

How Deborah Dash Moore's *At Home in America* Led Me to Study Small-town Jewish Life

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When I arrived in the United States from Poland in the late 1970s, I was basically unfamiliar with current American sociology and historiography. This included American Jewish studies, a field of particular interest to me. During the first decade of my stay in the United States, I read widely in all of these fields, especially in the area in which I wanted to pursue my own research: a historical sociology of immigration to the United States from the late-nineteenth century to the present, with special attention to East European settlers. Together with classics such as Irving Howe's *World of Our Fathers* (1976) and Moses Rischin's *The Promised City* (1962), Deborah Dash Moore's *At Home in America* provided me with a basic education in the history of East European Jews in New York City or, as I originally thought, America in general. Informative and engagingly written, *At Home in America* was a great pleasure to read, and I returned to it several times during the process of my American Jewish scholarly education to recheck specific details, the author's interpretations, and the sources upon which she relied.

At Home in America most tangibly influenced the location and agenda of my own research. The book concentrated on a large urban center with a large Jewish population, and with a good reason: By 1915, almost 50 percent of all Jews in the United States lived in New York City. Still, no less than one-quarter of East European arrivals made their homes in smaller towns—the type of location that attracted little attention from American Jewish historians. Even more important or challenging from my perspective was that Moore called the mode of Jews' economic and sociocultural adaptation in New York “the master pattern,” or “the grammar of American Jewish life”: a spectacular, collective climb up the mainstream educational and occupational ladder; a rapid “modernization” of the forms of social participation and religious life; and an active engagement with mainstream civic-political life on the part of the children of immigrants. Other socio-historical studies of New York Jews—for example, those written by Thomas Kessner (1977) and Suzanne Model (1988)—have similarly portrayed New York City as the basis for essential American Jewish patterns.

A comparatist by professional training and research practice, I was interested in testing this master pattern in a different configuration of socioeconomic, cultural, and political circumstances. I thought that setting a study of small-town Jews against a comparative framework of the experience of their New York City co-religionists, who had come to the United States from the same part of the world at the same time, but who had subsequently lived in a quite different environment, would be fascinating and sociologically elegant. I also hoped to rebalance American Jewish historical knowledge about the adaptation processes of immigrants and their children outside of New York City and other big cities by providing comparative information from a different setting. Limited in number, existing studies of Jews in smaller locations—e.g., Reznikoff and Engelman (1950) and Trachtenberg (1944), among the early ones, and, contemporaneous to the designing of my project, Sarna (1978), Toll (1982), Endleman (1984), and Smith (1985)—simply reported on their findings rather than treating them as a “tester” of the master pattern¹, so my project appeared exciting.

Because of its focus on the multitrack transformation of Jews' lives during the first decades of the twentieth century, *At Home in America* was, I thought, a perfect template for my study's research agenda. But first I had to find the appropriate location. I had in mind a compare-and-contrast type of investigation with my case study set against the existing comparative material—in this case, Moore's findings. So I looked for the place that most radically contrasted with New York City: small in size, isolated, dominated by heavy industry, with limited opportunities for other employment, non-union, overwhelmed by numbers and ruled by conservative-minded Anglo-Protestants who were unfriendly—or, at best, indifferent

(as long as they kept quiet)—toward foreigners. I consulted a number of American social historians for suggestions on such a place, and John Bodnar recommended Johnstown: a steel-producing town surrounded by a ring of coal-mining townlets in the hills of Western Pennsylvania, approximately 70 miles southeast of Pittsburgh. Until World War II, Johnstown was non-union under the enforced patronage of the Bethlehem Steel Corporation, which employed about 70 percent of the local working population throughout the first half of the twentieth century. It maintained an autocratic political order sustained by resolutely right-wing Republican politics, and its social system was marked by rigid stratification, with sharp ethnic cleavages between the established Anglo-Protestant elite and West European groups, on the one hand, and, on the other, new ethnic groups mostly of South and East European origin—"Hunkies," "Dagos," and "Hebrews," as they were referred to by members of the dominant groups. Jews in Johnstown numbered between 1,000 and 1,200 (about 1.2 percent of the general population) from the time when mass migration ceased in 1914 until the outbreak of World War II.

Johnstown appeared to possess just the characteristics against which I could test Moore's New York City "master pattern." It was also small enough for a conscientious historical sociologist with a lot of Sitzfleisch like myself, I believed, to be able to examine thoroughly the available sources. It took me twelve long years to complete my project: During the first 3½ years of my research, I practically lived in Johnstown, and, for the next four years, I visited it regularly for extensive periods to continue my fieldwork. Afterward, I continued to travel there occasionally to check an old record or to investigate a newly emerging issue, or simply to visit my friends in the local Jewish community. In 1996, I finally published my book on Johnstown, *Insecure Prosperity: Small-Town Jews in Industrial America, 1890–1940*.

Throughout my research, Deborah Dash Moore's *At Home in America* served as an agenda-setting guidebook. As I collected data on the occupational distribution and residential movements of Johnstown's Jews between 1890 and 1940, gathered information on Jewish community-building and functioning during that period, checked the town's voting and civic-participation records, and recorded the immigrants' and second generation's life-stories—followed by rounds of specific, issue-focused individual and group interviews—I returned again and again to Moore's book. I took and retook detailed notes from its chapters on a number of issues—the immigrants' search for a livelihood, the second generation's mass embrace of higher education, the transformation "from chevra to center" of Jewish communal institutions, the immigrants' children's involvement in the city's democratic politics, and those children's emergent collective identities. In each case, I translated these issues into specific research questions for my Johnstown project depending on its phase and particular focus, and then I moved into the field to examine archival data and talk to the local Jewish residents. I then compared, and compared again, my field notes with the figures and interpretations in Moore's book, making notes about the differences I discovered between New York and Johnstown with regard to the trajectories of Jewish economic, sociocultural, and civic-political adaptation. Throughout this long process of gathering data, Moore patiently answered (and answered again) my never-ending questions as they emerged from my ongoing investigations, and she tirelessly commented on my proposed interpretations of the findings.

My Johnstown study revealed that, because of the economic and sociopolitical conditions that had prevailed in the town and within the Jewish group throughout the interwar period, and in stark contrast to their New York City counterparts who went to college and then entered mainstream white-collar occupations, most of the small-town Jewish immigrants' children had joined their parents in shop-keeping. They served the needs of the large (gentile) East European worker population, and they did so without any apparent feelings of disappointment or frustrated ambitions about moving higher up in the world. They minimally modernized their communal institutions and Jewish religious practices, instituting only a few "Consorthodox" reforms (with the emphasis still on the latter—Orthodox). Their participation

in the town's civic-political affairs remained practically nonexistent throughout the interwar period, except for voting Republican "against their hearts" (as a number of them told me) so as not to stand out against the political profile of the dominant Anglo-Protestant elite. Rather than feeling at home in America, the Jews of Johnstown shared a sense of civic insecurity and a preference for remaining inconspicuous and keeping a low profile so as not to attract attention to themselves.

In short, my testing of the master pattern of Jewish adaptation in America, based on the New York case documented in Moore's *At Home in America*, demonstrated that there has been no such uniform pattern for this process. A wiser presupposition to inform studies in American Jewish history would be that of diversity rather than sameness. Still, inspired by Moore's book and its underlying argument or, more accurately, eager to test it further, I conducted a few more comparative studies (based on secondary sources) of the trajectories of Jews' adaptation in different localities. I sought to check whether Moore's master pattern applied to Jews residing before World War II in bigger cities, such as Boston, Cleveland, and San Francisco, and also in today's Philadelphia (Morawska 2001, 2004). I also wondered whether perhaps there existed in the prewar era a distinct small-town pattern of Jewish experience in places such as Charleston, S.C.; Greensboro, N.C.; Clinton, La.; and Johnstown, Pa. (Morawska 1994, 2001). Both of these exercises produced negative answers. Certainly, some specific similarities united the experiences of small-town Jews in contrast to big-city Jews, but the overall constellations of circumstances and their outcomes in terms of the general trajectories of Jewish adaptation were sufficiently different from each other in each case to justify recognition and separate treatment.

My comparative investigations revealed the main factors which in specific constellations differentiated Jewish experience in various locations. With respect to the surrounding society, these factors included the size of the city or town and the structure and dynamics of its economy, the degree of rigidity or fluidity of the social structure and civic-political climate (especially with regard to newcomers and outsiders), the degree of ethnic residential segregation, the relative level of competition and collaboration in ethnic relations, and the degree of social distance (separatism) of the dominant group(s) vis-à-vis the newcomers, and, specifically, Jews. With respect to the Jewish group, these circumstances included the size of the Jewish group and its proportion of the total population, the fit of the Jewish group's "collective human capital," particularly their educational and occupational skills, into the profile and dynamics of the local economy, the degree of parity or discrepancy between economic positions of the Jewish group and the dominant class and ethnic strata, the degree of the Jewish group's residential concentration and of the residential stability of its core, the scope of Jewish group members' participation in the local civic-political organizations and affairs, the scope of Jewish group members' participation in private social activities of the dominant group(s), the proportion of native-born American Jews in the group, the scope and hold of absorbing intra-group social-institutional networks and activities, and the degree of self-separatism of the Jewish group. These lists are by no means exhaustive, and should be treated as heuristic guideposts for researchers interested in pursuing comparative studies in American Jewish history.

Assuming that the story I have sketched out here of my investigations into American Jewish history that were directly or indirectly inspired by Moore's magisterial *At Home in America* has made a convincing argument for the advantages of the approach I have used in these studies, I would now like to identify different types and strategies of such comparative investigations. American Jewish historians have not, I believe, used a comparative approach to its full potential, so I offer here a brief overview of the advantages of such an approach, which, I hope, will be useful to researchers tempted to pursue comparative projects.

Comparative studies can be case- or variable-based. In his book *The Comparative Method* (1987), historical sociologist Charles Ragin provides an excellent guide to different strategies of this approach. They can aim at a high level of complexity by including as many

dimensions of the examined phenomena tested on as many groups in as many different locations as possible—the approach common in variable-based or quantitative analyses. Case-based investigations usually pursue the opposite strategy or they follow simple (or even deliberately simplified) setups: a comparison of similar actors in different settings, and a comparison of different actors in a similar setting.

As a historical ethnographer, I focused on case-based comparative analyses designed to pursue a comprehensive examination of cases treated as “wholes” (rather than, as in variable-based methods, collections of variables) and following just two simple setups as identified above. My Johnstown study, set against the research agenda and findings of Moore’s *At Home in America*, represents a comparison of similar actors in different settings. The other type—a comparison of different actors in a similar setting—has seldom been used by American Jewish historians, although the approach may bring important insights into the situation and practices of the group(s) that interest the researcher.²

Another useful study for researchers interested in comparative analysis, *Constructing Social Research* (1994) by Charles Ragin identifies four research goals whose realization is feasible through comparative, case-based studies. They include (1) testing and refining theories or specific claims/propositions; (2) interpreting the significance of an event, phenomenon, or process; (3) identifying historical (that is, time-bound and place-bound) patterns; and (4) exploring diversity. Each of these goals, alone or in combination with other goals, can inform both types of case-based comparisons: similar actors in different settings, and different actors in a similar setting. I already illustrated the testing of specific propositions in the example of my Johnstown project as compared to Moore’s New York study, and in the studies I conducted in different locations that explored diversity in other comparative investigations of American Jewish experience. In both cases, I used a research setup that compared similar actors in different settings. The second type of comparison, an interpreting-significance kind of comparative investigation, could focus, for example, on an examination of the importance of college education as a measure of life accomplishment and success among children of Jewish immigrants (of the same country/regional provenance who arrived in the United States in the same time period) in different locations, or among Jews and members of other comparable groups in the same place. The research goal of identifying historical patterns in the transition from Orthodox to Conservative Jewish religious practices and communal organization during the interwar era could be pursued through a comparison of the pursuits of Jewish communities in two different large cities or two small towns; or one could compare the mechanisms, forms, and contents of the transformation of socioreligious life of two (or more) immigrant groups, including Jews, in a particular location.

Finally, in reflecting on the wisdom generated by the investigations I conducted into the American Jewish history inspired by Moore’s book, I would like to note the unavoidable biases, blinders, and omissions in the historical (and any other) studies we produce, and to suggest ways to deal with these problems. As I was interviewing my Jewish Johnstownians about their lifestyles and aspirations, I kept asking them whether, in the interwar period, they had considered moving to New York, where the advancement opportunities were so much better and where life was so much more exciting and, if they never considered this—as most of them acknowledged—why not? My respondents did not seem to understand what I was talking about. Typical reactions to my inquiries were “... but it was our relatives from New York who envied us, not the other way round...” or, “how nicely you live here—my sister and her husband would say when they visited us in the summer—just look at us in our stone-set place (New York), and the noise and the rush...” It took me a good while to realize that I was imposing on past situations the understandings and evaluation criteria of the present: a quiet life in beautiful natural surroundings (by the late 1920s, the Johnstownians had moved from the immigrant quarters downtown to the suburbs on the woody hills) was at the time of greater value to my respondents than the opportunities for socioeconomic advancement in a big city.

An even more glaring bias of my Johnstown investigation emerged after I had already drafted the book manuscript. I had an agreement with the local Jewish community that, before the book went off to print, they would have the opportunity to review it. So members of the board did just that. Their reaction was that, yes, it was a nice story, but “we have quarrelled so much, there was so much [intra-group] fighting, and it is hardly there...” This was no bagatelle of a criticism, considering that the task of ethnographic research is to render the world as closely as it is or was experienced by the actors themselves—the subjects of the study. It was only then—after more than a decade of intensive research—that I realized that because I had approached this small town with a preset agenda taken from Moore’s study of New Yorkers and I had sought to test it in my fieldwork, I had not paid sufficient attention to an important aspect of the local Jewish life I had tried to reconstruct.

And one more example of the oversights resulting from the research agendas that guide our investigations. What both Moore and I had failed to do in our studies was to identify different subgroups within the same localities and to examine them vis-à-vis each other. Moore employed the “aggregate” New York Jewish experience as the analytic unit. My examination of Johnstownners would have been markedly different had I been comparing them to the “urban village” or to small-business segments of the New York Jewish population rather than to New York Jews as a whole. My compare-and-contrast research strategy had made me primarily interested in the differences between the two cases, and for this purpose, the aggregate comparisons made good sense. Similarly, had Moore in her analysis of the New York scene in the interwar period recognized the still enduring influence of the immigrants rather than focusing exclusively on the native-born American generation, the portrayal presented in her book would most likely have been different.

Should we, therefore, conclude that the biases of these studies render them useless? I do not believe so. I do not know how Deborah Dash Moore regards her *At Home in America* today. For me, twenty years after the publication of *Insecure Prosperity*, I still believe that the book is one of the best things I have written in my scholarly career. It is certainly not perfect, and it contains a number of gaps and loopholes, but I applied to it the best of my professional skills as a historical ethnographer, and I did not leave unturned one stone that seemed relevant to what I hoped to learn. Without adopting the radical relativist position of Jacques Derrida’s “everything is a text” genre, we can recognize that our representations of the world we study unavoidably reconstitute it, that is, they transform rather than reproduce it in a mirror-like image. Researchers who identify with this position acknowledge, therefore, the impossibility of “true” or purely objective representations of the world they study, yet they are committed to the canons of disciplined research. Their acknowledgement of the transformative function of research practices does not automatically invalidate the accounts produced by these practices. It means, rather, that the degree of verisimilitude, or approximation, of the researcher’s accounts to the experience and situations examined should be evaluated on a case-by-case basis, through the critical assessment of the problem agendas, sources, and research methods used in the study on the one hand, and, on the other, in a self-reflexive account of the investigator’s unintended interventions into his or her project in its different phases.³

In an essay entitled “A Historical Ethnography in the Making: A (Self-)Reflexive Account,” (1997) I self-critically accounted for the biases and omissions in my Johnstown study and for their implications for its findings. I still believe—and I convey this conviction to my students in seminars on the epistemological traditions in the social sciences, ethnographic research methods, and the critical assessment of documents used as sources of information in historical-sociological research—that admitting the limitations of one’s research strengthens rather than weakens it. By raising these issues here, I by no means imply that Moore was not aware of them or that she does not share at least some elements of the position outlined above. One could perhaps argue that her study of Jews in Miami and Los Angeles (1994)

has been a test of sorts of the New York master pattern from her classic *At Home in America*, and, then, one could hope that she will continue such investigations in the future.

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Footnotes

1. Perlmann's (1987) report on the occupations of Russian Jews in small Jewish communities at the beginning of the twentieth century, entitled "Beyond New York," has been the exception.
2. The social-history paradigm-informed studies of Kessner (1977), Model (1988), and Smith (1985), which compare Jews' occupational pursuits and family strategies with those of other immigrant groups in selected locations, do not pertain to the field of American Jewish history *sensu stricto*, that is, they do not account for Jewish practices in the context of their broader sociocultural and deeper-seated personal, ethno-religious commitments.
3. Recognition of the "crafted" character of historical knowledge acknowledges as well the need for self-reflexivity, that is, the critical examination of and accounting for the researcher's own impositions on the course and outcome of the investigation. Such critical self-reflection should involve all stages of the study, including research design, data-gathering, the interpretation of findings, and the writing up of the story. Good discussions of researcher's various entanglements in the project, or the issues to pay attention to in the process of self-reflexivity, can be found in Atkinson (1990), Hammersley (1990), Norman (1991), Swidler and Ardit (1994), and Van Mannen (1995).