ten years before the 1890 Evarts Act created the U.S. Courts of Appeals in order to be able to assess any changes the restructuring of the Circuit Courts may have had on the selection of legal reasoning by the judges. I wish I had been more cognizant of judicial history when I began the project. Because I had so much time in immigration, including my previous professional work experience in the field, I knew U.S. immigration history fairly well, but I had to learn judicial history as I was doing the project. In retrospect, it would have been more efficient to get up to speed on judicial history before I designed the project rather than learning it in bits and pieces as I went.

The project is now slowly morphing into a book manuscript that is a more ambitious and sophisticated project than the original dissertation. It includes an additional 250 cases from the Supreme Court, 800 more cases from the U.S. Courts of Appeals spanning the years 1880–2000, and some additional Circuit cases from 2002 (to assess any post-9/11 effects). These additional cases bridge the gap between the original two ten-year periods, thereby producing a true longitudinal study. The additional cases will allow a better assessment of the judges’ selection of legal reasoning regarding changing institutional norms over time. The additional cases will also allow a review of the evolving relationship between the Supreme Court and U.S. Courts of Appeals with a focus on the evolution of the respective norms, rules, and structures of each court.

Conclusion
I wish I could tell graduate students and young scholars that many of the research
design and methodology choices made in this project were the result of careful and deliberate effort or were often intuitive, or even that once they start working intensively on the project a momentum will develop to carry them through, but that would simply be untrue based on my experience. Instead, many choices I made in this project were because of serendipity, random luck, and trial and error, including a painful 18-month search to find an appropriate methodology to answer the research question. I do hope that this narrative has shown that although some research choices may indeed be intuitive, many others are not. What my experience with the project that began with my dissertation ultimately taught me is that even the most carefully planned research projects will run into unanticipated difficulties and dead ends that will require the researcher to make adjustments on the fly. Perhaps the best advice to give is to expect the unexpected and know that when it happens, it is not the end of the project.
Works Cited


Using Publicly Released Data Files to Study Immigration: Confessions of a Positivist

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http://www.ssrc.org/pubs/researching_migration.pdf
For much of the twentieth century, prevailing patterns of population movement, including rural to urban migration, interregional migration, suburbanization, counterurbanization, and immigration, have structured the study of population movement. While defining research questions, the availability of data has heavily influenced empirical research within the social sciences. In the context of the United States, this made the 1940s a watershed period, with the inclusion of the question “Where did you live five years ago?” in the 1940 Census and the introduction of the Current Population Survey (CPS). Since then, a wealth of new data sources has become available, including cross-sectional files such as the U.S. Census Bureau’s Public Use Microdata Sample (PUMS), and longitudinal files such as the Population Survey of Income Dynamics (PSID), the National Longitudinal Survey of Youth (NLSY), and the new American Community Survey (ACS).

This wealth of data has enabled social scientists to make important contributions to understanding the demographic trends that have shaped our society, along with their spatial (i.e., geographic) consequences. The widespread use and availability of census and other public data are due in large part to their validity and the degree of geographic, social, and economic detail embedded in the files. Moreover, the growth of data has corresponded to increasing computational abilities and a refinement and broadening of the analytical tools used within population research, including the ability to test hypotheses through inferential techniques and to gain insight into the causes and consequences of population movement.

Since the 1990s, population research has increasingly focused upon immigration and the adjustment of immigrants within their host society. As one cornerstone of population research, geographic inquiry has focused upon the settlement and spatial assimilation of immigrants within the United States, with the geographic perspective valued for its emphasis on the role of space and place, location, regional differences, diffusion, and its ability to bridge disparate issues (Pandit 2004; Weeks 2004). For instance, the importance of geography is exemplified by the outcome of California’s Proposition 187. Representing a reaction to the perceived concentration of illegal immigrants in Southern California, the debate took on different meanings, roles, and expressions at alternate spatial scales (Clark 1998).

The existence of high-quality, publicly released data files enabled much of this research, with the use of such files often accompanied by theoretical approaches commonly grounded in positivistic science, the goal of which is to verify (or falsify) empirical observations and to construct laws that can be generalized to a wide variety of models and theories (Bailey 2005). However, their use can also be problematic. In part, such data files are often considered incomplete. For example, they often miss details and the motivations for immigration and assimilation and instead rely upon empirically quantifiable notions of
movement and acculturation and statistical inference. Even the immigrant population is typically broadly defined to include all individuals born outside the United States, failing to distinguish between legal immigrants, illegal entrants, and refugees. Similarly, questions have arisen over the continued use of public data files and positivistic methods as a primary insight into immigration, its effects, and its outcomes both at the personal and societal levels.

The purpose of this chapter is to contribute to the discussion and comparison of methodological and theoretical approaches by providing an overview of immigration research based upon large, publicly released data files. This chapter is written from a positivist perspective, meaning that the use of terms such as “test” is purposive, stemming from the viewpoint that publicly available data and their corresponding studies typically entail. That is, following the scientific paradigm, researchers will hypothesize and test relationships and offer a degree of statistical certainty to the observed relationship. In addition, and through the empirical example of Jamaican assimilation in the New York metropolitan area, the chapter is meant to convey both the benefits and the shortfalls of empirical analyses that are rooted in positivism. Throughout, the discussion draws upon my experiences to illustrate the advantages and disadvantages of this approach to immigration research.

**Framing the Research Question: So Many Questions, So Little Time . . .**

Before researchers put pen to paper, or fingers to keyboard as the case may be, they must start with a research question. What is of interest to them? What are the major questions and objectives to be pursued? Frequently, I find that one research question or project leads to the next, such that the research portfolio grows organically over time. For instance, my doctoral research focused upon domestic migration, that is, the movement of individuals across the country, and reflected an outgrowth of a fourth-year undergraduate research paper. By the time I started my graduate studies, I had a good grounding in statistics, an interest in geographic and population processes, and had a passing knowledge of large datasets and their strengths and weaknesses (see, e.g., Foulkes and Newbold 2005). At the start of my graduate career, my supervisor passed a book to me that explored residential mobility in the United States, saying, “I think we can do something similar in the Canadian context,” a conversation (and suggestion) that ultimately led to a long-term affair with large datasets and the use of statistics. So, despite my interest in other data sources, including qualitative work, my research career and the use of large datasets reflected a simple choice at the time based on a conversation with my supervisor and my ease with statistics. Immediately after I completed my PhD, my interests turned to the domestic movements of the foreign-born, again using large datasets that offered a broad view of geographic scales and comparisons across place-of-birth groups. Ultimately, this line of research led
to projects looking at the settlement and assimilation of new arrivals, with a particular emphasis on geographical differences (see, e.g., Newbold 1999, 2004a). More recently, it has evolved again, linking immigration with the health literature (see, for example, Newbold 2005; Newbold and Danforth 2003).

While experience helps in defining a research project and its specific questions, turning to the existing literature for clues is also a key step in the process to frame the problem or analysis. Systematic reviews of the existing literature, while time-consuming, lay the foundations for successful research by identifying the significant contributions to the literature, theoretical backgrounds used by other scholars, methodologies, data sources, and any existing gaps and questions. Moreover, the systematic and careful review of the literature will not only aid in defining the research question, but will also prove invaluable in writing the research proposal and subsequent papers. Review papers are particularly fruitful, providing a “state of the field,” gaps in the literature, and salient references. I also like to set my research questions within the context of policy.

Who would be interested in this research? How does it apply within a broader arena that extends beyond academics? Policy documents frequently reveal shortcomings in the existing literature and provide a great starting point for framing a research question or agenda, particularly when embarking on a new project or in a new direction.

In a perfect world, our data (regardless of whether it is primary or secondary) would capture everything that we need. The reality, of course, is far different, such that public data will potentially disallow some questions to be answered, meaning research questions must be framed relative to the data source. If, for example, someone asked about the movement of illegal immigrants within the United States, all that I could say would be “the data doesn’t allow that to be ascertained” or “there is no information.” In other cases and for other questions, we might be able to say that “there is some limited evidence . . .” Alternatively, key assumptions regarding relationships, measures, or definitions within the data must be made in order for the analysis to proceed. For example, how is the immigrant population to be defined? Working with Census or other public data files, as I commonly do, I typically define immigrants on the basis of when they entered the country, age at Census day, and country of origin, therefore ensuring a relatively consistent group upon which to base comparisons, track assimilation, and build an argument. However, the type of immigrant—whether refugee, illegal, or legal, along with the type of legal immigrant (i.e., family reunification or economic entrant)—cannot normally be identified, one of the shortcomings of working with such data. In this way, the research question and agenda may need to be reframed in light of the available data sources and the information contained within them. Still, finding shortcomings in the data after the research question is set out is better than
looking at the data first and then asking what the data can answer—so-called data-driven research. Doing research this way implies that the cart is before the horse, with a disjuncture in the research process (McCain and Segal 1982).

**Using Publicly Released Data**

With few exceptions, the vast majority of my research draws upon publicly available data files, typified by the PUMS published by the U.S. Bureau of the Census. PUMS files are large samples (one or five percent) of the U.S. population based upon the decennial census, containing records for a sample of housing units with information on the characteristics of each unit and each person in it. Information includes citizenship, country of origin, period of arrival, ethnicity, race, education, and other socioeconomic and sociodemographic data. While preserving respondent confidentiality, these microdata files permit users to prepare virtually any number of tabulations, such as the number of Chinese immigrants in the United States, New York State, the New York Census Metropolitan Statistical Area (CMSA), or New York City. Similarly, we could examine various measures of assimilation, such as the degree of naturalization, distribution vis-à-vis other immigrant groups or the native-born population, or economic advancement.

In large part, the flexibility offered by large data files is attractive in that they allow generalizable results and research questions to be asked from a number of different perspectives, and that statistical inference can be brought to bear upon the analysis, strengthening the interpretation of results. In addition, the reliance upon these data files from a geographer’s perspective is also somewhat pragmatic, owing to our interest in the role of space in immigration, migration, and assimilation (or other phenomena that occur over space). Generating “space” through other means, such as individual one-time surveys, is typically cost (or time) prohibitive as the sample size either needs to be large to adequately represent a particular location, and/or replicated across space to capture spatial differences. The only way to really grasp the role of geography is by using large public data files, with such sources enabling analysis at a variety of different geographic scales including the nation, region, state, county, metropolitan area, and within metropolitan areas.

**Methods: How Do We Get Data to Talk?**

While good research is predicated on questions that address gaps in the literature and based upon appropriate data, methodology is also important. How, for example, are the data operationalized? What analytical methods are to be used? How a process such as immigration is defined and measured may alter its empirical measurement and the derived conclusions. The international migration/immigration literature, for example, distinguishes temporary international migrations, such as the seasonal movement of “snow birds” between colder and warmer climates, transnationalism, or more permanent forms
of immigration. Other issues including the length of interval over which immigration (or any movement) is captured, the size, shape, and characteristics of the receiving and sending regions, and the composition of the sample population affect the analysis. For example, is the immigrant population composed of refugees, legal, or illegal immigrants? What is the origin of the immigrant population? Frequently, the data will (at least partially) dictate how the immigration event is defined (see Plane and Rogerson [1994] or Newbold [2004b] for a more detailed discussion). Variables may also be derived or constructed with the assistance of other sources.

Once measurement and definition are reconciled, the analysis can begin. Researchers using public data files typically draw upon a set of analytical and statistical tools that provide a measure of statistical significance and therefore either prove or disprove initial hypotheses. The analysis frequently begins with descriptive statistics, such as the number of immigrants, their destination, origin, and other salient characteristics, providing an overview of the process. Analytical methods also typically include multivariate analysis.

Drawing on Experiences: The Use of Public Data Files to Evaluate the Assimilation of Jamaicans in New York

The use of large datasets is illustrated by my SSRC-related work, which was broadly concerned with differentials in assimilation across space (see, e.g., Newbold and Foulkes 2004; Newbold 2004a). Using Portes’s segmented assimilation framework as a lens through which to view assimilation and settlement (Portes 2001; Portes and Rumbaut 1996; Portes and Zhou 1993), the goal of this research was to add to the understanding of the geography of assimilation, with the research proposing a spatial extension to the segmented assimilation framework that recognizes the potential for assimilation of the same group to vary across or within space. In doing so, the research explored the settlement and assimilation process of immigrants to the United States, evaluating whether the apparent assimilation of similarly defined immigrant groups, defined by origin, age, and period of arrival, differed across selected areas.

Evaluating the assimilation of a group is difficult, given the temporal nature of the process. That is, assimilation occurs over time, while much of our data is cross-sectional in nature. Although not perfect, one option is to carefully define immigrant arrival cohorts and compare measures of assimilation for each group over space. Data were drawn from the 1990 five percent PUMS, selecting foreign-born Jamaicans ages 20 to 64 who resided within the New York CMSA, which includes New York City and portions of New York and New Jersey, in 1990. The analysis focused upon labor force–age immigrants to avoid “tied migrations” and their impact upon the settlement behavior of the young and elderly. Four cohorts defined by period of arrival in the United States were selected, including;
Very quickly the utility of datasets such as the PUMS becomes apparent, as the analyst has a number of options in terms of defining (and redefining) the immigrant population. For example, the sample population could be easily altered to look at different origin groups, periods of arrival, or geographies, although users are constrained by the questions included in the survey, potential responses, and how the data are actually presented to the user. Above all, while researcher-constructed surveys could ask retrospective questions regarding assimilation experiences, I could not have derived this amount of information through my own research. Imagine trying to find a sufficiently large sample to conduct meaningful statistics while working within the frame of the above-noted cohorts. If you are reading this as a new graduate student, you can start your data collection now, and we’ll talk about the analysis in a few years’ time.

In this case the research considered a specific group of immigrants (Jamaicans) defined by age and period of arrival in order to evaluate various measures of assimilation across space. In particular, I was interested in the assimilation process at relatively small, neighborhood scales, proxied by Public Use Microdata Areas (PUMAs), which provide the smallest geographic unit of analysis available within the PUMS. Rather than analyzing all PUMAs within the CMSA, three were selected for inclusion. Selections were based upon the total population of Jamaican immigrants, with the simple criteria that the selected PUMAs had to have a large immigrant population within each arrival cohort and were representative of different locations (i.e., boroughs in New York City vs. the remainder of the CMSA). In other words, selected PUMAs captured areas of known spatial concentration.

To test notions of variation in assimilation over space, assimilation was measured by variables that captured social, economic, and geographic attributes, and was compared across PUMAs and arrival cohorts (Allen and Turner 1996; Newbold and Spindler 2001). Variables measuring cultural assimilation included English language ability (percentage speaking English not well or not at all) and US citizenship (percentage naturalized citizens). Economic assimilation was measured by
tenure (percentage who owned their home), occupation (percentage in professional occupations), education (percentage with a high school education or less), and poverty status (percentage below poverty line).

Mobility measures and neighborhood further contextualized differences in assimilation over space, particularly with respect to race or ethnicity (Portes and Zhou 1993; Skop 2001). Focusing upon the percent that had changed residence in each PUMA, mobility rates of the foreign-born were used to evaluate who moved and where they moved from, reflecting differential adjustment and migrant selectivity over space. Rates were restricted to those residents in the country in 1985. Finally, neighborhood context was evaluated with respect to the native-born population, including the proportional share of non-natives, African Americans, and Hispanics. The native-born were also evaluated with respect to criteria identified above, including home ownership, poverty status, education, and occupation.

Population Distribution

The geographic distribution of immigrants within the New York metropolitan area provides clues as to the historical, economic, or other processes that shape the settlement system, along with their relative success. Although Jamaicans already represented a large immigrant group in the New York CMSA prior to the liberalization of immigration laws in 1965, immigration flows were supercharged in the post-1965 era (Portes and Rumbaut 1990). In terms of absolute numbers, New York’s Jamaican population exceeded 151,000 individuals in 1990, making it the largest concentration of Jamaicans in the United States, with the majority (67.3 percent) settling within New York City proper, suggesting the importance of networking, access to nearby conational communities, housing, and transportation within the city (Table 1). Sampling PUMAs from both Brooklyn (PUMA 5317), where Jamaicans have settled in predominately African American areas such as Bedford-Stuyvesant, and the Bronx (PUMA 5010), corresponding to the Woodlawn and Wakefield neighborhoods, there does not appear to be any one area containing an overwhelming proportion of the Jamaican population. In fact, less than nine percent of the sample population resided in each of these PUMAs, although both had somewhat larger shares of more recent arrivals, even as the city’s share declined from 68 percent among pre-1965 arrivals to 65.7 percent of the most recent arrivals.

Although home to only 3.8 percent of the Jamaican population within the CMSA, a third PUMA (4405) encompasses parts of southern Westchester County, including Mount Vernon and New Rochelle, New York, areas immediately adjacent to concentrations in the Bronx. Long considered a white suburb, Westchester County has witnessed increasing settlement by immigrants in the recent past. In addition to Jamaicans, who represented half of the inflow during the 1990s, immigrant origins have included East Asia, Europe, and English-speaking Caribbean nations, with these new arrivals
seemingly bypassing traditional areas of settlement within the city. While the area’s share of Jamaican immigrants remained relatively unchanged regardless of arrival cohort (averaging approximately four percent), larger proportions of recent arrivals have settled outside New York City, with nearly 35 percent of the 1985–1990 arrival cohort settling outside the city, compared to approximately 32 percent of pre-1965 arrivals. Together, this suggests that Jamaicans are increasingly settling outside the city at the time of immigration and during subsequent residential searches, while New York City itself is declining in importance.

Spatial Differences in Levels of Assimilation: Personal Attributes

While the spatial distribution of the foreign-born is of interest in itself, the spatial variation in measures of assimilation across the New York CMSA is of real interest and starts to provide insight into the potential role of space within the assimilation process. Of particular interest is whether or not these skills and human capital vary across space with respect to arrival cohort, suggesting that place (after controlling for period of arrival, age, and origin effects) affects assimilation. Table 2 summarizes the various measures of assimilation with respect to location and period of arrival.

Arrival cohort clearly has a significant impact upon measures of human capital and socioeconomic status, reflecting initial stratification and continued sorting, along with socioeconomic advancement with increasing duration of residence. Among Jamaicans arriving between 1985 and 1990 and settling in Brooklyn, for example, approximately 81 percent had a high school education or less. In comparison, 55.6 percent of those who arrived prior to 1965 had a comparable level of education. A similar gradient was observed across most other measures. In general, recent arrivals tended to have poorer English abilities, were more likely to be below the poverty line, less likely to own a home, relative to earlier arrivals.

Since the assimilation process is not defined simply by time to a given end point, differences across space in the measures of assimilation were also pronounced and frequently significantly different when period of arrival is held constant. Among recent Jamaican arrivals, residents of the Bronx were more likely to be engaged in professional occupations and less likely to be poorly educated, relative to those settling in Brooklyn. However, recent arrivals in the Bronx also fared worse than their Brooklyn counterparts on a number of other measures, including poverty status and home ownership. Similar differences are also noted when the Mount Vernon PUMA is compared with the two New York City PUMAs, with residents of the Mount Vernon PUMA generally less likely to be below the poverty line, more likely to have a professional occupation, and less likely to be poorly educated. Although home ownership tended
to be significantly less than residents of the Bronx, this varied by arrival cohort.

Contextualizing Space and Assimilation
Given that the observed differences in the measures of assimilation exist, the question becomes “What accounts for the differences?” In other words, what could account for the apparent differences in assimilation after controlling for age and period of arrival? Although caveats remain, including the use of cross-sectional data to capture an inherently temporal process, the initial sorting of immigrant arrivals, subsequent migrations, and the neighborhood will potentially alter the assimilation process.

With reference to mobility, Table 3 records the major in-migrant flows to each of the selected PUMAs. With relatively small in-migration streams from elsewhere in the United States, the majority of Jamaicans entering the selected PUMAs sourced either from elsewhere within the New York CMSA (pre-1985 arrivals) or abroad (post-1985 arrivals). The Bronx received the largest number of immigrants from abroad (1,462). Internally, the Bronx and the adjacent Mount Vernon/New Rochelle areas had immigration from neighboring areas within the New York CMSA. Moreover, suburban Mt. Vernon received 619 immigrants from abroad, perhaps reflecting its role as an emergent “ethnoburb” (Li 1998), and undoubtedly guided by the existing Jamaican community.

Table 4 summarizes overall in-migration rates (which report the proportion who move into PUMA X regardless of distance) by selected variables, allowing the analysis to address whether differences in assimilation were due to “in-place differences” versus changes due to in-migration. Not surprisingly, those arriving between 1980 and 1984 were significantly more likely to move, with earlier cohorts tending to be less mobile. Although all cohorts were defined as ages 20–64 in 1990, earlier arrival cohorts tended to have a higher mean age and were more likely to have accumulated ties to place, reducing mobility levels. Nevertheless, the findings are consistent with Newbold (1999), who noted elevated rates among recent arrivals as compared to like-aged counterparts who arrived earlier, indicating movement associated with the adjustment to settlement location shortly after arrival. Moreover, a large proportion of this movement was local, as noted in Table 4, allowing proximity, concentration, and interaction within a familiar environment. Mount Vernon had the highest in-migration rate (43.1 percent), a finding that extends to most other indicators, including a high in-migration rate among those resident within the country for the greatest period of time. It was not, however, consistent in its attraction of, for example, positively selective in-migrants. For instance, while in-migrants tended to be homeowners, high in-migration rates were also noted among those with less education (44.3 percent), and those below poverty (21.6 percent), suggesting a broad-based appeal among in-migrants. The Bronx, on the other hand, had the lowest overall in-migration.
rate (34.1 percent), with lower rates among those resident in the country for an extended time, naturalized citizens, individuals below the poverty line, and professionals.

**Once a Positivist, Always a Positivist?**

Regardless of the viewpoint from which research is undertaken, the type of data used, or the methodology, all research is (or at least should be) supported by theory which provides the lens through which we may interpret our results. But are we defined by the tools and “isms” that we are trained with as academics? Several years ago I was reading the reviews associated with a grant proposal that I had submitted to a major funding agency. Part of my proposed research agenda included a qualitative analysis of individuals’ intentions to migrate based upon surveys and interviews, to which one reviewer noted something to the extent that (and I paraphrase) “the researcher proposes to continue positivist-oriented, quantitative research methods and theories.”

To me, this sounded like I was to be forever slotted as a positivist. To be sure, positivists do not necessarily rely upon large datasets such as the U.S. Census, but can apply the theoretical and methodological aspects to smaller datasets such as those derived from researcher-created surveys. That is, if my research methodology was to include a survey administered to movers and nonmovers, asking questions relating to their intention to move, reasons for moving, and so on, I could perform a statistical analysis on the responses in much the same way as if the data were generated by the Census Bureau. It would just be that my sample size would be smaller, and I would have lost some of the power and the ability to generalize the conclusions, but my positivist insights would remain intact.

At the same time, it made me wonder if academics, having chosen a particular “ism” and set of methodological tools, could never “re-tool” later in their career. Recognizing that one of the criticisms of the positivistic approach is its deterministic nature, either neglecting or quantifying (i.e., as in notions of economic rationality, human capital, or utility theory) human behavior, feelings, values, and experiences, the proposed research agenda worked to overcome this deficiency by moving from my methodological strengths in quantitative analysis to the application of new (at least for me) qualitative methods. The necessary bridging was built into the proposal, ensuring that I acquired the needed skills and sought professional assistance to complete the agenda. Moreover, by pitching the proposal as a mixed-methods research agenda, the project promised to integrate the strengths of both qualitative and quantitative research methodologies, increasing its potential to inform policies (Mattingly and Falconer Al-Hindi 1995) and reducing biases associated with a one-method approach to research (Cook and Campbell 1979).

Unfortunately, the proposal was not funded. While breaking away from one’s dominant research tradition is unlikely to be easy, it is (fortunately) possible to re-tool...
later in an academic career. Although using quantitative methods set within a positivistic framework remain my forte, I have been involved in research projects that have used as their basis a very different set of skills and research areas than I finished my PhD with, including ethnographic techniques and other qualitative methods. More recent research has been cross-fertilized by my interests in health geography, such that my current research agenda considers the health status and health care utilization of new immigrant arrivals in Canada.

The reviewer’s comment also made me question if different techniques to collect data were treated with relative sameness, at least in terms of interpretation. Despite the fact that the proposal clearly stated that qualitative techniques including interviews with key informants and focus groups would be used, it appeared as if the reviewer was suggesting that my analysis of the qualitative data would be tinged by my quantitative, positivistic background and expectations. In other words, irrespective of the data source, it would be interpreted through the same lens. To be sure, the application of surveys implies some quantitative analysis, but open-ended questions or probes must be interpreted and analyzed through the appropriate framework, and the results triangulated with other studies or sources.

**Conclusions**

Population geography, like demography in general, has retained a strong empiricism and has contributed to the understanding of domestic migration, immigration, and its spatial manifestations. The advantage of working with large public data files lies in the generalizability, reliability, and replicability that their use implies. Clearly, the use of large data files can be problematic and the methods and techniques criticized, but these issues are not insurmountable. Problems can often be minimized by defining the research question(s) appropriate to the data at hand, by recognizing the potential limitations of the data, by careful analysis using appropriate techniques and methods, recalling that the use of large data files provides one way of looking at an issue, and linking research results to the broader literature and hands-on knowledge.

Beyond data issues per se, recent geographic literature has suggested that much of the existing research has been data-driven and fails to recognize the linkage between data, methods, and the influence of theory upon both. Instead, the literature has charged that research methods tend to accommodate available data (Graham 1999; Long and Nucci 1997; Pandit and Davies-White 1999). The very availability of population data has supported empirical research that has placed a greater emphasis on the opportunities provided by datasets than upon the wider question of relationships, with the nature of the data influencing the choice of the method and research strategies that accommodate the type of data available. In other words, research questions have been limited and, in many cases, defined by the availability of public data. To be sure, there is some
element of truth to these statements, as demonstrated by my own earlier comments, with these sentiments echoed elsewhere in the literature. Long and Nucci (1997, p. 5), for instance, assert that “research issues are framed not simply by events but by data.” Empirical work and data have dominated theory formation, meaning that research has tended to concentrate within data-rich areas, while others remain relatively unexplored. The implication of this commentary is that theory formation has been disadvantaged given the emphasis placed upon data and methods. Instead, theory development has been driven by new and better data that have expanded the outlook of migration questions.

Questions have consequently arisen over the continued reliance on census or other published data sources as the primary insight into immigration and, more generally, migration and population movements (Behr and Gober 1982; Halfacree and Boyle 1993; McHugh et al. 1997; Roseman 1992). Often, details and the motivations for immigration and assimilation are based upon empirically quantifiable notions of movement and the population. Publicly available data cannot, for instance, capture the complexity and subtleties of immigration, the nature of which emerges, for example, in the transnational immigration or gendered literatures. So, while public data remains an important tool, other data sources or research methodologies are frequently employed.

Alternatively, if the challenge facing population geography is to allow theory to guide research methods and questions, a number of nonexclusive alternatives exist. First, while the shortcomings of positivism have long been debated, alternative methodologies promise to provide additional insights into important population issues (Bailey 2005; McKendrick 1999; Pandit and Davies-White 1999). This is not to say, however, that researchers should abandon every tool and data source in the rush to embrace other ideas and methods. In fact, theory and methods have advanced in the intervening years such that human behavior is often incorporated in quantitative and positivistic research. For instance, the increasing availability of individual-level or “microdata” often means that dimensions of human behavior, values, and experiences can be recognized within the research. Statistical models also reflect this need to evaluate the role of the individual, rather than the group, with models such as the nested logit model including individual behavioral effects in a theoretically and empirically meaningful way (Kanaroglou et al. 1986a, 1986b).

In addition, the apparent divide between quantitative and qualitative techniques should be seen more as a continuum, with mixed-methods research programs providing the complimentary insights. Alternative methods and theories should inform, rather than confront, each other. However, a cautionary note is sounded here: Just like quantitative work based upon large public data files, other methodologies and theoretical approaches will not provide all the answers on their own. Moreover, attempts should not be made to reinvent the wheel. Although calls
have been made to recognize the importance of noneconomic issues associated with sociocultural processes such as immigration, migration, and assimilation, these are not new. In an analysis of Canadian intermetropolitan migration, Shaw (1985) devoted considerable energies to exploring noneconomic determinants of domestic migration in Canada, providing evidence of the declining importance of economic effects, while Fielding (1992) conceptualized migration as a cultural issue.

Second, changing the research strategy may also imply that multiple data sources are used, with data defined in its broadest forms. As McKendrick (1999) points out, positivism and empirical analyses encompass a variety of approaches, including survey/questionnaire, interviews, and fieldwork. In itself, data is not a self-defining category. Instead, sources of information become data when they are identified as useful (theory) and ways to use them are developed (method). Where this is absent or underdeveloped, they can limit what is seen as “data.” The use of other, less mainstream data sources can be used to inform commonly used sources (or as a stand-alone source), allowing the exploration of relatively underdeveloped areas of research. Other data sources may offer similar opportunities to inform the process and the development of theory (see, e.g., Foulkes and Newbold 2005).

Third, no researcher is an island unto him- or herself. When I partook in the SSRC-funded research, I was employed by the University of Illinois, an institution that placed great emphasis on solo research as the marker of research eminence and ability. Since then, I returned to my native Canada, where national funding agencies and postsecondary institutions themselves have placed increasingly greater emphasis on interdisciplinary research. This emphasis, while having tangible benefits that can be measured in many dimensions, allows researchers to extend their research expertise beyond their core strengths. Personally, that has led to the transfer of my demographic skills to research projects including transportation, the environment, health, and aging populations, and to engage in work with others outside of my discipline and university.
Works Cited


### Table 1. Spatial distribution of Jamaicans and Cubans (%) by selected New York CMSA PUMAs: Ages 20–64, 1990.

<table>
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<tr>
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<td>60.2</td>
<td>67.5</td>
<td>75.6 *</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total</td>
<td>45.9</td>
<td>50.6</td>
<td>47.9 *</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Education: High School or Less</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>1985–1990</td>
<td>81.3</td>
<td>66.6</td>
<td>64.0 *</td>
<td></td>
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<tr>
<td>1980–1984</td>
<td>54.8</td>
<td>63.3</td>
<td>66.3 *</td>
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<tr>
<td>1965–1979</td>
<td>58.5</td>
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<td>37.4 *</td>
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<tr>
<td>Pre-1965</td>
<td>55.6</td>
<td>49.8</td>
<td>33.1 *</td>
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<tr>
<td>Total</td>
<td>60.0</td>
<td>61.7</td>
<td>48.2 *</td>
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</table>

Note. Sig1, t-test of differences in proportions between columns 1 and 2, p < .05. Sig2, t-test of differences in proportions between columns 1 and 3, p < .05. Sig3, t-test of differences in proportions between columns 2 and 3, p < .05.
Table 3. Major Jamaican in-migration flows to selected New York PUMAs by 1985 place of residence.

<table>
<thead>
<tr>
<th>Location</th>
<th>Jamaicans</th>
<th>Migrants</th>
<th>%</th>
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<tr>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Brooklyn 5317</td>
<td>4,474</td>
<td></td>
<td></td>
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<tr>
<td>Immigrants</td>
<td>3,042</td>
<td>68.0</td>
<td></td>
</tr>
<tr>
<td>Within Brooklyn</td>
<td>1,194</td>
<td>26.7</td>
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</tr>
<tr>
<td>Rest of U.S.</td>
<td>238</td>
<td>5.3</td>
<td></td>
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<tr>
<td></td>
<td>4,474</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Bronx 5010</td>
<td>4,132</td>
<td></td>
<td></td>
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<tr>
<td>Immigrants</td>
<td>1,462</td>
<td>35.4</td>
<td></td>
</tr>
<tr>
<td>Within Bronx</td>
<td>1,957</td>
<td>47.4</td>
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<tr>
<td>Brooklyn</td>
<td>182</td>
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<tr>
<td>Queens</td>
<td>181</td>
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<tr>
<td>New York suburbs</td>
<td>197</td>
<td>4.8</td>
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<tr>
<td>Rest of U.S.</td>
<td>153</td>
<td>3.6</td>
<td></td>
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<tr>
<td></td>
<td>4,132</td>
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<td></td>
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<tr>
<td>Mt. Vernon, NY 4405</td>
<td>2,152</td>
<td></td>
<td></td>
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<tr>
<td>Immigrants</td>
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<td>Within NY suburbs</td>
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<tr>
<td>Rest of U.S.</td>
<td>58</td>
<td>2.7</td>
<td></td>
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<tr>
<td></td>
<td>2,152</td>
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Chapter Six  Understanding Migrants’ Remittances: Evidence from the U.S.–Nigeria Migration Survey

By Una Okonkwo Osili
Associate Professor of Economics
Indiana University-Purdue University at Indianapolis

http://www.ssrc.org/pubs/researching_migration.pdf
.... the story we had to tell could not be told for us
—Chinua Achebe
*Morning Yet On Creation Day*

It is difficult to remember the exact moment when I started asking questions about international migration. A straightforward answer is that my interest in the subject dates back to my very name and childhood. My first name, “Una,” translated from Igbo, a Nigerian language, fittingly means “going home.” I was born in New York City, and six months later my family left America and returned to southeastern Nigeria. Upon their return, my parents dwelt briefly in a newly constructed family home, which was built on ancestral family land over a period of two years and which they had financed with their hard-earned U.S. savings. Although I did not realize it at the time, this home, built during my family’s sojourn in the United States, would occupy my thoughts and provide rich fodder for my doctoral dissertation.¹

As a young child growing up in Nigeria in the 1970s and 1980s, I welcomed several uncles and aunts who had returned from overseas. I was also exposed to economic migration by neighbors—hailing from nearby countries such as Ghana and from lands as far away as India, Pakistan, and Bangladesh—who had migrated to take advantage of the new opportunities created by Nigeria’s oil boom and sent money—termed “remittances”—to their relatives back home. During my teenage years, as Nigeria’s economic fortunes declined with the collapse of global oil prices, I heard family members and friends discuss visas and job prospects in London, Johannesburg, and Riyadh.

But as I soon learned, in many cases, migrants do not truly leave their home countries. The rising economic profile of global remittance in volume and impact is only one piece of evidence.² Over time, I observed the growing importance of remittances and international migration to Nigeria and other parts of sub-Saharan Africa. Billboards now grace the major streets in Lagos, Port Harcourt, Warri, Ibadan, Enugu, and other lesser-known Nigerian cities, advertising how to receive money transfers safely, discreetly, and quickly from anywhere in the world. New terms have emerged to describe migrants

¹This research was funded by grants from the International Migration Program of the Social Science Research Council and the Ford Foundation Dissertation Year Fellowship. The Program on African Studies, the Center for International and Comparative Studies, and the Dissertation Year Grants Committee at Northwestern University provided financial assistance. This paper has benefited greatly from discussions with Joseph Altonji, Sara Berry, Meagan Berger, Joseph Ferrie, Jane Guyer, Leslie Kersey, Ifeanyi Osili, Peter Rangazas, Anna Paulson, Steve Russell, Thomas Wiseman, and Christopher Udry. I am grateful to Edith Nkwonta, Rina Okonkwo, Paul Okonkwo, and Uzochi Ukwu for their research assistance during my fieldwork in Nigeria.

²In 2005, remittance flows from international migration to developing countries through official channels were estimated at close to $180 billion and exceeded foreign investment or aid flows to many developing countries (World Bank 2003). The Bureau of Economic Analysis estimates that remittances from U.S. immigrants to their origin countries reached $25.5 billion in 2003 (Congressional Budget Office 2005).
and their destination communities—many hometowns have special celebrations to include their “foreign-based” sons and daughters in Yankee, Jand, and Italo.  

For many origin families in Nigeria, the transfers that migrants send from abroad have induced a visible expansion in their economic and social opportunities. Even more impressive are the migrant-funded businesses and community development projects that are now part of the landscape in many towns in southeastern Nigeria. A persistent question concerns why some migrants send generous sums of money for food, health, and education expenses for their relatives at home—and even toward hospitals, schools, and libraries in their home towns—while other international migrants decide not to invest in their families and communities in Nigeria. Having a son or daughter abroad means a brand-new house and upward socioeconomic mobility for many parents, yet for others the dollars never arrive, leaving them with the bitter complaint that their children have been seduced by the comforts of living in the richer West.

Several people have asked me why I chose the study of remittances as a dissertation topic. My response is that the topic seemed so intriguing, important, and timely, yet woefully underresearched. I was particularly interested in learning how origin families and communities use remittances. And I wondered whether remittances represent merely an attempt by migrants to fulfill obligations of the societies they left behind—to provide for aged parents and other relatives, for example—or whether the process was more complex.

Today, the bulk of international migrants are from developing countries, and remittances represent a form of income redistribution from the affluent West to poor countries in the developing world. A number of recent studies have also emphasized that migrants’ remittances can function as investment capital in the origin country (Dustmann and Kirchamp 2002; Mesnard 2004; Woodruff and Zenteno 2002). Did migrants’ remittances have any long-term implications for economic development in the communities that migrants left behind? In recent debates, policy makers and researchers have increasingly argued that the migrant remittances can have these long-term implications particularly if invested in the origin country. My instincts were to investigate remittances and to uncover their impact on both senders and recipients of these transfers.

**Acquiring Tools and Methods**

In graduate school, I was exposed to economic models of transfers and international migration. My coursework in development economics provided a formal opportunity to grasp economic theory and to take on challenging topics that had intrigued me for much of my life. I was very fortunate as a second-year graduate...
student to enroll in Professor Joseph Ferrie’s American economic history course, which offered a wealth of historical and economic perspectives on U.S. migration. In my coursework, I intently studied the earliest literature on international migration, devouring many of the texts, highlighting relevant passages in bright yellow, and taking copious notes.

It was in this American economic history course that I first realized that remittances were a long-standing feature of the migration experience. Today’s international migrants seem to mirror some patterns evident at the turn of the twentieth century. Although accurate statistics are difficult to obtain, the evidence suggests that remittances were an important outcome of U.S. immigration in the early twentieth century. The New York Post Office sent 12.3 million individual money orders to foreign countries between 1901 and 1906, with 50 percent of the dollar amount going to Italy, Hungary, and Slavic countries (Wyman 1993). According to the Immigration Commission, the sum of remittances to Italy totaled $85 million in 1907 (Foerster 1919, 374). Beginning in 1890, the influx of savings—whether through international money orders, letters, immigrant banks, or returning migrants—became commonplace for many Italian households. According to Cinel (1991, 123),

American remittances created a lot of converts to the soundness of emigration. . . . The first convert was the average Italian. Remittances became a household item. New York, Rio de Janeiro, and Buenos Aires were closer to the heart of the average Italian than Rome or Florence. Fare l’America turned into the main goal for young people, almost a sign of manhood. Egisto Ors reported that in some southern Italian towns young men who had never crossed the Atlantic had a hard time finding a bride.

These statistics and my initial readings were enlightening, but they also left many unanswered questions. Although an extensive amount of literature deals with the adaptation of immigrants to their host communities, much less is known about the impact of migration on the origin communities. The long history of international remittances suggested some unanswered questions on economic linkages that tend to persist between migrants and their origin families. My dissertation was born out of a desire to contribute to knowledge on this topic.

Unpacking Remittances

Finally, I had a topic—the study of remittances—but as any good graduate advisor will tell you, this step was only
a start. I had read several theoretical and empirical studies of remittances, had prepared a critical review of the literature, and had discussed these studies extensively with my main advisor, Professor Chris Udry. But I still had a lot to do. I needed to develop new economic models of migrants’ remittances, which I hoped would yield a set of clear testable hypotheses. My preliminary models involved the senders and recipients of remittances. I was also interested in distinguishing between classes of remittances—migrants’ transfers to family members versus their savings in the origin country. In addition, I sought to understand to what extent migrants contributed to community development projects in their origin communities. I began the search for relevant data sources.

By the beginning of my third year in graduate school, I had inched closer to my goal—I now had some reasonably well-developed models of remittance behavior. It was clear that my research questions would involve disaggregating remittances, literally “unpacking” the forms of economic exchange that take place between international migrants and their families in origin countries. This task was bound to be challenging, because remittances are maintained over time and across large geographical distances. As I gained familiarity with existing data sources, I became well acquainted with their limitations. Many appeared inadequate because they treated remittances in an aggregate fashion or assumed that remittances were sent only to support the consumption needs of the origin household.

As I searched for statistics on remittances to Nigeria, I discovered that a large share of the transfers from Nigerian migrants take place through various informal channels, and few accurate statistics on migrants’ remittances and other economic linkages exist. I also discovered that recent migrations from sub-Saharan Africa to the West were just starting to attract the attention of researchers and policy makers on both sides of the Atlantic, and very little information on the size and scope of these growing communities was available.†

Beginning Field Research: “You May Need to Collect Your Own Data”

After several meetings with my dissertation committee, it became apparent that I would need to collect my own data on remittances. But, unlike anthropology and sociology, graduate students in economics are seldom trained in survey methods and field research. Instead, most graduate programs in economics are geared to equip students with sophisticated econometric techniques and modeling tools to allow them to answer questions by using existing data sources. This approach works well for formulating and researching questions in the United States and other developed countries because a dizzying array of datasets is available on household-level decision making and behavior. For many developing

† Some of the growth in African migration to the United States may be due to changes in the U.S. immigration policy that increased the number of employment visas for skilled workers and to the introduction of the U.S. diversity visa program.
Preparing for Field Research

As I prepared to enter the field, I kept a journal. During the summer of 1996 I conducted preliminary field research among Nigerian emigrants in Chicago. I started by attending Nigerian immigrant association meetings and events. Fortunately, I was warmly received at these gatherings. As a Northwestern graduate student—and a Nigerian—many emigrants were happy to share their views and experiences. Because I had no formal survey or questionnaire yet, I had an unconstrained opportunity to learn about the environment in which remittances are transferred.

Initial conversations with Nigerian migrants in Chicago were illuminating. I asked detailed questions to gain an understanding of the various classes of remittances and listened to lengthy discussions as migrants themselves debated the various aspects of how and why they sent money to their families back home. During this period, I learned about three main classes of remittances: transfers to family,
investments, and contributions to community development initiatives. I also learned about migrants’ investments in their origin communities.

As I developed a survey instrument, I needed an awareness of people, institutions, and locations. I wanted to learn as much as possible about how migrant households and communities were organized. Identifying the unit of analysis was important, but as I discovered, the flow of remittances across international borders suggests an enhanced notion of “household,” encompassing more than simply a group of people who share the same roof or eat from the same pot. Although migrants reside in different and distant geographical locations, they continue to participate in family decision making and the familial pooling of resources with faraway relatives.

The flow of remittances also challenged the notion of space and led me to increasingly think about migrants and their families as transnational agents. Nigerian migrants often maintain strong economic and social linkages to their origin communities. Informal conversations provide a unique opportunity to study the ties that tend to persist across transnational locations and the process of sending transfers to family members in the country of origin. As I soon learned, a large number of Nigerian migrants did not plan to live in the United States permanently; many hoped to return to their home communities at some point in the future—upon retirement, after children left for college, or, more elusively, when conditions improved in Nigeria. If migrants perceived migration as temporary, this view was likely to affect the scope of remittances from international migration.

I also observed the importance of social networks and institutions in the remittance process. In particular, migrants’ hometown associations assume a highly visible role in the context of international migration. In addition to providing information about the social and political issues that affect their communities of origin, hometown associations can also play an instrumental role in mobilization of remittances from international migrants towards various community development initiatives.

Field Research in Two Connected Worlds
Collecting Data in Chicago—A Relative Insider’s Perspective
I began the next stage of my dissertation research with some advantages. Preliminary field research had given me confidence in my ability to undertake a full-scale survey. I employed several strategies during this research to build trust and confidence with the survey respondents. My goal was to provide respondents with a lot of information about the dissertation project and my background—who I was, where I was from, and why I posed certain questions. Also, I was a relative insider. I was familiar with the migrant community in which I would be conducting my research. I spoke Igbo, the language commonly spoken in southeastern Nigeria. I also learned while conducting preliminary work in the field that migrants did not appear inherently suspicious of a graduate
student conducting research. Many seemed at ease during our conversation and relatively open to questions, even those that centered on very sensitive issues such as remittances and income. Finally, I had pretested my survey instrument and significantly revised the questionnaire, improving or eliminating poorly devised questions.

The next stage involved the selection of a random sample of immigrant households. To obtain a random sample of Nigerian emigrants, I searched Chicago area telephone listings by surnames and first names, selecting distinctly Igbo names. I spent many weeks at the Evanston Public Library searching through the database and extracting phone numbers and addresses. To facilitate eventual location of and interviews with the home families of the initial survey respondents, I restricted my U.S. sample to the Igbo of southeastern Nigeria. Each respondent in the U.S. sample would complete a required information sheet, identifying the head of their origin household and two other adult family members who would be available for interviews in Nigeria.

Beginning in March 1997 I began the actual field research process designed to broadly investigate the economic ties that exist between migrants and their communities of origin. I planned to interview at least 100 Nigerian migrants in Chicago. The small sample size would permit me to conduct personal interviews with each survey respondent and to develop the more qualitative aspects of the study. Many of the interviews lasted two hours, but some lasted much longer. I also allowed time for informal conversations and for migrants to share their experiences with sending money home. Because my research methodology involved the development of theory, data collection, and empirical analysis, I enjoyed extended conversations of remittance patterns with migrants as they allowed me to refine formal models of remittance behavior.

Another important strategy that I employed in the field was to start each interview with a general set of questions that dealt with basic demographic information about age, sex, education, employment, and U.S. migration experience for household members. A household was defined as all family and nonfamily members who lived and ate in a particular household for at least six months out of the past year. This definition was expanded to include children of the head who were away at school but continued to receive financial support from the household being interviewed. I collected more sensitive data on immigration status, networks in Nigeria, and income toward the end of the survey.

Using the migrant survey instrument, I obtained information on the various methods that migrants use to transfer income to their home families and the types of remittances that were sent during the
I heard migrants’ stories of transnational lives—the decision to leave Nigeria, adjustments to Chicago winters, and the challenges and joys of raising Nigerian American children. I also collected detailed information on the migrants’ asset holdings in the country of origin and the host country. Discussions of asset holdings emerged as a highlight of many interviews. Several migrants retrieved documents and photos of their houses and landholdings in Nigeria. The final U.S. sample comprised 112 Nigerian migrant households in Chicago, Illinois. I shared with the Chicago migrants my plans to interview their families in Nigeria and emphasized that my research questions involved studying both sides of remittances—the senders as well as the recipients.

Field Research in Nigeria: “Why Do You Want to Know?”
I arrived in Nigeria in June 1997 ready to start collecting data from the origin families. My experience with the migrants in Chicago had gone smoothly and I expected to have few difficulties interviewing the origin families. I spoke Igbo fluently and would not require an interpreter to conduct my interviews. I felt optimistic that I would readily gain the trust of origin family members in Nigeria.

But my first interview did not go as planned. The origin family that I interviewed seemed uncomfortable and suspicious, inquiring several times why I needed to ask them so many questions. My attempts to explain that I was a doctoral student conducting dissertation research did not alleviate their concerns. I began to realize that I was no longer considered an insider to the origin families because I had lived outside Nigeria for nearly a decade. My interview techniques, which had worked so well in Chicago, appeared less suitable for the origin families in Nigeria. I decided to take some days off from the surveys to reacquaint myself with the Nigerian environment. My parents, who had some experience in conducting field research and data collection in Nigeria, provided useful advice and suggested that I start by introducing myself not as a researcher, but as an individual. Few Nigerian households had direct experience participating in a research survey, so explaining the goals of the survey would yield only limited benefits. Instead, my parents recommended that I build trust and confidence by sharing with the respondents information about myself, my hometown, my parents—and perhaps most important, my relationship with their family members in Chicago.

My second interview proved more successful. The migrant in Chicago had already informed the origin family in advance that I would be contacting them upon my arrival in Nigeria. I approached the interview differently this time, giving information about myself and providing...
ample opportunity for them to ask questions about their family member in Chicago.

I did confront some additional challenges in conducting field research in Nigeria. Locating origin families involved extensive travel and many of the interviews in rural areas could not be scheduled in advance, making it difficult to plan ahead. In rural areas, I was typically able to find the respondents at home at the time of the interview and to locate the origin household using the information provided by the migrant household. Locating the origin household was more difficult in urban areas of Nigeria where streets tend to be unmarked. I relied heavily on inquiries within the general vicinity to locate the residence of the origin household. I traveled to both urban and rural areas at the peak of the rainy season in southeastern Nigeria. Most Chicago migrants refer to their hometowns as villages, irrespective of the town’s level of urbanization. Some of the towns are well-defined settlements, while others are merely clusters of houses. Land area, population, urbanization, and level of development were included in the sample.

<table>
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<th>Insights from Field Research: Developing Formal Models</th>
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<td>By the time I arrived back in the United States in September 1997, I had experienced the same physical journey that many of the migrants in my Chicago sample had undertaken. I accomplished even more than I had expected. Originally, I set out to examine the economic linkages between migrants, their families, and their home communities, but I also learned about the social institutions in Nigeria and Chicago that facilitated these flows. I was now beginning to form a picture of two worlds connected by migration and remittances. My research had the potential to shed light on the specific connections that migrants maintain with their origin families and communities.</td>
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</table>

After looking over my field notes and reflecting on my conversations on both sides of the Atlantic, I initially concluded that remittances were more complex than mere capital transfers that migrants sent to their families for food and consumption needs.
My interviews and data analysis from the U.S.–Nigeria migration study suggested at least three main classes of migrant remittances: family transfers, investment-related transfers, and community transfers. I was particularly interested in developing models of investment-related transfers, or remittances that are sent to finance the migrants’ own investments in the country of origin, as well as community transfers, or remittances that are sent to support development projects in the migrant’s origin community.

**Mixing Methods:**

**Migrants and Housing Investments**

A key observation from my data collection was that nearly half of my sample of Nigerian immigrants in Chicago had initiated substantial housing investments in their communities of origin in southeastern Nigeria. I had started my research with a general interest in remittances. By now, I realized that I could not ignore the importance of migrants’ housing investments. I began to develop questions related to those investments. Why did Nigerian migrants invest in housing in their origin communities while they lived and worked in the United States? Why were housing and real estate the dominant investments that migrants initiated in their origin communities?

During my fieldwork, I caught glimpses of many houses in southeastern Nigeria and was warmly received by nearly all the people that I visited. Some houses belonged to migrants who lived in Nigerian cities such as Lagos, Kano, Abuja, Port Harcourt, and Enugu. But many houses belonged to migrants who lived in cities and towns in the United States, the United Kingdom, Saudi Arabia, Italy, and other destination countries. Migrants’ houses could be distinguished by their imposing two- or three-story red-brick structure, unusual in villages of small cement-block and mud bungalows.

My field research experience provided some clear directions towards understanding migrants’ housing investments in their country of origin. To illuminate the motivations for migrants’ housing investments, I developed three main theoretical models informed by insights from field research. They suggest that all three classes of migrants’ remittances can be modeled as a function of migrant and origin family characteristics, as well as hometown variables.

Home ownership is a near-universal symbol of economic achievement. In the Nigerian context, housing represents a less risky class of investment. Housing also has several desirable properties including durability, low monitoring costs, and visibility. However, I could not rule out the possibility that the dominance of housing could also reflect limitations in the investment choices available to migrants in the country of origin. My goal was to uncover the primary factors that formed the migrants’ decision to invest in housing. Field research provided a unique opportunity to learn about the motivations behind housing investment decisions through direct...
observations and informal discussions with migrants and their home families.

The evidence from Nigeria suggests that migrants invest in housing to strengthen and maintain their membership rights in their origin communities. During informal discussions, the Nigerian migrants in the United States emphasized the link between housing and membership in the home community. My fieldwork in Nigeria confirmed this relationship. In many villages, there was relatively good information about migrants who resided outside Nigeria, particularly when these migrants owned houses in the hometown. Within the hometown environment, home ownership served as a means of identification. Migrants could be distinguished within the hometown according to whether they owned a house(s) or whether they were currently building a house there.

Discussions with migrants highlighted the role of the home family in the decision to invest in housing. In some cases, migrants undertook housing investments in order to provide a flow of housing services to their home families. In fact, about half of the migrants’ housing investments in home villages were occupied by the migrants’ home families in Nigeria in the absence of rental payments.

While return migration plans are clearly important, they alone cannot fully explain the timing of migrants’ housing investments. Migrants incur the opportunity costs associated with forgone savings by investing now, rather than upon their return. If there are no obvious economic returns from investing in housing now, migrants could postpone their housing investments until they return to Nigeria. Within the sample of Nigerian emigrants, nearly 30 percent of these community of origin residences (twelve houses) were reported to be vacant. In addition, incomplete housing markets in southeastern Nigeria limit rental and resale transactions involving housing assets.\(^\text{11}\)

By analyzing data on migrants and origin families, I found evidence that houses, even when they are vacant, play an important role in signaling migrants’ resources and support of their origin family. Specifically, I found that housing investments provided information about the migrants’ resources abroad and the home family’s access to these resources. Informal interviews during the course of my fieldwork verified the role of migrants’ housing investments. According to one village member, “A house is a highly visible sign of accomplishment and wealth. Few people in the hometown can observe the migrant’s income level or the social status that a migrant has achieved in the United States, but the entire community can observe the size and quality of houses that the migrant has built in the hometown.” The flow of information about the migrant’s resources, and the migrant’s resource connection to the home family, offers benefits to the home family within the hometown environment.

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\(^{11}\) There were only two residential houses within the home village currently being rented out for cash payments.
Beyond Migrants’ Housing Investments

By studying migrants’ housing investments, I gained insights into other aspects of migrants’ remittance behavior. In more recent work, I have returned to my original set of questions, with the goal of unpacking remittances. The U.S.–Nigeria migration survey provided an opportunity to investigate remittances using a matched sample of migrants and their origin families.

Using my data, I constructed detailed measures of migrants’ families, investments, and community transfers. Family transfers are defined as the total remittances sent to the origin family. In contrast investment transfers are defined as the sum of all investment-related remittances sent by the migrant to finance own investments in origin country assets in the survey year. Finally, community transfers refer to the total remittances that are sent towards community development projects in the origin community.

My results from Nigeria suggest that investment-related flows differ in important ways from family transfers. Family transfers appear to be motivated by economic considerations, with poorer origin family members in Nigeria receiving larger transfers, other things being equal. A different picture emerges from investment-related transfers, which tend to flow toward wealthier origin households.

There is descriptive evidence that migrants maintain direct economic ties with their communities of origin. The unique data from the U.S.–Nigeria migration survey allows me to investigate the likelihood that the migrant has initiated a community transfer as well as the total amount sent towards community development projects in the hometown. The main prediction of an economic model of community transfers is that less-developed hometowns should receive more community-related transfers, other things being equal.

The findings from the U.S.–Nigeria migration survey suggest that hometown characteristics play an important role in the migrant’s decision to send a community transfer. However, migrants tend to send community-related transfers to more developed hometowns, and not to less developed communities. I used population and the distance from urban centers to capture important aspects of hometown development. I also examined the impact of additional indicators of development, including the number of higher education institutions, access to a major road, electrification, and

12 Within the African setting, hometown associations formed by migrants play a prominent role (Attah-Poku 1996; Egboh 1987; Smock 1971). They are of considerable importance in mobilizing migrants’ contributions toward the construction of schools, hospitals, roads, and the provision of other amenities in the community of origin.

13 Other measures of village development delivered similar results. For example, empirical results using a dummy variable that took on the value of 1 if the hometown is an urban or semi-urban community, and 0 otherwise also contradicted the predictions of the community-investment model, since more urbanized hometowns had a higher likelihood of receiving migrants’ community-related transfers, other things being equal.
access to potable water. These results did not support the altruism model of community transfers in that migrants appeared less likely to send community transfers to less-developed villages.

Conclusions

My research questions led me on a journey across the Atlantic. Like many migrants, I had ties on both sides of the ocean. However, unlike most migrants returning home, I found myself in a unique position—returning home not just to visit relatives, to attend a wedding or a funeral, but also to find answers to questions on remittances. There are so many questions about what happens to these families and communities when their sons and daughters move away from the developing regions of the world that they know so well. I felt uniquely placed to answer some of these questions. America represented a distant land for many origin families in Nigeria. However, during my field research, both worlds were in full view to me, the researcher. Remittances from international migration have increased rapidly, generating debates on both sides of the Atlantic Ocean on how best to channel these transfers toward economic development. To a large extent, the eventual impact of remittances will depend on the end use of remittance flows in the origin country.

My travels in Chicago and Nigeria suggest that some migrants, over the course of their migration experience, can impact the lives of their families left behind. My research also allowed me to reflect on my own family’s experiences, beginning with my parents’ decision to return to their home country and their decision to build a house in our hometown in southeastern Nigeria. In my case, my parents’ sojourn abroad allowed them to erect a shiny cement and glass house to replace the mud house their family once inhabited.

Investing in housing provides migrants with an opportunity to learn about investment conditions in the origin country. Migrants can then apply the institutional knowledge gained from housing investments towards a wider set of investment objectives. Massey and Pirrado (1994) argue that migrants’ savings are first channeled toward providing for the consumption needs of the home family, and once basic needs are secured, then migrant families allocate their savings towards investment goals. If migrants begin with housing investments in their origin communities, but then go on to provide initial capital for a new family business and to contribute to schools, hospitals, erosion control, and other projects in their hometown, remittances will have even greater implications for long-term economic development in sending regions.

14 Unobserved heterogeneity across hometowns (such as the size and strength of migrant networks, investment technology, and construction costs) may also influence the migrant’s community transfer decision.
Works Cited


Chapter Seven

Changing the Research Question: Lessons from Qualitative Research

From
Researching Migration: Stories from the Field

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http://www.ssrc.org/pubs/researching_migration.pdf
In 1996 I entered a master’s degree program in sociology after completing one in political science at the University of Memphis. I was excited about designing a project that would combine my interests in violence and conflict, gleaned from my training in political science, with the qualitative research methodology I was learning in sociology.\(^1\) A sociology professor suggested that a study of refugee resettlement in Memphis might accomplish that. I met with staff from the local branch of Refugee Services of Catholic Charities, Inc., and made arrangements to meet with Somali refugees. After a few preliminary meetings with the refugees and after reviewing the literature on refugee resettlement in the United States, I formed my original research question: How do Somali refugee women experience changes in gender status during resettlement?

Sociology provided me with a wealth of scholarship that supported the value and contribution of qualitative research (Lofland and Lofland 1995; Denzin and Lincoln 1994; Reinharz 1992). One criticism that sociologists pose regarding qualitative research design is that social scientists tend to choose such methods—interviewing, participant observation, ethnography, and archival research—only when they encounter a paucity of available quantitative data (Ragin 2004). Sociologists are likewise critical of how scholars who favor quantitative methods often view qualitative research as supplemental to quantitative research or inferior to the “theory → hypotheses → data collection → analysis → conclusion” model used in quantitative studies (Blee 2004, 55). I was convinced that qualitative research had value not as supplemental data or as an inferior alternative to quantitative methods; its value was its ability to answer the research question.

I chose a qualitative research design for this project because these methods best answered my research question on how female Somali refugees experienced changes in gender status. I conducted in-depth interviews and participated in various events sponsored by Catholic Charities because I was interested in gathering ethnographic data on the process of how Somali women experienced and understood refugee resettlement. I interviewed six Somali women about their lives in Somalia, upheaval during the civil war, the Kenyan refugee camps, and adjustment to their new homes in the American South. During one interview, Fathia\(^2\) angrily recalled the day she left Somalia: “My mother and I, we tried to gather all the gold before we ran. But we could not. There was so much gold, we could not carry it all. When we got to Kenya, the military took it. They took everything from us” (Oxford 1998, 62). In inquiring about the day Fathia left Somalia, I had

\(^1\) This material is based upon work supported by the National Science Foundation under Grant No. 0211694 and by a fellowship from the International Migration Program of the Social Science Research Council with funds provided by the Andrew W. Mellon Foundation.

\(^2\) All interviewee names for my master’s thesis and dissertation are pseudonyms.
anticipated an account of the experience of leaving a country torn by civil war, an account adorned with details of violence and harrowing escapes from death; instead, I heard one of regret over the loss of material wealth. This interview became a turning point in my research. Had I asked a narrower question about her departure, I may not have gleaned insight regarding her class status in Somalia, which led to a consequential piece of information for my research.

From this interview, I learned that interview guides in qualitative research should be understood as just that—guides for conducting interviews. When an interviewee answers a question in an unexpected way, thereby illuminating the research problem, the interviewer needs to incorporate this new information into the current interview and into the guide for future interviews. My preconception that these women had been poor in Somalia obscured my ability to design an interview guide that tapped questions about changing class status. The initial interview guide included questions about changes in gender status, such as employment in the paid labor market and the Muslim dress code of veiling. After this interview, I changed the interview guide to include questions about class. Thinking of the interview guide as flexible instead of immutable allowed me to develop an argument about how refugees experience and perceive downward mobility when they come to the United States. I changed the research question from “How do Somali refugee women experience changes in gender status during resettlement?” to “How do Somali refugee women experience downward mobility during resettlement?”

Because conformity across interviews is important for comparability, altering the interview guide presented the following conundrum: Is it preferable to ensure reliability across interviews by asking everyone the same questions, or to ensure validity of experience by asking different questions? I chose to alter the research guide and to ask questions about class background in the remaining five interviews. Because I interviewed Fathia first, there was no trade-off between reliability and validity. However, this methodological challenge emerged in my dissertation research well into the interview stage, which necessitated further thinking and decisions about interview guides and data comparability in qualitative research.

Developing Research Questions for New Social Science Research

After completing my master’s degree, I entered a doctoral program in sociology at the University of Pittsburgh. I took a class with an anthropology professor who had served as an expert witness on an asylum case. Knowing of my interest in refugee women, she encouraged me to write a research paper on gender-based asylum, which served as the basis for my dissertation project. Designing and implementing a research project on a topic that has not been explored by social scientists is a daunting task. Migration scholars have studied various aspects of gender and forced migration that
include violence in refugee camps (Martin 2004), changing status during resettlement (McSpadden and Moussa 1993), and economic adaptation (Montgomery 1996). Yet none have examined gender-based persecution and asylum claims in the United States.

Gender-based persecution laws and policies allow immigrant women to claim asylum based on forms of harm that historically were not considered persecution. On May 26, 1995, Phyllis Coven, director of the International Affairs Office of the Immigration and Naturalization Service (INS), issued a memorandum that provided asylum officers with “guidance and background on adjudicating cases of women having asylum claims based wholly or in part on their gender” (Coven 1995). This policy memo, along with growing case law, created legal recourse for women claiming persecution based on gendered forms of harm, such as female circumcision, coercive population control, honor killing, domestic violence, rape, forced marriage, and repressive social norms.

My training in feminist theory and gender studies provided me with a conceptual framework for designing a research project on gender-based persecution. A mainstay of feminist theory is a critique of how gender often serves as a synonym for women (Lorber 1995). According to feminist scholarship, the conflation of gender (a relational concept) to women (a descriptive category) is present in much nonfeminist scholarship. Based on my initial reading of the INS policy that allowed women to claim asylum for gender-based persecution, I formulated three preliminary research questions that guided my review of the legal scholarship on asylum. The two concepts that shaped my thinking on this subject were gender and persecution. What is gender? What does it mean to be persecuted? What does it mean to be persecuted on account of one’s gender? These questions served as my original research questions as I canvassed the academic literature on asylum.

In preliminary research on gender-based asylum claims, I found that academic debates about gender-based persecution and asylum originated and have overwhelmingly remained within the disciplinary boundaries of legal studies (Anker, 2001; Goldberg 1993; Kelly 1993). Legal scholars understand gender-based asylum laws and policies within the context of U.S. case law, international law, and INS regulations, making the law—not people—the subject of inquiry. Such a focus ignores the social relations and networks of people who make, implement, use (and misuse), and rely upon the law. My training as a qualitative social scientist led me to ask different questions about gender-based persecution than those asked by legal scholars. Unlike legal scholars, who focus on the outcome of a case, I privileged process over outcome in my research questions and design. By this, I mean that I was less interested in laws and policies per se and more interested in how human beings who created, implemented, and relied upon these laws and policies understood gender-based persecution.
Similar to the research question from my master’s thesis that addressed the process of how Somali refugee women experience downward mobility, I was interested in the process of how what I later termed “differently situated participants” (asylees, immigrant service providers, immigration attorneys, immigration judges, INS asylum officers and supervisors, and human rights activists and organization employees) interpret gender-based persecution. After perusing scholarship on gender and asylum, I began by asking what it means to be persecuted on account of gender. I revised the initial question and designed the project around the following three research questions: How and why did gender-based asylum policies and laws in the United States come to exist? How are gender-based asylum laws and policies applied? How do asylees and human rights activists interpret ideas about gender-based persecution?

**Designing Qualitative Research**

In order to answer my research questions, I chose qualitative research methods because to question the interpretation of concepts such as gender and persecution requires dynamic data. Quantitative data such as that found in INS and Executive Office for Immigration Review (EOIR) statistical yearbooks are limited: They provide information on how many women and men apply for and either gain or are denied asylum, but not on how gender-based persecution laws and polices emerge; how they are implemented; or how law and policy makers, asylees, and human rights activists respond to them. I had to decide where to locate the fieldwork site, what types of data were accessible, and which qualitative methods were appropriate to answering my research question.

While researching asylum and immigration, I learned that there are eight INS asylum offices in the United States. I chose Los Angeles as the fieldwork site because its immigration court and asylum office have the greatest number and diversity of applications in the United States (U.S. Executive Office for Immigration Review 2002; U.S. Immigration and Naturalization Service 2002). Upon arriving in Los Angeles in August 2001, I e-mailed the district director of the San Francisco INS asylum office, whom I met during a summer program on refugee studies at York University. The director put me in touch with Michael, a supervisor at the Anaheim office, who facilitated my interviews with asylum supervisors. Michael made arrangements for me to receive copies of the INS asylum officer training materials as well as memos and letters regarding gender-based asylum claims. I contacted four refugee resettlement organizations, one organization that serves torture survivors, and two human rights organizations. These organizations served as initial sampling points from which I was able to locate interviewees. I met with personnel from these organizations and made arrangements to work as a volunteer and with immigration attorneys who agreed to facilitate my access to asylum applications and contact with asylees.
Based on my initial contacts, I surmised that I would be able to collect the following data: interviews with asylees (persons with a grant of asylum), immigration attorneys, immigrant service providers, INS asylum supervisors, and human rights activists; document analysis of asylum applications and INS asylum officer training manuals; and—through volunteer work with the organizations mentioned above—observation of participants in immigration court hearings and of interactions with asylees, immigrant service providers, and attorneys.

I developed an interview guide, keeping in mind that it should be flexible enough to accommodate new questions, a lesson learned from my interviews with the Somali refugees. I divided the interview guide by category of interviewee. For example, I did not ask all immigration attorneys and human rights activists the same questions, but I did ask similar questions of all attorneys and all activists. With the exception of asylees, nearly everyone I interviewed was involved in asylum through their employment. Therefore, I designed a guide that asked questions such as “When did you begin working with the INS?” or “How long have you practiced immigration law?” as a way of getting respondents to speak about their experiences with asylum. I did not ask all participants in a particular category the same questions. For example, not all asylum seekers are detained when they arrive in the United States. Questions regarding experiences in INS processing facilities are only relevant if the interviewee was in detention.

I situated this study in what sociologists Barry Glaser and Anselm Strauss termed “grounded theory,” a mode of inductive inquiry (Glaser and Strauss 1967). This method of inquiry begins with observations and then seeks to discover patterns based on those observations. There are no dependent or operationalized variables. I did not define gender or persecution, the two most salient concepts in this project, prior to data collection. Instead, I created an interview guide with open-ended questions that would allow participants in the asylum process to describe how they engaged in practices that define gender and persecution. The sampling method was purposive, with the sample size determined by availability of subjects, sampling of legally recognized gender-based claims, and multiple sites of snowball sampling. Nonprobability sampling is limited in its generalizibility, but probability sampling was impossible. Because of how the INS and EOIR collect data, there is no way to know how many or what types of gender-based claims are received. No list of all gender-based asylum claims is available in the United States to serve as a sampling frame.

During my interview with Michael, the asylum supervisor at the Anaheim office, he explained, “In our database, asylum officers have to mark which grounds the person claimed. If you want to know how many gender persecution cases were granted or denied, you are not going to get any usable figures out of this system, because it only says race or nationality.
I granted a domestic violence case once for religious reasons. If you searched the database for that application, it would show religious persecution, not domestic violence.” By “marking the grounds,” Michael refers to the five legally accepted grounds of persecution that include race, religion, nationality, political opinion, and membership in a social group. His example of domestic violence demonstrates that type of harm is not recorded, only the ground on which that harm is based. This means that the INS and EIOR have no official (or at least publicly available) data on how many types of gender-based asylum claims—such as female circumcision, honor killings, or domestic violence—are adjudicated. One of the strengths of my methodological design is that the information gleaned from my research serves as the only available empirical data of how gender-based laws and policies are applied and interpreted in the United States.

Data Collection and Data Analysis: Interactive Stages

The linear model that structures quantitative methods dictates that research happens in stages. The researcher begins with a theoretical framework, formulates a hypothesis, collects data, analyzes the data, and then draws conclusions that either support or refute the theory. Qualitative methodology eschews the linear model of research through its simultaneous data collection and analysis (Becker 2004). One of the strengths of qualitative research is its ability to refine the research question during and after data collection and analysis. In this section, I outline the challenges and rewards of treating data collection and data analysis as simultaneous research steps and the ways in which I refined my research questions through method choices of participant observation, interviewing, and document analysis.

The following example shows how a telephone discussion with an immigration attorney served as an opportunity to collect and analyze conversation data about the meanings of gender-based persecution. In an effort to understand the patterns that structure everyday social exchanges, sociologist Harold Garfinkle (1967) advanced ethnomethodology as a reflective method of data collection. Garfinkle demonstrated how assumed understandings of concepts operate in verbal exchanges among friends, students and parents, and consumers and store clerks. Throughout this study, my exchanges with INS supervisors, judges, attorneys, service providers, and activists took the form of Garfinkle’s questioning techniques regarding the meaning of gender. For example, an exchange with one attorney over the telephone was as follows:

CONNIE: I was given your name by ___ because I am writing a dissertation on gender-based asylum. He/she mentioned that you’ve represented these types of cases.
ATTORNEY: What do you mean by gender-based claims?
CONNIE: I mean cases like honor killings,
female circumcision, or domestic violence.

**Attorney:** I’ve never had a domestic violence case.

**Connie:** What about other types of gender claims?

**Attorney:** I’ve never had any of those cases. Do you know other attorneys who have had those types of cases who you could put me in touch with?

**Attorney:** (Pauses before responding.) I’ve had clients who were raped, is that what you mean?

**Connie:** Yes, I’m interested in rape cases.

**Attorney:** I don’t know if I would call that a gender case; most of my clients have been raped.

This exchange supports Garfinkle’s contention that familiar social interactions are structured by assumptions that merit a “sociological inquiry in its own right” (Garfinkle 1967, 136). While my contacts with attorneys and service providers were for the purpose of gaining access to asylees with gender-based claims, these exchanges also provided data by answering my research question regarding what constitutes gender-based harm. This example reveals a paradox about gender-based claims by showing how rape, a legally recognized form of gender-based harm, is not initially understood as such because of its pervasiveness across all asylum claims. Moreover, in my insistence that rape cases be included, this exchange reveals my preconceived notions about what types of acts are considered gender-based harm. By treating data collection and data analysis as simultaneous stages, I was able to incorporate my analysis of how gender-based harm is interpreted into future conversations with immigration attorneys.

I did this by asking attorneys whether they had represented cases that included specific types of harm (such as those mentioned in the above exchange: rape, domestic violence, female circumcision, etc.) instead of using the legal term “gender-based persecution” with no examples of what types of harm those cases might include.

A second example of how data collection and analysis occurred in simultaneous stages during my research is the following interaction between an asylum seeker and her attorney. In order to gain access to asylees, I volunteered with organizations that assist asylum seekers. Volunteering afforded me the opportunity to observe extensive interactions between asylum seekers and their service providers. In the spirit of client advocacy, I was occasionally drawn into disagreements between clients and their service providers and attorneys. While organizations expect employees and volunteers to support their clients, I was sometimes presented with situations in which I was expected to concur with service providers and attorneys concerning the assumed best interests of their clients. While volunteering for one such organization, I was drawn into a conflict between an asylum seeker and her attorney.

During a master calendar hearing for a woman seeking asylum from Cameroon whom I accompanied to court, her attorney
In this exchange, I became a pawn in a struggle for authority by the attorney. My expertise in having “been to court” was invoked in an effort to persuade the applicant to continue her hearing in three months rather than three days. I tacitly agreed with the attorney because I was fearful that her client’s defiance would invoke negative repercussions such as poor legal representation, resulting in a denial of her claim. Prior to this day, the questions from my interview guide assumed a benign relationship between attorneys and clients. Questions such as “Did you have a lawyer?” or “How did you locate your attorney?” were the limits of how I probed asylees about their relationship with their legal representatives.

After this observation, I altered the interview guide to include questions regarding conflicts between asylees and their attorneys or service providers. This exchange occurred in June 2002, nine months into my 19 months of fieldwork. At the time of this exchange, I had interviewed six of the twenty-one asylees included in this project. Although

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The Illegal Immigration and Reform Act of 1996 legislated the “one-year rule” that requires all asylum seekers to file an application within one year of their arrival in the United States. The judge offered a hearing date three days from the calendar hearing, advising that the documentation might not matter, since the greater issue was one of resettlement in Nigeria, not of timeliness in filing the application. However, at the attorney’s insistence, the judge issued a continuance for three months, and the applicant, attorney, and I retreated to a waiting area outside the courtroom.

The applicant became angry with her attorney and yelled for her to “go back in there and tell the judge I want to be heard in three days. I am tired of waiting.” The attorney responded in similar fashion, yanked her cell phone from her briefcase, and began dialing the number for the asylum seeker’s psychologist, curtly telling the applicant that it was her responsibility to make sure her therapist was available to testify, that her testimony was ready, and that all documentation was available. I witnessed this exchange with the assumed gaze of invisible researcher until the attorney turned to me and exclaimed, “Tell her. Tell her that there is no way I can prepare her case in three days with all that I would have to do. I don’t even have her work papers from Nigeria and I still have to prepare her testimony. If we go forward in three days we will lose. Tell her it is better to wait. You have been to court. You know how asylum works.” I turned to the Cameroonian woman who stood in silence with tears pouring down her face and told her that I knew that she was frustrated for having to wait, but that whereas postponing a hearing could not hurt her case, a premature hearing could prove problematic.

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requested a continuance in anticipation of arriving documentation that her client had been working in Nigeria just before she fled to the United States. This documentation was important as evidence that the woman had filed her asylum claim within the legally accepted time frame. The Illegal Immigration and Reform Act of 1996 legislated the “one-year rule” that requires all asylum seekers to file an application within one year of their arrival in the United States. This documentation was important as evidence that the woman had filed her asylum claim within the legally accepted time frame. The Illegal Immigration and Reform Act of 1996 legislated the “one-year rule” that requires all asylum seekers to file an application within one year of their arrival in the United States.
altering the interview guide strengthened the project in that I was able to capture data about negative exchanges between asylees and attorneys, a corresponding weakness was that I had no data on this subject for approximately one third of my sample.

A third example of how data collection and analysis occurred in simultaneous stages was through the acquisition of asylum applications. Through the assistance of immigration attorneys, I was able to procure a total of 19 asylum applications, four of which were from asylees I was able to interview. Because I gained access to asylees through their attorneys and service providers, I was often given extensive information about their asylum applications prior to meeting them. Some attorneys and service providers revealed that their clients had been raped, circumcised, kidnapped, and detained (in their country of origin and in the United States). Sometimes asylees would discuss the same information with me in our interview, but, overwhelmingly, interviewees did not discuss instances of rape documented in their applications. For example, Mary, an asylee from Congo, did not mention a gang rape that her attorney discussed with me in detail and instead focused during our interview on the two assassinations carried out against her family. Conversely, asylees also gave me information during our interview that they did not discuss with their attorney. Miriam, an asylee from Iran, discussed how she was raped repeatedly while detained after a protest for women’s rights. During our interview she told me that she did not discuss the rapes with her attorney, an Iranian man, because she “knew what men from my country would think about such things.”

These examples presented a challenge of data recording in ethnographic research. If an attorney or service provider reveals that a client was raped but the asylee does not mention the rape during the interview, should the researcher record “rape” as an example of gender-based persecution in the data? Instead of regarding this seeming conflict as a discrepancy in the data, I included both accounts as data. Because I was interested in knowing ways in which differently situated participants understand and articulate gender-based harm, I consider both the attorney’s discussion and the client’s silence about her rape data. By treating data collection and analysis as interactive—not linear—stages of research, I was attuned to how differently situated participants are willing to divulge stories about gendered harm, such as rape. After hearing varying accounts from asylees, their service providers, and attorneys about the same case, I was convinced that I needed a question that tapped the multiplicity of accounts of persecution in asylum claims. These examples persuaded me to include another research question: How are narratives of persecution created in gender-based asylum claims?

**Research Findings and Limitations of the Data**

I found that, while gender-based persecution laws and policies laid the groundwork for gender equality in asylum adjudication,
asylum officers, immigration attorneys, judges, and service providers participate in what I term a “gender regime” of asylum practices that exacerbates inequality for some migrant women. For example, I found that a differentiation emerges in gender-based asylum claims between what I term “ethnocentric” and what I term “exotic” harm. While asylum officers and judges are more reluctant to consider cases of ethnocentric harm (harm that American women experience, such as domestic violence) as persecution, they overwhelmingly consider exotic claims (harm that non-Americans experience, such as female circumcision and honor killings) as persecution. Moreover, immigration attorneys and service providers encourage female asylum seekers who are circumcised to claim persecution based on female circumcision regardless of the actual reason they left their country. None of the five interviewees who gained asylum because of female circumcision left their country because they considered their own circumcision to be persecution. Instead, they migrated to the United States and sought asylum because they had been detained, tortured, or had lived with threats of torture of a spouse.3

My high school English teacher was famous for her advice to students regarding their behavioral decisions: “Choices cause consequences” was her steadfast motto. Her directive reaches beyond the scope of personal decisions and is pertinent to research design and methodology. Throughout this project, I was confronted with choices about my three types of research methods: participant observation, interviewing, and document analysis. My goal was to collect data across sources; ideally, I would observe an asylum seeker’s hearing, interview her after she gains asylum, obtain her application, and interview her attorney, service providers, and the judge who adjudicated the case. Unfortunately, this was not possible for at least three reasons. First, the time span between filing an asylum application and its adjudication in immigration court can take years, and the data collection was limited to eighteen months. Second, all subjects in the project were willing participants, and some asylees, attorneys, and judges whom I approached refused an interview. Third, I was unable to locate some participants for interviews. Attorneys lost contact with clients, asylees couldn’t remember their judges’ names, and human rights activists were no longer in touch with the detainees they assisted. A shortcoming of this project was incomplete data with regard to comparability across sources.

I was confronted with choices that included whom I should (or could) interview. I conducted a total of 102 tape-recorded interviews with asylees, refugees, VAWA claimants (VAWA refers to the 1993 Violence Against Women Act provision for immigrant women who are abused by their U.S. citizen or permanent resident husbands), trafficking survivors, immigrant service

3 This gender regime is documented in Oxford (2005).
providers, immigration attorneys, paralegals, immigration judges, former immigration judges, INS processing center employees, INS supervisors/former asylum officers, INS assistant district counsel supervisors, former INS supervisors and asylum officers, former INS border patrol agents, human rights activists and organization employees, legal scholars, policy makers, United Nations officials, language interpreters, U.S. attorneys, and immigration reform activists and organization employees.

However, I was unable to interview many immigration attorneys, service providers, or their clients for a variety of reasons. The gendered nature of caregiving accounted for why some female respondents were unable to schedule interviews. Five female immigration attorneys were unavailable for interviews because they were on maternity leave, caring for aging and ill parents, or caring for a special-needs child. One supervisor at the INS refused an interview because of discomfort with how she might be portrayed. During one of my visits to the Anaheim asylum office, she stated, “I know how they made people look in that film. I just don’t want that.” The film she referred to is the documentary *A Well-Founded Fear*, which chronicles asylum hearing interviews and interviews with asylum officers in the Newark, New Jersey, asylum office. The most disappointing refusal was that INS headquarters would not allow me to interview current asylum officers. After a year of my persistent phone calls and office visits to the Anaheim asylum office, Michael yielded to my request to interview him and five other supervisors whose previous employment included serving as asylum officers. When I met Michael for an interview, he explained INS headquarters’ reluctance to allow me to interview current asylum officers: “I’ve left many messages trying to get approval. I don’t think it’s about your project. They just have a lot going on.” Personnel at INS headquarters had “a lot going on” indeed during this time; my year-long persistence continued from September 2001 through 2002, a year marked by upheaval within the INS and its responses to the 9/11 terrorist attacks.

The greatest strength of my research is that it is the first (and to date only) empirical study of gender-based asylum in the United States. My findings are the only data available that address how gender-based asylum laws and policies are applied and interpreted. The greatest methodological strength of my research was its multi-method approach: extensive data collection; its qualitative approach that allowed me to collect and analyze data simultaneously; and its methodological design that allowed me to refine my research questions throughout the project. While the decision to change research questions and data instruments is problematic in regard to issues of comparability across data sources and reliability across interviews, I chose this approach because it best answered my research questions about gender-based persecution.
Works Cited


Chapter Eight  
Thoughts on the Use of Semistructured Interviews in Exploring Ethnic and Gender Inequality in Silicon Valley

From  
Researching Migration: Stories from the Field

By Johanna Shih  
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http://www.ssrc.org/pubs/researching_migration.pdf
This book affords me the unique opportunity to apply a hindsight gaze to the choice I made in using in-depth, semistructured interviews as the primary method for my dissertation research. My purpose in this chapter is to consider how the results from my research were connected to and shaped by this methodological choice. Clarifying this relationship illustrates the importance of having a good fit between method and research goals, as well as understanding a priori what types of results you can (and cannot) garner. I offer my post-hoc thoughts here, on the lessons I learned in my first research project, with the hope that it can be of some use to other junior scholars.

I begin with a brief description of my research project, and the process by which I chose the research site, key questions, and research method. I then detail two sets of findings from the study in order to discuss how they were shaped by my use of interview data and consider the lessons I learned while collecting and interpreting these data. I emphasize the utility of semistructured interviews as an exploratory device well suited for relatively new and less understood contexts and the importance of viewing the collection and interpretation of this type of data as an ongoing conversation between researcher and “subjects.”

**Choosing the Project and Method**

In a nutshell, my research project investigated the mechanisms of ethnic and gender inequality in the high-tech region known as Silicon Valley, and I did this primarily through collecting and analyzing fifty-four in-depth semistructured interviews with white and Asian male and female engineers. The process by which I chose my research site and my primary method was simultaneously difficult and straightforward. When I started, I knew I was interested in studying the interaction of gender, ethnicity, and immigration in the labor market, and I was also aware that the analytical focus of the field had undergone a decided shift from analyzing race and ethnicity as static phenomena toward understanding how contexts shape the salience of these characteristics. I was also personally attracted to research on ethnic and racial relations that were firmly located in a regional context; thus my task at this point was to identify a research site that would allow me to investigate these social phenomena.

I think my logic and strategy until this point was fairly straightforward, but since this is a book about “war stories,” I should also admit that it took me a (painful) year to identify a feasible site that suited my interests. When I finally turned my attention to Silicon Valley, however, it became an easy choice, because what was happening there at the time (the late 1990s) represented a nexus of trends that were pertinent to my interests. There were three main reasons why the high-tech industry of Silicon Valley represented a good opportunity for exploring the relationship between labor markets, ethnicity, gender, and immigration.

First, there had emerged from the continuous media coverage an ideology that
depicted the region as a wondrous place of riches representing the triumph of free-market capitalism and the meritocracy it could bring (where, for example, CEOs such as Steve Jobs of Apple Computing promised that “Silicon Valley is a meritocracy. It doesn’t matter what you wear, it doesn’t matter how old you are, what matters is how smart you are”). Second, the economic organization of the high-tech industry was receiving significant attention from scholars who saw it as an ideal example of “flexible specialization,” a mode of production that was seen as a shift away from the Fordist model of mass production and that is particularly suited to the demands of global capitalism. Third (and perhaps most important for my purposes), the region had attracted a significant migration of highly skilled workers. In particular, it had in a very short time become an area with the fourth largest concentration of Asians in the United States, most of whom had immigrated because of the high-tech industry. This meant that it was a region and industry that was experiencing rapid ethnic and racial changes, which made it an exciting site to explore. In addition, the high-tech sector had a higher proportion of technically skilled women, in comparison to the nation overall. Taken together, this convergence of factors seemed to me to represent a unique opportunity to study the meanings of ethnicity, immigration, and gender in a new organizational context, in a burgeoning industry at the heart of the “new” economy, and amidst significant demographic changes. I thus identified my main guiding question as: How do the particular characteristics of flexible specialization in Silicon Valley’s high-tech industry affect mechanisms of ethnic and gender inequality identified in other types of economic arrangements?

Once I defined this research topic and question, choosing the method was relatively easy. Indeed, in retrospect, I cannot honestly recollect which came first—whether I chose my method after deciding upon my research site, or whether I chose Silicon Valley partly because it suited my choice of method. I had some experience in analyzing semistructured interview data because I had the opportunity to work on my mentors’ datasets, and frankly, I suspect that most scholars in their first project use the methodologies with which they are most familiar. I was definitely no exception.

Regardless of which came first, what is true is that I was careful to consider the “goodness of fit” between what I was studying and the method I used. To my mind, semistructured interviews are particularly well suited to exploratory research in a relatively new and less understood context. This was true of Silicon Valley’s high-tech industry, because although there were a number of researchers looking at the economic organization of the region, the experiences of the skilled individuals who worked in this “new economy” had been under-studied. In addition, it was also unclear whether the gender and ethnic mechanisms of inequality that had been well identified in myriad organizational studies had been ameliorated or exacerbated
in flexibly organized industries, or if new mechanisms had emerged. So it seemed to me that semistructured interviews would be a suitable choice because the method’s strength is that it is investigative, that is, it allows respondents to describe and identify experiences and phenomena that are only partially understood by the researcher. I chose to conduct semistructured interviews rather than unstructured interviews because I had already identified a set of issues that I was primarily interested in and I wanted to have some control over the shape of the discussion.

On a more practical level, given this research site, it is unlikely that I would have been able to use the two other methods I considered: a large-scale survey that would allow for quantitative analysis, or an ethnographic method. Security in Silicon Valley firms was quite tight (for instance, although I was considered a “personal” visitor when I met respondents to interview them at the workplace, I was always asked for identification and given a badge; at some firms, I was asked to sign privacy waivers). Thus it was extremely unlikely that I would have been able to gain permission to survey workers in an organization or to engage in ethnographic research, because I had no personal contacts in the region prior to embarking on this research. (After developing some contacts through the research, I did eventually gain access to a privately held survey dataset on engineers, but I view this as supplementary data.)

For these reasons, I chose to interview Asian and white men and women, using a snowball strategy that I would end at two to three links. Starting out, since I had no initial contacts, I quickly realized that I should have no shame as well. Indeed, I tried to leave any social hang-ups at the door, trying to garner connections by attending meetings of engineering societies in Silicon Valley, introducing myself to professors of foreign institutions who were known to send their students to Silicon Valley, asking to post listings on alumni boards of educational institutions in northern California, and asking people I knew if they knew anyone who might speak with me. In the course of a year and half, I ended up interviewing 54 engineers who worked in start-ups and larger organizations and who represented both the junior and senior levels of these firms.

This was the basic process by which I chose my research project, method, and collected the data. In the remainder of this chapter, I describe two sets of my findings to show how semistructured interviews turned out to be a suitable exploratory device for this project and to also discuss the mistakes I made and the lessons I learned along the way in collecting and interpreting this type of qualitative data.

**Example One:**

*On Investigating the Meaning of Race, Ethnicity, and Gender in the Careers of Silicon Valley High-Tech Engineers*

I indicated earlier that the pervasive ideology about Silicon Valley characterized it as a meritocratic region that was willing to reward anyone with the “right skills and ideas.” Given this, at the basic level, I
wanted to know how Asian immigrants and white women in Silicon Valley viewed their own experiences regarding the salience of race, ethnicity, and gender in their careers thus far. As a sociologist, I was skeptical of the glowing ideology, because certain characteristics of the region—most notably, the centrality of networks to the livelihood of both organizations and individuals—would not seem to auger well for gender and ethnic equality in organizations. This was because lack of access to networks and mentorship had been consistently identified as problematic in previous research on inequality within organizations.

However, when I asked respondents the basic question of whether their race, ethnicity, or gender had shaped their careers in any way (I purposefully asked this question during the final third of the interview, when respondents were most likely to be relaxed, comfortable, and open), I was surprised to find that almost everybody answered that these ascribed characteristics did not affect their careers in Silicon Valley. This was frankly difficult for me to accept initially, and I was sorely tempted to present a “false consciousness” argument by interpreting their views as a function of the overriding dominance of the Silicon Valley ideology about capitalism and meritocracy. Of course, false consciousness is a problematic argument, and it is particularly hard to justify with this method because the rationale is partly based on the premise that it is important to allow people to give their own accounts of their experiences.

I decided to take a step back, sit down, and systematically read through all the interviews to try to understand the entirety of the picture my respondents presented of working in Silicon Valley. How did they view mobility in the region, and what did they identify as the factors that shaped it? (The interview schedule included earlier sections asking about people’s career trajectory, the shape of their work day, and their views on mobility.) Here I was surprised once again, because I realized that, when talking about their career histories and their views about mobility in the region, 80 percent of Asian men and women and white women I spoke with did voluntarily tell me stories of incidents where they believed their ethnicity or gender did matter. This was puzzling, as it made their accounts seem internally contradictory. What was I missing?

As it turned out, the key to resolving this contradiction was that almost all of these respondents went on to explain how they got out of the situation they perceived to be discriminatory—by job-hopping to firms (and employers) they viewed as more egalitarian. Job-hopping was a particular feature of this industry and economic organization, and it has been more generally identified as a career mobility strategy of workers and as a necessary characteristic of an industry in which organizations no longer felt responsible for their workers. What respondents were telling me was that they were able to use this feature of the region’s economic organization to navigate around discriminatory bosses, colleagues, or workplaces. They also detailed one
important reason why they were able to do so by describing how they were forging their own ethnic and gender-based networks that had the resources to help their members successfully job-hop. So this became the story that I eventually identified.

*Making Mistakes*

When I think about the circuitous path I took in figuring out what people were telling me, I realized that this path was shaped by methodological mistakes. My initial and incorrect impression of the situation was shaped by the way I as the researcher asked the question and interpreted the answers. I asked respondents if race or gender shaped their careers—they said no, which I automatically took to mean that they viewed race and gender bias as absent in their careers and in Silicon Valley. However, what respondents really meant was not that issues of race or gender were absent, but rather that they believed they were able to find their way out of the situation. It was only by retracing my steps and reading through the interviews that I was able to see where I went wrong—that I had assumed in my question that the presence of bias also meant that respondents had little recourse to circumvent it. My assumption was shaped by the literature on organizational inequality, which had largely depicted white women and racial minorities as relatively limited in their ability to resist mechanisms of discrimination.

This was not the case in Silicon Valley during this time period, and my understanding of this situation changed when I looked at how respondents described their experiences using their own frameworks. In fact, even if they said yes (and one can see that if I had rephrased the question, for example, to ask “have you ever felt that your gender, ethnicity or race mattered in terms of how you were viewed by your bosses, clients or colleagues?”) I might have garnered a lot of “yeses” in that respondents did give accounts where they believed that race, ethnicity, or gender mattered), I still would have been wrong in my analysis because the presence of discriminatory employers and organizations was not the end or even the heart of the story. The heart of the story in Silicon Valley at the time was the particular confluence of events that allowed for some white women and Asian men and women to forge their own resource-rich networks that allowed them to job-hop into better circumstances, effectively turning the tables on discriminatory employers. Of course, I did not try to argue that this is always possible; my task from there was to try to root respondents’ experiences in the particular characteristics of the region and the particular contexts of their groups that made their job-hopping possible. This was a story I would have missed had I not taken a step back to reconsider whether my own framework as a researcher was submerging what respondents were trying to say. That is, rather than letting respondents tell me about how they viewed the meaning of race, ethnicity, and gender in their careers, I had, with too heavy a hand, imposed frameworks that I had garnered from research literature about other settings.
This mistake was partially shaped by the fact that I did develop tentative hypotheses going into the research. One of these was that the reliance on networks in Silicon Valley would exacerbate ethnic and gender inequality in organizations. In hindsight, it was a mistake to develop hypotheses, because they inherently require the implementation of researcher frameworks prior to the collection and analysis of data, and the results are used essentially to confirm or reject one’s initial assumption. This was problematic in my case because the phenomena that I was interested in were still relatively under-studied, and thus I had no clear concept of what the important variables were and how they could be connected with one another.

Put differently, I was simply not using the strength of in-depth interviewing as an exploratory device—it can tell us a great deal about social worlds about which we know relatively little. Had I conducted a survey with close-ended questions, I obviously would not have come away with these results, simply because I did not know enough about the key issues to ask the right questions. Indeed, my interpretational journey would probably have ended at the point where I would have concluded that few thought race and gender mattered at all in the region. This would have been an example of an inaccurate analysis that stemmed from a methodological error.

*Contextualizing the Data*

When I began the process of “writing up the data,” I was faced with a challenge that I had not foreseen. I realized then that in using interview data (with a relatively limited number of cases) as a primary method, one runs the risk of being “stuck in the middle of nowhere,” between a true ethnographic method and a survey method. This is a problematic place to be, because interview data have neither the breadth of data garnered by surveys, nor the depth of data garnered by ethnography. This is particularly the case when you are not able to collect a large number of interviews—my previous experience in a master’s project and an interim research project used interview data that were collected by my mentors. These were very large, very well-funded projects that had the resources to collect large amounts of qualitative data. I, unfortunately, as a grad student, somehow missed the obvious point that I would not be able to collect a large amount of qualitative data if I wanted to finish my degree in reasonable time. So although it was quite time consuming for me to collect the interviews and visit thirty-odd companies, I just did not have enough cases to make generalizations. I also did not have the richness of data (as well as the longitudinal perspective) that ethnographic work can bring, because I did not spend extended amounts of time at any one place or with any one set of people.

This left me in a neither here-nor-there situation, and it became tricky to navigate the interpretation and decide what kind of inferences I could make. To complicate matters further, it seems to me that there is not yet a “blueprint” for presenting this type of research.
of data that there is for more tried-and-true methods, and this is potentially a difficult situation in terms of publishing, because one lays oneself open to critiques about the claims being made and becomes a relatively easy target for methodologists at either end of the spectrum.

One solution to this problem is to gain depth of data by embedding the interviews in a well-developed context, effectively grafting one’s own data with what is already known. This is in essence a grounded theory method of analysis, where architecture is built to better understand and interpret respondents’ accounts. To give the specific example, my interpretive strategy for this portion of the data involved layering respondents accounts of their own experiences with four contextual tiers. These were (1) the networks and social relationships in which the individuals were embedded; (2) the characteristics and history of the group with which they identified in respect to Silicon Valley; (3) the economic organization, ideology, and history of the high-tech industry; and finally (4) the prevailing literature on ethnic and gender inequality in more traditionally organized firms and economies. These contexts rooted the experiences of the individuals by providing logical parameters within which the experiences could be understood without claiming that these individuals “stood” for everyone. The data also gain breadth and depth by connecting one’s findings with that of other researchers to create a more comprehensive, albeit woven together, picture.

This strategy simultaneously solved another interpretational dilemma. In making sense of interview data, it seemed to me that researchers can span a continuum, ranging from one end where the goal is simply “letting people speak” because these voices are the “authentic” ones, to the other end where the researcher’s analysis always trumps the respondents’, which leads, for example, to a “false consciousness” argument. Providing the context for respondents’ accounts provides a middle ground for this dilemma because it does not insist upon a definitive interpretation of the data; rather, it provides the reader with the necessary information that mediates or moderates the respondents’ voices.

**Example Two:**

*Temporal Pressures in the High-Tech Industry*

One of the secondary goals of this study was to investigate gender inequality by taking into account both what was happening in the public sphere of work and the private sphere of home. In terms of the high-tech industry, I wanted to understand whether the notoriously hectic pace of Silicon Valley disadvantaged women who were balancing both the career of the family as well as the career of work, or else were planning to do so. The high-tech industry expanded exponentially in the 1990s, and all reports suggested that this expansion was paralleled by an acceleration in the pace of work as competition over product and service innovations and the race for time to market dominated life for high-tech workers.

In order to understand the toll that this sped-up pace had on workers, particularly
on women with responsibilities at home, one baseline question I asked respondents was how many hours a week on average they worked. This is a standard question both in surveys charting the amount of time people spend at work, and also in charting average differences between the number of hours that men and women work when both the labor market and the domestic sphere are taken into account.

This question might be standard, but it certainly baffled my respondents who were, in other portions of the interview, generally quite articulate and thoughtful. Respondents stopped speaking suddenly, frowned, cocked their heads, stammered, and hesitated in a variety of other ways before saying that they really could not hazard a good guess, because it depended on the project and where they were on the project. It could be 30 hours, or it could be over 100 I was told, it all depended. After pushing them further to explain why they could not give me an answer, it turns out that respondents’ work weeks were not organized by any 9-to-5 type of schedule, but rather was determined by the project cycle, which lasted anywhere from a few months to a year or more.

Organizing work through project cycles has several implications for people’s experiences at work, which I explain further below.

First, however, I note that the fact that respondents were able essentially to correct and reframe my question illustrates clearly how the method of semistructured interviews shaped my results. Had respondents been asked this in a telephone survey with close-ended questions, it is far more likely that they would have committed to an answer, perhaps by “guesstimating” what the average weekly hours would be when the total number of hours for a project is divided by the weeks worked. This would have been a useful finding in and of itself, as these kinds of results could have revealed, for example, whether female and male engineers work, on average, the same number of hours in the labor market, or whether those who work in start-ups put in more hours than those in large organizations, or whether those who work in the high-tech industry were putting in longer work weeks than those in other industries.

However, by asking respondents this question within the course of a conversation and by using open-ended questions, another set of results emerged that led me to different conclusions. On one level, this method allowed respondents to tell me about how their work was organized, and how this diverged from the 9-to-5 schedule. On another level, it allowed people to explain their experiences with work time from a qualitative perspective—and by qualitative, I mean that respondents were able to describe their experiences with time as a non-linear entity. There are three examples of this. For one, regardless of the amount (quantitative) of time someone is putting in, it is clear that the leisurely feel that accompanies the beginning of a project is quite different from the harried pace at the end, when one is under significant pressure to meet deadlines, and it is indeed these last few weeks that lead to frequent burnout among high-tech
engineers. Second, respondents were also able to explain how the line between working and not-working is increasingly blurred, because there are many times in the day that are not easily categorized. Checking e-mail or answering work-related phone messages at home, or trying to solve a problem when showering, as one respondent put it, represents ambiguous space, especially in an occupation that is knowledge intensive. Third and finally, a nonlinear conception of time is also exemplified in terms of people’s perspectives of their life course, where there appears to be a shift from viewing career and family as simultaneous events to one that is sequenced.

Giving respondents the chance to describe their temporal experiences at work in their own words really clued me in to the particular ways in which peoples’ personal time becomes co-opted in Silicon Valley, and the vehemence with which this was explained to me also indicated that this issue was important to those who worked in the region. It thus became evident that while the hectic pace of work does indeed create gendered results, the more compelling point seemed to be its impact on shaping the experiences of everyone I spoke with. This turned into a story about how high-skill, high-tech workers’ lives in Silicon Valley became synchronized to the escalating rhythms of the global capitalist market and the factors that created and reproduced this synchronization. For example, when I asked people to tell me more about the demands of the project cycles, they explained to me how the pace of innovation and development in the high-tech industry was accelerating, creating increasingly unreasonable deadlines in the rush to “get it to market.” The very characteristics of flexible specialization which were seen to give places like Silicon Valley an advantage because it made the region better able to adapt quickly to the changing demands of the global capitalist market were the same characteristics that bound their workers more tightly to the vicissitudes of the global marketplace.

Ongoing Adaptations, Corrections, and Dialogues
This set of findings emerged from two adaptations: the first in regard to the data collection and the second in regard to interpreting and framing the data. These adaptations occurred in large part because what respondents said led me down an unexpected path. As indicated in the description of the case, this was a situation in which I had to change my interviewing strategy on the spot in response to what I was hearing because my question about a set work schedule did not really make sense from the respondents’ viewpoints. (So, for example, I would ask “If there isn’t really a set work schedule, how would you describe the time you put in for work?” or “Can you tell me more about what the project cycle is like?”) In this sense, I eventually found that it was best to see the collection of my data as an open dialogue in which the initial questions are merely the starting point of the conversation over which I, as the researcher, did not have total control.
Of course, I did not manage to adapt as much as I wished I had. From a hindsight perspective, I should have asked more direct questions about how particular characteristics of the high-tech industry affected work conditions and whether respondents had developed any strategies to resist the pressures to put in extraordinary hours. I also could have asked them to tell me more about the changing visions of the life course that seemed to be emerging from the region as a whole. In listening to and transcribing the interview tapes, there were innumerable moments when I regretted that I was not quicker at digesting what someone was saying to me and asking “corrected” questions. Follow-up interviews would have been appropriate here, and I regret not conducting them.

In any case, because I had been able to adapt some of my questions, I subsequently needed to reconsider the context from which I viewed this portion of the data. As I noted above, the original framework from which I saw respondents’ experiences was through the lens of gender inequality, and, more particularly, the literature that argued that both the public sphere of work and the private sphere of home must be taken into account when considering women’s labor market trajectories. While I still believed that the demands of the high-tech industry on workers’ time were producing gendered outcomes, I also believed that this issue was not solely about gender, but was also about labor relations in the new economy. Given this shift in my interpretation, I needed to find the right framework to understand my results. Finding this framework entailed a constant back-and-forth with the actual data, the concepts and themes that I had identified in memos to myself as emerging from the data, and the prevailing research that could be used as a comparison (and here, I benefited greatly from incisive feedback from others). For example, when I considered what respondents were telling me in terms of how their lives were bounded with the project cycles, I began to wonder why they complied. This led me to go back through their interviews to find the answer, from which I realized that another two themes were the individualist ideology in the region that constructed individuals as being entrepreneurs of their own careers, and the managerial effort in blurring the line between private and work time. Both these trends functioned to legitimate and facilitate the hectic pace of work in Silicon Valley.

At this point, I went back to reading other research and used as a historical comparison the literature on managerial ideologies of coercion during industrialization (as juxtaposed to coercion in flexibly organized economies), and a comparison between the “clock time” of industry with the “project time” of high-tech. The general point I’m trying to illustrate is that the ongoing dialogue between my data, my preliminary identification of concepts and themes, and other research eventually allowed me to embed my data in what I considered to be a more accurate framework.
Conclusion

In sum, my experiences illustrate the utility of semistructured interviews in investigating relatively new and less-studied contexts. One key strength of this method is that it allowed me to establish some measure of control and uniformity over the interviews in order to explore the key issues and questions that I was interested in, yet at the same time the method also allowed for the space and flexibility that was necessary for respondents to tell me about the key issues that they identified as important to their lives. My results subsequently reflected both my own as well as my respondents’ understandings of living and working in Silicon Valley’s high-tech industry and incorporated both the deductive and inductive reasoning characteristic of a grounded theory method of analysis.

I conclude this discussion with one practical bit of information that I think I would like to have known before I embarked on this project. I indicated that the interpretive core of the semistructured method is generated from the constant adaptations and corrections that emerged from the open dialogue between what we as social scientists know and what respondents were telling me about the social world in which they lived. It seems to me, though, that this is also precisely what makes this method terribly messy and challenging. Facing hundreds (or thousands) of pages of data is daunting and sometimes even panic inducing, and there is no real “recipe” that tells you how to begin to digest this material in order to identify the main themes and trends. There is, of course, increasingly sophisticated software for qualitative analysis such as QSR Nvivo, but even these require the researcher to identify the core themes in order to set up the data effectively. This means that there is likely to be an extended period of time in which you may simply be in fear that you “have nothing” or, alternatively, you may be seized with the belief that you will never be able to “wrestle the data down.” Both of these fears were ones that I certainly faced. As a practical matter, then, it seems to me that in order to live comfortably and peacefully through the entire process of the research, one has to become relatively tolerant of ambiguity, essentially taking on faith that what respondents have to say about their own lives is valuable, that their experiences reflect an integral part of the social world we seek to better understand, and that discernible shapes will eventually emerge from their accounts.
Multisited Ethnography in Peru, Japan, and the United States

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http://www.ssrc.org/pubs/researching_migration.pdf
On December 17, 1996, at 8 P.M., a bomb exploded in the Japanese ambassador’s residence in Lima. When the four-month-long “Peruvian hostage crisis” began, I was in the middle of my field research on Japanese Peruvians. In the midst of the annual cocktail party celebrating the Japanese emperor’s birthday, 600 guests, most of them notables, were taken hostage by the Tupac Amaru Revolutionary Movement, a Peruvian terrorist group, known by its Spanish acronym, MRTA. Among the hostages were Peruvians (government officials), Japanese (embassy officials and businessmen), and representatives of the community of Japanese Peruvians, or more precisely, Peruvian nationals and residents who claim some Japanese ancestry.

Throughout the crisis, the media described this group, comprising Japanese immigrants, many of Okinawan origin, and their Peruvian-born descendants, in many ways—sometimes as “Peruvian” and other times as “Japanese” or “Okinawan”—while frequently classifying them under such distinct labels as “Peruvians of Japanese origin,” “Japanese Peruvians,” “Peruvian Japanese,” or simply “Nikkei” or “Nikkeijin” (overseas Japanese). The Japanese media, whose utmost concern was the safety of Japanese from Japan, distinguished Japanese emigrants and their descendants from “Japanese,” but they also were classified differently from other Peruvians, as “Nikkei” and “Nikkeijin.” On the other hand, the Japanese Peruvian community media defined its group membership on the basis of “blood,” and each time hostages of Japanese ancestry were released, it listed their full names, regardless of their nationality and place of residence.

To the Okinawan media, what mattered most was Okinawan heritage. It initially reacted to the hostage crisis, “14 Okinawans Are Taken Hostage!” (Ryukyu Shimpo, December 19, 1996), and distinguished hostages with any ties to Okinawa from “non-Okinawans.”

The Peruvian media, preoccupied with hostage government officials and national image, classified hostages first by occupation and rank: Japanese Peruvians were lumped together as “Peruvians,” although sometimes they were treated as “Nisei” (second-generation Japanese immigrants) or simply as “Japanese,” distinct from other Peruvians. The U.S. media portrayed this group as a special type of Peruvian, using labels to simultaneously denote nationality and ethnicity: “Japanese Peruvian,” “Peruvian Japanese,” or “Peruvians of Japanese ancestry.” Thus, Japanese Peruvians were sometimes Peruvian and sometimes Japanese or Okinawan; other times, they were simultaneously both or neither of these.

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2 Literally meaning non-Japanese nationals of Japanese descent.
ambiguous national membership intriguing and perplexing—so intriguing and perplexing that they, I hoped, would help me understand what it means and takes to belong to a nation. I also found this population interesting because it was geographically mobile and dispersed across countries—Peru, Japan, and the United States.

Initially, from 1899 through the 1930s, more than 33,000 Japanese migrated to Peru as agricultural contract laborers under the sponsorship of the Japanese government. Many were from Okinawa, the southernmost island group, which was a sovereign nation with its own culture and language before Japan annexed it in 1879. Almost a century later, their descendants began to migrate “back” to Japan as contract factory workers. This so-called return migration was triggered directly by Japan’s ethnicity-based immigration policy, allowing foreign nationals of Japanese descent (Nikkeijin) to enter and work in Japan without any restrictions. Some of these return migrants then migrated again, from Japan to the United States, partly because of personal networks stemming from prior migrations (some Japanese Peruvians had migrated from Peru to the United States during the 1970s and 1980s as laborers, and many had emigrated from Okinawa to the United States before World War II), and also because they perceived the United States as more open to immigrants. Of the 120,000 or so Japanese Peruvians, about a third lived in Japan as contract laborers, and the rest resided in Peru and the United States.

I first encountered a Japanese Peruvian through a government-sponsored cultural exchange program in Japan in 1990. I was asked to work as a Spanish interpreter and assistant for the program, since I had acquired a command of the Spanish language in college and through language training in Mexico and Spain. There I met Christy, a third-generation Japanese Peruvian born and raised in Peru who was visiting Japan for the first time. As a leader of a Japanese Peruvian youth group in Lima, Christy passionately told me about the Japanese Peruvian community and its “unique culture,” about which I knew so little. She did not speak a word of Japanese, and her complexion was darker than the average Japanese. I remember wondering why she did not even retain a Japanese surname. My Japanese colleague told me, “Descendants of Japanese emigrants lose things Japanese quickly.” Until years later, I did not quite realize that her surname was Okinawan, and that the spelling of her name was changed when her grandfather first arrived in Peru from Okinawa before World War II.

Years later, when I began to search for a dissertation topic, the so-called return migration had peaked, and Christy returned to Japan, along with many fellow Japanese Peruvians, to take up factory work. I decided to explore this phenomenon during my pilot study in 1995. Christy introduced to me many Japanese Peruvian return migrants. Among them was Lucho, who then introduced me to his relatives in Lima; they warmly welcomed me to their home the following year. So began my research
and long-term relationship with Japanese Peruvians.

It all started with a simple question: What does it mean to be “back” in the country of one’s ancestors, and what do “ethnic ties” really mean? I was curious to find out how Japanese descendants adapted in Japan, a country of their ancestors, but one that seemed so foreign and different from their own. As I learned during my pilot study that many Japanese Peruvian families were also dispersed in the United States, I also added a comparative question: How differently or similarly did they adapt in, and relate to, Peru, Japan, and the United States? Ethnicity is commonly defined as a sense of identity based on shared culture, history, and ancestry. How, then, I wondered, do Japanese Peruvians of multicultural backgrounds perceive shared culture, history, and ancestry? What kind of culture and history do they perceive to share, and with whom? And what happens to their sense of ethnic ties when they move from one place to another? How does ethnicity shift, in general, and when does ethnicity become thick and thin (Cornell and Hartmann 1998), pan-ethnic (Lopez and Espiritu 1990), symbolic (Gans 1979), or simply a matter of choice (Waters 1990; Song 2003)?

With these questions in mind, I decided to conduct multisited, cross-national field research in the Japanese Peruvian “subcommunities,” beginning in Peru (Lima), Japan (Greater Tokyo), and then the United States (New York, New Jersey, and Los Angeles). Following my pilot study in Japan in 1995, I spent six to eight months in each of these countries, interviewing more than 150 individuals, and observing and participating in Japanese Peruvian daily and communal activities. I chose an ethnographic method, because I believed it was the best—indeed the only—method to study such a small and geographically dispersed population. There were no large-scale surveys about Japanese Peruvians. I also believed that conducting a large-scale survey myself would be difficult and insufficient to explore the meaning and manifestations of ethnicity in depth. In retrospect, however, I should have combined ethnographic research with a survey, even a small-scale one, that would have allowed me to compare across regions more systematically. That way, I might not have struggled so much in writing it up as I did, as described later.

**Multisited Research**

To examine global networks or the circulation of cultural meanings, objects, and identities, it is important to be able to follow the people or the issues concerned (Marcus 1995; Hendry 2003). As the subjects of our research become transnational, researchers, too, need to become transnational and learn to cope with different situations, languages, and cultural contexts (Ishi 2003; Nyiri 2002). In an attempt to understand the processes and consequences of transnational movements, I ended up crossing the Pacific Ocean eight times between 1996 and 1999, flying four times to Tokyo, three times to Lima, once to Okinawa, and twice to Los Angeles from my base in New York.
While multisited research was an indispensable part of my project, it was nonetheless a challenge. Becoming familiar with local languages, histories, and ways of life in multiple countries was a daunting task, as was establishing and renewing contact each time I began research in a new site. Multisited research naturally multiplies the logistical difficulties of single-site fieldwork (Freidberg 2001; Kurotani 2004). It is also costly. In the following section, I address issues raised in conducting cross-national research, that is, how I conducted my research and what I learned in the process.

**Getting In and Lessons Learned**

Entry, or “admission to a setting for the purpose of observing it or access to individuals for the purpose of interviewing them” (Lofland and Lofland 1984, 25), is the first problem an ethnographic researcher faces, and it can be a major obstacle for a newcomer. While the resident researcher may have the advantage of already having access to key personnel and knowing whom to contact, I had to exploit the few personal networks I had previously established and build new ones to gain entry and acceptance in the Japanese Peruvian community.

Personal networks were particularly effective in Lima. Christy, the former leader of a Japanese Peruvian youth association whom I met earlier in Japan, was instrumental in pointing to key organizations and persons. My contacts snowballed quickly. Lima’s Japanese Peruvian community was cohesive and well-organized on the basis of quasi-kin relations (Takenaka 2003b). Since “everybody knows everybody” within the community, as they told me, I, too, got to know pretty much everybody. That, in turn, required me to reach out to non-affiliates, but they, too, were found through personal networks. The contacts established in Lima eventually expanded to Japan and the United States. This approach of relying on personal networks proved particularly effective in the tight-knit kin-based community. It was effective in tracing and examining their networks and also in reducing the rate of refusal. Contacting potential interviewees through personal networks, I received virtually no refusal.

Gaining access to community activities was easiest in Lima, because, unlike in Japan and the United States, there were numerous institutions and organized activities in centralized locations. The Japanese “colonia,” as the Japanese Peruvian community was known in Peru, consisted of physically bound sites, including a ten-story cultural center with a Japanese garden, an athletics stadium, schools, a theater, and a hospital. Housed in these locales were about seventy voluntary associations, including recreational, regional, educational, and business organizations. Since my Japanese Peruvian host family was actively involved in community activities, I often accompanied them both to participate in and observe such events as festivals, singing contests, religious services, athletic meets, birthday ceremonies, and weddings. Through participation in such activities, I met more people to interview.
Entry also was relatively easy because Japanese community membership in Lima was defined on the basis of “blood.” Usually measured by phenotypical features and surname, “blood” served to distinguish them from others (“Peruvians”) more clearly than in the other locations (Japan and the United States). Because of this membership definition, I had easy access to Japanese Peruvian institutions. Although many institutions were physically guarded with closed gates and (non-Japanese Peruvian) security officers, I often was given automatic access. Guards at the main entrance to the Japanese Cultural Center also let me in, while they checked the belongings and identification documents of each non-Japanese-looking person. I was also given access to Japanese Peruvian homes; although I was treated as if I were “one of the family,” many hosts pointed out that they would not let “foreigners” (non-Japanese Peruvians) into their homes.

Within the community compound, I frequently observed Japanese Peruvians talking about how “Peruvians” (of non-Japanese descent) were “intruding” into their space, and how that led to increased thefts. Some Japanese Peruvians also spoke in my presence about how some Peruvians were nice, responsible, and trustworthy “as if they were Japanese,” or “even though they were not Japanese.” Although I was, and certainly felt like, a total outsider to the community, in their view, I was not, because I shared symbols, such as surname, that bound their community. When I approached Japanese Peruvians, the first question they would ask me was my surname, followed by which Japanese prefecture I hailed from.

Identifying surname and place of origin in Japan (i.e., your family background) was an important way to recognize one another among Japanese Peruvians whose sense of community and trust was based on quasi-kin relations. In fact, Japanese Peruvian associational membership was officially defined by “blood,” measured by surname; those who carried at least one paternal or maternal Japanese (Okinawan) surname were eligible. Subsequently, I was reminded at times that I was, indeed, eligible to become a member of their community associations. Through my efforts to gain entry and acceptance to their community, I learned a great deal about how they defined their group membership.

As I moved from Peru to Japan, I experienced an abrupt shift in group boundaries. In Japan, where Japanese Peruvians were treated as foreigners despite their Japanese descent, I represented the “other,” the majority Japanese, from which they also distinguished themselves (Takenaka 2003a). Boundaries between insiders and outsiders are indeed permeable (Merton 1972). To gain acceptance among them, I constantly tried to distance myself from the Japanese by stressing my residence in the United States and my prior experience in Peru. This effort, together with my command of the Spanish language, helped reduce the distance between us. Previously established personal contacts in Lima also helped (and served as a good
excuse to approach them), as did, I believe, my proximity to them in age, since most Japanese Peruvians in Japan were third-generation, in their 20s and 30s. What turned out to be more difficult was getting into a factory to work side by side with Japanese Peruvians in Japan.

Although it was not part of my original plan, I wanted to work in a factory as a participant–observer. I learned through interviews with Japanese Peruvians that their lives in Japan were quite work-centered. Not only did they spend most of their time in factories, they also talked, and complained, much about work in their spare time. “Life in Japan is work, work, and tough work,” as one person put it. “There is no time for anything else in Japan.” I wanted to see what “tough work” really meant, and how Japanese made them work as “robots” and “donkeys,” as they frequently told me. Entering a factory, however, proved to be a challenge.

I faced difficulty, first of all, because I was treated as Japanese in Japan. I obtained a list of labor brokers and hoped to gain employment like Japanese Peruvians. Most Japanese Peruvians in Japan worked in factories on short-term contracts through labor brokers. Most brokers were Japanese, and some were Japanese-speaking Argentineans or Brazilians. Some operated legally, while others were illegal. I called several brokers hiring Peruvians, but they flatly rejected my request because of my fluent Japanese: “We hire only foreigners,” they told me, without asking anything about my background. These jobs were reserved for foreigners, I learned, and employers based foreignness on Japanese language fluency. I sensed a thin barrier between “Japanese” and “foreigners.”

There were other reasons for rejection. One subcontract employment agency told me that they only had men’s jobs, meaning physically tough work. Women were not allowed to do such work, and there were no “women’s jobs” on factory floors. The agent told me that they would be in touch “in case women’s jobs might come up,” and asked me if I knew how to type before hanging up. Others declined because they did not want to deal with a researcher. Some did not understand what research meant, and most simply wanted workers. The only option left was to enter as a worker, just like Japanese Peruvians. I did not have to lie, I told myself, but neither did I need to tell explicitly of my role as a researcher. As a Japanese citizen, I had no legal obstacle to work in Japan. Yet this strategy made the entry process and subsequent research difficult.

One afternoon, I showed up at Sabo, a subcontract employment agency, to talk to Marcelo. A Japanese Peruvian I befriended had already talked to him on my behalf. A Japanese Brazilian in charge of contracting South American laborers, Marcelo was expecting me and told me a story, in “portuñol,” Portuguese mixed with Spanish, about how badly they were in need of workers at a nearby computer assembly factory. There were 200 Peruvian and other South American (Brazilian and Argentinean) contract workers employed in the factory,
he said, but about half of them quit within a month. Since my major concern was my Japanese citizenship, I asked him if it presented any problems. “No, no,” he said, as there were indeed some Japanese working in the factory. He then began to explain the job, according to the information sheet in front of him, as if it were routine.

About ten minutes later, he suddenly stopped, looked up, stared at my face, and said: “But are you really Japanese?” I showed my Japanese passport, which I thought was the prime evidence. Examining my passport, he kept shaking his head: “Are you Japanese? I mean, were you born and raised in Japan? Some Nikkei Peruvians were born in Japan but raised in Peru, you know?” My response was, “Yes, I am ‘Japanese Japanese,’” but Marcelo remained skeptical. “But do you speak Japanese, I mean, really well?” “I suppose.” “Then why do you speak such good Spanish?” He leaned toward me with eyes wide open. I said I was in Peru for four months. He screamed, as if in desperation, “I’ve been in Japan for four years and still don’t speak Japanese.”

While looking at my passport, he spotted my U.S. visa. “Wow, you have a U.S. visa. Why is it?” I was mortified; my status as a graduate student might be questioned. But his interest was something else: “So, do you speak English, too?” Relieved, I smiled and said, “A little.” But then, he noticed my English handwriting in my notebook. “Do you write in English, too?” He almost fell out of the chair. “I’ve never met any Japanese like you.” Then, after a pause, he asked me in a more serious tone, “Why do you want to work in a factory?” This question was expected; I responded, as I had been prepared to, that I had Japanese Peruvian friends working there and just wanted to work temporarily with them. Marcelo paused, smiled, leaned close and said, “You know what,” looking at my eyes through his glasses, “why don’t you work in our office as an interpreter? Pay is much better.”

At this point, a Japanese Argentinean couple came in, and Marcelo stood up to assist them. As I looked over, he checked their visas, made copies of them, let them fill out an application form—and they were set to work in ten minutes. Few questions were asked. Few words were exchanged. Marcelo came back and told me to fill out an application form (in Spanish), too. The form asked only basic self-identification data (name, address), type of Japanese visa, and work experience. At the bottom were questions about the name of the last school attended and whether or not I had graduated from that school. Luckily, there was no question about the years of school attendance or earned degrees. So, I put down “Columbia University” and “not yet graduated.” While filling out the form, I overheard two female office workers, a Japanese and a Japanese Brazilian, talking about me. The Japanese Brazilian woman said, “She speaks perfect Spanish, but she is Japanese.” She came up to me and asked the standard set of questions: when, how,
and how long have I studied Spanish? “You know, we need someone like you who speaks both languages,” said the Japanese woman, “because we constantly have communication problems with Nikkei workers [South Americans of Japanese descent].”

Meanwhile, Marcelo returned with his boss, the Japanese head of the firm, to convince me to work as an interpreter. I politely declined the offer, expressing my preference to work in a factory. After a half-hour conversation back and forth, about why and why not, we settled that I would consider working in their office upon graduation. Interestingly, the head did not ask about my personal or educational background. His main concern was why I would not work in their office.

I was happy to have passed the test and that I could finally work in a factory. While I was picking a factory uniform, Marcelo and the entire staff watched, repeatedly saying “mottainai” (“She deserves a better job”). Just before I left the office, Marcelo whispered to me, “It’d really be nice if you decided to work in our office. Then you can teach me Japanese and English.” Perhaps these employers were more concerned about their own interests than about who I was. It probably did not matter whether I was a student or not, completing my BA or PhD. After all, their main job was to contract laborers for manual work.

Once begun, work was a painful process; due to my entry as a worker and hiding my role as a researcher, I felt guilty and frustrated. It was difficult to engage in open dialogue with both workers and employers. When I asked many questions about the factory and the line operation, people became suspicious. When I took notes when the line stopped, people asked what I was writing down. Worse still, I seldom had satisfactory answers. My ambiguous role in the factory prevented me from engaging in the in-depth conversations I would normally have had as a researcher. The nature of the work itself made it difficult to conduct research. The assembly line was so loud and so fast that I was hardly able to talk to my coworkers. Breaks were so short that it was difficult to observe interactions fully. When I took extra time observing factory floors and counting people during breaks, a Japanese boss shouted at me to get back to my work position. Lunch breaks also were too short for in-depth conversations.

Yet I did learn from this experience how foreigners and Japanese communicated minimally with each other and how they segregated themselves during breaks and meals. I was able to verify that men and women often engaged in similar types of work (although men always earned more than women, supposedly because they did heavier and dirtier work). Turnover was indeed high, and work was tough, primarily because it was simple. My job on an assembly line was as simple as cutting Scotch tape and putting it around computer monitors, but the line moved so quickly that the task had to be completed in 15 seconds, and the procedure was repeated 240 times in 60 minutes. After a week of repeating this task some 9,600 times, my
back and neck ached, and my fingers shook to the extent that I had trouble writing down my field notes. My Japanese Peruvian coworker consoled me: “Once you become ‘roboticized’, you’ll get used to the work and even to the pain itself.” Albeit with pain, this experience helped assure my rapport with Japanese Peruvians outside the factory. I was finally able to share laughs with them in talking about how our Japanese bosses shouted at us, and how we all worked “as if we had never worked in our entire lives.”

**The Role of the Researcher in Ethnographic Study**

Throughout my research, I struggled to cope with the dialectic relationship of being both a participant and a researcher. As my experience in a factory demonstrates, hiding the role of a researcher can be detrimental. At the same time, I strived to be a total participant wherever possible, in order to access data in natural social settings. As Gans (1968, 305) states, “People soon forget why the researcher is there, and react to him as a participant. They treat him as a person even if he treats them as subjects of study.” Indeed, several Japanese Peruvians complained that I was constantly asking them about “Nikkei stuff,” and urged me to go dancing instead. I often went dancing, to play the role of a total participant. But even while dancing, I took mental note of everything I saw and heard to be jotted down later (and I sometimes ran to the bathroom to write down my observations on the spot). Even though people treat you as a person, and you want to be treated as such, you are privately a researcher. Ethnographic research is deceptive and, consequently, is psychologically strenuous and emotionally draining (Gans 1968).

Because of the personal nature of ethnographic research, it is important to study how personal elements affect the data-gathering process and the gathered data (Tsuda 1998; Gans 1968). It also is important to be aware of how I, as a participant-researcher, played a part in the very interactions that I studied. To do so, I tried to (1) triangulate data and (2) examine people’s reactions toward me.

Triangulating data is a way to back up one’s findings as well as to seek consistencies and contradictions in what people do, say, and what they say they do and should do, by using multiple methods in multiple settings. In an attempt to examine how I played a role in the data I collected, I tried to engage in multiple methods—participation, observation, and face-to-face interviews with individuals. In addition, I conducted several group interviews in Japan, partly to observe dialogue and partly to control for the researcher’s effect. To do so, I occasionally brought in a Japanese Peruvian moderator to see if that would yield different results. To facilitate further group discussions, I formed an association with six Japanese Peruvian members, named it “the Nikkei Peruvian Discussion Group,” and registered it in the city of Fujisawa in order to become eligible for municipal conference facilities. After two group meetings, however, the group gradually
became indistinguishable from the salsa dance club formed by the same members. It failed, partly because of a lack of structure and also because of already familiar, and not strategically selected, membership. Members also preferred dancing to discussing Nikkei identities. As I found that semistructured group meetings did not yield much new information anyway, we devoted ourselves to salsa dance lessons—that lasted for a long time.

I also examined people’s reactions toward me. In addition to what they said in response to my questions, I learned a great deal from the questions they asked me. Some of their questions concerned why Japanese are so cold, why Japanese look down on foreigners, or what Japanese do on weekends and holidays. Others demanded that I teach positive things about Peru to Japanese people, because, according to them, Japanese knew only negative things about Peru and Peruvians. These reactions provided me with clues as to how Japanese Peruvians wanted to be treated by Japanese, and how little they knew about Japanese on a personal level. Marcelo’s reaction to me in the employment agency also showed the kinds of Japanese they were used to dealing with—factory laborers, most of whom were without higher education.

Throughout my research, many Japanese Peruvians, like Marcelo, reacted to me with surprise, which, in turn, surprised me. They were amused by the fact that I spoke Spanish well, occasionally taking me to fellow Peruvians for a “show.” “Look how well she speaks Spanish, but she is real Japanese,” ‘Japanese Japanese.’” I learned these terms for the first time from Japanese Peruvians in Japan, because Japanese natives in Japan, as well as Japanese Peruvians in Lima, rarely thought of themselves as “real” or “fake” Japanese.

Peruvians in Japan also found my command of the Spanish language useful. Since most of them lacked sufficient command of Japanese, they frequently asked me for help. Rumor spread quickly about “a Japanese who spoke Spanish” in town, and even strangers would call about various personal issues, from medical to marital problems. I frequently served as an interpreter for them, accompanying them to public offices, factories, and stores. I translated letters and filled out their application forms for credit cards, visas, and special bank accounts. While it was a way for me to reciprocate, it also proved helpful in learning about the needs and problems they encountered in Japan. At times, however, I felt frustrated. I was even asked to negotiate with their employers over pay (“It must be a Japanese Japanese talk, not between a Japanese and a foreigner”) or to accompany them to their factory just to tell the boss why they missed work the day before. Japanese Peruvian friends warned me, “You must watch out, because Peruvians abuse people. They feel empowered by doing so.” I had to learn to say no at times, but everything I did, heard, and observed in helping them provided useful data for my research.
Writing and Comparing Data

Multisited fieldwork provided abundant opportunities to collect data, but in order for anything to become data, it had to be written down. Writing down notes can be a painful process in any fieldwork, but it was particularly so in multisited research. It took me some time to devise a special coding system and to learn to quickly jot down interview notes conducted in three languages (I seldom recorded interviews). Sometimes I felt overwhelmed by an abundance of data. Living with a Japanese Peruvian family in a foreign country, for instance, was round-the-clock fieldwork. Everything I lived, heard, and saw was so fresh that I had to write it down, quickly. I constantly struggled for the right words to describe what I felt and sensed. Time always passed by more quickly than I was able to jot down everything I wanted.

Even more challenging was to organize and write about information collected in multiple sites. Once I put all my notes into a computer, I sorted them, first by country and then by questions of my interest: why they migrated or did not migrate; how they related to each country; how they perceived the way they were treated; how they related to the Japanese Peruvian community in each country; and what their future plans were. I also sorted data according to key categories and issues: Nikkei culture and identity (the terms they frequently used and talked about); transnational links (an issue of my interest); and the impact of the return migration (an issue I found important during research).

I was able to compare data across countries to the extent that I asked similar questions to similar populations in each country. On the other hand, cross-national comparison was difficult, as the amount of data for each category varied from place to place. There was more data about “community” in Peru than in Japan and the United States, where Japanese Peruvians perceived there was little or no community. Likewise, there was a lot more about “Nikkei identities” in Peru and Japan than in the United States, where some had not even heard the term Nikkei. Research in the United States yielded more data on transnational links, while there was relatively little on the topic in Peru. Since I set out to follow a population across countries, rather than doing an explicit cross-national comparison, I also lacked exactly comparable background data for each country for the same time period (Japanese immigration to Peru occurred around the turn of the century, much earlier than the subsequent migrations to Japan and to the United States; see Takenaka 2004).

These differences eventually led me to organize the whole thing by geography (i.e., Japanese Peruvians in Peru, Japan, and the United States). I wrote each geographical section in such a way as to make sense of cross-national differences or locally specific characteristics. Organizing the material, however, was a painful process. I spent several months moving data around, debating over whether to organize it by place (differences) or themes (similarities) before settling on the former. What proved useful in
the process were field reports, or summaries of research activities and findings, that I wrote upon completing fieldwork in each research site, as well as a narrative outline I wrote, and rewrote, before I began the final writing process.

**Conclusion**

Multisited research was a challenge as much as a “journey of discovery” (Hendry 2003, 507). The journey grew out of my research, and my project evolved in the process. By engaging in multisited research, I learned not only why people migrated and who did, but why others stayed behind or returned home after migrating. I learned not simply where migrants were coming from but also how their perceptions and expectations were formed relative to other places, most often their places of origin. Following my transnational subjects in multiple contexts also helped me learn the extent to which migrants and nonmigrants formed, and severed, ties in the process of migration. It also provided me with a basis for comparison: how Japanese Peruvians related to each national context (Peru, Japan, and the United States) relative to others, and how their perceptions compared before and after migrating vis-à-vis actual experiences.

Cross-national research taught me that knowledge, while context specific, is also shaped by institutions, policies, discourse, and symbols in multiple locations (Nyiri 2002; Kurotani 2004). While specific cultural symbols varied locally, there were striking similarities in the way culture was used in discourses about “us” and “them.” The way Japanese Peruvians talked about “unique Japanese values” (hard work, warmth) in Peru was exactly the same as the way they described “Peruvian culture” in Japan in distinguishing themselves from others (hardworking, warm Peruvians, in contrast to cold, materialistic, individualistic, and lazy Japanese, who were unwilling to put in long hours of overtime work in factories, unlike themselves). In the United States, Americans also were described as “cold, materialistic, and individualistic,” in contrast to “warm” and “family-oriented” Latinos. Discovering these similarities, and differences, across countries shaped my questions and ultimately pushed my research.

It is difficult to tell when research is finished. I left each site when I began to hear the same thing after forty or so interviews. While I “officially” left the field in 1998, my fieldwork, in a way, has gone on. Ever since then, I have acquired the habit of counting everything countable, and carrying a notepad in my pocket; ethnographic research was good training for observing, asking questions, and approaching and interacting with a variety of individuals in multicultural and multilingual contexts. As I keep crossing national borders, I continue to learn these skills every day.
Works Cited


Chapter Ten  

Immigrants and “American” Franchises: Research Challenges in New Lines of Inquiry

From

Researching Migration: Stories from the Field

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http://www.ssrc.org/pubs/researching_migration.pdf
When I walked into a Burger King restaurant in downtown Brooklyn and asked for a job several years ago I had no idea that I was about to embark on a long journey toward understanding immigrant labor in American corporate franchises. It just seemed to be the best way to fulfill a course requirement in graduate school. I needed to study a work culture. I was interested in the relationship between poverty and the transforming global labor market. A fast-food job in Brooklyn, I thought, would provide an ideal glimpse into the lives of people trying to survive on the bottom rungs of major growth sectors of the mainstream economy. It had not occurred to me to study immigrant workers until my on-the-job experience provoked this interest. During my three months of employment I witnessed this workforce transform from a predominately African American staff to a diverse group of immigrants from all over the world. The processes that underscored this type of labor force transformation became interesting to me. They seemed pivotal to an understanding of race and ethnic stratification at the bottom rungs of the growing service economy and their affect on the chances for mobility for different groups of workers, including immigrants and the native born.

This paper gives me an opportunity to elaborate on how this research developed into a dissertation project on the big fast-food chains and immigrant employment in New York City’s immigrant neighborhoods and the challenges I faced in developing and implementing a qualitative research design. I begin by discussing some of the theoretical quandaries I dealt with in trying to develop a framework for a topic that had not been studied before and how my methodology evolved out of my theoretical questions. I then turn my attention to issues of access and rapport, two methodological concerns I struggled with while attempting to implement an interview-based research design in a giant corporate franchise organization characterized by low-wage, low-status, temporary, and part-time employment. I highlight two examples of “unintended” research findings that evolved directly from my choices in methodological approach in order to illustrate the inextricable link between research methods and results. Finally, I address the data analysis phase and share how I approached the difficult task of interpretation of a large body of variably ordered text and some of the methodological questions that arose from it.

**Defining a Research Question: Theoretical and Methodological Challenges**

Had I been interested in the study of immigrant employment from the start I do not think I would have chosen to study a fast-food restaurant franchise. First, the fast-food literature and modern corporate organizational studies lacked a focus on immigrant employment. Second, studies involving immigrant workers who relied on low-level jobs in the host society tended to focus on traditional “ethnic economies,” in which immigrants and employers share the same ethnicity (co-ethnicity) and are relatively segregated from the mainstream.
Researching Migration  
http://www.ssrc.org/pubs/researching_migrations.pdf

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economy, such as in Chinese garment factories and ethnic restaurants (Light 1973; Portes and Bach 1985; Waldinger and Bozorgmehr 1996; Zhou 1995). For example, the ethnic enclave literature was a dominant resource for students interested in immigrant employment. The ethnic enclave was conceptualized as the geographical concentration of “ethnic economies” in immigrant neighborhoods. It was argued that the geographical concentration of ethnic business and employment activity provided the economic foundation for the preservation of cultural traditions from homelands, including language, religious customs, and family practices (Portes and Bach 1985). I thought it was interesting that at the same time this literature was channeling students into studies of these traditional ethnic sectors the globalization literature was growing in influence. Globalization studies, and particularly the homogenization thesis, was envisioning a “McDonaldized” world culture in which Western-type corporate institutions were devouring local culture and institutions (Ritzer 1993).

After my experience working at Burger King and then reviewing this literature I decided it would be interesting to try and explore how these two schools of thought related to each other in the real world of the giant franchise economy. I first decided to conduct informal research to gain an idea of the extent to which the big franchise companies were present in New York’s immigrant neighborhoods. After pounding the pavement for four months and talking to more than a hundred fast-food restaurant participants in all five boroughs of New York City, it became clear to me that the big fast-food chains were, indeed, making deliberately planned inroads into immigrant neighborhoods, often with specific strategies to attract a new immigrant clientele and new immigrant workers. It seemed that immigrant neighborhoods having strong cash flows and a workforce eager to “Americanize” were ideal targets for the fast-food industry.

But the extent to which the industry was drawing on an immigrant labor force and how this was related to consumer strategies to attract new immigrant groups to fast food was less clear. Was the industry drawing on the same people who worked in traditional “ethnic economies,” or was it drawing on different groups altogether? Were immigrants carving out occupational niches in American fast-food restaurants, an industry long perceived as the quintessential employer of American teenagers? Were immigrants using jobs that the American public considered dead-end and last resort as a stepping-stone into the American mainstream culture and economy? Did fast-food restaurants in immigrant neighborhoods provide the same opportunities as traditional ethnic enterprises? Or were fast-food jobs in immigrant neighborhoods simply dead-end opportunities reflecting the already polarizing forces of the American city and the economy at large? In the long run, did the fast-food restaurant help reinforce or
break down the traditional norms of ethnic community in the immigrant neighborhood and ethnic enclave?

It seemed that answering these questions and developing an appropriate methodology to study them required moving beyond the homogenization thesis as well as the image of immigrant communities as homogenous and immigrant workers who depend on low-level jobs as socially and culturally isolated. James Watson, in his work on the global expansion of American-style fast-food restaurants in East Asia, had argued that the spread of global cultural institutions is not a unilateral process, as the homogenization thesis tends to proclaim; nor are local cultures immune from these processes, as the ethnic enclave literature was seeming to suggest. Rather, it is a two-way street. This idea of linking the global with the local among immigrant communities in American cities came to frame my methodological approach to the study of immigrant fast-food workers (Watson 1998; Portes and Bach 1985).

**Developing a Methodological Framework**

Most studies of the immigrant enclave or ethnic economy, I learned, relied on fieldwork: either survey data or face-to-face interviews with immigrant employees and owners. Likewise, my methods were primarily qualitatively based with a heavy emphasis on semistructured, face-to-face interviews with restaurant participants at all levels of the hierarchy—from entry-level crew members and managers to owners. I conducted in-depth face-to-face interviews with 52 people including crewmembers, managers, and owners. They came from more than a dozen different countries in Asia, Africa, Europe, and Latin America.

My questions—open-ended rather than hypothesis oriented—were best treated through a kind of grounded theory approach that would allow me to move back and forth between empirical findings and theoretical framework to continually redirect my research questions. I thought that a survey method, usually associated more with a positivist methodology, would not give me this kind of flexibility. Semistructured interviews (what I referred to as an “interview guide”) would give me the freedom to orient and develop an interview design that would revolve around respondents’ experiences and knowledge, enabling me to gather information I could not have anticipated during the interview interaction.

In order to capture local variations (including how ethnic relations play a role in labor market processes) I used a neighborhood comparative approach. I focused on two fast-food chains/brands and seven restaurants located in three different immigrant neighborhoods. The criteria for choosing the immigrant neighborhoods I studied (Manhattan’s Chinatown, the predominantly Dominican neighborhood of Inwood/Washington Heights, and downtown Brooklyn) were based on a number of criteria. First, the immigrant community had to be relatively large. Second, the neighborhood had to include
both a traditional ethnic enclave and the big fast-food chains. Finally, convenience and personal access were important—I lived in Inwood, my research job as a graduate student was in downtown Brooklyn, and Chinatown lay between these neighborhoods. I decided to focus on the two biggest hamburger chains, McDonald’s and Burger King, because they seemed to have the biggest and most aggressive marketing strategies among the major fast-food chains and they were similar in organizational structure. It was also because I was familiar with both of these chains. I had worked in both of them—at Burger King as a participant–observer and at McDonald’s when I was a teenager.

In retrospect, I could have chosen virtually any immigrant neighborhood or any fast-food chain(s). I wanted to understand how local neighborhoods and giant corporate franchises interacted to create specific global–local patterns, rather than understand how a specific immigrant group or company functioned within it. This, I thought, would tell a larger story about the cultural strategies and operations of modern corporate organizations in the global economy and how they interacted with immigrants’ strategies of survival and mobility in American society.

Therefore, what was important was limiting the number of variables I studied while being careful not to make it a study about a particular company, immigrant group, or neighborhood. For example, if I had focused on only one company—say, McDonald’s—it could have been interpreted as a study about a specific company. If I had broadened my focus to include several different chains then there would have been too many structural variables related to the work process to make reasonable comparisons within and between restaurants and neighborhoods.

### Issues of Access in American Corporate Franchises

Accessing respondents is always a concern in qualitative research, and there were issues that were particular to the giant corporate restaurant chains. Would formal authorization be required to interview respondents, either from corporate headquarters and/or the local restaurant? Could I avoid getting authorization? Would anyone even talk to me, given the hectic and low-status nature of fast-food jobs? Since my unit of analysis was the restaurant (rather than a specific ethnic group) it seemed appropriate to seek out respondents where they worked (at the restaurant). At the same time I realized the importance of finding a balance between going through appropriate channels of authorization and being in a situation where respondents could talk freely and honestly without their job being threatened.

Thinking in the context of political economy or, more generally, labor studies, I originally thought that it would be important to talk to employees independently, away from the restaurant setting and the managers’ purview. But accessing enough employees independently in each restaurant to have a consistent sample size was problematic.
Being a part of the flex-based global labor market, employees’ work schedules are individually staggered. There could be hours between when employees arrived or left the restaurant, and they were always alone. Second, if an employee agreed to be interviewed then I found myself negotiating for an interview in another context, the employee’s family, which posed its own set of challenges. Third, it was difficult for employees to find the time outside of work. They were either students balancing school and work schedules, parents balancing the demands of work and children, or people struggling with two or more jobs. Time outside of work, in other words, was scarce and hectic.

I was skeptical of the alternative; pursuing formal permission to interview employees. Originally I thought that I would be required to gain permission from corporate headquarters of each chain in order to interview on-site restaurant participants. Given this, I was apprehensive about what this would imply—from flat-out rejection to a research design controlled and/or limited by corporate headquarters. This turned out not to be the case. Franchise owners, as I was told by headquarters representatives, are not restricted by corporate headquarters in what they can say as spokespeople of their restaurants. Likewise, managers can consent to their own interviews and authorize their employees to give interviews on the job if they choose to.

The freedom this gave me came with its own set of methodological nightmares. Given the standardized nature of the industry I had assumed that a uniform approach could be applied. But this was not the case. I found the industry to be enormously complex, with immense organizational diversity at the local level, not simply in terms of the diversity of people they hire, but also structurally. The decentralized nature of the social organization of this industry, in fact, became a central theoretical finding of my research, and a good example of how methodological approach often informs the direction of larger theoretical questions. In other words, it formed the basis of my critique of the rationalization thesis (McDonaldization), a critique that emphasized the significance of local agency and variation in globalization processes (Ritzer 1995).

On a structural level, for instance, I was surprised to find an absence of social linkages (both formal and informal) between restaurants, which meant that each of my seven restaurants had to be approached entirely anew. A snowball approach (which I assumed would be an effective approach in this industry) would be limited to connections that existed within each restaurant. Second, the local autonomy afforded to restaurant owners meant that each restaurant could have its own organizational culture and I would likely need to negotiate access to respondents in each restaurant differently.

As I discovered, some restaurants were ruled by a strong top-down authority structure in which issues of access were more formalized, requiring permission from the franchisee (local owner). For
example, one restaurant franchisee among my respondents was the biggest restaurant corporation in New York City. I was required to submit a formal letter of request to the central corporate administration. It took several weeks and numerous phone calls to get a response. In cases like this, where a lot of persistence was required, I felt that permission was finally granted just so I would stop bothering them. It would have been easy (even reasonable) to give up after a series of unreturned phone calls. In this industry, though, and especially when dealing with a large local ownership structure, persistence is crucial.

Some restaurants had a looser, flatter chain of command that gave managers and crew members more flexibility in their interactions with outsiders. In one restaurant, for instance, a manager gave me an interview as soon as I walked in off the street. As I later learned, the franchise owner of this restaurant, also an owner of a small retail shop in the Vietnamese ethnic economy (within Chinatown), had no involvement with the restaurant because of his limited language skills. He did not speak either language (Spanish or English) spoken by the Colombian-born general manager and the predominately Latino workforce.

While the structure of the social organization determined how I gained access to respondents, it also affected how interviews were conducted. Spontaneous interviews, such as with the Colombian-born general manager, seemed ideal at first. But like any relationship, the immediate pace of involvement and intimacy had its own drawbacks. This manager, for instance, sat with me for over two hours, providing me very rich material about his perceptions and experiences in the industry over the past ten years. But when I returned the next day to finish the interview with him and to interview members of his staff (as he had committed to) he seemed much less engaged, as if he had rethought his involvement. While he kept his word to allow me to interview his staff, he seemed to do so with a different attitude. Had he not expected me to come back? Had I done something wrong? Was he suddenly uncomfortable about what he had disclosed to me so readily the day before? Or was he simply too busy that day to deal with me? In the spontaneous interview the establishment of formal boundaries and expectations are often sidestepped to make way for receiving the immediate and unfiltered thoughts of an eager respondent. But this comes at a risk of possibly leaving the researcher without a formalized context in which to negotiate further research needs in the organization.

While the organizational culture and owner–management relations helped determine how and if I would gain access to restaurant staff (as well as written material, memos, etc.), it was also impossible to know what this structure was like until I interacted with it face to face. Ironically, it seemed that the more formal the process of gaining access, the greater the ease I had in conducting interviews and gathering data. Authorization from the top worked not only as a license for restaurant staff to talk to me
(and, usually, in the absence of this top-level authority) but also afforded the time during working hours for interviews. In retrospect, the informality of the spontaneous interview provided a unique opportunity to gather “off-the-cuff” data, which almost always probed new territory. On the other hand, the more formalized the interview context, the greater the ease with which people talked and the greater consistency there was in my respondent sample.

**The Interview:**  
*Establishing Trust and Rapport in Corporate Retail Franchises*

In-depth interviews seem to provide one of the most important contexts in which data is collected among immigration scholars, yet little has been written about the interview process itself. Data seems to rely heavily on personal encounters, yet investigators of the immigrant economy tend to remain silent and hidden in the literature (Goodwin and Horowitz 2002) with little reporting of reflexive processes (Bourdieu 1993; Wacquant 1999). The relative absence of writing about personal encounters may be related to the fact that the interview situation itself is so highly personal, relying heavily on personality or ethnic affinity rather than learned traits.

Without undermining the importance of the personal, I will discuss particular challenges I came across in establishing rapport in the giant corporate retail sector. Part of the challenge in this industry pertains to the diversity and multisituational identities of the people it hires; rapport had to be negotiated differently depending on the situational identity of each respondent and raises questions about how identity is produced in the research process (Robb 2004). I refer to two types of respondents (common to the giant corporate retail sector at large) to illustrate the strategies I developed to overcome particular challenges to establishing rapport: (1) teenagers who tend to resist being identified with the brand names and companies they work for, and (2) authority figures (including managers) who tend to frame their responses in the context of corporate rhetoric rather than their own experiences. But first I will discuss the concept of rapport, and particularly two elements of rapport that I found helpful to distinguish in this industry: trust and getting respondents to buy into the research project.

I refer to **trust** as that which gives respondents confidence that the information they provide is everything the researcher says it is—important, confidential, and so on. This is partly based on building a relationship based on the interactive elements of the interview, finding a balance between the roles of listening and probing (Tusini 2004). But trust, in some cases, became a secondary concern for me over how to get respondents to buy into the interview goals. A respondent, as I learned, may trust your scholarly motives, but be completely uninterested in your project or not take it seriously. Likewise, a respondent may be completely interested in your project but be suspicious and therefore withholding, or even misleading. In some cases a
respondent may be trusting and interested in your project, but to such an extreme level that you are forced to establish boundaries around your professional interests. Given the diversity of my respondents, not simply their ethnic backgrounds but also their socioeconomic backgrounds, age/maturity levels, and gender, these three elements of rapport were important to distinguish.

They are also important to distinguish because issues related to trust need to be thought of differently in the fast-food industry than in traditional ethnic sectors of the low-wage economy. In many studies of the traditional immigrant economy researchers need to be cautious about their respondents’ vulnerable and often undocumented status. Issues of trust significantly revolve around ensuring confidentiality so that employees do not perceive their job status and lives in the new country to be threatened. Since the most vulnerable immigrants tend to rely on co-ethnic relations within their own communities as a means of survival, establishing “trust” in the interview situation has tended to revolve around establishing “indigenous identity” or “ethnic insider” status. Recent studies have shown that about half of immigration scholars in the United States are themselves of immigrant stock (Gans 1999; Kusow 2003; Rumbault 1999). It occurred to me that the importance placed on “indigenous authenticity” and ethnic insider status may have influenced a methodological tendency within U.S. immigration studies to focus on immigrant group (rather than organization) as an analytical starting point.

Establishing trust among respondents in a fast-food chain restaurant requires a different kind of thinking than in traditional co-ethnic establishments. There are two relevant issues to consider. First, the ethnic diversity of the staff means that a particular ethnic background of a researcher may not necessarily be advantageous for approaching a diverse population. Second, immigrants who work in fast-food sectors are different from those who work in traditional co-ethnic sectors, most notably because they do not necessarily attach their ethnic identity to their job position or the organization they work for, at least not as a primary status. Immigrants in fast-food sectors tend to be a step above their counterparts who rely on co-ethnic employment. They are employed on a legal basis and are socioeconomically diverse, ranging from the poor to the middle class. Many are upwardly mobile and few plan on lingering in the industry for very long. Those who do plan on becoming managers. In fact, their fast-food job status is not typically a primary status at all.

Their more privileged status in an ethnically diverse environment allows them to step beyond both ethnicity and job position as primary statuses in an American organization. The American fast-food restaurant for many of my immigrant respondents represented a kind of transitional space between their own immigrant communities and mainstream America. Moreover, they occupied a whole set of social statuses, not simply an ethnic
one (Kusow 2003; Merton 1972). Other social statuses, such as socioeconomic status, student, future career, age, and gender, play central roles in their identity as “American” employees.

Immigrant Teenagers and the Fast-food Social Stigma

Rapport, then, was negotiated in light of these multiple situational identities. I, as an interviewer, played on my own multiple identities, as a student, a former fast-food crew member, an American, a woman, and a young person, according to whom I was interviewing. For instance, there was a set of issues that particularly concerned the younger, especially teenage, employees. One of these pertained to the social stigma attached to fast-food work. Being a fast-food worker carried so much social stigma that employees usually chose to work in fast-food restaurants outside of their own neighborhoods to avoid being taunted, embarrassed, or pressured into breaking rules and giving away food. Although employees did not believe their jobs deserved the low social status they were associated with, they were acutely aware that the rest of society did. Establishing trust as an interviewer usually depended on being able to convey, first of all, that I did not share in the social stigma and that I recognized their more “primary status” as a future doctors, basketball players, teachers, or simply people who were trying or wished to do better. My status as a former fast-food worker and a current student helped convey the degree to which I could communicate an understanding that their current status was not really what they were all about.

In my initial interviews, without being attentive to this, my youngest respondents seemed defensive. This defensiveness, as I interpreted it, stemmed from my respondents’ feeling subjugated to an inferior status, perceiving me as an outsider who was interested in fast-food workers as “losers” or illustrations of failure. As one teenager asked me, “Why would you want to study fast-food workers? There are much better jobs out there.” By emphasizing my own experiences as having had jobs in the industry and starting out the interview by talking about respondents’ primary status and future goals I was better able to reconfigure status distinctions between interviewer and respondent, elevate the respondents’ status, and negotiate a more even playing field. Asking respondents for help also contributed to elevating their status to informant versus object of research.

These strategies affected the degree to which employees bought into the project. For example, without reconfiguring status distinctions in the interview relationship, these young respondents viewed the project differently—as a study about them, in the objective rather than informant sense. Once they realized that their thoughts and experiences could actually shape the study’s story, rather than felt their lives were being interpreted by it, they were more apt to express what seemed like genuine interest.
Still, it was a challenge to get some of the teenagers to talk, especially fourteen- and fifteen-year-old boys of the lowest socioeconomic status. With some it felt like I was prying information out of them, getting only one-word responses. My gender, I believe, helped mediate this kind of social distance among my female respondents of equivalent socioeconomic status and age (Hall 2004). On the other hand, respondents’ youth and temporary and part-time attachment to the industry also seemed to reflect the fact that many people who work in this industry (and giant corporate retail chains in general) simply do not have the depth of experience or conscious investment in their jobs to be able talk about workplace dynamics. In these cases, the interview focused more on personal issues, that is, respondents’ relationship to the industry rather than their particular roles within it.

In hindsight, I believe it would have been productive to conduct focus-group sessions among the teenagers rather than rely exclusively on one-on-one interviews.

Given the complex issues surrounding struggles over identity and social stigma in the industry, as well as temporary attachment to the industry, focus groups (involving both male and female teenagers) may have been an effective strategy for capturing both individual and interactive dynamics in the workplace and illuminating issues that were difficult (or simply uninteresting) to express for respondents on an individual level. On the other hand, it may have been difficult, on a methodological level, to orchestrate focus-group sessions. It is unlikely that managers would have allowed more than one or two crew members at a time to be taken away from their job tasks while on the clock. Furthermore, given the individual staggering of work schedules and the fact that employees do not live in the neighborhoods where they work, it would have been difficult to get enough crewmembers together outside of work hours to conduct focus-group sessions.

Getting Beyond Corporate Rhetoric: Interviews with Fast-food Managers and Franchisees

Another concern when doing research in corporate organizations is how to get beyond corporate rhetoric when interviewing authority figures—in my case, fast-food managers and franchisees. Among these people, establishing trust was factored on different grounds, and inspiring interest in the study was usually not necessary. Becoming a manager, first of all, already implied having some investment (including time and interest) in the industry. Most managers did not feel comfortable being interviewed until they fully understood what the study was about, what it would be used for, and that they were assured of confidentiality. At first, managers’ frame of mind was typically situated in the context of corporate rhetoric, using it as a shield from personalizing the issues or simply interpreting my interests in this context—that I was there to find out how the organization is supposed to work. In nearly every
interview with managers I needed to shift the framework of conversation away from the corporate philosophy, or rhetoric, in general, to managers’ personal perceptions and experiences in the industry.

In doing this, it was helpful to draw on my own experiences as a fast-food worker or to recount what other respondents had told me. This served a number of purposes. First, it allowed me to negotiate an insider status as a former crew member and someone familiar with the nuts and bolts of the industry. Second, it elevated my respondents to a consultant or informant status, rather than an object of analysis, or mere interpreter of corporate codes. For example, consider the following exchange in an interview with a general manager:

JPT: Do you tend to hire certain kinds of people for particular jobs in the restaurant? For example, do you need a certain kind of person to work on the front line?
M: That person would need to have a cheerful personality and communicate well.
JPT: Are there certain kinds of people, say, men or women, who are better in these roles?
M: No, anyone can be given the opportunity to be on the front line.
JPT: Last week, a manager in another restaurant explained to me that he prefers to have women on the front line because of (elaboration). Also, in my experience working at a Burger King, none of the guys wanted to work on the front line because they said it was a “girl’s job.”
M: Yes, yes, it is like that here too. While the opportunity is there for everyone, I notice that the girls do a better job, they are more friendly with the customers, are more attentive to their appearance. See, the guys, they don’t pay attention to things, they come to work with their hair uncombed, their fingernails dirty, things like that…I just don’t put the guys on the front line.

It seemed easier for managers to verify and elaborate on a trend rather than explain how things worked in contradiction to the corporate philosophy, partly because their job responsibilities were defined in the context of corporate manuals even though their day-to-day activity was completely different. Presenting a trend was also a way for me (the interviewer) to communicate what I was really interested in without posing a direct question such as: Are there ever any exceptions to the corporate organizational philosophy? Once the conversation shifted away from corporate rhetoric, managers usually seemed highly motivated to talk. Managers, as I learned, are true middlemen, caught between corporate rules and local realities, that is, between standardized policies and diverse immigrant cultures. Several told me that they had not had an outlet for these concerns and had rarely had the chance to place their everyday experiences into real thoughts. The interview, then, was an outlet for many of the managers to voice
the everyday contradictions inherent in this middleman position.

The large degree of negativity voiced by many managers, especially concerning the nature of their jobs and company expectations, led me to believe that they did not feel particularly constrained or self-conscious while being interviewed “on the job.” An interview with a manager from India at a Burger King at 5:00 one Sunday morning became one of the saddest personal critiques I had ever heard about the industry. In a three-and-a-half hour interview this manager explained how his life was a pathetic story and his position shameful, located at the bottom rung of the U.S. socioeconomic ladder. He discussed the “despicable” nature of pay hierarchy in the United States, the unfairness of minimum wage, and the grueling nature of fast-food work. Managers, I came to understand, held positions of power but felt powerless to change the structural conditions of the industry. This was why they could sympathize with the employees who complained about their conditions and at the same time be the object of blame on the part of these same employees. (This was also why employees did not feel particularly constrained when complaining about their work conditions—because the real authority structure was amorphous to the local organization.)

Establishing Professional Boundaries: When Interview Relations Become Too Personal

In some cases both trust and interest levels became personalized, and I found myself in the awkward situation of having to establish my own personal and professional boundaries. In one particular case an immigrant from Cuba, a former military officer and Burger King crew member, asked to meet at a restaurant where he would give me an interview. It was apparent that he was financially and emotionally down and out and represented the dire situations of about 10 percent of my respondents. The interview lasted well into the night and he asked for more time to continue the interview the next day. The second day he asked for a third meeting. The relationship began to feel awkward. I decided that as long as our discussions continued to be relevant to the research project, I would continue to meet with him. We met for three consecutive days, after which I explained to him that he had made a valuable contribution and that I needed to move on to other participants. He had shared his life history, the trajectory of his financial decline (Burger King representing the rock bottom of his career), his divorce, the days he spent with his son, his one-room living conditions, his failed childhood dreams, his nightmares, and the details of his current work life. He had given me a lot, a glimpse into the misery that sometimes exists in this industry, and mires people in what one manager referred to as “the cycle that never ends.” I felt guilty taking his story when he went right back into this cycle of poverty and misery. Moving between my graduate student life and the lives of many of my respondents
The Relationship between Methodology and Research Findings: Two Examples of Unintended Findings

The inextricable relationship between research methodology and research findings is most prevalent in the case of unintended research findings. I provide two examples to illustrate this. The first example refers to how interactive elements of the interview, which often go beyond the control of the interviewer, can affect the content of discussion and hence the nature of the data collected. The second example refers to how a comparative approach illuminates unanticipated findings through what I call a reflective experience—or what a grounded theory approach would refer to as “conceptual reexamination”—looking back on previous observations and noticing what had gone unnoticed or passed as insignificant.

First Example

The first example, referring to the relationship between interactive elements of the interview and the nature of the interview discussion, is made most clear in a particular situation I found myself in when attempting to interview a recent Chinese immigrant in a Chinatown restaurant. The languages spoken by most of the Chinese immigrant employees at that time were Mandarin and Cantonese. I did not speak a Chinese language and the interviewee did not speak enough English to grant an informative interview. The general manager, who was fluent in Cantonese, Mandarin, Taiwanese, and English, offered to translate. I suggested that someone of the interviewee’s own status (i.e., another crew member) might be better and the manager obliged, although he hovered over the interview, often interjecting his own views and insights. At first the manager’s presence seemed like an annoying intrusion, stifling the voice of the crew member and sometimes even interrupting and offering his own interpretation of the respondents’ answers. I felt stuck between trying to conform to my methodological principles and displaying tolerance for the general manager’s “helpfulness,” because he held the reins to my access to future respondents and company memos. But as it became clear that these were the dynamics that would characterize this interview I decided to politely finish but not include the material among my data. After writing it off and simply letting the conversation go on between the general manager, the crew member translator, and the respondent, I realized how interesting the conversation had become. It became a kind of focus-group session about matters relating to relationships that had evolved between Chinese managers, crew members, and their parents, particularly the role that Chinese McDonald’s managers were playing in socializing Chinese families into the norms of American teenage jobs and culture.
topic was approached when I asked one of the questions on the interview guide: “How do your parents feel about your job here?” The manager responded by pointing out how the interviewee’s and the translator’s parents had different perspectives on their children’s fast-food jobs and how he (the manager) dealt with these parents.

Up to this point the role fast-food managers were playing in socializing new immigrant families into American teenage work culture was not an issue I had even considered, but it was extremely significant for understanding the way multinational chains successfully incorporate themselves into ethnic communities. In this way it became apparent that the interactive elements of the interview itself can often determine the content of discussion and hence the nature of the data collected. It is a good example of how analytical categories can be derived directly from the methodological approach, reflecting the interaction between the observer (investigator) and the observed (respondent), even when transcending the methodological “rules” (Charmaz 2006).

Second Example
Through a reflective experience I discovered specific ways in which technologies can play a role in facilitating and shaping a multiethnic workforce in this industry. Because the literature on modern technology and work relations stressed tendencies toward homogenization, I assumed that workforce diversity was correlated with rationalization processes, or what Ritzer (1993) described as the “robotization” of the workforce. In other words, various language groups could work together precisely because their individuality was irrelevant to the functioning of the modern workplace. But what I found was quite the opposite: While it was true that advanced technologies facilitated diversity, it was also true that diversity and ethnic identity played a proactive rather than an irrelevant role. A comparative approach where restaurants had different levels of technology made this apparent.

When I studied a Burger King restaurant in Chinatown I assumed that the absence of Chinese employees was due to a combination of factors that I had observed and that had been explained to me by the general manager. These included a lack of an existing Chinese network in the restaurant, the general manager’s expressed bias against the Chinese (including stereotypical views that they were not friendly with the customers and emphasized studying over jobs), and the lack of a local marketing strategy aimed at attracting Chinese residents to fast food.

It was only when I went across the street to study a McDonald’s restaurant that it became apparent that technology also played a highly significant role in the fast-food restaurants’ hiring practices concerning Chinese immigrants. New computer technologies had altered modes of communication in the restaurant, affecting who could be hired, the jobs they could do, and the diversity of the people who could work together. Restaurants that still had the
old microphone technology (like the Burger King) required front-line workers to verbally call out customer orders on a microphone to the food preparers in the back. Therefore, a common language spoken among all employees was required and had obviously prevented the restaurant from hiring non-English-speaking employees and, hence, recent Chinese immigrants.

With the advanced technology, verbal communication was not necessary between the front line and back food-preparation area. Front-line workers at the McDonald’s simply pressed the required code button on their register, which was then transmitted to a computer screen in the back. This technology significantly affected the fast-food organizational culture. It helped place a greater emphasis on face-to-face communication between front-line workers and customers, enabling the organization to hire foreign language speakers who can communicate with customers in their same language. In the McDonald’s in Chinatown, Mandarin- and Cantonese-speaking workers were placed on the front line to help Chinese customers who were new to the McDonald’s system order their food. In the meantime, these employees did not have to verbally communicate with the Spanish, Russian, and African workers who were preparing the food in the back, and could instead concentrate on their customers. It was through comparative methodology and the resulting reflective experience that the significance of technology and its interactive effects on hiring policies, ethnic divisions of labor, and marketing strategies became apparent.

**Conducting Data Analysis and Considering Interpretation**

One of the dilemmas of qualitative research involves how to appropriately analyze and interpret the data collected. Rather than ending up with a consistently ordered set of survey answers that can be neatly coded, quantified, analyzed, and tested against a hypothesis (as in the case of a positive-oriented survey design), a qualitative researcher often ends up with (after word-for-word transcription of interviews, field notes, memos, industry paraphernalia, etc.) a very large quantity of rich, detailed, often disordered text—in my case, thousands of pages of it. My qualitative approach had prioritized giving voice to my respondents’ lives and stories, hoping that the text that emerged would provide meaningful understandings about the immigrant experience in the context of global economic transformation and the growth of the giant corporate chains. But I had not anticipated how difficult it would be to analyze, interpret, and present the data.

While the literature on grounded theory analysis emphasizes the “back-and-forth” movement between theory and fieldwork, in reality it felt like moving forward (in the writing and presentation stage) required favoring one over the other as a starting point. Should the theory I had been dealing with reflect what I consider to be most empirically important? Or should the empirical material shape the theoretical
framework? If I choose the former, will I risk biasing my interpretations or even repressing the voices of my respondents? If I choose the latter, do I risk spending time on an analysis that is not adequately relevant to the major theoretical questions?

The path I took to get around this dilemma involved taking a broad thematic approach, broad enough to capture nuances in the data, yet specific enough to be relevant to existing theoretical discussions and my original framework. These themes paralleled the topic areas of the interview guide, while also including new angles of understanding that had emerged during the data collection phase. For example, one of the themes pertained to the way employment networks worked in the industry. Co-ethnicity was central to understanding employment relations in the ethnic enclave literature and I had begun my research wanting to understand the extent to which social structures, including ethnic relations, operated similarly in the fast-food chains. What I found was that the ways in which co-ethnicity operated in the fast-food industry were just as significant, but almost diametrically opposite in form. Co-ethnic relations in traditional ethnic enterprises were premised on immigrant vulnerability, mainstream marginalization, and cultural identity. But in the fast-food industry they were premised on cultural capital, commercial interests, and coexisting and multiple ethnic chains. A thematic approach toward data analysis allowed me to both theoretically contextualize the data and stay open to new forms of interpretation.

I coded and analyzed the data using both quantitative (SPSS Statistics Software) and qualitative (content analysis) tools. The “factual” data I collected (e.g., demographic data, numbers and ethnic makeup of employees, commuting times, how jobs were obtained, etc.) could be coded and quantified. Other data based on employees’ interpretations of experiences and observations were qualitatively analyzed using a text-based content analysis.

The comparative framework (between neighborhoods and restaurants) provided the basis for theoretical sampling on the micro level to examine variations between categorical patterns. Theoretical sampling allowed discovery of the features of categories, the conditions which give rise to them, and how they were connected to other categories (Charmaz 2006). It also provided the basis for understanding local variations and the global–local relationships that characterized the industry. The comparative framework provided a way to turn around the nagging question that often haunts qualitative researchers regarding the extent to which findings are generalizable. In other words, my data showed that employment patterns in the industry simply were not generalizable, given the autonomy and variation that existed at the local level. This defied the logic inherent in the homogenization thesis within globalization theory and provided a theoretical roadway to highlight local–global relationships and the significance of local ethnic economies in the
In retrospect, one issue I regret not addressing more fully in the beginning of the analysis phase is the different ways I might have treated and presented respondents’ own voices. Should their voices speak for themselves or should their voices be used to identify and establish patterns, patterns that are interpreted by the interviewer (me)? This goes back and is somewhat related to the original question I address in this section pertaining to what priority should be given when starting out analysis—to the theory or to the empirical material? But it is different because it more particularly addresses how and the extent to which the interview text, itself, is eligible for varying degrees of interpretation. Is it possible for the text to stand on its own without interpretation? Is it possible to overinterpret text? What about text that is left out? Is it even possible to be entirely accountable for the choices qualitative researchers make independent of recognizing and legitimizing the fact that specific choices are, in fact, an integral component of analysis and interpretation (Frosh and Emerson, 2005)?
Chapter 10: Immigrants in “American” Franchises: Research Challenges in New Lines of Inquiry

Works Cited


