# Numbering the Jews: Evaluating and Improving Surveys of American Jews

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by

Benjamin Phillips

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ABSTRACT

Numbering the Jews: Evaluating and Improving Surveys of American Jews

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American Jews have long understood the vitality of the Jewish community by its numbers: initially its population size and later its characteristics. Acquiring such data, though, has always been complicated by the exclusion of questions on religion from the U.S. Census of Population and Households. In the absence of official counts of the Jewish population, American Jews have sought other ways of counting themselves. The methods used to this end have, however, varied considerably across the decades, as have the quality of the methods used. Initial attempts to count the Jewish population relied on institutional censuses of Jewish congregations supplemented, and ultimately supplanted, by guesstimates of dubious quality from local reporters. From the beginning of the twentieth century, these efforts were joined by proxy methods that relied on readily ascertainable statistics with some type of relationship with Jewish identity, which also failed to generate accurate estimates. More accurate counts were finally arrived initially in censuses based primarily on organizational lists and later via various forms of geographically based samples. List-based methods were no longer sufficient as the number of Jews not

found on lists grew due to assimilation. Costs for these methods, though, were too great and the 1980s saw a move to random digit dialing methods. Costs, however, continued to increase, driving studies to become more economically efficient. The late 1990s saw a decline in response rates to surveys of all types, further raising costs and increasing methodological concerns. The long-awaited National Jewish Population Survey of 2000-01 was the culmination of these trends, with a vast cost of $6 million and multiple methodological flaws. I examine a variety of responses to the challenges of the present survey research environment. Indirect screening questions appear to be less effective than a direct approach. Use of expanded list frames drawn from the 10 or so largest Jewish organizations in a community in combination with random digit dialing appears to be the most efficient sampling scheme. I conclude that cost-variance optimization for estimating Jewish incidence leads to dramatic increases in accuracy compared to nonoptimized distribution of the sample across frames and strata.

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# Chapter 1

## The Population Wars

The release of the National Jewish Population Survey (NJPS) 2000-01 was to have been a landmark achievement of Jewish social science and its sponsoring agency, the United Jewish Communities. Its predecessor, NJPS 1990, was best known for its estimate of a 52 percent rate of intermarriage between Jews and non-Jews in the years 1985-90 (Kosmin et al. 1991a:14-15),[1] far higher than many American Jews had suspected and a catalyst for renewed efforts at promoting "Jewish continuity" (Herring 2000).[2] The new study was eagerly awaited in order to determine whether the intermarriage rate had plateaued or was still climbing. Major sponsors expected data on a variety of issues, including the impact of childhood Jewish education on adult behavior and philanthropy.[3] NJPS 2000-01 was also of great importance to social scientists interested in the Jewish community, as NJPS 1990 data formed the basis of the overwhelming majority of quantitative analyses of American Jewish life in the 1990s and early 2000s, which were becoming increasingly out of date.

In the face of these manifold expectations, NJPS 2000-01 was repeatedly delayed. Telephone interviewing was originally scheduled to begin in January 2000 (Gootman 1999b), but was delayed in order to allow the new leadership of United Jewish Communities to give "full consideration" to the study (Goldman 1999b; Gootman 1999c; Hyman 1999). Full consideration having been given, the survey was fielded in mid-May 2000, with interviewing scheduled to be completed by November and the results released in mid-2001 (Wiener 2000a). By the end of the year, this relatively optimistic estimate was superseded, with higher than expected refusal rates cited as a problem (Wiener 2000b; Wiener et al. 2001). The situation continued to deteriorate in 2001, with the revelation that more interviews were needed to achieve the desired sample size. This failure was due to a massive overestimate of the proportion of households in the United States with Jewish members by Jim Schwartz, then research director of United Jewish Communities, and Jeff Scheckner, a research consultant, as well as an apparent overestimate of the likely response rate by the RoperASW, the survey research contractor (Goldman 2001b). By the end of 2001, a firm release date was no closer, with only a vague commitment to release the results "some time in 2002" (Goldman 2001a). In tandem with the lengthening timeline, the cost of NJPS 2000-01 grew dramatically, beginning at $2.5 million to $3.5 million (Stoll 1997), touching $3.6 million in April 1998 (Cohen 1998), increasing to $4 million by May 1999 (Goldman 1999a; Gootman 1999a; Greenberg 1999), $5 million by August 2000, before leveling out at a reported $6 million (Berkofsky 2002d; Goodstein 2003). The last million dollars was in large part a product of a poorly structured contract that left United Jewish Communities liable for the expenses involved in correcting RoperASW's mistakes (Goldman 2001b).

Personal acrimony, played out in the Jewish press, was a constant accompaniment to the study. Steven M. Cohen—a prolific and peripatetic sociologist of American Jewry—complained that he, Jack Ukeles, Gary Tobin, Calvin Goldscheider, and Bethamie Horowitz were all excluded from the National Technical Advisory Committee (Berman 1998b). The advisory committee was chaired by Frank Mott, a sociologist of children and youth with a background in large-scale longitudinal surveys, and the late Vivian Klaff, a sociologist and demographer at the University of Delaware. Cohen was later added to a subcommittee (Berman 1998a) and was subsequently hired as a consultant (Wiener 2000b), though the other researchers were left outside the process and remained critical. Goldscheider, then professor of sociology and Judaic studies at Brown University, characterized the attitude regarding the study as "No matter how badly we did the first one, we want to make sure we do the same thing next time" (Goldberg 1998) and complained that he was treated in a cavalier fashion (Gootman 1999a). Barry Kosmin, chair of the technical advisory for NJPS 1990 and then director of the Institute for Jewish Policy Research in the United Kingdom, implicated Ukeles, Tobin, Cohen, and Goldscheider in "outflanking" the National Technical Advisory Committee and attempting to doctor the intermarriage rate, claiming that they had "wanted to control the 1990 study, and we saw them off then" (Anonymous 2000).

Complaints about the instrument were also commonplace. Ukeles, Goldscheider, Tobin, and Cohen were among the signatories to a letter to United Jewish Communities, objecting to the inclusion of the number of questions focused on demographic issues at the expense of items that could inform communal planning (Gootman 1999d). Jonathan Woocher, executive vice president of the Jewish Education Service of North America,

expressed concern about the lack of questions regarding adult Jewish development and "spiritual dimensions of Jewish life" (Gootman 1999b). Bethamie Horowitz, a social psychologist, former research director of UJA-Federation of New York, and author of a seminal study on Jewish identity (Horowitz 2000), pressed for questions dealing with respondents' subjective experiences of being Jewish (Gootman 1999c). William Rapfogel, executive director of the Metropolitan New York Coordinating Council on Jewish Poverty, asked for more questions on social services (Gootman 1999a; 1999c). Abraham Foxman, national director of the Anti-Defamation League, felt the number of questions on antisemitism was insufficient (Gootman 1999c). Samuel Heilman, a sociologist at the City University of New York and scholar of Orthodoxy, was concerned that the survey distinguish between "fervently Orthodox" and "non-Haredi elements" (Gootman 1999a). Rela Mintz Geffen, then a sociologist at Gratz College, feared that intermarriage-related issues would be focused on at the expense of understanding the "mainstream" community (Gootman 1999a).

The initial results were finally released in October 2002, the highlight being the study's estimate of 5.2 million Jews, down from 5.5 million in 1990. The release, though, was quickly followed by a firestorm of articles about the survey's methodology in the Jewish press. The *Forward* editorialized against the findings, pointing to putative undercounts of the Russian- and Israeli-born population, the large number of assumptions behind the weighting scheme, and the development of a population estimate via alleged negotiation between scholars' favored totals (Anonymous 2002). Barry Kosmin (2002) charged that the study's findings were politically convenient, fueled by methodological choices that excluded marginalized Jews. Others, among them Calvin Goldscheider and

Sergio DellaPergola, chairman of the Institute for Contemporary Jewry at the Hebrew University of Jerusalem and a demographer of world Jewry, argued that methodological flaws should not distract attention from its underlying utility for understanding the American Jewish population (DellaPergola 2002; Goldscheider 2002). James Tisch (2002), chair of the United Jewish Communities board, disparaged opponents of the "honest and accurate snapshot" of the American Jewish community.

The initial results were, however, withdrawn just over a month later when Stephen Hoffman, president of United Jewish Communities, became aware that RoperASW had lost data from two-thirds of screening interviews where eligible households refused to answer the main questionnaire (Berkofsky 2002c; Cohen 2002b; Kadushin, Phillips, and Saxe 2005; Radler 2002). In response to these revelations, Hoffman appointed a task force to investigate what had happened, led by Bernard Shapiro, chancellor of McGill University (Anonymous 2002; Cohen 2002b). A very public spat ensued in which Hoffman disclosed that Jim Schwartz and Lorraine Blass, the NJPS project director, had been notified of the problems in May but had not informed him, and declared he had "lost total confidence" in Frank Mott and Vivian Klaff, chairs of the National Technical Advisory Committee, who "had an excuse that says, 'it's not significant' on a number of occasions" and kept "explaining things away" (Berkofsky 2002b).[4] Mott, in turn, described Hoffman as "misinformed" and the problems amounting to little more than a "molehill" (Berkofsky 2002b), while Klaff declared that "the UJC [United Jewish Communities] leadership is in chaos" (Radler 2002). Another member of the National Technical Advisory Committee, the late Egon Mayer, chairman of the Brooklyn College sociology department, director of the North American Jewish Data Bank, and founding director of the

Jewish Outreach Institute, ascribed political motives to the decision, noting that "it may be there is a kind of inherent tension between the independence of social scientists and an organization that wants to use their work and at the same time control it" (Heilman 2003).

The lost screener data was not, however, the only problematic element of the study. The task force found "a range of serious issues of conception, of data collection, and of analysis," with a United Jewish Communities spokeswoman projecting an April 2003 release of data (Berkofsky 2003b). RoperASW had misprogrammed the skip patterns in its computer aided telephone interviewing system, which led to the failure to ask certain questions of eligible respondents (United Jewish Communities 2003h). In addition, the study used an unusual definition of Jewish identity that Jim Schwartz had insisted on over the objections of the National Technical Advisory Committee, which had the effect of systematically excluding relevant respondents from certain questions when combined with the reduced set of questions asked of "Persons of Jewish Background," severely curtailing analysis of many important issues (Kadushin et al. 2005:14-15).

In April, however, the release was delayed again, as UJC appointed Mark Schulman, president of the survey research firm Schulman, Ronca & Bucuvalas, Inc. and then president of the American Association for Public Opinion Research, to chair an independent review of the study's estimates (Berkofsky 2003c). This move drew criticism from members of the National Technical Advisory Committee, including Egon Mayer, who claimed that the review panel injected "a note of unnecessary concern" and would "undermine the credibility" of the committee and the data (Berkofsky 2003c; Cattan 2003b).

If the starts, stops, and reverses of NJPS 2000-01 were not confusing enough, two other national studies of American Jews were released during this period. The first of the

other national studies was the American Jewish Identity Survey, sponsored by Felix Posen, founder of the Center for Cultural Judaism and passionate advocate of secular Judaism, and conducted by Egon Mayer, Barry Kosmin, and Ariela Keysar (Mayer, Kosmin, and Keysar 2002), key researchers in NJPS 1990. The study was in part a reaction to the National Technical Advisory Committee's decision to use sampling methods and criteria for interviewing that differed from those used in NJPS 1990, and to change the wording of many questions. While the differences were often improvements, the changes also complicated the analysis of trends. By contrast, the American Jewish Identity Survey replicated many of the elements of NJPS 1990. In comparison with either NJPS 1990 or 2000-01, though, the study asked a much more limited series of questions and focused on how American Jews "define themselves with respect to a continuum of beliefs ranging from religious to secular" (Posen 2002:50), it was primarily this question that the reports (Mayer, Kosmin, and Keysar 2003; Mayer et al. 2002) addressed. The American Jewish Identity Survey did not, however, differ significantly from the NJPS estimate of 5.2 million American Jews, estimating the Jewish population at 5.3 million.

Yet another estimate of the U.S. Jewish population became public just before NJPS was first released in 2002. Gary Tobin, an urban sociologist and director of the recently formed Institute for Jewish & Community Research, claimed the Jewish population was 6.7 million in his Survey of Heritage and Religious Identification, plus 6.6 million with some other tie to Jewish identity (Berkofsky 2002a; Cohen 2002c; Schleifer 2002; Tobin 2002). Tobin's conclusions were, not surprisingly, challenged. Ira Sheskin, a human geographer at the University of Miami and prolific surveyor of Jewish communities, noted that "there are so many people today in gray areas that, depending on how you

define who is a Jew, you will get different answers" and claimed that Tobin had "stretched the definition of being Jewish more than most might" (Schleifer 2002). The estimate was, however, revised to 6.002 million when the study report was released in 2003, with a note stating that "the original estimate of 6.7 million has been revised downward" (Tobin and Groeneman 2003:1), but no further explanation.

Debate over the utility of NJPS resumed with the final release of the data in August 2003, with heated rhetoric flying between supporters and opponents. Gary Tobin declared NJPS worthless, declaiming that "you cannot make chicken salad out of chicken s—t" (Cattan 2003c). An anonymous critic described the survey as "an unmitigated disaster," having been "poorly designed, implemented and evaluated" (Rosenblatt 2003). J.J. Goldberg (2003), editor of the *Forward*, charged United Jewish Communities (UJC) in the *New York Times* with knowingly misrepresenting the data due to a combination of "ideology, ego, [and] incompetence." On the other side, Hebrew University's Sergio DellaPergola (2003) was particularly outspoken in his defense of NJPS, criticizing an unnamed opponent "who, facing a disconcerting discrepancy between his own theory and the data from the field…blamed the data," while others "were pollsters who might have liked a hefty share of the several million dollars invested by UJC."

A related—and equally heated—shouting match took place between J.J. Goldberg and members of the National Technical Advisory Committee regarding the intermarriage rate, which was calculated on a different basis to that of NJPS 1990. Goldberg accused NJPS 1990 authors of having "fraudulently reported" a "sexed up" intermarriage rate of 52 percent (Goldberg 2003), while Stephen Hoffman of United Jewish Community described the claims as slanderous (Cattan 2003d).[5]

On the whole, the study's supporters took the position that its estimates of population size or characteristics were valid, while critics advocated its utility for understanding elements of Jewish identity, but not providing accurate population estimates. Steven Cohen broadly defended the study, asserting that its results were "quite credible, believable and sound," and that while certain data would be "a little shakier," they were close enough for policy purposes (Berkofsky 2003e), though he elsewhere said his "gut feeling" was that the study undercounted the Jewish population by 300,000 to 500,000 (Berkofsky 2003d). Lorraine Blass of United Jewish Communities found the study to be reliable and comprehensive, as "in all cases we examined, it checked out favorably" (Cattan 2003c), as did her superior, Stephen Hoffman, who declared that the study had "full value" (Berkofsky 2003e). On the other side, Leonard Saxe, a social psychologist and director of the Cohen Center for Modern Jewish Studies at Brandeis University, and Charles Kadushin, emeritus professor of sociology at the City University of New York and distinguished scholar at the Cohen Center, argued that while accepting the population estimates required "a leap of faith," they were nevertheless a rich source of data on the impact of background on beliefs and behaviors (Saxe and Kadushin 2003). Kadushin observed:

> Every survey has its share of "kick me" moments. Does this survey have more than its share of kick-me's? Yes, probably. Is it a total loss? No. Lay people want to know numbers first. Analysis is secondary. But these kinds of studies are best at analysis, so in a sense the best is yet to come. (Cattan 2003d)

This assessment proved prescient. Although the population estimates have remained controversial, interesting and substantive analyses of NJPS data have been produced.

While the national surveys took up the majority of ink, studies of individual communities also suffered from delay and controversy. The 2002 Jewish Community Study of New York, conducted by Ukeles & Associates, a research firm that has conducted many studies of large Jewish communities, was slated to be released in December of that year (Ain 2002). It took until mid-2003, however, for the summary report—a collection of PowerPoint slides—to appear. (The final report was released in October 2004.) Three members of the New York study's advisory panel resigned over methodological concerns: Charles Kadushin of Brandeis University, Bethamie Horowitz, director of the previous study of New York, and Andrew Beveridge, a sociologist and demographer of New York City at Queens College (Berkofsky 2003a; Cohen 2003). The study also found unexpectedly high numbers of Jews in poverty and surprising growth in the Orthodox community (Berkofsky 2003a; Cohen 2003; Treiman 2003a; 2003b). Similar delays afflicted the Ukeles & Associates study of the Jewish community of Greater Phoenix, which also faced difficulties in completing sufficient interviews (Cohen 2002a).

For the public—and most communal professionals, lay leaders, and funders—the spectacle of the survey wars of the 2000s must have been deeply unsettling. Results were delayed, released, and then withdrawn with limited explanation. Different surveys came to very different answers about even so basic a figure as the number of American Jews. Anomalous findings were common. Experts disagreed vocally about the accuracy and value of the findings, and even the motives of researchers and sponsors. The dueling op-eds echoed the controversy surrounding the 52 percent current intermarriage rate reported in NJPS 1990, only reinforcing the notion that all Jewish social science is contingent and

problematic. In the public eye, NJPS 2000-01 is Jewish social science, and its tarnished reputation rubs off on the entire enterprise.

The implications of distrust of Jewish social science are troubling. The potential for social scientific research to impact policy depends on its findings being seen as unbiased, accurate, and relevant. The first half of the 2000s instead has seen Jewish social science portrayed as driven by external agendas, contingent, and of dubious accuracy. We should not be surprised if its findings play less of a role in the development of communal policy. The effect on funders is also likely to be considerable. United Jewish Communities drew deeply on its reservoirs of goodwill to raise the six million dollars that NJPS 2000-01 cost. What the sponsors got was an ongoing source of controversy, which may well influence support for future research. And all this in an environment where giving to Jewish Federations—which have historically been major sponsors of Jewish social science—had been declining in favor of contributions to single-issue Jewish organizations (Tobin 1992; Wertheimer 1997).

As worrisome as the public perception of Jewish social science may be, the main issue is whether studies of Jewish populations provide accurate data. This is vital because Jewish self-understanding is to a surprisingly large degree quantitative. Witness the perennial interest in the size of Jewish communities and whether they are growing or shrinking, or the sense of crisis that accompanied the report of a 52 percent current intermarriage rate. Incorrect data are not purely academic, but lead to inaccurate self-image. As the Jewish community's understanding of itself drives hundreds of millions of dollars of giving and spending every year and countless policy decisions, many of which pro-

foundly affect people's lives, seemingly small methodological questions can have large effects in the world outside the academy.

Beyond policy, however, the quality of survey data has profound implications for contemporary Jewish studies. Understanding of contemporary American Jewry is dependent on empirical data, which are produced by three methodologies: qualitative studies based on ethnography and interviews, content analysis, and quantitative methods. While there is some overlap in the information provided by each approach, they are largely complimentary and nonsubstitutable. Qualitative data is useful for understanding meaning, texture, and process. Content analysis, which codes texts like speeches or novels for key words or concepts, can illuminate patterns of thinking and the construction of social problems. Quantitative research addresses questions of incidence and causality, and can provide data on trends. Survey research is the best known source of quantitative data on the Jewish community, but researchers have at times managed to use other sources such as immigration statistics. Of the three, quantitative methods are the most commonly used.[6] Invalid quantitative data would thus have a tremendous impact on the field, potentially leading to false conclusions on virtually any topic. The veridicality of surveys of Jewish populations is, therefore, the key question in the study of contemporary Jewry, as so much else is dependent upon it.

In addition to the concerns of the Jewish community and contemporary Jewish studies, the problems experienced by Jewish surveys are part of broader trends in survey research. Response rates have dropped precipitously since the late 1990s, increasing numbers of households have fallen outside of standard telephone sampling frames, and concerns about the representativeness of sample surveys have grown. Together with these

trends, interest in nonresponse has grown considerably in the form of the total survey error school of thought, which emphasizes the detection and correction of bias over the narrower focus on response rates (American Statistical Association 2006; Biemer et al. 1991; Biemer and Lyberg 2003; Groves 1989; Groves and Couper 1998; Groves et al. 2004a; Lyberg et al. 1997; National Institute of Statistical Sciences 2006; Weisberg 2005). These problems are particularly severe for studies of rare populations, like American Jews.

American Jews, however, pose special challenges for the study of survey quality. For many populations of interest to survey researchers, inaccurate survey results can be identified and corrected by reference to nonsurvey sources of data such as the U.S. Census and administrative records, or "gold standard" surveys such as the Current Population Survey of the Bureau of Labor Statistics and the American Community Survey of the Census Bureau. The U.S. Census, however, has a tradition of not including items on religion—in large measure a product of Jewish activism (Good 1959).[7] This was codified in 1976 by Public Law 94-521, which forbade mandatory questions on religious affiliation (U.S. Census Bureau 2005). While no legal bar exists, many government sample surveys do not ask about religious affiliation, including the American Community Survey and the Current Population Survey.[8] Items on religion are most commonly found on government-sponsored health surveys, but have too few cases per survey to provide reliable estimates of the size of the American Jewish population, let alone the population in a given locality, though meta-analysis may provide a solution to the national problem (Kadushin et al. 2005).

*A Reflexive Postscript*

Before I turn to the analytic chapters, it is important to outline my stance *vis-à-vis* the subject matter of this dissertation. Qualitative sociology emphasizes the role played by the researcher's viewpoint in efforts to gather and interpret data. While the methodology employed in this study is not qualitative, and is predicated on the notion that objective knowledge is desirable and at least partly attainable, the author's stance plays a significant role, particularly in certain areas. Evaluation, for instance, compares observed conditions to an ideal state, and the observer's conception of the ideal state will determine the result of the evaluation. Historiography, for another, is particularly sensitive to the author's selection of data and interpretive framework. While I have made every attempt to be objective and to clearly delineate the criteria used for evaluations, I have been a part of the environment I seek to describe and evaluate, and a brief summary of my involvement allows readers to understand any biases I have unconsciously introduced.

I was directly involved with NJPS 2000-01 before its second release. As a graduate research assistant at the Cohen Center for Modern Jewish Studies at Brandeis University, I assisted Leonard Saxe and Charles Kadushin in reviewing technical documentation and conducting analyses of a preliminary version of the dataset. In mid-2003, the Mandell L. Berman Institute—North American Jewish Data Bank moved from the City University of New York to the Cohen Center, and I administered it on a day-to-day basis as a research fellow, again under Kadushin and Saxe. My duties included supporting academic use of the NJPS 2000-01 dataset, as well as other studies of Jewish communities housed at the data bank, which involved considerable methodological analysis. Following the transfer of the data bank to the University of Connecticut in late 2004, I prepared

methodological analyses and co-wrote a paper on the proper use of NJPS 2000-01 (Kadushin et al. 2005) as a research associate at the Cohen Center and, more recently, the Steinhardt Social Research Institute. The design and implementation of the 2005 Boston Jewish Community Study, the centerpiece of this dissertation, was profoundly influenced by these analyses. Saxe and Kadushin have been generally counted among critics of NJPS 2000-01, a position I share with my mentors. This criticism was constructive in intent and founded on social scientific norms, which are discussed below.

The presentation and analysis of the Boston study is necessarily an insider account, as I managed the project from its inception under Leonard Saxe as principal investigator and Charles Kadushin as senior methodologist, and participated in every stage of design and implementation. While insiders are predisposed to see their projects as successes, a number of factors have, I hope, mitigated this tendency. This dissertation does not address whether the study as a whole was a "success" or "failure" (subjective as these concepts are). Instead, the study is analyzed as a series of tests of ways to survey American Jewish communities. The criteria used for evaluation are presented as clearly as possible, together with the data used to reach conclusions, permitting falsification (Popper 1959). Analyses focus on relative efficacy rather than binary conceptions of acceptable and unacceptable results, drawing on comparative data and making use of simulation to analyze counterfactual scenarios. While I cannot step outside my viewpoint, ample opportunity exists for the reader to make an informed evaluation of my conclusions.

ENDNOTES

[1] This estimate was challenged by Cohen (1994), who claimed an intermarriage rate of 43 percent based on reweighted data. As the details of the reweighting apparently were never made public, it is difficult to evaluate this estimate.

[2] As Phillips and Fishman (forthcoming) point out, however, a 52 percent intermarriage rate is exceptionally low in comparison to other white ethnic groups, particularly given the low odds of meeting a Jewish partner by chance.

[3] The question of whether sponsors bought the right to ask questions remains unclear. Increasing levels of donations apparently bought greater levels of interaction. Diamond Trustees ($250,000 and up) were able to meet with the research director to review the survey instrument and make specific suggestions, while Gold Trustees ($100,000 plus) had the right to comment on the instrument as it was being prepared (Berman 1998c). On the one hand, Yossi Praeger, the director of the AVI CHAI Foundation, which contributed to study, complained about the lack of "personalized services" and felt the Council of Jewish Federations "made it clear from the beginning that they're not selling questions" (Berman 1998c). The reported comments of at least one sponsor, Richard Wexler of the United Jewish Appeal, suggested otherwise. Wexler reported conditioning the organization's $75,000 donation on the presence of "certain critical philanthropic-related questions" (Gootman 1999a), which suggests that at least some sponsors felt that donations bought questions. The view from the other side of the table was similar. Frank Mott, a co-chair of the National Technical Advisory Committee for NJPS 2000-01 described considerable pressure from sponsors: "Pressures come in. Groups that contribute certain amounts of money feel that they have a right to have certain questions answered" (Stoll 1997).

[4] Jim Schwartz was removed as research director of United Jewish Communities in January 2003 and laid off in October (Cattan 2003a). Lorraine Blass was dismissed in mid-2005, with a number of other senior United Jewish Communities staff (Pomerance 2005).

[5] The 1990 estimate of the intermarriage rate was based on all persons raised as Jews while that of 2000 was based on all persons currently Jewish. Both are well-established ways of defining religious exogamy (Kalmijn 1998; Phillips and Fishman forthcoming). Goldberg's allegations of fraud were simply wrong; see Sheskin's (2003c) well reasoned reply.

[6] I analyzed articles in peer-reviewed journals between 2000 and 2005 listed in a *SocINDEX* (EBSCO Publishing 2006) search of articles containing the words "Jews" AND ("United States" OR "America") in the subject field. The time-frame chosen should, if anything, be biased against quantitative analyses, as NJPS 1990 data was at least a decade old, while NJPS 2000-01 data only became available in August 2003. The search yielded 91 items, 4 percent of which were classified as using qualitative methods, 8 percent as content analyses, 30 percent as quantitative, 3 percent as using a mix of quantitative and qualitative methods, and 41 percent as using historical methods, while 11 percent were nonempirical, 1 percent were literary analyses, and 2 percent were book reviews. Of the 41 classified as using social scientific methods (qualitative, quantitative, or content analysis), 10 percent were qualitative, 17 percent used content analysis, 66 percent were quantitative, and 7 percent used mixed methods. Within articles that exclusively used quantitative methods, 37 percent used NJPS alone, 22 percent used NJPS and another dataset, and 41 percent used a dataset other than NJPS.

[7] The U.S. Census Bureau did, however, conduct the Census of Religious Bodies between 1850 and 1936; see Stark (1992) and Anderson (1998) for further details. I discuss the topic at greater length at pp. 39-49.

[8] The March 1957 Current Population Survey included a question on religion as a pretest of the abortive attempt to introduce a question on religious affiliation into the 1960 census (Good 1959).

## Chapter 2

## Jewish Population Studies in the United States

Shimon Rawidowicz (1998) famously declared that Jews are an "ever dying people." By this he meant that each generation of the Jewish people has seen itself as the final link in the chain of tradition. Rather than sounding a death knell for Jewish existence, Rawidowicz explained, fear of imminent demise made possible the continued existence of Jewry. The keenly felt fear that Jewish life would disappear had the paradoxical effect of calling forth extraordinary efforts to avert this fate. While hardly as momentous as the continuation of Jewish peoplehood, similar pessimism has pervaded students of American Jewish statistics. The first Jewish-sponsored census of American Jewish institutions began by noting the lack of available data:

> For many years past, it has been desirable to collate statistical information of the Israelites in the United States, not alone for an exhibit of mutual prosperity or status as a religious community, but...[to] enable them to act in greater concert in all that concerns them as children of the Abrahamic faith. (Union of American Hebrew Congregations 1880:3)

Twenty seven years later, the compiler of another compendium of Jewish statistics felt it necessary to caution her readers to take the results with "the proverbial grain of salt" due to "wide, even extreme" variations among population estimates (Szold 1907:432). These and other problems moved yet another author to denounce the state of the field in 1943:

> A great many persons believe that the extent of the Jewish population of the United States is reliably known. Some even labor under the illusion that the figures have been determined by a federal census. This false impression is strengthened by the prevailing practice of citing precise totals rather than round numbers regarding the Jewish population of the country or of a particular city. Very few of those who see or hear such estimates stop to consider the nature of these calculations or their degree of accuracy. (Robison 1943d:1)

Her view of the accuracy of such calculations was predictably bleak. The various methods in use were, respectively, "utterly untrustworthy" (p. 2), "[begged] the question" (p. 2), and "valid at best for only a short period" (p. 4). The dawn of a new decade brought no surcease to such negativity. The most comprehensive review of American Jewish statistics to date began with the bold admission:

> Comprising the largest Jewish national grouping in the world, American Jews are as yet unable to ascertain with any degree of precision how many persons make up that grouping, where they live, how old they are, where they came from, and how they earn their livelihoods. (Seligman 1950:3)

Two decades later, a distinguished demographer, writing in the same periodical, characterized the social scientific data in the intervening period in strikingly similar (albeit a little more delicate) terms:

> …there is no single authoritative source of information on the demography of American Jews. Under the circumstances, a variety of sources must be used, each varying in the comprehensiveness, representativeness, and quality of its data. (Goldstein 1971)

Another two decades passed, and the demographer found some improvement, but little cause for celebration:

> While [community surveys] have greatly added to our knowledge, that knowledge remains incomplete. The surveys have varied considerably in scope and quality: They have relied on different questionnaires, varying sampling designs and coverage of the Jewish population… (Goldstein 1992:80)

Leaping ahead 11 more years, the sitting president of the American Association for Public Opinion Research offered a mixed assessment of the problematic National Jewish Population Survey (NJPS) 2000-01, which had been intended as a landmark study of the state of American Jewry:

> The NJPS 2000-01 will provide researchers with a broad profile of the Jewish population of the United States. The questionnaire and study design reflect the complexity of this undertaking and raise many research issues and questions which cannot be fully resolved at this time. (Schulman 2003:16)

Like the perpetual fear of dissolution in Rawidowicz's account of Jewish survival, the striking negativity of these assessments is a sign of growth rather than decay. The commentators were practitioners who were intent on improving the field and contributed to its development.

While the initial picture of the methodologies used to study American Jewry is one of startling diversity of research designs, closer examination shows a pattern of punctuated equilibrium (Eldredge and Gould 1972), where strategies that are stable for an extended period break down, followed by the rapid acceptance of a new methodology. The cause of the evolution of research methods is the social and technological environment they inhabit. What worked at a given point in time may not work when the conditions change. And like the Cretaceous-Tertiary and Permian-Triassic boundary events—and the mass extinctions and rapid evolutionary change that accompanied them—change in the research environment can be extremely rapid.

Research methods have no Platonic ideals. Instead, research methods exist in a dialogical relationship with their context: a given method has a degree of fit or adaptation with the social and technological environment of the population to be studied. Methodological change in survey research follows what might be called a "fit-misfit cycle." Methodologies are initially developed to "fit" a given social and technological environment. However, changes in the research environment lead to a period of declining levels of fit. At first, these problems silently accrete unnoticed by researchers. Eventually, though, the level of "misfit" reaches the point that results diverge grossly from expectations. This process is analogous to Kuhn's (1962) conception of the emergence of anomalous observations not explained by existing theories prior to the emergence of a new paradigm:

> Discovery commences with the awareness of anomaly, i.e., with the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science. It then continues with a more or less extended exploration of the area of anomaly. And it closes only where the paradigm theory has been adjusted so that the anomalous has become the expected. (pp. 52-3)

In this case, rather than a decline in the goodness of fit between theory and observations as a method becomes increasingly ill-adapted to a population, observations will increasingly diverge from expectations.

In the case of studies of Jewish populations, the driving factors besides technological developments and broader societal trends have been acculturation and assimilation, which have changed the nature of Jewishness and required adjustments to research strategies, as well as the continuing geographic transformation of the Jewish population from urban ethnic enclaves in major cities of the Northeast and Midwest to mixed suburban communities in large metropolitan areas across the nation. These themes recur throughout the history of American Jewish population studies.

## BIBLICAL PRECEDENTS

The desire to enumerate "Jewish" populations predates even Judaism itself. [1] While questions of dating and historicity are critically important to properly understanding Biblical texts, there is little consensus on these points, and the issue can be bracketed off for the purposes of this discussion. Regardless of whether the censuses of Num. 1-4, Num. 26, 2 Sam. 24 and 1 Chron. 21 actually occurred and when these passages were composed, the fact that early texts express both an interest in counting themselves and a clear understanding of how to do so is a reminder that contemporary interest in the size of Jewish populations is not unprecedented.

The level of detail of the Pentateuchal texts is particularly striking, as can be seen from the description of the population to be studied in Num. 1:2-3 (all quotations follow the Jewish Publication Society 1985 translation), when the Lord directs Moses to:

> Take a census of the whole Israelite community by the clans of its ancestral houses, listing the names, every male, head by head. You and Aaron shall record them by their groups, from the age of twenty years up, all those in Israel who are able to bear arms.

As this count expressly excluded Levites, Num. 3 describes a parallel census of male Levites from the age of one month and up. Additional enumerations of specific groups of Levites aged between 30 and 50 (and thus capable of performing tasks for the Tent of Meeting) are called for in Num. 4. Another census is described Num. 26. The method is almost identical, except that Moses' co-enumerator is Eleazar rather than his brother Aaron, and the number of Levites increases from 22,000 to 23,000, while that of the Israelites decreases from 603,550 to 601,730.

In contrast to this wealth of detail, early 20th century Jewish population studies often contain little or no information about the manner in which the sample was gathered nor who was eligible to be counted, more closely resembling the vague descriptions of the Davidic census of 2 Sam. 24 and 1 Chron. 21: "The king said to Joab, his army commander, 'Make the rounds of all the tribes of Israel, from Dan to Beer-sheba, and take a census of all the people, so that I may know the size of the population'" (2 Sam. 24:2).

While the early interest in numbering the Israelite community is fascinating, these texts are best seen as instructive parallels rather than as evidence for a continuous, or even intermittent, tradition of self-enumeration.[2] There is little if any evidence of similar attempts to count Jews in the intervening eras, and nothing in the first modern studies of

Jewish populations to suggest that researchers saw themselves as participating in a living tradition. Indeed, *halakhah*, rabbinic law, actively discourages counting Jews. In Ashkenazi communities, the presence of a *minyan*—the quorum of 10 Jewish males over the age of 12 traditionally required for public prayer—is ascertained by counting "*not* one, *not* two, *not* three," etc. or using a Biblical verse with 10 words (Sarna 2004:356). If anything, the tradition of *not* counting Jews is more closely tied to the Bible. The accounts of David's census in 2 Sam. 24 and 1 Chron. 21 depict counting people (as opposed to substitutes like half shekels) to divinely ordained pestilence.

The Biblical census of Num. 1:2-3, however, serves another purpose, introducing key concepts in survey methodology, which I cover briefly below.

*Census vs. Survey*

The Biblical passages discussed above are all censuses, which are attempts to count (and possibly collect additional information) from all units of a given type with a certain set of characteristics. In the Bible the unit of interest is the individual, but it can also be the household (as is the case with certain questions in the U.S. Census) or a corporate entity, like the Census Bureau's studies of businesses. Whatever the unit of interest, the census-taker attempts to collect information from every eligible unit. In the census of Israelites described in Numbers, an eligible unit was a male Israelite (in the narrower sense of a Hebrew who was not a Levite) more than 19 years old, corresponding to the group eligible to bear arms.

A survey, by contrast, infers the characteristics of the population from a set of randomly selected cases (the *sample*) taken from the sampling *universe*, the population of

units eligible for inclusion in the survey. The way in which a sample is used to measure the population is well described in homely fashion in a promotional brochure accompanying the Los Angeles Jewish community study report of 1951:

> Community surveys are well-established devices that make use of various scientific techniques to get their results. Fortunately, it is not necessary to interview every Jewish family in Los Angeles to adequate (though never perfect) information about all Jewish people any more than it is necessary to drink a whole bowl of soup to find out how the soup tastes. But even with well designed "samplings" of Jewish people (resembling well stirred spoonfuls of soup), the effort required to get the facts is considerable.

Were Moses to have surveyed the Israelites instead, his universe would have been non-Levite Israelite males aged 20 and above.

*Coverage and Bias*

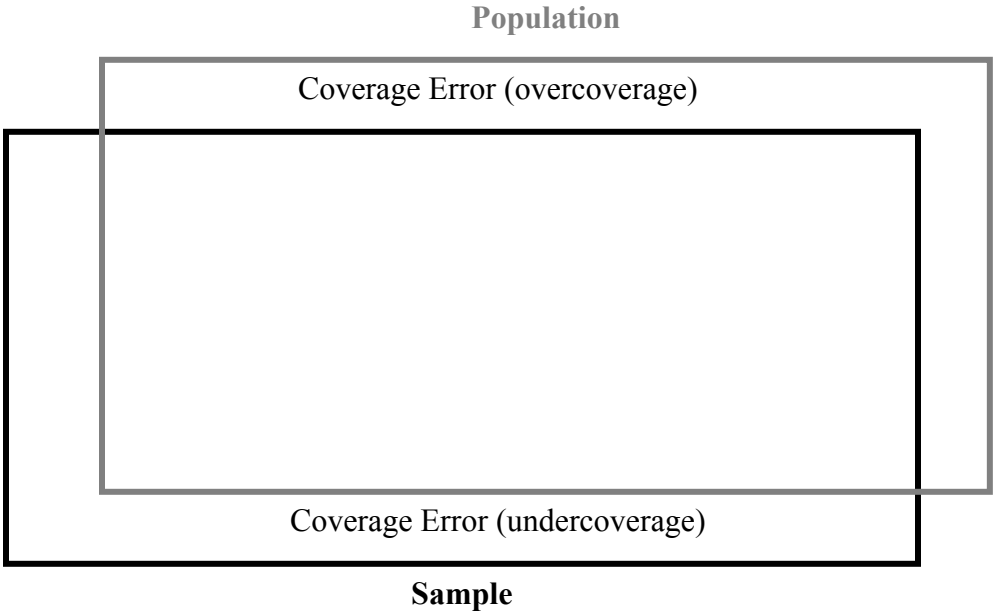
Inference from a sample to a population depends on the properties of the sampling frame, the means by which the sample is generated. The cases that are included in the sampling frame constitute the sampling universe. For example, had Moses surveyed every tenth Israelite as they lined up, this procedure would have been his sampling frame. Three types of error typically occur moving from the target population to the final estimate: coverage error, sampling error, and nonresponse error.

*Coverage error.* The first type of error to consider is that of coverage error, the extent to which a sampling frame differs from the target population (I use the terms "error" and "bias" interchangeably). The census described in Num. 1 enumerated the Israelites by the tribal rosters. The Lord assigned census-takers to Moses, one from each tribe.

Moses and Aaron then assembled the community and the enumerators counted each Israelite male aged 20 and above as they declared themselves by name and ancestry and ancestral house.

The adequacy of this frame depends on its coverage, the extent to which the sampling frame includes all eligible units of the sampling universe and no ineligible units. Unless special provisions were taken, one might expect that anyone who was seriously ill would not have been able to stand in the desert sun until it was his turn to announce himself, leading to undercoverage and, consequently, underestimates of the population size. The frame also depends, apparently, on self-reports of age. It would not be unreasonable to think that some individuals may have been disabled and unable to report their age by themselves. Overcoverage is also a possibility. Lev. 24 describes the case of the unnamed son of an Israelite mother (Shlomit bat Divri) and an Egyptian father who blasphemed and was stoned to death. It is unclear whether he was considered an Israelite or not. Lev. 24:10 describes him as quarrelling with another Israelite (suggesting that he was so regarded), while in Lev. 24:16 the Lord pointedly notes the blasphemers, whether alien or not, are subject to death (suggesting the alternative classification). Had he lived, he might have presented himself for mustering (he did leave Egypt, after all) although he was not, quite possibly, included in the sampling universe. Had this been the case, the population of Dan would have been slightly overestimated. Figure 2:1, below, illustrates coverage error.

Figure 2:1. Coverage Error



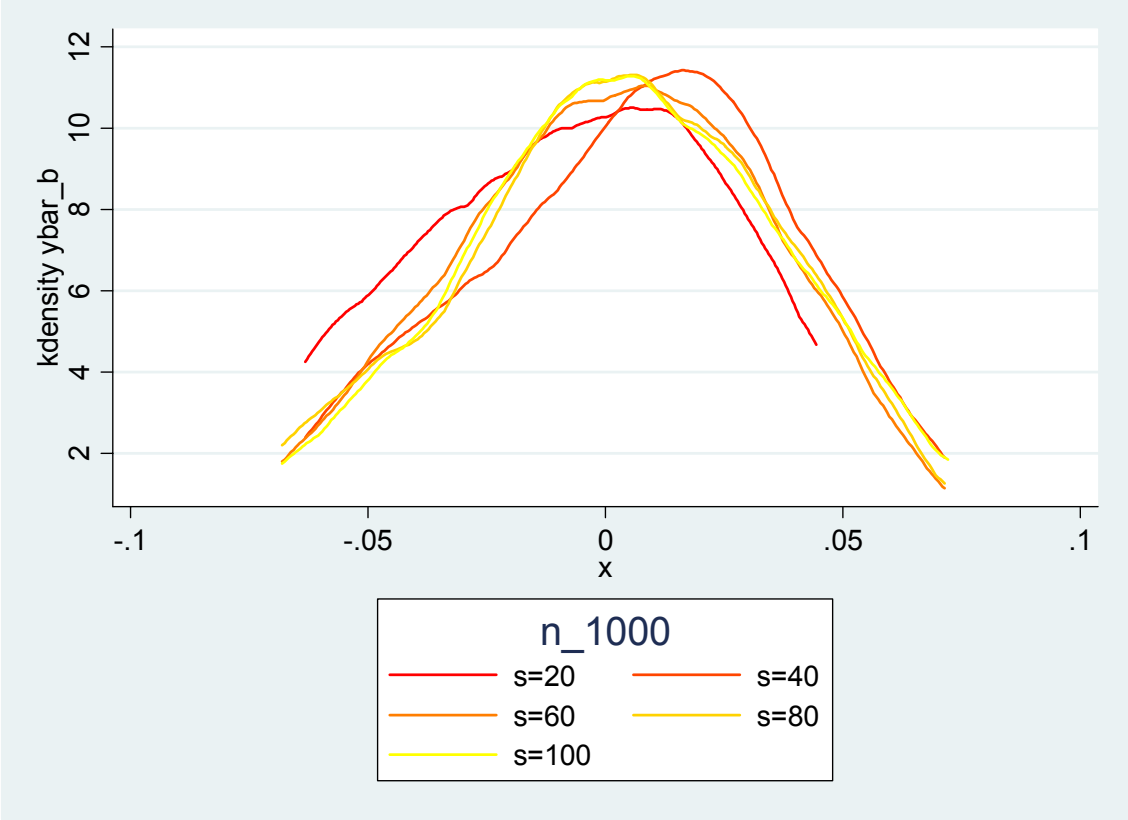

*Note:* Adapted from Groves *et al.* (2004a)

Coverage error can also occur in a more conceptual sense: the difference between the sampling universe and one's conception of it. The census of Num. 1, for example, clearly spells out the universe: it excludes women, children, and non-Israelites. By contrast, King David simply instructs Joab and his military staff to "Make the rounds of all the tribes of Israel, from Dan to Beer-sheba, and take a census of all the people, so that I may know the size of the population" (2 Sam. 24:2). The plain meaning of the request encompasses women, children, and non-Israelites. However, Joab returns with the pleasingly imprecise (albeit disconcertingly large) estimates of 800,000 men fit for military service in Israel and 500,000 in Judah. A sampling frame that is mismatched to the sampling universe is biased to the extent that the estimated value differs from the true value

of the population. In this case, the census clearly systematically underestimated the total size of the population.

*Sampling error.* The next type of error is not a function of the sampling frame, but its realization (i.e. the particular set of units selected from a sampling frame). A randomly selected sample is described as an unbiased estimator. That is to say that were an infinite number of samples of size *n* (the number of units sampled) to be selected, the means ($\bar{x}$) of a variable of interest ($x$) would be normally distributed (i.e. the familiar bell curve) around the mean of the population ($\mu$). The grand mean, which is the mean of the sample means ($\bar{\bar{x}}$), will always equal to $\mu$. To illustrate this, Figure 2:2, below, shows the sampling distribution of *S* realizations (separate samples) of a normally distributed population ($\mu = 0, \sigma = 1$) of $n = 1{,}000$ cases (where *s* is the standard deviation of a sample).

Figure 2:2. Sampling Distribution of *S* Realizations of Normal Samples of *n*=1,000




The lighter lines represent greater numbers of realizations and tend toward a closer approximation of the normal distribution, where $\bar{\bar{x}} = 0$. (The choice of 0 is purely arbitrary. If $\mu = 10$, then $\bar{\bar{x}} = 10$.) In other words, the mean of a particular sample ($\bar{x}$) may deviate from the mean of the *population* (i.e. all units in the sampling universe: $\mu$), but it will not be systematically *biased* in one direction or the other.

The level of dispersion (*variance*) of the sample means ($\bar{x}$) around the grand mean ($\bar{\bar{x}}$) is a function of the dispersion of values within the population ($\sigma$) and the sample size ($n$). In the most extreme case, we have a population where $x$ is invariably equal to one. Every sample will only consist of ones, so there is no variance ($x = \bar{x} = \bar{\bar{x}} = \mu$). If we have a population where $x$ is equal to one half the time and equal to zero the other half of

the time, different samples will have different means because they contain different numbers of ones and zeroes. Finally, if we take an intermediate case where a population has more ones than zeroes, different samples will have different means, but they will be more tightly clustered around $\mu$ than would samples drawn from the population with equal numbers of ones and zeroes. A population with a narrower dispersion of values will have sample means closer to the population mean than would a population with greater variation.

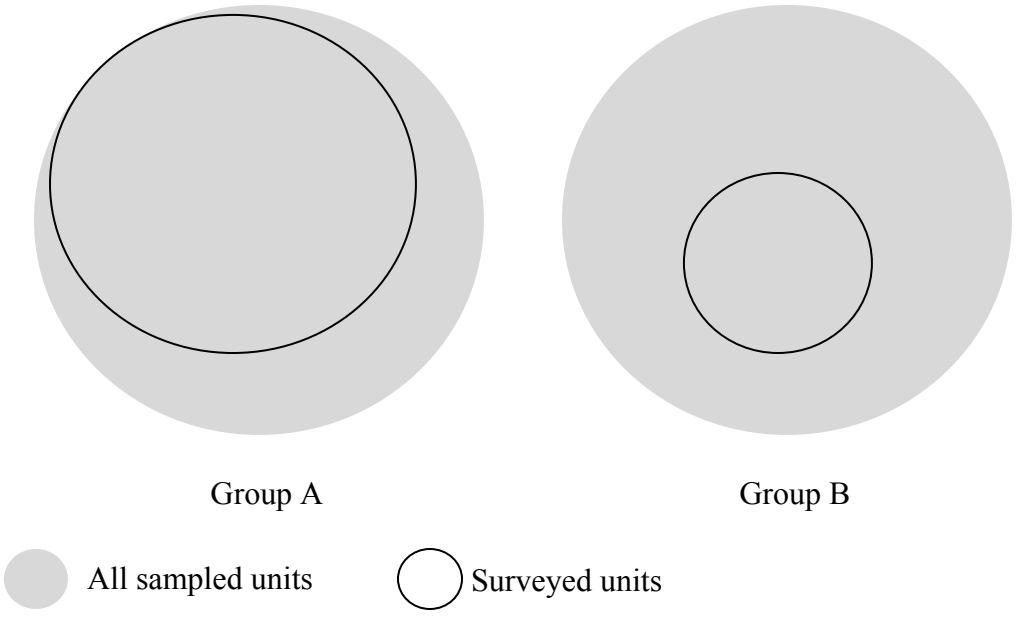
Similarly, the smaller the *n* (the number of cases in the sample), the greater the variance is. To take the most extreme example again, a sample of $n = N$ (where $N$ is the number of units in the population, i.e. a census) will have no sampling variance ($\bar{x} = \mu$) because all units are always included. A sample of $n = 1$ will have the maximum value of variance. In a population with equal numbers of ones and zeroes (i.e. $\mu = 0.5$), $\bar{x}$ can never equal $\mu$ where $n = 1$. A sample of intermediate $n$ ($1 < n < N$) will have some variance (different samples will have different numbers of ones and zeroes), but less so that where $n = 1$. As $n$ approaches $N$, the variance will grow ever smaller, because the value of a single case will have less and less impact on $\bar{x}$. Thus a survey with an $n$ of 499 will have smaller confidence intervals for a population of $N = 500$ rather than $N = 500{,}000$.

The dispersion of the sampling distribution, the *standard error of the mean*, is $\sigma/\sqrt{n}$. While $\mu$ and $\sigma$ remain unknown, as sample size grows the odds that the standard deviation of the sample ($s$) will equal the standard deviation of the population ($\sigma$) rapidly increase in predictable fashion. With sufficient $n$, $s$ can be assumed to equal $\sigma$, and the standard error of the mean can be calculated as $s/\sqrt{n-1}$, and the possible range of val-

ues around $\bar{x}$ in which $\mu$ would be found at a given rate of success can be calculated. Thus, although $\mu$ and other characteristics of the population remain unknown, the *confidence interval* (the range of values in which they can be found with 95 percent or some other level of confidence) can be determined.

*Nonresponse error.* The final source of error is neither a function of the sampling frame nor its realization, but the participation of sampling units. Had there been many Israelite draft dodgers, the Lord may have been well advised to require the tribal enumerators to check for malingerers and men gone AWOL. In most surveys, the bias arising from nonresponse error occurs when nonresponse is associated with a characteristic of interest. Should a given tribe have had more than its fair share of nonresponders, estimates of its relative size would have been too low. Figure 2:3, below, illustrates nonresponse error.

Figure 2:3. Nonresponse Error



Group A and Group B (represented by the grey circles) have the same size samples. Group A, however, has an 80 percent response rate, let us say, while Group B has a 20 percent response rate (circles not drawn to scale). Unless otherwise corrected for, Group A will comprise 80 percent of the final sample. If we further assume Group A has an average score of one ($\overline{x}_a = 1$) and Group B has an average score of zero ($\overline{x}_b = 0$), it follows that the overall average should be $\overline{x} = 0.5$. However, the estimate will be $\overline{x} = 0.8$, so the resulting bias will be 0.3.

Having provided an overview of concepts used throughout this dissertation, I will now turn my attention to the history of Jewish population studies in the United States.

## GOVERNMENT ENUMERATIONS

The United States is unusual in that its national census does not ask about survey respondents about their religious affiliation or have a national population register that includes religion.[3] Some of the earliest social scientific studies in Jewish demography relied on official statistics garnered from censuses. The Berlin-based *Bureau für Statistik der Juden* (Bureau for Jewish Statistics) published the *Zeitschrift für Demographie und Statistik der Juden* (Journal for Demography and Statistics of Jews) from 1905 to 1931, which analyzed Jewish demography using data from German and Austro-Hungarian censuses (Blau N.d.).[4] Incidentally, the concerns of these pioneering Jewish demographers anticipate those of their American counterparts. Arthur Ruppin and Felix Theilhaber described a looming crisis for the Jewish community brought on by low birth rates and high levels of intermarriage (Blau N.d.).

The Jewish community apparently felt that church-state separation was, in fact, the reason for the absence of a religion item on the Census of Population. The first volume of the *American Jewish Year Book* stated that "…the census of the United States has, in accordance with the spirit of American institutions, taken no heed of the religious convictions of American citizens, whether native-born or naturalized…" (cited in Diamond 1977:251). The reality, however, appears to be more prosaic. The census was conceived as an instrument for apportioning representation and for providing data for informing taxation.[5] No less influential a figure than James Madison tried to expand the census' purview to collect data on age, sex, race, and occupation. He succeeded with the first three but failed on occupation because (in Madison's words to Jefferson) it was considered "a waste of trouble and supplying materials for idle people to make a book" (quoted in Anderson 1988:14) despite its clear economic implications. The early censuses were shoestring operations, further hampered by the absence of technology for tabulating large amounts of data. Goals accordingly had to be limited. Religion simply was not that important. It was not until 1850 that the census began to collect individual-level data. In addition, marshals were instructed to collect "social statistics" which were generally collected from sources other than individuals, including, as Anderson lists, taxes collected, schools, newspapers, pauperism and crime, wages, and libraries, as well as religion (p. 37). I discuss the Census of Religious Bodies in the following section. Any possible contention about the inclusion of religion would have been forestalled by the bitter argument over the enumeration of slaves that took place at that time. The first time at which there may have been a compelling reason to ask for religion was as a way counting migrant groups, like Jews, that could not be determined from their country of origin.

As part of this effort, which culminated in the decision to restrict migration to the United States in 1924, the census added a question on mother tongue in 1910. Were Jews the only target of antimigrant sentiment, a question on religion would have been an attractive option. This would not, however, suffice to count other European migrant groups that could not be distinguished by religion. The omission of religion from the U.S. Census, accordingly, appears to be a product of circumstances rather than a concerted choice.

*Census of Religious Bodies*

While less well known than the Census of Population and Housing, the Census Bureau conducts a number of institutional censuses, including the quinquennial Economic Census and the Census of Agriculture. Instead of households, the reporting units for these censuses are businesses and farms, respectively. From 1850 to 1936, the Census Bureau also conducted a census of religious institutions every 10 years or so, the Census of Religious Bodies. This census contained a number of variables of interest, including the number of members of the congregations surveyed.

The accuracy of these estimates is, however, questionable and they were generally ignored by sociologists of religion. Rodney Stark (1992), however, asserted that the Census of Religious Bodies was in fact a source of useful and quite accurate data, though he did not address counts of Jews. Jewish opinion of the accuracy of Census of Religious Bodies estimates is, however, decidedly negative. Harry Linfield (1938) found that the number of Jewish congregations was underestimated in the 1850 census, and probably those of 1860 and 1870, by reference to directories of Jewish congregations. He ascribed the underenumeration to the rather strict definition of a "church" used by the Census of

Religious Bodies, which may not have encompassed many recently founded Jewish congregations. The 1850 and 1860 censuses specified that assistant marshals take an "account of churches, and other places of worship, belonging to all religious denominations in the town or county described, if statedly used as public places of worship" (quoted in Engelman 1947:128), a definition that may not have been though appropriate for some Jewish congregations. The 1870 survey declared that "A church to deserve notice in the census must have something of the character of an institution. It must be known in the community in which it is located. There must be something permanent and tangible to substantiate its title to recognition" (quoted in Engelman 1947:131). In his review of Jewish statistics in the Census of Religious Bodies, Engelman (1947) noted that this definition probably excluded many small Jewish congregations. By 1906, however, the level of coverage had improved, and Engelman reports that existing synagogues heretofore excluded were finally counted.

Table 2:1, below, shows estimates of the number of synagogues between 1850 and 1890, including the results of a Jewish enumeration of 1876-78 carried out by the Union of American Hebrew Congregations (1880), which is discussed at greater length on pp. 50-52. While religious statistics were collected in 1880, they were never published, allegedly due to a lack of money and disinclination on the part of the superintendent of the U.S. Census (Engelman 1947:133-134). Despite the massive influx of migrants from Eastern Europe from 1880, the number of synagogues reported by the Census of Religious Bodies was generally little higher in 1890, although a considerable increase is observed in New York, which was the main port of disembarkation. One might expect that the *landsmanschaften* (associations of migrants from the same region

providing social and religious services) and synagogues established by the recent migrants were particularly difficult to enumerate given their informal nature and the presumably limited skills in English of their officers and members. (The extraordinary estimate of 23 synagogues in Maine in 1870 is an apparent mistake.)

Table 2:1. Number of Synagogues

| | Year | | | | |
|---|---|---|---|---|---|
| State | 1850 | 1860 | 1870 | 1880* | 1890 |
| Alabama | | 2 | 2 | 7† | 8 |
| Alaska | | | | | |
| Arizona | | | | | |
| Arkansas | | | 1 | 4‡ | 5 |
| California | | 5 | 7 | 11§ | 15 |
| Colorado | | | 1 | 1 | 5 |
| Connecticut | 2 | 1 | 5 | 3** | 8 |
| Delaware | | | | | |
| District of Columbia | | | 2 | 3†† | 2 |
| Florida | | | | 1§§ | 2 |
| Georgia | | 1 | 6 | 8*** | 9 |
| Idaho | | | | | |
| Illinois | | 3 | 10 | 10††† | 24 |

*Notes:*

* Data from Union of American Hebrew Congregations (1880).

† Excludes 3 noncongregational organizations.

‡ Excludes 3 noncongregational organizations.

§ Excludes 7 noncongregational organizations.

** Excludes 1 noncongregational organization.

†† Excludes 3 noncongregational organizations.

‡‡ Excludes 1 noncongregational organization.

§§ Excludes 1 noncongregational organization.

*** Excludes 5 noncongregational organizations.

††† Excludes 3 noncongregational organizations.

| State | Year | | | | |
|---|---|---|---|---|---|
| | 1850 | 1860 | 1870 | 1880[*] | 1890 |
| Indiana | | 2 | 5 | 14[†] | 23 |
| Iowa | | | 5 | 3[‡] | 6 |
| Kansas | | | 2 | 2[§] | 6 |
| Kentucky | 1 | | 3 | 4[**] | 7 |
| Louisiana | 1 | 5 | 5 | 13[††] | 13 |
| Maine | | | 23 | | |
| Maryland | | 3 | 5 | 23[‡‡] | 12 |
| Massachusetts | 1 | 2 | 5 | 10[§§] | 9 |
| Michigan | | | 5 | 6[***] | 10 |
| Minnesota | | | | 1[†††] | 5 |
| Mississippi | | | | | 6 |
| Missouri | 1 | 2 | 4 | 5[‡‡‡] | 17 |
| Montana | | | | | 1 |
| Nebraska | | | | 1 | 6 |
| Nevada | | | | 1 | |
| New Hampshire | | | | 1 | |
| New Jersey | | 1 | 1 | 7[§§§] | 24 |
| New Mexico | | | | | 1 |
| New York | 9 | 20 | 47 | 52[****] | 179 |
| North Carolina | | | 1 | 3[††††] | 4 |

*Notes:*

[*] Data from Union of American Hebrew Congregations (1880).

[†] Excludes 11 noncongregational organizations.

[‡] Excludes 2 noncongregational organizations.

[§] Excludes 2 noncongregational organizations.

[**] Excludes 4 noncongregational organizations.

[††] Excludes 6 noncongregational organizations.

[‡‡] Excludes 9 noncongregational organizations.

[§§] Excludes 5 noncongregational organizations.

[***] Excludes 8 noncongregational organizations.

[†††] Excludes 2 noncongregational organizations.

[‡‡‡] Excludes 7 noncongregational organizations.

[§§§] Excludes 3 noncongregational organizations.

[****] Excludes 30 noncongregational organizations.

[††††] Excludes 4 noncongregational organizations.

| State | Year | | | | |
|---|---|---|---|---|---|
| | 1850 | 1860 | 1870 | 1880* | 1890 |
| North Dakota | | | | | 1 |
| Ohio | 3 | 8 | 7 | 19† | 34 |
| Oklahoma | | | | | |
| Oregon | | | | 2‡ | 3 |
| Pennsylvania | 8 | 12 | 15 | 38§ | 35 |
| Rhode Island | 1 | 2 | 1 | 2 | 5 |
| South Carolina | 3 | 3 | 3 | 4** | 3 |
| South Dakota | | | | | |
| Tennessee | | | 4 | 6†† | 9 |
| Texas | | | 1 | 7‡‡ | 11 |
| Utah | | | | 0§§ | 1 |
| Vermont | | | | 1 | 1 |
| Virginia | 1 | 3 | 8 | 7*** | 11 |
| Washington | | | | 0††† | 1 |
| West Virginia | | | 1 | 2‡‡‡ | 3 |
| Wisconsin | | 1 | 4 | 3§§§ | 8 |
| Wyoming | | | | | |

*Sources:* Bureau of the Census (1980); Haines (2004); Union of American Hebrew Congregations (1880).

*Notes:*

* Data from Union of American Hebrew Congregations (1880).

† Excludes 13 noncongregational organizations.

‡ Excludes 3 noncongregational organizations.

§ Excludes 15 noncongregational organizations.

** Excludes 3 noncongregational organizations.

†† Excludes 5 noncongregational organizations.

‡‡ Excludes 14 noncongregational organizations.

§§ Excludes 1 noncongregational organization.

*** Excludes 4 noncongregational organizations.

††† Excludes 1 noncongregational organization.

‡‡‡ Excludes 2 noncongregational organizations.

§§§ Excludes 2 noncongregational organizations.

In addition to the number of religious bodies, 19th century censuses included estimates of the value of congregational property (which I do not reproduce here), as well as the number of spaces in congregational pews (Table 2:2). This, however, tells us little. Were women's sections counted toward this figure in congregations with separate seating? We do not know. It is also unclear whether these counts included temporary seating for the High Holidays. The definition used in the first Census of Religious Bodies specifies "the number of seats for individuals in such places of worship, or the number of persons they will accommodate" (quoted in Engelman 1947:128), which does not make matters entirely clear. The larger question remains even more obscure. The number of seats is only of interest to the extent it tells us something about those who would fill them. Unfortunately, this relationship remains obscure. The 1890 census included questions on both seating and membership. In Massachusetts, seats outnumbered members 4,215 to 2,501, while in New York, 45,807 members squeezed into 40,172 seats, indicating that these estimates are of limited utility.

Table 2:2. Number of Synagogue Seats

| | Year | | | |
|---|---|---|---|---|
| State | 1850 | 1860 | 1870 | 1890 |
| Alabama | | 950 | 1,650 | 3,050 |
| Alaska | | | | |
| Arizona | | | | |
| Arkansas | | | 300 | 1,450 |
| California | | 1,460 | 3,610 | 5,375 |
| Colorado | | | | 1400 |
| Connecticut | | 800 | 1,850 | 1,350 |
| Delaware | | | | |
| District of Columbia | | | 800 | 975 |
| Florida | | | | 318 |
| Georgia | | 150 | 1,400 | 3,100 |
| Idaho | | | | |

| | Year | | | |
|---|---|---|---|---|
| State | 1850 | 1860 | 1870 | 1890 |
| Illinois | | 1,500 | 3,950 | 8,820 |
| Indiana | | 450 | 1,900 | 4,700 |
| Iowa | | | 150 | 1,160 |
| Kansas | | | 300 | 260 |
| Kentucky | 600 | | 1,500 | 1,025 |
| Louisiana | 600 | 1,250 | 2,200 | 3,450 |
| Maine | | | 7,315 | |
| Maryland | | 4,300 | 2,750 | 5,100 |
| Massachusetts | 200 | 660 | 1,500 | 4,215 |
| Michigan | | | 1,300 | 4,050 |
| Minnesota | | | | 1,124 |
| Mississippi | | | | 1,750 |
| Missouri | 400 | 1,000 | 2,100 | 4,133 |
| Montana | | | | |
| Nebraska | | | | 600 |
| Nevada | | | | |
| New Hampshire | | | | |
| New Jersey | | 228 | 300 | 4,995 |
| New Mexico | | | | |
| New York | 5,600 | 10,440 | 21,400 | 40,172 |
| North Carolina | | | 200 | 580 |
| North Dakota | | | | |
| Ohio | 1,300 | 5,300 | 4,000 | 9,810 |
| Oklahoma | | | | |
| Oregon | | | | 1,200 |
| Pennsylvania | 3,425 | 3,295 | 7,750 | 10,842 |
| Rhode Island | 300 | 279 | | 620 |
| South Carolina | 2,400 | 850 | 900 | 850 |
| South Dakota | | | | |
| Tennessee | | | 1,100 | 4,400 |
| Texas | | | 400 | 2,380 |
| Utah | | | | 750 |
| Vermont | | | | |
| Virginia | 600 | 700 | 1,890 | 2,550 |
| Washington | | | | |
| West Virginia | | | | 650 |
| Wisconsin | | 400 | 750 | 2,030 |
| Wyoming | | | | |

*Sources:* Bureau of the Census (1980); Haines (2004).

From 1890, the Census of Religious Bodies counted denominational membership (Table 2:3). What exactly the populations counts of individuals represent, to the extent they indicate anything at all, is difficult to say. The 1890 census stated that in Jewish congregations "only the head of the family is counted. The members of the family are represented by one person. The number given as communicants, therefore, does not indicate the number of members of a synagogue. Members of families may, in attaining their majority, rent a pew and be counted as a member of a synagogue or Temple, but they seldom do so until they have a household of their own" (quoted in Engelman 1947:134-135). Orthodox synagogues of the time had few full members and many who bought seats for the High Holidays, Engelman believed, and kept poor records of the latter, presumably leading to undercounts. Reform synagogues were better organized. In 1906 the definition was apparently further tightened, counting only heads of families. Engelman saw this as incompatible with the 1890 definition, for reasons that are not apparent at this remove. Perhaps as a result of the difficulty of reconciling synagogal practice with the alien notion of "communicants," nonresponse was rife, with 35 percent of synagogues not reporting their membership in 1906 (Engelman 1947:150). Women, I presume, were largely excluded from the count, as were children and those unable to pay membership dues, not to mention Jews who were unwilling to join a synagogue or those who lived in an area without a congregation. In 1916, the formal definition of "communicant or member" used in the previous censuses was abandoned in favor of "members" as defined by the reporting institution. It appears that this led to a more egalitarian definition, with women making up 39 percent of reported members (membership breakdowns by sex were not reported in previous years). The 1926 and 1936 census saw yet another change

in Jewish reporting practices. Engelman (1947:159) quotes the faintly disappointed description offered by the Census Bureau: "The Jews, in contrast with the definition employed at earlier census periods, now consider as members all persons of the Jewish faith living in communities in which local congregations are situated." Engelman accurately describes these estimates as "bordering on guessing" (p. 159). Guesses though they may be, the population counts of the 1920s and—with rare exceptions—the 1930s, are, however, gloriously variegated and creative, the methods of which I outline later in this chapter.

Table 2:3. Number of Synagogue Members

| | Year | | | | | |
|---|---|---|---|---|---|---|
| State | 1880[*] | 1890 | 1906 | 1916 | 1926 | 1936 |
| Alabama | 254[†] | 3,168 | 1,141 | 2,947 | 9,218 | 10,135 |
| Alaska | | | | | | |
| Arizona | | | 20 | | | |
| Arkansas | 165[‡] | 744 | 673 | 1,451 | | |
| California | 599[§] | 6,179 | 2,028 | 8,178 | 122,724 | 151,596 |
| Colorado | 31 | 1,062 | 853 | 2,356 | | 20,116 |
| Connecticut | 169 | 1,621 | 1,733 | 8,688 | 90,165 | 90,614 |
| Delaware | | | 207 | | | 6,292 |
| District of Columbia | 144 | 976 | 698 | 3,225 | 16,000 | 18,350 |
| Florida | 0[**] | 147 | 323 | 746 | 11,975 | 18,769 |
| Georgia | 298[††] | 2,086 | 897 | 2,880 | | 19,545 |
| Idaho | | | | | | |
| Illinois | 567 | 10,171 | 5,286 | 33,377 | | 377,809 |

*Notes:*

[*] Data from Union of American Hebrew Congregations (1880).

[†] Excludes 30 members of noncongregational organizations.

[‡] Excludes 42 members of noncongregational organizations.

[§] Excludes 1,887 members of noncongregational organizations.

[**] Excludes 20 members of noncongregational organizations (note that the only congregation did not report membership).

[††] Excludes 77 members of noncongregational organizations.

| State | Year | | | | | |
|---|---|---|---|---|---|---|
| | 1880[*] | 1890 | 1906 | 1916 | 1926 | 1936 |
| Indiana | 378 | 3,617 | 1,383 | 5,461 | | 25,494 |
| Iowa | 69[†] | 537 | 412 | | 12,714 | 11,888 |
| Kansas | 56[‡] | 486 | 175 | | | |
| Kentucky | 285 | 955 | 1,147 | 2,695 | | 16,339 |
| Louisiana | 495[§] | 3,374 | 1,618 | 4,284 | 13,845 | 13,464 |
| Maine | | | 205 | | 7,582 | 8,386 |
| Maryland | 61[**] | 3,575 | 2,153 | 12,056 | 69,974 | 75,225 |
| Massachusetts | 317[††] | 2,501 | 4,388 | 38,867 | 213,085 | 257,994 |
| Michigan | 192[‡‡] | 3,693 | 1,530 | 5,383 | 83,161 | 99,366 |
| Minnesota | 28 | 1,424 | 1,725 | 2,740 | | 39,160 |
| Mississippi | | 1,370 | 746 | | 2,871 | 2,897 |
| Missouri | 478[§§] | 4,450 | 2,392 | 8,347 | | 83,358 |
| Montana | | 140 | 152 | | | |
| Nebraska | 20 | 1,062 | 435 | 1,695 | | 12,922 |
| Nevada | 29 | | | | | |
| New Hampshire | 20 | | 80 | | | 2,996 |
| New Jersey | 241[***] | 4,276 | 4,603 | 15,720 | 219,455 | 257,761 |
| New Mexico | | 50 | 120 | | | |
| New York | 3,406[†††] | 45,807 | 35,342 | 113,924 | 1,899,597 | 2,197,418 |
| North Carolina | 82 | 386 | 234 | | | |
| North Dakota | | 30 | 12 | | | |
| Ohio | 992[‡‡‡] | 8,889 | 5,678 | 20,151 | | 175,942 |
| Oklahoma | | | 72 | 1,166 | | |
| Oregon | 60 | 1,165 | 414 | 1,315 | | 10,862 |
| Pennsylvania | 2,235[§§§] | 8,029 | 15,479 | 32,276 | 393,517 | 423,943 |
| Rhode Island | 105 | 910 | 1,025 | | 24,034 | 27,311 |

*Notes:*

[*] Data from Union of American Hebrew Congregations (1880).

[†] Excludes 22 members of noncongregational organizations.

[‡] Excludes 45 members of noncongregational organizations.

[§] Excludes 778 members of noncongregational organizations.

[**] Excludes 966 members of noncongregational organizations.

[††] Excludes 475 members of noncongregational organizations.

[‡‡] Excludes 52 members of noncongregational organizations.

[§§] Excludes 703 members of noncongregational organizations.

[***] Excludes 278 members of noncongregational organizations.

[†††] Excludes 9,978 members of noncongregational organizations.

[‡‡‡] Excludes 2,038 members of noncongregational organizations.

[§§§] Excludes 3,682 members of noncongregational organizations.

| State | Year | | | | | |
|---|---|---|---|---|---|---|
| | 1880* | 1890 | 1906 | 1916 | 1926 | 1936 |
| South Carolina | 110† | 800 | 312 | | 3,956 | 4,408 |
| South Dakota | | | | | | |
| Tennessee | 260‡ | 1,760 | 919 | 2,022 | | 23,275 |
| Texas | 210§ | 1,994 | 1,676 | 6,258 | | 43,970 |
| Utah | 25** | 100 | 183 | 670 | | 2,710 |
| Vermont | 19 | 44 | 166 | 590 | 1,433 | 1,727 |
| Virginia | 276†† | 1,187 | 915 | 2,690 | 22,414 | 23,564 |
| Washington | | 150 | 488 | 2,950 | | 17,119 |
| West Virginia | 58‡‡ | 350 | 220 | | 4,936 | 5,391 |
| Wisconsin | 95 | 1,231 | 1,199 | 2,612 | | 37,375 |
| Wyoming | | | | | | |

*Sources:* Bureau of the Census (1980); Haines (2004); Union of American Hebrew Congregations (1880).

*Notes:*

* Data from Union of American Hebrew Congregations (1880).

† Excludes 20 members of noncongregational organizations.

‡ Excludes 71 members of noncongregational organizations.

§ Excludes 165 members of noncongregational organizations.

** Excludes 25 members of a noncongregational organization.

†† Excludes 161 members of noncongregational organizations.

‡‡ Excludes 61 members of noncongregational organizations.

The inclusion of estimates of synagogue members (Table 2:3) was not the only change that took place in 1890. For the first time, Jews were deputized as special agents by the Census Bureau to collect statistics. Philip Cowen and Henrietta Szold served as the special agents in the 1890 and 1907 censuses respectively (Linfield 1938). From 1917 to 1936 the Bureau of Jewish Statistics and Research of the American Jewish Committee was in charge of enumerating Jewish religious bodies for the Census Bureau (Diamond 1977). From 1907, the special agent deputized by the census bureau to collect statistics on Jewish religious bodies was also responsible for the *American Jewish Year Book* estimates (see pp. 52-57), and sensibly combined their duties, asking for information on the

total Jewish population as well as the mandated questions. These estimates, however, had their own problems, which are discussed subsequently.

Why exactly the Census of Religious Bodies was ended is unclear. Marcum (2003) suggests that the program was cancelled in part because of opposition from Christian Scientists. It seems likely that the program found few avid supporters because of the inaccuracy and limited utility of its estimates, as well as the general sense that government interest in religious matters was contrary to the spirit of the constitution, though neither the free exercise clause in the first amendment nor the religious test clause of Article VI could be construed as barring this census.

*Individual Census Returns*

Each census of the United States dating back to 1790 has included at least the name of the head of the household (Gauthier 2002). In 1850, this was expanded to include the names of all free persons in the family normally residing at that place of residence. While "nationality" was not recorded in the first censuses, the datasets held by the Inter-University Consortium on Political and Social Research (Haines 2004) includes codes for persons of "Hebrew nationality" among other national origin groups. I have been unable to determine when this was coded and what procedures were used, though presumably this involved using the name of the head of the household. I discuss the difficulties of using distinctive Jewish names below (pp. 74-78). Beyond the usual problems, the names of Jews in 1790 would have been very different from those in later eras, given the predominance of Jews of Sephardic backgrounds whose names tended to be of Portuguese or Spanish origin. Had the coder assumed that the names of Jews were Germanic in origin

(excepting names like Cohen and Levine), they would have underestimated the Jewish population. It is reported that there were 599 heads of household of "Hebrew nationality" in Maryland, 385 in New York, 85 in South Carolina, 49 in Massachusetts, 44 in Maine, 21 in Pennsylvania, 9 in Rhode Island, 5 in Connecticut, 1 in North Carolina, and none in New Hampshire, Vermont, or Virginia. Totals for Delaware, Georgia, Kentucky, and New Jersey are missing as a result of Federal buildings burned during the War of 1812 (Rosenswaike 1961).

Another approach was used by Rosenswaike (1961; 1964b; 1985; 1989), who compared the names of heads of households to names Jews of the era obtained from "congregational membership lists, tombstone inscriptions, marriage and death notices and public documents such as wills and land records" (Rosenswaike 1961:24). Other than membership lists and tombstones, however, it is unclear how Rosenswaike determined who was a Jew. Judging by his references and the comments accompanying the names of the people identified as Jews, a good deal of his information came from local histories and other secondary sources. Relying on official records and local histories carries the potential to identify well-known Jews at the expense of those who were not prominent members of general or Jewish society and those who may have not emphasized their Jewish identity, as well as those living in areas with established Jewish communities. This appears to be the case, as Stern (1964) makes some amendments and Rosenswaike's (1964a) response concurs, noting "possible" households. To the extent that Rosenswaike relied on Jewish names, his estimates would be subject to the problems outlined above. An unexpected problem with the census data noted by Rosenswaike is underenumeration: individuals known to be on membership rolls who were not listed on census returns. In

any case, this method is primarily of historic interest, as named census returns are not available for an extremely long period of time and individually tracing known Jews is only possible for a small population.

*1890 Census*

Curiously, the only formal study of Jewish household-level records by the Bureau of the Census itself is unremarked in the literature on Jewish population studies. John S. Billings (1890), a surgeon in the U.S. Army, presented an analysis of the births, deaths, and marriages of 10,618 Jewish households. As the census did not ask about religion, the addresses of the households were provided from rabbis and presidents of congregations throughout the country. The purpose was avowedly assimilationist: "to determine whether the important characteristics in the vital statistics of this people which have been observed in Europe also exist in this country, or to what extent they have been modified by their new conditions of life here" (p. 3). In keeping with the goal of understanding the effect of living in the United States, attention was paid to longer term residents who had lived in the nation for five or more years. The resulting sample is clearly skewed toward well-to-do second and third German Jews: about two-thirds of households kept one or more servants.[6] Readers familiar with contemporary concerns in Jewish demography will find Billings's comments on Jewish marriage and fertility strikingly familiar. He reports a very low marriage rate and older average age at marriage (compared to other Americans) as the main causes of the low Jewish birth rate.[7]

*1960 Census*

In 1956, the U.S. Census Bureau announced it was considering adding a religion question for the 1960 census. A test question on religion, in fact, made its way onto the March 1957 version of the Current Population Survey, a monthly survey of 35,000 households conducted by the Census Bureau for the Bureau of Labor Statistics that is the gold standard of American surveys. Data from this survey has been extremely important to researchers of American Jewry (Ritterband, Kosmin, and Scheckner 1988). The proposal foundered on opposition from a number of groups, mostly Jewish, including the American Civil Liberties Union, the Anti-Defamation League of B'nai B'rith, the American Jewish Congress, the Union of American Hebrew Congregations, and the Jewish Statistical Bureau, as well as from individual Protestant and Catholic ministers, in spite of their denominations' approval of such a question (Good 1959).[8] From today's vantage point, it is ironic that Jewish communal opposition contributed to the absence of a question that would provide extremely valuable information to the community, but barely a decade after the Holocaust, fear of that the data could be used to identify Jews (and others) in a future wave of prejudice loomed large.

*Shipping and Immigration Records*

While not strictly a government enumeration, Joseph Jacobs (1914) of the Bureau of Jewish Statistics of the American Jewish Committee used information on the number of Jews arriving in the United States in third class on immigrant ships to estimate the size of the American Jewish population. Unfortunately, this method required many additional estimates to be made for which there are little data. First, government figures were in-

complete for the period 1881-99, requiring Jacobs to assume that the proportion of Jews was identical in the unenumerated ships. Second, the figures did not account for deportation and out-migration. Third, upper-class passengers were not counted, as were people who immigrated via Canada. Fourth, an estimate had to be made for the size of the Jewish population prior to 1881. This was apparently based on the Board of Delegates of American Israelites estimate of 1876-78 (see pp. 50-52), as well as migration in the intervening years, which were in turn derived from the unreliable guesstimates of local reporters. Finally, births and mortality had to be accounted for, and Jacobs's calculations did not inspire confidence, lacking individual-level data on the population:

> But both this original quarter of a million and the net immigration added to it in the thirty years 1881-1910 have increased naturally by the usual excess of births over deaths, and in order to ascertain the full Jewish population in 1910, an attempt must be made to calculate this natural increase. Roughly speaking, populations of a type that has so large a proportion of "viable" lives and of marriageable men and women who are not addicted to race-suicide, would have a death rate ranging around 15 per thousand, a marriage rate of 10 per thousand, and a birth rate of 35. Subtracting the death rate from the birth rate would result in an addition of 20 per thousand, or 2 per cent per annum. (Jacobs 1914:342)

Population projections are notoriously sensitive to the slightest changes in inputs, so were Jacobs to be off by even a slight margin (as he undoubtedly was given the impossibility of obtaining accurate data on births and deaths), his estimates would have been askew. The problem is made worse by Jacobs's assumption that the compound growth formula applied. Unlike money in a bank account, newborns added to a population do not immediately contribute to further growth. Instead, population growth is a function of the age

structure of a population and age-specific birth and death rates, which Jacobs was unable to calculate.

### *Other Estimates*

Occasionally, state or federal governments have directly enumerated the Jewish population. Oppenheim (1918) reports a 1790 census estimate of the Jewish population of New York City. While not strictly an enumeration, Indiana and Iowa collected information on the religious preferences of brides and grooms in the middle of the twentieth century (Rosenthal 1963).

## JEWISH ENUMERATIONS

Besides government censuses, various organs of the Jewish community also made attempts to enumerate the Jewish population. The two major efforts in this vein were the Union of American Hebrew Congregations census of congregations in 1876-78 and the various estimates published by the *American Jewish Year Book*, which I discuss now.

### *Statistics of Jews of the United States, 1880*

Whether via luck or foresight, the Board of Delegates of American Israelites chose to undertake a similar enumeration to the Census of Religious Bodies in cooperation with the Union of American Hebrew Congregations in the interval that the 1880 census should have appeared. Despite the very similar methods employed, the Board and Union apparently felt the results of the Census of Religious Bodies were inaccurate, describing their

own effort as "the first systematic effort to ascertain our strength" (Union of American Hebrew Congregations 1880:3). Like the Census of Religious Bodies, the survey authors attempted to contact congregations directly, with mixed success:

> The plan adopted to secure the desired information, was sending out blanks and circulars to almost every place in the Union. These were sent out in the years 1876-7-8. In towns where no congregations existed, the secretaries of the Lodges of the various Jewish Orders were addressed, and influential citizens in sections where there were neither congregations nor societies. It was difficult, in some instances, to procure a response: but, notwithstanding many obstacles, it is believed that this—the first systematic effort to ascertain our strength,—is nearly as accurate as we would expect, and will furnish a basis on which future committees can work with a fair prospect of accomplishing yet more fully the desired object.
>
> Where there are no statistics from a city, town or congregation, it is because no reply was received, notwithstanding urgent solicitation.
>
> Some congregations declined furnishing the information sought, so that it had to be procured from other sources, but replies were received from nearly one thousand places, and the result shows that there were in the United States, in the year 1878, 278 congregations, with a membership of 12,546, and the number of Israelites, as far as could be ascertained, was 230,257, so that making a fair allowance for such cities, towns or villages from which no information could be derived, the Jewish population of the United States can safely be placed at 250,000. (Union of American Hebrew Congregations 1880:3)

The authors are, perhaps, too sanguine about the accuracy of their count of synagogue members. Of the 12 congregations of Baltimore, for instance, 10 refused to provide any information and other, less dramatic, instances of missing data can be found throughout the work. Indeed, the estimated number of members seems considerably lower than 1890 Census of Religious Body estimates, even allowing for the explosive growth of the Jewish community in this period (see Table 2:3).

The inclusion of estimates of synagogue members was not the only change that took place in 1890. For the first time, Jews were deputized as special agents by the Census Bureau to collect statistics. Philip Cowen and Henrietta Szold served as the special agents in the 1890 and 1907 censuses respectively (Linfield 1938). From 1917 to 1936 the Bureau of Jewish Statistics and Research of the American Jewish Committee was in charge of enumerating Jewish religious bodies for the Census Bureau (Diamond 1977), and chose to estimate the total Jewish population rather than synagogue members, to the Bureau's displeasure.

In contrast to the estimates of the Census of Religious Bodies, the 1880 organizational census includes estimates of the total Jewish population for various towns, including those with no synagogue or other Jewish organization furnished by local reporters, coming to a total of 250,000 after guesstimating the size of the uncovered population. As this is the procedure adopted for most of the estimates published in the *American Jewish Year Book*, I will address this method next.

*American Jewish Year Book Population Estimates*

From its first issue (1899-1900), the *American Jewish Year Book* has regularly featured articles on American Jewish demography, usually including state by state estimates of the Jewish population. Like the 1880 congregational census, the *Year Book* relied on estimates from local reporters. The basis for these estimates is unknown, but in most cases probably amounted to little more than a guess by the reporter. While this could be quite accurate in small, tightly-knit communities, its accuracy in large communities almost certainly left a lot to be desired. These estimates could and did differ widely. In the

course of gathering information for its directory of Jewish organizations in the United States, Henrietta Szold (1907) sent questionnaires to organizations and congregations inquiring into the "Number of Jewish Residents (Souls) in the Town" (p. 432). The multiple answers received at times disagreed significantly:

> In some cases there happily was complete agreement among correspondents whose positions in their communities presumably rendered them competent to judge. Thus the necessity for a further exercise of judgment seemed removed. In other cases the variations were wide, even extreme, ranging in one city from 75,000 down to 30,000. For such cases certain warranted instances suggested a gauge, not infallible, by any means, but also not without some claims to reliability, in so far as confidence can be placed in figures not the result of an actual count. In a general way, this gauge consists in the number, the character, and the duplication of organizations,—the number of the congregations and the character and duplication of charitable societies and institutions, regardless, in a measure, of the size of the official membership of each or of all combined. (Szold 1907:432)

The 1908 edition of the *Year Book* provides a natural experiment in interrater reliability, providing a table comparing their 1907 estimates with the similarly derived 1905 estimates published in the *Jewish Encyclopedia* (also solicited by Szold). Figure 2:4 presents a scatter-plot of estimates of the Jewish population for states and territories of the United States from the *Jewish Encyclopedia* and the *Year Book*, excluding New York. As can be seen, the encyclopedia's estimates are usually lower, which may be partly a function of immigration. While the two sources are in broad agreement concerning the relative size of communities, specific estimates of size vary considerably.

Figure 2:4. Estimates of State Jewish Population, 1905-07

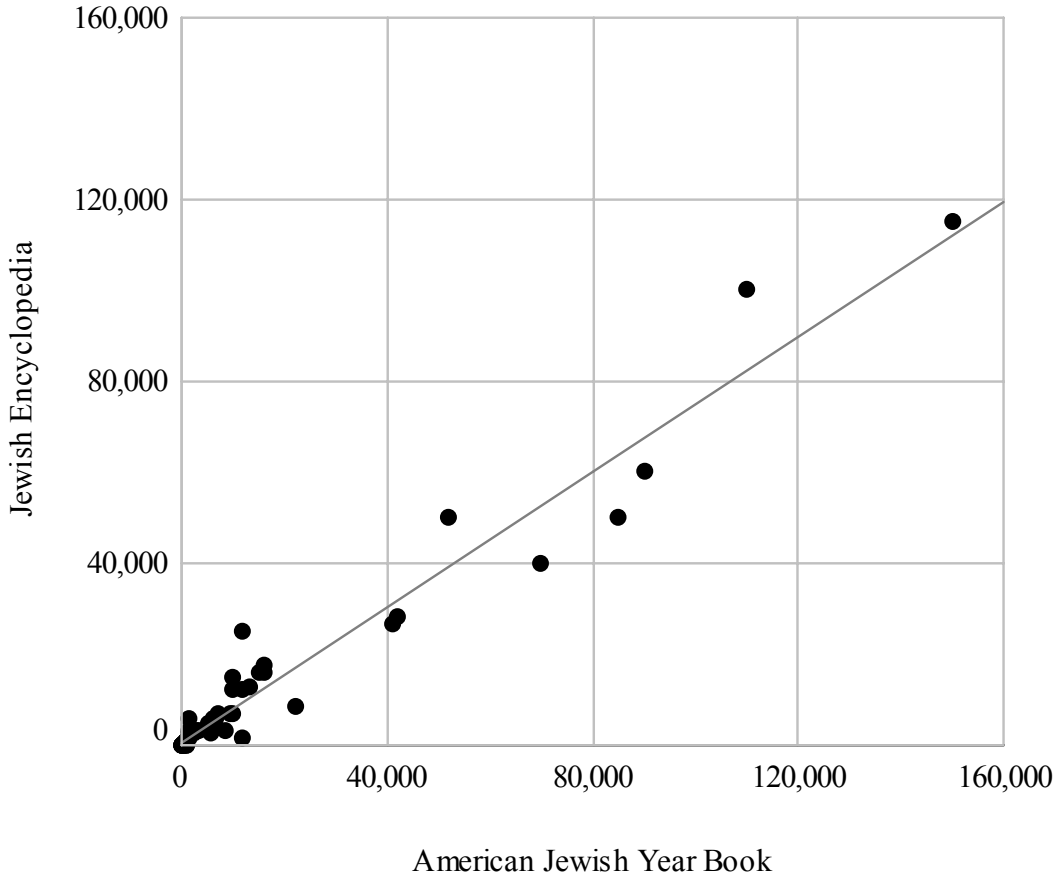
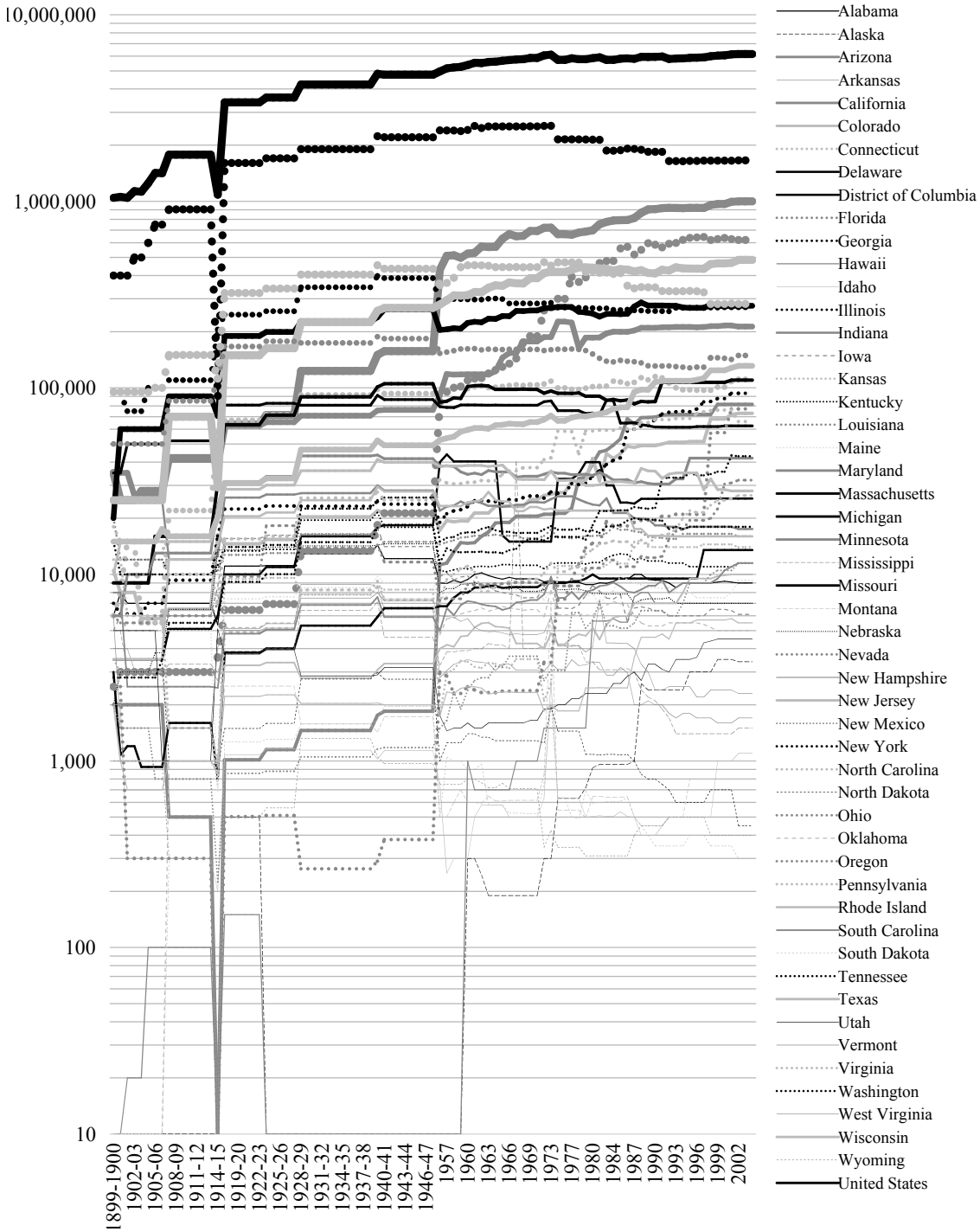



Figure 2:5, below, shows state by state estimates for each year reported in the *Year Book*. While the chart is difficult to read, given the sheer number figures, the large number of up or down spikes indicate sudden, unsustained changes in estimates, the effects of a change in reporter or reporting standard. More rarely, they represent a printing error. The New York state estimate plunged from 905,000 in the 1913-14 *Year Book* to 90,604 in 1914-15, before rebounding to a reported 1,603,923 in the 1917-18 edition. While clearly a typographical error, the national total reflects the erroneous New York estimate.

Figure 2:5. American Jewish Year Book Population Estimates




*Source:* American Jewish Year Book.

The problematic nature of the estimates was widely acknowledged. Joseph Jacobs (1914) began his discussion of Jewish population statistics with a rather poetic lament:

> But, as a consequence, any attempts to ascertain the number of Jews, or of any other religious denomination, in the United States must take the form of estimates, which are notoriously untrustworthy, and as a rule overshoot the mark, because few persons are aware how large a few hundreds of human beings bulk in the real or imaginative eye. Yet, inadequate as such estimates usually prove to be, they are obviously better than nothing, or than the mere guesswork that often shoots wildly in such cases. (Jacobs 1914:339)

While local counts may result in overestimates, Jacobs noted that early population estimates were inadequate because they did not account for small populations of Jews. The list of towns Jews were settled in by the Industrial Removal Office far exceeded those enumerated in the *Year Book*, demonstrating the underenumeration was indeed a problem.

At various times the *Year Book* supplemented its state by state estimates with a different national estimate. The bases of such estimates were often problematic. The 1923-24 *Year Book*, for instance, heaped surmise on top of uncertainty by applying the natural increase of the entire population of the United States (despite the clearly different age structure of the Jewish community) on earlier state by state estimates derived from reports by key informants (Linfield 1923). Similarly, Linfield (1928; 1938) estimated the population of the cities with the largest Jewish communities using a combination of census estimates of the Yiddish speaking population and the number of Jewish school children, both of which are problematic for reasons I subsequently expand upon.

I cannot better Sophie Robison's (1943d) acerbic assessment of the value of the *American Jewish Year Book* estimates:

> In 1914-15 figures on the Jewish population in the United States and in individual communities were obtained by correspondence with key persons in the larger cities and in those smaller communities in which it was possible to obtain correspondents. Little reflection is needed to prove that a method which consists of adding one guess to another is entirely untrustworthy. Subsequently, instead of attempting direct contact with all of the numerous townships, the device of applying the results of the questionnaires returned from towns of similar size was employed to estimate the number of Jews in many hundreds of town of the same class. This is clearly a highly questionable procedure, since there is no way of telling whether the sample for which the data on Jewish population were returned is representative of the situation in all other communities of similar size. (Robison 1943d:2)

United Jewish Communities (the descendent of the Council of Jewish Federations and Welfare Funds) did not update the estimated published in the 2003 edition of the *American Jewish Year Book* as a result of its staff's "preoccupation with the delayed NJPS" (Singer and Grossman 2003:158), and decided to cease publishing estimates for good the next year. At the time of writing, the American Jewish Committee (publisher of the *Year Book*) has discontinued estimates rather than collecting the data on its own.

## COMMUNITY "SURVEYS"

An early and unusual genre of self-studies of Jewish communities has gone largely unnoticed in the literature. These "surveys" have their origins in the English social improvement movement and do not accord with surveys as the term is presently understood: a

study based on a representative sample of a given population, but rather the older sense of a survey of the landscape (Converse 1987).[9] The progenitor of this line of study is Charles Booth's (1840-1916) *Life and Labour of the People in London*, a massive multi-volume study of poverty in London undertaken between 1889 and 1903. Booth's work was based on interviews with school board visitors, from questionnaires of church workers involved in social relief, participant observation, and use of preexisting statistics garnered from diverse sources. The nearly contemporaneous 1896 study of the black community of Philadelphia by W.E.B. DuBois similarly drew on preexisting data and participant observation, but supplemented by interviews with the central population of interest. Closer still to Jewish communal studies were studies directed by Paul Kellog and sponsored by the Russell Sage Foundation. The first of these, a 1907 survey of Pittsburgh, drew upon existing sources of data rather than conducting interviews or sending out questionnaires. Further studies on this model were carried out, including a study of the Polish residents of Buffalo, New York.

This magpie-like approach to data collection may appear haphazard to contemporary readers accustomed to systematic data collection (whether quantitative or qualitative), but is a product of the social and technological environment of the time. Administering standardized questionnaires was difficult in a time when a high proportion of the population did not speak English and a notable fraction of those who did were illiterate. Surveying required a field staff of interviewers at considerable expense, raising the level of financial commitment required to mount a survey. Simply tabulating the results presented a major task, as the first tabulating machines for punch cards were only introduced in the 1890s and presumably were rather expensive (da Cruz 2004). Simply

analyzing the joint distribution of two variables defied automation until the card sorter was developed for the 1900 agricultural census (International Business Machines N.d.).

In addition to the practical difficulty of counting, much of the methodological apparatus of modern surveys was still lacking. Systematic sampling of human population was only introduced in the mid-1890s by Anders N. Kiaer, the director of Norwegian statistics, and was still hotly contested in the first decade of the twentieth century (Converse 1987:43). Much of the theory of undergirding human populations was developed for agricultural purposes (Converse 1987:41). Random sampling only began to take hold in the 1930s and its utility was still contested by proponents of quota sampling at the first meetings of the American Association for Public Opinion Research in the late 1940s (Converse 1987).[10]

Before turning to the early studies of Jewish communities, it is important to look at the difficulties of accessing early material. I fear that it is impossible to account for every study of a Jewish community. As an example, an early guide to Boston Jewry (Taylor 1924) is listed in only one library on WorldCat.[11] It is not unreasonable to think that similar documents might lie in poorly archived collections. Appendix B lists every demographic study of a Jewish community in the United States I am aware of.

It may also be the case that some reports never survived at all. I contacted Combined Jewish Philanthropies of Greater Boston, the successor to the organization that published the guide, regarding early studies, and was told that archiving was minimal. This holds true for much more recent studies, as well. Important methodological information on the 1995 survey of Boston Jewry proved to be impossible to locate. This is not to suggest that Combined Jewish Philanthropies is unusually lax. Maintaining working

archives for nearly a century requires considerable foresight and resources. Both tend to be in short supply. Rawidowicz's (1998) notion of the Jews as an "ever dying people," perpetually on the brink of catastrophe, only to survive to the next generation by the efforts generated by the fear of extinction, is a fair description of Jewish organizational life. Such an atmosphere necessarily focuses on present exigencies to the exclusion of organizational continuity. However abundant, resources in communal life are inevitably oversubscribed and directed toward the most pressing concerns. Another factor is that communal studies tend to be treated as internal reports rather than publications and may not be widely distributed, lengthening the odds they ever make it into a library. The early communal studies I discuss are the products of an extensive search of WorldCat listings and should be seen as illustrative examples.

*Worcester, Massachusetts, 1915*

The first study of a Jewish community directed at discerning its characteristics—other than Billings's (1890) study in conjunction with the 1890 census—comes in the form of a Master of Arts dissertation by Joseph Talamo (1915), who does not appear to have been Jewish.[12] This sympathetic study of patterns of assimilation in the Jewish community of Worcester, Massachusetts appears to draw upon what may be the first individual survey of American Jews (3,500 of them), though not perhaps a representative one.[13] Despite the apparent innovation of surveying ordinary Jews, I nevertheless classify Talamo with other surveys of the era because of the diversity of the material used.

Beyond the apparent survey, Talamo appears to have searched the directory for businesses with Jewish owners (presumably determined by their names), engaged in

observation (he noted the lack of space for children to play in tenements on Water and Harding streets, and the apparently unusually grimy nature of Jewish dwellings), examined records of a nonsectarian charity (9 of 690 were Jewish) and the Immigration Commission (1 of 220), talked to key informants including the overseer of the poor farm ("the Jews take care of their own," p. 32) and the town jailor ("they usually have the smallest percentage of any nationality," p. 32), and even took the trouble to enumerate the number of Jewish inmates (1 of 226).

In keeping with the broader genre, Talamo focused on assimilation and social problems (poverty and crime). Reinforcing this impression, Talamo's work was informed by the broader progressive effort: he cites a Jewish block in Pittsburgh studied by a Miss Reed, undoubtedly part of the Pittsburgh studies sponsored by the Russell Sage foundation, though unfortunately this reference was omitted from the bibliography. No attention is paid to ethnic behavior or religious observance; even the number of synagogues goes unremarked.

*Boston, Massachusetts, 1921*

The earliest Jewish-sponsored community "survey" I have found comes, fortuitously, from Boston. Ben Rosen (1921) studied the location of a new community building on behalf of the sponsoring agency, Federated Jewish Charities. This required estimating the current location and composition of the Jewish population and its future trends. Apparently, data-driven decision making was then rare in the Jewish organization world, as Rosen proudly noted that "instances where Jewish communities have planned the loca-

tion of communal buildings with direct reference to the future population are so rare as to excite comment" (p. 9).

Like many other studies of the era, data collection was limited to interviews with key informants and analyses of preexisting data.[14] Rosen paid particular attention to the general level of growth in areas of the city of Boston, showing that the Jewish population was concentrated in some of the fastest growing districts (Ward 21 of Dorchester, which is now Mattapan).[15]

To estimate the proportion of the population that is Jewish, Rosen used the Yom Kippur absence method (see pp. 69-74), making the assumption that the Jewish population had the same age distribution as all Massachusetts residents. However, as many Jews were immigrants and probably younger than the general population, it seems likely that children represented a relatively larger proportion of the Jewish population, and Rosen may have in fact overestimated the size of the Jewish population. Conversely, Jewish birth rates have always been relatively low compared to the population as a whole, so there is potential for underestimation. Perhaps mindful of the problems of extrapolating from age projections, Rosen also made an estimate for the Jewish population of the second precinct of Ward 21, using Jewish names found on the 1916 and 1920 "police lists" (municipal listings of residents). How Rosen defined Jewish names is undocumented and may or may not have been systematic.

*Other Studies*

The Boston study appears to be only the first of a series apparently focused on locating facilities for the social welfare of the Jewish population. No less than seven studies were

for communities in New York and New Jersey (and one in St. Louis) were published the following year (National Jewish Welfare Board 1922a; 1922b; 1922c; 1922d; 1922e; 1922f; 1922g), followed by studies by the Bureau of Jewish Social Research of Cleveland (Jewish Community Federation 1924) and Detroit (Bureau of Jewish Social Research 1923) the next.[16] The next known studies are of Greater New York (Bureau of Jewish Social Research 1928a; 1928b; 1929) and Omaha, Nebraska (Lapidus 1929), both by the Bureau. A Master of Arts dissertation was written at Columbia University regarding establishing a "Jewish community center organization" in Passaic, New Jersey (Levitan 1930).

## PROXY MEASURES

A proxy measure is simply a variable that is assumed to be more or less related to an unobservable characteristic of interest. For instance, social scientists will often use education as an indicator of socioeconomic status, even though the correlation between these items is imperfect. The charm of proxy measures is that one can use readily available data to estimate the absolute or relative size of a group with a particular characteristic that would be impossible or difficult (and expensive) to estimate in another fashion. The problem with proxy measures is that an imperfect relationship exists between the variable measured and the characteristic it is assumed to be related to. In the case of estimating the size of the Jewish population, accurate use of proxy measures depends on knowing the proportion of the Jewish population with and without the characteristic used as a proxy, as well as the proportion of the non-Jewish population with that characteristic. Because

there is no external source of accurate data about the Jewish population—which is why proxy measures are alluring in the first place—such data would have to be derived from a survey. Were survey data to be available, of course, proxy data would not be needed. Thus, the paradox of proxy estimates. Nevertheless, numerous attempts have been made to estimate the size of the Jewish population via proxy measures, which I review below.

*Census Derived Proxies*

While the U.S. Census does not ask about religion, several of its items can, with a little ingenuity, be seen as imperfect reflections of the Jewish population. The particular advantage of census proxies is that they allow for estimates of population characteristics as well as population size by cross-tabulating the proxy with other census items. For this to be realized, however, the proxy itself must be adequate.

*Immigration statistics.* From 1899, the Commissioner-General of Immigration kept records of the total number of Jews and other migrants from each nation (Jacobs 1914). Accordingly, it was possible to obtain the proportion of Jewish migrants from each country and multiply that figure by the total number of residents of the United States from that country, as reported in the U.S. Census. However, as Jacobs pointed out, the problem is that the Jewish composition of the migrant stream was ever-changing. The proportion of Jews among British migrants was relatively high in the years measured compared to British migration in previous times, for instance, due to the United Kingdom's role as an intermediate stop for many Jewish migrants from Eastern and Central Europe. On the other hand, Jacobs reports an increased level of non-Jewish migration from Russia and Austria-Hungary at the time of writing. Beyond these potential biases,

this method did not account for third and later generation Americans because the census only asked questions about the nativity of household members and their parents. Jacobs guesstimated a population of 150,000 based on little more than the traditional statistics, together with problematic assumptions regarding population growth (see p. 49). An additional problem, which Jacobs does not touch upon, is that of plural ancestry: having two parents born in different nations. It is not clear how Jacobs reconciled data from people whose parents (or grandparents) were born in different countries. These represented insuperable problems for deriving accurate population estimates, and the method was a curiosity rather than a viable proxy indicator.

Whatever utility this measure had was peculiar to its time and place. As a greater proportion of the Jewish population became third generation or later, greater and greater levels of reliance on other means of estimating the size of this population would have been required. With the end of mass migration a decade after the publication of Jacobs's (1914) article, this situation came to pass. More recently, the census has included an item on ancestry in general that would seem to obviate the problem, which I address subsequently.

*Yiddish speakers.* In addition to providing estimates based on migration, Jacobs (1914) developed estimates based on the number of individuals reporting Yiddish as their (or their parents') mother tongue in the 1910 census, the first time the question was asked. Naturally, not all American Jews spoke Yiddish or grew up in a Yiddish speaking household. (It seems reasonable to assume that no non-Jews had Yiddish as a mother tongue.) In order to develop estimates of the Jewish population, Jacobs therefore had to estimate the size of the Jewish population with parents that arrived before the period of

mass migration (see p. 49) as well as the number of first and second generation immigrants who did not report Yiddish as a mother tongue. Jacobs estimated the number of non-Yiddish speaking first and second generation Americans at 250,000 without any explanation of the derivation of the figure.

As was discussed previously, the 1927 and 1937 *American Jewish Year Book* estimates for the largest Jewish communities drew upon census estimates of the number of Yiddish speakers (Linfield 1928). The procedures adopted were no more scientific than those of Jacobs. To cover apparent underreporting of Yiddish and overreporting of Russian as a mother tongue in the 1920 census, Linfield made a complicated and flawed estimate of the number of non-Jewish Russian speakers on the basis of people with "Slavic and Lettic mother-tongues" (p. 167). To estimate the "corrected" proportion of Jews in New York with a Yiddish background, he used Laidlaw et al.'s (1932) estimate of the Jewish population of New York as the denominator. *Ad hoc* adjustments to this ratio were made for other cities, not informed by any data, scientific or otherwise.

*Russian ancestry.* The most recent proxy-based panacea for estimating the number and characteristics of American Jews was the Russian ancestry approach, which was introduced in articles by Rosenswaike (1963) and Rosenthal (1975). Rosenswaike (1963) examined data from the 1935 local census of Hamilton County, Ohio encompassing Cincinnati that included questions on religion and ancestry. He showed that areas of Jewish concentration had the highest proportion of foreign born whites, and more particularly the greatest proportions of the Russian-born population.

Rosenswaike's confidence in the method was, however, belied by his own data. He wrote that "the strikingly high correlation between the distribution of the Russian-

born and the Jewish population in the classic Cincinnati portrait must be emphasized for it is precisely the absence of data along religious lines elsewhere which necessitates the use of statistics by birthplace to secure some reliable indication of the extent and distribution of other American Jewish communities" (pp. 46-47). Elsewhere, however, he noted that the proportion of foreign-born Jews in census tracts ranged from 40 percent to 13 percent (p. 45). In other words, while the Russian birth method worked (in 1935) to identify areas of Jewish population concentration, it could not be used to estimate the size of the Jewish population. This latter point is a recurrence of the usual proxy paradox—accurately estimating the size of the Jewish population from a proxy measure requires knowing the proportion of Jews and non-Jews with the proxy characteristic.

Even the modest achievement of identifying Jewish areas was, however, problematic. As the Russian-born population was presumably concentrated in areas of first settlement, estimates of population concentration would likely be biased in that direction. Nor could the Russian-born population be used to understand the characteristics of the Jewish community. Native-born Jews on the whole had higher socioeconomic status than did immigrants, rendering estimates of the characteristics of the Jewish population biased.

An additional problem arises should one wish to compare communities. The ancestry of Jewish communities in the United States differs considerably from one to another. To take two recent examples, recent migrants from the former Soviet Union are concentrated in the New York City area (particularly around Brighton Beach in Brooklyn) and Jewish migrants from Arab countries are particularly common in New York City and parts of New Jersey and in Los Angeles. The same holds true for historic Jewish migra-

tion. Earlier migrants from German-speaking countries had different settlement patterns to Eastern European migrants. Estimates derived from the Russian-born population would be biased in favor of areas that were favored destinations of Eastern European migrants.

With the end of mass migration in the early 1920s, the proportion of Jews who were Russian-born (or even had Russian-born parents) decreased steadily over time, requiring greater and greater extrapolations from a declining pool of cases. A potential remedy was introduced in the 1970 census, which asked "What is your origin or descent?" in addition to the usual questions on parental nativity. ("Jewish" was not an acceptable option and was counted as "unclassified" together with answers like "white," "American," and "mixed.") There are two significant and related problems with this question. First, intermarriage between national origin groups ensures that most third or later generation Americans have ancestors from multiple countries. While the Jewish intermarriage rate is comparatively low, it has increased significantly from the 1970s, leading to increased difficulties for ancestry-based methods. Even people with two Jewish parents are likely to have ancestors who migrated from different nations. Second, as Waters (1990) convincingly demonstrated among white Catholics, recall of ancestry is a subjective process. Certain ancestries (e.g., Irish, Italian) are "popular" and more likely to be remembered than others (e.g., Scottish, French). It is not clear how Jewish ancestry would be treated. Even if Jewish ancestry is relatively popular, contemporary Jewish culture appears to have collapsed ancestry into a generic "Eastern European Jewish" or even "European Jewish" category that does not comport with the census question. Rosenthal's (1975) presentation, like Rosenswaike (1963), whom Rosenthal cited extensively,

was limited to showing the rough equivalency between the 1957 Current Population Survey estimates of the characteristics of the Jewish by religion population and the Russian ancestry population in the 1970 census and several subsequent iterations of the Current Population Survey. While it seems beyond doubt that most Russian migrants to America were Jewish and that most American Jews have some Russian Jewish ancestry, the problem is that the relationship is imprecise enough that it is impossible to know with any confidence how much these estimates differ from the Jewish population.

*Yom Kippur Absence*

The Yom Kippur absence method is perhaps the most original of all proxy measures for estimating the size of the Jewish population. The method used is the essence of simplicity. One simply subtracts the average number of absentees of absentees from the public school system from the number absent on Yom Kippur to estimate the school age Jewish population, and multiplies this estimate by the inverse of the expected proportion of school age children in the Jewish population as a whole. The originator of the Yom Kippur absence method is unclear. Ritterband et al. (1988) noted it was first used in London in 1892 and Philadelphia in 1904, citing an unpublished mimeograph of much later date.

Originality, unfortunately, does not result in accuracy. Robison's (1943d) terse and well reasoned critique ably presents the major problems:

> The "*Yom Kippur* method" makes several assumptions, some of which beg the question. Stated briefly, the reasoning behind this technique for calculating the number of Jews in a large city is that *all* Jewish children attend public schools and that all Jewish parents keep their children out of school on *Yom Kippur*. The number of absences from each public school on that day, corrected for the expected number of absences for each school, it is

> assumed, will equal the number of Jewish children at each school. The proportion that the school children bear to the total population in a census year is the co-efficient by which the number of Jewish children is multiplied to obtain the estimate of the total Jewish population. The validity of this calculation is based on the unproved assumption that the proportion of school children in the Jewish population is identical with that of the school population in the total population. (Robison 1943d:2)

Concerns about the accuracy of the Yom Kippur absence method were far from purely theoretical. Taylor (1943) directly compared the number of Jewish students in the Pittsburgh school system (which apparently recorded students' religion) to survey data, and found the population was underestimated by 20 percent. This was a product of low birth rates. The estimating system expects a normal age distribution for the Jewish population when in fact the age pyramid is elder-biased (the end of mass migration led to a rapid increase in the age of the Jewish population). In fact, school age children constituted about 15 percent of the Jewish population, compared to 22 percent of the general population. Robison's (1943a) own study of Minneapolis Jewry showed that Jews had much notably lower birth rates than native-born whites, which would lead studies based on the Yom Kippur absence method to underestimate the size of the Jewish population.

In certain circumstances, the Yom Kippur absence method did, however, provide accurate estimates. The *American Jewish Year Book* estimate of New York City in 1953 was based on this method and estimated that Jews constituted 26.4 percent of the population, while a sample study undertaken by the Health Insurance plan of Greater New York estimated the proportion of Jews at 27 percent (Chenkin 1955). One reason for the more accurate New York estimate was undoubtedly that Jews constituted such a high proportion of New Yorkers that census estimates of the city's age composition were deeply

influenced by the Jewish community. This accurate estimate may, however, have been a fluke. The New York Federation of Jewish Charities used the Yom Kippur absence method with 1957 data and arrived at a much higher estimate of the Jewish population than did the Health Insurance Plan (Chenkin 1960; 1961).

Robison rightly directs attention to the problematic assumptions that all Jewish children were in public schools and that all Jewish children were kept out on Yom Kippur. To the extent that the Yom Kippur absence method was ever effective, its utility depended on the social structure of the first half of twentieth century. With the general lessening of prejudice against Jews after the Second World War, it became increasingly possible for Jews to be enrolled in private schools, especially as more such schools were established without religious affiliation (or became less overtly religious). The so-called Jewish love affair with public schools has also waned, with Orthodox Jews the first group to depart in significant numbers with the establishment of day schools following the Second World War, later picked up by the Conservative movement's establishment of Solomon Schechter schools, and the more recent establishment of Reform and community day schools. The universality of Yom Kippur observance is also a problematic assumption. Some secularists took particular care to disregard *halakhah*, Jewish religious law. The famous Grand Yom Kippur Balls of the Lower East Side of New York City that celebrated the new year with "Kol Nidre, music, dancing, buffet; Marseillaise and other hymns" (Goodman 1971:330). While ideological secularism may have been confined to a relatively small proportion of the Jewish population, increasingly large portions of the Jewish community arrived at a more workaday version through a general decline in the salience of Jewish identity, further depressing estimates of Jewish population.

As the most significant set of estimates based on the number of Jewish school children, Linfield's (1928) defense of the method is worth considering. He concedes that the ratio of Jewish children to adults probably differs from that of the population at large:

> The ratio of Jewish children to the total number of Jews is probably larger than the ratio of the whole number of children to the total population. These errors would have a tendency to render too large the estimate of the number of Jews arrived at on the basis of this method. On the other hand, it is undoubtedly incorrect to assume that fully 100% of the Jewish children in the large cities are recorded as absent on the day in question. A certain percentage are undoubtedly recorded as "present." From this point of view, the number of Jews arrived at would be too small. Is it not possible that the errors neutralized one another or nearly did so in the fall of 1927? (Linfield 1928:172)

Far from the Jewish community having a higher birth rate than the non-Jewish population, Taylor (1943) and Robison's (1943a) studies of a decade later found precisely the opposite, upsetting Linfield's precariously balanced offsetting biases. That Linfield saw fit to use the same method 10 years later when its problems were well known is deeply problematic. He was harshly critiqued by the chairman of his own advisory committee (Ritterband et al. 1988), as well as his successors at the *American Jewish Year Book*, who wrote that:

> The national estimates of Jewish population made by the Jewish Statistical Bureau in 1926 and 1936 in conjunction with the decennial Census of Religious Bodies of the United States Census Bureau might have been thought by some lay readers to be sufficient, but demographic experts have found it to be adequate only for purposes of rough calculation. (Seligman and Swados 1949:651)

Considering its glaring flaws, the Yom Kippur absence method was used for an unconscionably long time. Its apex appears to have been around 1927, when Linfield (1928) reports the *American Jewish Year Book* estimates received from local reporters for New York City, Newark, Boston, Detroit, Philadelphia, St. Louis, and Baltimore "checked on the basis of the number of children that abstained from attending school on the Day of Atonement in the fall of 1927" (p. 115). In Cleveland and Pittsburgh the number of Jewish students was estimated by social workers. The only other cities estimated to have 50,000 or more Jews not checked via school records were Baltimore, Chicago, and Los Angeles. The final counts for these cities were apparently leavened with "the number enumerated in the census as persons of foreign birth with Yiddish as their mother-tongue and their children" (p. 118). The manner in which these estimates were combined is not presented. Linfield's estimate is particularly problematic as he took a single day as being representative of the overall level of absenteeism rather than averaging truancy across a longer period. He did not detail the basis by which he extrapolated from the putative population of Jewish children to the total Jewish population other than to refer to it as "the coefficient to the given city," which appeared to assume that Jews had the same age structure as the general population which was deeply problematic and known to be so at the time. The Yom Kippur absence method was used for estimates of larger Jewish communities in conjunction with the 1936 Census of Religious Bodies, a practice that continued throughout the 1950s (Seligman 1950; 1951b; 1953). Seligman and Chenkin (1954) used the method to calculate the size of the Jewish population of New York. The final use of the Yom Kippur absence method I am aware of was an estimate of the Jewish population of New York in 1968 (Chenkin 1969). The fact that a method known to be flawed

and inaccurate could be used to estimate the size of the largest Jewish community in the world when scientific methods of sampling were well established and had been used for Jewish populations is astonishing and demonstrates a basic disinterest in obtaining accurate data.

*Distinctive Jewish Names*

The idea behind the distinctive Jewish names method is extremely simple: Jews whose names are identifiably Jewish are used as stand-ins for Jews whose names cannot be so distinguished. It is used in three ways. Distinctive Jewish names can to generate estimates of Jewish communal size by itself. This requires a list of the total population of interest, as well as estimates of the proportion of Jews with "non-Jewish" names (false negatives) and the proportion of non-Jews with "Jewish" names (false positives). Like other proxy methods, distinctive Jewish names rises or falls on the accuracy of these estimates, as well as the coverage of the list of the total population. (Telephone directories, for instance, will omit those who cannot afford a telephone, overestimating the size of the Jewish community to the extent that Jews are of higher socioeconomic status than the rest of the population.) Of course, accurate estimates require survey data, obviating the need for distinctive Jewish names. The second way in which "Jewish" names can be used applies to the rare circumstances in which preexisting data is available with surnames, allowing estimates of characteristics other than the size of the Jewish population. The final way in which distinctive Jewish names can be used is as a sampling method. Once again, one uses a list of Jewish names, but surveys them rather than simply counting

them. This approach, which eliminates the problem of false positives, is discussed subsequently (pp. 118-128). I will focus on the use of distinctive Jewish names as a proxy.

*Estimating Jewish population size.* Ritterband et al. (1988) identified the first use of the procedure with Joseph Jacobs's (1912) study of New York Jewry which he termed the "Cohen" method. Jacobs, as Ritterband et al. described, underestimated the size of the New York Jewish population by drawing on a biased list of Jewish names: donors to the German Jewish United Hebrew Charities. Distinctive Jewish names of Eastern European origin tended not to be present on this list, consequently underestimating the size of the Jewish community. In cities with large populations of non-Jews of German descent, this method may lead to overestimates of the Jewish population (Abrahamson 1986).

The distinctive Jewish names method was expanded and popularized by Samuel C. Kohs in the 1940s (American Jewish Historical Society 1946), though it was never a primary component in population estimates.[17] The methodology by which Kohs's list was developed remains obscure, which is unfortunate as contemporary lists of distinctive Jewish names are very similar (see Appendix C for a comparison of several lists of names and Appendix D for a list of the most commonly occurring surnames in the 2005 Boston Jewish Community Study). Lists of "Jewish" names still tend to be Euro-centric. As this may not be appropriate for parts of the United States with large numbers of Sephardic or recent Russian migrants some attempts have been to construct supplementary lists (although these have been used for sampling rather than as a proxy measure). Ira Sheskin, for instance, created a list of Russian Jewish first names as a part of his recent survey of Minneapolis-St. Paul (2004c) and a list of distinctive Sephardic surnames for his study of Monmouth County, New Jersey (1997a). Lists of "Israeli" names could also be devel-

oped. Another source for Jewish names that has been used occasionally (Immerman and Jacobs 2004) are commercial databases of ethnic surnames. Unlike lists of distinctive Jewish names, these data bases assign all names to an ethnic category based on both first and last names. Someone named David Phillips, for instance, might be classified as Welsh, while an Antwan Phillips may be classified African-American or a David Ortiz as Latin American. The derivation of these classifications is proprietary and almost certainly extremely complex. I discuss such databases at greater length on p. 249.

The utility, such as it is, of distinctive Jewish names is predicated on a distinctive Jewish population. While this was a reasonable assumption for Jacobs and Kohs to make, it is less so now. The growing proportion of intermarried Jews (Phillips and Fishman forthcoming) drives up the rate of false negatives, cases where a Jewish person or household is classified as non-Jewish. In addition, to the extent to which the children of intermarried couples do not identify as Jews in later life, there will be an increasing proportion of false positives, non-Jewish households with "Jewish" names. As the proportion of intermarried households may increase relatively rapidly, contemporary estimates of the rates of false positives and negatives are required and, as the rate of intermarriage differ dramatically from one area to another, cannot be inferred from data from other communities. The paradox of proxy measures thus comes into full play—accurate use of distinctive Jewish names requires survey data that render it irrelevant.

*Estimating Jewish population characteristics.* Where preexisting data containing surnames is available, distinctive Jewish names can be used to estimate information on population characteristics. While household returns for the U.S. census are not made available to researchers for an extended period of time, the same is not necessarily true of

state censuses. While these may lack information on religion and/or ethnicity, they have the names of household members, which can be used to identify Jewish households via the distinctive Jewish names method. Henry J. Meyer (1943), for instance, estimated the sociodemographic characteristics of Detroit Jews from the 1935 Michigan Census of Population and Unemployment. While representing a significant advance on typical implementations of distinctive Jewish names insomuch as characteristics of the population other than size can be obtained, the fundamental problems remained unanswered. Accurate estimates require knowing both the proportion of Jews with "non-Jewish" names and the proportion of non-Jews with "Jewish" names. Meyer (1943:109) claimed 90 percent accuracy in a test, although he failed to separate these two figures. It is also unclear whether he relied on intuition or a list of Jewish names.[18] In addition, as Meyer acknowledged, it is also possible that Jews with distinctive names may differ from other Jews.

Meyer's (1943) survey is notable for another reason: he referred explicitly to the use of Hollerith cards in analyzing the data from 3,016 schedules of households with "Jewish" names. As I mentioned earlier, the lack of a rapid means for tabulating and cross-tabulating data was a major impediment to Jewish and other studies around the turn of the century from both the perspective of cost and depth of analysis. Given that most sponsors want results relatively quickly, the slower the methods employed for tabulation, the more basic the statistics presented will be. Talamo (1915), for instance, presents no cross-tabulations at all, while Meyer (1943) is able to present highly detailed tables drawing on up to four variables. The costs of using punch cards were, however, not inconsiderable, as nearly 9,000 cards needed to be hand-punched for the Detroit study. All later studies included cross-tabulations, indicating wider availability of card sorters.

In another example of this method, Alexander Dushkin and Meir Isaacs drew a random sample of "school census" cards for 4,200 families (representing 10,332 children) kept by the New York City Board of Education, covering both public and private schools. They determined which families were Jewish by the first and last names of the parents, their nativity, year of immigration, country of emigration, the occupation of the father, the school attended, and the place of residence (Oppenheim 1918). Unfortunately, the accuracy of such a method is unknown unless the estimates are verified by, for example, contacting some or all of the households and directly determining whether the inhabitants were Jewish. This appears not to have happened. In addition, being an estimate of Jewish school children, extrapolating to the total Jewish population required the additional step of estimating the proportion of the Jewish population made up of school children. The resulting estimate of 1,412,500 was, however, dismissed as too conservative by Oppenheim when compared to labyrinthine but unsystematic calculations involving Jacobs's (1914) unsatisfactory estimates derived from immigration records, census data on Yiddish as a mother tongue, the rate of natural increase, a guesstimate for internal migration, Jewish death rate information from the actuaries of the Metropolitan Life Insurance Co., a guesstimate of recent changes in the Jewish marriage rate, and the (unjustified) assumption that the Jewish birth rate was no lower than that of other New York residents.

*Jewish Funerals*

Using Jewish deaths to predict the size of the living Jewish community rests on a slightly different set of assumptions than the other proxy measures discussed. Instead of knowing the proportion of the Jewish and non-Jewish populations with a given characteristic,

accurate estimation using data on deaths requires knowing the proportion of total Jewish deaths represented by the enumerated data. Determining whether a given death was Jewish was usually determined by a combination of the funeral home, cemetery, and the name of the deceased. While this was probably relatively accurate in the first half of the twentieth century, the ability to determine which decedents are Jewish has almost certainly declined with the diminishing effectiveness of distinctive Jewish names as a criterion and the likely growing number of Jews opting for cremation and/or burial in a general cemetery. Knowing the number of deceased Jews is only part of the problem. It is also necessary to know the death rate of the Jewish population. This is the major stumbling block, because accurate estimation of the death rate requires knowing the size of the living population, at which point using death rates would be moot. Attempts to determine the size of Jewish population by this method inevitably tie themselves into knots estimating the death rate, with invariably unsatisfactory results.

Another problem with using Jewish funerals is the sheer difficulty of getting such data. Jacobs (1914) reports that the Bureau of Jewish Statistics attempted to obtain the number of Jewish funerals for St. Louis, Louisville, Philadelphia, San Francisco, Boston, Cincinnati, Kansas City, New York, and other cities but "the returns came in so slowly that it was decided for the present to abandon this method" (p. 356).

*Maryland, 1903.* The first known use of the death rate method in the United States was by George Barnett (1903) of Johns Hopkins University for the Jews of Baltimore.[19] The use of death rates was explicitly conditioned on cost: "It is not possible without a very large expenditure of money to count the number of Jews living in large cities such as Baltimore" (Barnett 1903:46). Barnett consulted the Baltimore Health Department's

record of deaths and determined whether a decedent was Jewish by the cemetery in which they were buried. To account for the limited number of cases in which Jews were buried in non-Jewish cemeteries, the renowned Henrietta Szold determined Jewishness based on names and her own knowledge of the Baltimore Jewish community. Barnett recognized the problems associated with assuming that Jews had the same death rates as other white Americans, noting that Jews tended to live longer than their socioeconomic peers due to "racial longevity" (p. 48) and that as a primarily immigrant population, the Jewish community would include relatively few newborns and elders (both highly vulnerable populations). These tendencies were, however, mitigated by the poor conditions in which the new migrants lived and worked. To partly control for these problems, Barnett had Szold classify decedents into immigrant and nonimmigrant categories based on the choice of burial ground and her considerable knowledge of the Baltimore Jewish community. The non-immigrant death rate was guesstimated at 14 per 1,000, based on Billings's (1890) estimates for the Census Bureau and the death rates of Jews in Germany. The immigrant death rate was estimated to be 13 per 1,000 using U.S. Census records for Russian migrants in New York City, the vast majority of whom were Jewish. Characteristically for early studies, Barnett did not use a "pure" death rates method. Besides Szold's apparent use of distinctive Jewish names and census data on people of descent as a proxy for recent Jewish migrants, Barnett determined the Jewish population of the rest of Maryland from local reporters in communities outside Baltimore. The question, of course, is whether these assumptions were accurate. While this was a valiant effort, the problem is that even one false assumption can lead to a biased estimate.

*Other studies.* Like the earlier Maryland and New York death rate estimates, Jaffe's (1943) estimate of the Jewish population of Chicago in 1930 was based on death certificate records of the undertaker used and birth and death rates obtained directly from data on Jewish households. Unfortunately, no further details are given about the methods used to derive birth and death rates, which were taken from Hauser's (1938) unpublished dissertation on differential fertility and mortality rates among ethnic groups in Chicago. Chenkin (1955) reports that studies in Detroit and Chicago used the death rates method.

### *Evaluating Proxy Measures*

The use of proxy measures to study Jewish populations is a study in failure. While these efforts were highly creative, they foundered on the lack of reliable external data on the Jewish community that would have provided the necessary means of understanding the relationship between the measure used as a proxy for Jewishness and Jewish identity. The irony is that external data would have made proxy measures irrelevant. To make up for the lack of external data these studies used other proxy measures to build up their estimates, failing for lack of a firm place on which to stand. As the shortcomings of these castles built on air were well understood in their own times, the tenacity of such methods is nothing short of astonishing. Their continued use in the largest Jewish communities decades after better alternatives were available represented nothing less than a dereliction of duty on the part of the Jewish Federations responsible.

## LIST-BASED CENSUSES

I now turn to actual studies of Jewish populations which collected data on individuals and households, allowing far more detailed analyses that were possible with institutional censuses, proxy data, or anecdotal reports. Surveying Jews tautologically requires finding Jews to survey. The first method used for this purpose involved lists kept by Jewish organizations.

*Minneapolis, 1936.* The earliest use of lists by a Jewish organization to study a Jewish population is unclear, but apparently predates 1936, as Robison's (1943a) account of the 1936 census of the Jewish population of Minneapolis refers to following "the methods outlined by the Council for Jewish Federations and Welfare Funds for gather Jewish population data in small communities" (p. 152). For this study she developed a master list from subscribers to the Minneapolis Jewish Federation campaign, members of Jewish women's organizations, families known to the Jewish Family Welfare Society, Jewish families on public welfare, members of the Downtown Talmud Torah, and members of two (unnamed) large synagogues. To account for households not included on the list, "the 1935 Minneapolis City Directory furnished many additional names of families presumably Jewish which did not appear on organization lists" (Robison 1943a:152). This use of distinctive Jewish names does not cause problems for the sample, except with respect to cost and efficiency, as the Jewish status of each household was directly determined by an interviewer. In addition to these methods, Robison described a "house-to-house canvas in the more densely populated Jewish sections of the city and, in the more sparsely settled sections, a canvass of those households which were designated as Jewish" (p. 152). Beyond these methods, every surveyed household was asked to list the names of other

Jewish families who lived in the neighborhood, presumably a labor intensive process for those with many ties. The proportion of households that did not respond is not given.

The feasibility of such an approach depends on factors specific to the time and place. Canvassing all houses and apartments in high density areas requires a ready supply of inexpensive but intelligent interviewers. Two factors came together to make this possible. First, the Depression must have eased labor costs and increased the supply of suitable interviewers. Second, although Robison does not say one way or the other, it is plausible that such studies drew on Jewish women as volunteers. As Sylvia Barack Fishman (1993) has noted, organizations like Hadassah furnished careers for married Jewish women, with a managerial hierarchy ascended by virtue of training, skill and seniority, considerable responsibilities, and long hours, differing from the rest of the workforce mainly in the matter of pay (or the lack thereof). The 1938 studies of Jews in Buffalo, New York and Pittsburgh, Pennsylvania, drew on another kind of cheap labor, using college and graduate students (Engelman 1943; Taylor 1943). Even with an inexpensive source of suitable labor, enumerating a large Jewish community would stretch virtually any organization to the breaking point, all the more so when a large central office staff would be needed for tasks that are now easily computerized: tracking interviewers, making sure all addresses are visited (but not surveyed multiple times), collating completed interview schedules, and maintaining and updating the master list of Jewish households. In addition, the research plan rested on a specific set of characteristics of the Jewish community. The community must be concentrated in certain locales, those areas must be very densely populated by Jews, and few Jews should live beyond these areas. Were the bulk of the Jewish community not to be found in a few neighborhoods, a much larger

section of the city would have to be canvassed in its entirety, with concomitant increases in cost and difficulty. It is also necessary that Jews be strongly connected to Jewish organizations and fellow Jews—having "bonding social capital" in Putnam's (2000) terminology—otherwise the sampling scheme would be inadequate. Should a large number of Jews have not belonged to Jewish organizations or had social ties to other Jews, they would have gone unenumerated, causing serious bias. Finally, the use of Jewish names to identify otherwise unknown households assumed that a high proportion of Jews had distinctive names shared by few non-Jews.

While such a procedure may seem unscientific to contemporary eyes, it is important to recall that few alternatives then existed. Neyman's key papers on sampling had just been published and more accessible material would not be available until after the Second World War. Evaluating the adequacy of a contemporary survey is difficult enough, but nigh unto impossible at such a distance. Bearing in mind these limitations, it remains a worthwhile exercise. It is likely that the study was, in fact, quite accurate. Relying on a relatively tight-knit community was appropriate at a time when Jews were still excluded from large portions of non-Jewish society, were relatively close to the immigrant experience, and were concentrated in ethnic neighborhoods (including areas of second or third settlement). The study was well designed to take advantage of these factors, with multiple ways for household to be included (living in a high density area, belonging to any of a number of Jewish organizations, having a distinctive Jewish name, or having Jewish friends). Soon, however, the social environment would change again.

*San Francisco, 1938.* Similar studies took place in Trenton and Passaic, New Jersey, Norwich and New London, Connecticut, and San Francisco, California. The 1938

San Francisco study (Moment 1943) based its master list on 70 Jewish organizations, supplemented by Jewish hotel guests (presumably determined via their names), and names obtained from other Jewish households by a presurvey mailing and via interview. The study initially intended to enumerate all known Jewish households, but was forced to undertake a systematic sample of one in three Jewish households after the study was about halfway complete, due to financial difficulties. This was accounted for by assigning a weight of three to the sampled cases, the first known use of weighting in a survey of an American Jewish community. Weights are used in surveys to compensate for unequal probabilities of selection and to project to the total target population. In this instance, had these cases not been "weighed" (essentially counting each case three times), they would have ended up being underrepresented in the final sample, together with any characteristics that were more common among this group.

To understand undercoverage, complete enumerations were undertaken in ten groups of four blocks in different census tracts. How these tracts were assigned is not clear, although it does not appear to be determined by Jewish population incidence. In any case, relying on such a small sample introduces considerable uncertainty into the estimates of the total Jewish population, although the concept of sampling error was not widely appreciated among social researchers at the time. Indeed, no list-based study of a Jewish population included any reference to sampling error. Nevertheless, this was a sophisticated and ambitious community study by the standards of the day.

*Trenton, 1937.* The 1937 study of Trenton, New Jersey forewent a house to house canvass of high density areas, but added Jewish names on the check-lists of ward leaders (the political affiliation of the ward leaders is not mentioned), as were married sons and

daughters listed by their parents as living in Trenton (Robison 1943c). The fact that no attempt was made to interview households of unknown status in high density areas is problematic, as the seed of the sample is limited to those with Jewish names or who were members of Jewish organizations. Nevertheless, the breadth of the list sample was likely enough for adequate coverage.

*Passaic, 1937.* The Passaic study of 1937 cut yet more corners, limiting the initial seed to Jewish names and contributors to the Jewish Welfare Fund (Robison 1943b). Rather than asking for the addresses of all Jewish families, Jewish households were only asked about families on their block. Instead of complete enumeration of blocks known to have Jewish households, only "two or three addresses at which no Jewish family was listed" were canvassed (p. 22). On streets with no known Jewish households, "three houses on each side of the block were canvassed and inquiry made in the stores" (p. 22). This design problematically mixes features of a census (complete enumeration of known Jewish families) with those of a sample (probabilistic sampling of unknown households). Absent weighting for the different probabilities of selection, the results will be biased toward the most connected Jewish households. Had a master list developed from many organizations been used the potential for bias would be mitigated somewhat, but this was not the case. The procedure for selecting households may also have been problematic, if left to the interviewers' discretion, as they may have opted for the more inviting looking residences.

*New London and Norwich, 1938.* The 1938 study of New London and Norwich (Wessel 1943) went further in the direction of using the list frame by completely omitting any households not found on organizational lists or known to other Jewish households. Its

major innovation was its use of multiple modes of contact, first mailing questionnaires to all known Jewish households and then following up in person with the 75 percent that did not respond.

The preoccupations of these studies remain true to the social welfare model of their predecessors. Migrant status and naturalization remained a key concern, as did occupation. The move of second and later generation Jews from trade to the professions was noted. Fertility was also of interest, with studies of this era typically noting the low Jewish birthrate. Other than brief mentions of intermarriage (Robison 1943c) and participation in Jewish schools (Moment 1943; Robison 1943b), the focus on Jewish behavior that is the bread and butter of contemporary population studies is entirely absent, speaking to insecurity about the community's socioeconomic standing coupled with the implausibility of a serious decline in Jewish distinctiveness.

*Bureau of War Records Studies.* An interesting lineage of list studies has largely been excluded from treatments of surveys of Jewish communities. During the Second World War a massive effort was undertaken by the National Jewish Welfare Board to memorialize and enumerate Jews serving in the armed forces (Bureau of War Records N.d.; Kohs 1944; 1945a; 1945b; Kraft 1945).[20] This undertaking was prefigured in the First World War, where a record of service was developed from a combination of the reports of agents of the Jewish Welfare Board at camps, ships, and the front lines, posters with mail-back cards in camps, Jewish newspaper articles, Jewish organizations, and official records to show that "the Jews of America are acquitting themselves magnificently, as soldiers and citizens, in this war; that their contributions of men and means tend to exceed, by a generous margin, their due quotas; that the Jewish soldiers at the front fight

with no less valor than their comrades; that their losses are as great—and their rewards no less" (Leavitt 1918:112). The same desire to preempt antisemitic accusations that Jews shirked their duties undergirded the later effort. Kohs (1945a) wrote that:

> Subversive elements are at work sowing seeds of suspicion and distrust regarding the activities or the lack of patriotism of various racial, religious, national, ethnic, social, and economic groups. Jews are not immune to such attacks. We must be prepared with the facts and we must publicize the truth. It is that ounce of prevention which will immunize many innocent minds against the poisons and infections of anti-Semitic propaganda. (p. 153)

To fulfill this need, Jewish communities across the United States were urged, and if necessary coerced, to conduct local studies to determine the proportion of eligible men and women serving in the armed forces. A byproduct of these efforts was a record of the size of the Jewish community and information on its age distribution and occupational patterns. The methods used in large communities are unclear. The New York figures mention confidence intervals for the first time in regard to a study of a Jewish community, suggesting that some kind of sample was used, although the report itself is not available to provide further details. Much better documentation is available for small to medium communities in the form of the *Handbook of Instructions for Survey Consultants* (Kohs 1944). The instructions contained in these records are identical to the methods used in the early list studies described above. A master list was to be developed from Jewish organizations, references from other Jews, and likely names in telephone or other directories. Where the Jewish population was densest, a house to house canvass was to be

considered. Most surveys were to be completed in-person by an overwhelmingly female corps of volunteers.

The essence of these surveys was haste rather than detail or quality. The schedule for a survey of Denver in 1944 given as an example by Kohs (1944) called for a field period of four days, including the tabulation of returns (June 12-15), a day to finalize matters (June 18), and a week to write the report. The data gathered was extremely limited, containing only the names of household members (both at home and in the service), each member's relationship to the head of the household, occupation, and whether or not they were in the service. Surprisingly, no attention appears to have been paid to whether household members were Jewish. Kohs (1944) includes a sample interview schedule with a Denver family that consisted of Henrietta McGillis (mother, age 62, a housewife), June McGillis (daughter, age 40, a bookkeeper), John H. McGillis (son, age 34, a policeman serving in the Marines), Sarah Jane McGillis (daughter, age 30, a beautician), and Joseph Levy (boarder, age 57, retired). While names can be deceiving, McGillis certainly is not a common Jewish name and commonsense would indicate that the household was surveyed because of the boarder. Nevertheless, the example includes a war record card for John H. McGillis, who presumably was counted in estimates of the record of service of Denver Jewry. Whatever their worth, very few of these studies are available today. None of the various communities that conducted studies attested in the sources I have cited has a report that appears in any library catalogue.

## SURVEYS OF JEWISH POPULATIONS

To this point all list studies described, with the possible exception of the war records study of New York, have been censuses, which attempt to contact every household in the study universe. The great advantage of a survey, which randomly selects a sample of the target population, is one of cost. Instead of trying to make contact with 10,000 Jewish households, one might need to contact 1,000 of them in order to have acceptably precise estimates of the size and characteristics of the Jewish community. The resistance of larger Jewish communities to actually studying their members, as opposed to relying on unreliable proxy measures, may be in large measure have been a reflection of the underutilization of surveys in studies of Jewish communities.

In this, the Jewish community was a part of its time. Public opinion pollsters and market researchers used surveys throughout the 1930s, but their use outside this niche was limited (Converse 1987).[21] The Second World War played a critical role in the development of surveys in the United States with a veritable explosion of survey-based programs. These programs impacted the development of survey research as a field in a variety of ways. They trained a generation of social scientists in survey methodology who went on to staff the polling companies and academic research centers, in addition to those alumni who went on to other academic, commercial, and governmental positions and would demand such data. The experience of the Second World War also whetted the government's appetite for survey data, with a massive increase in social scientific research following the war. To this day, government-sponsored surveys constitute a tremendous proportion of the work carried out by commercial and academic survey research centers and continue drive developments in survey methodology. The Second World War

also saw a continuation in the development of the methodology of sample surveys and, more importantly, established them as the gold standard for government-sponsored surveys, moving the field in this direction.

*Proto-Surveys*

The prewar studies of Minneapolis, Passaic, Trenton, and San Francisco shared some features with purely geographically-based samples that was then a hallmark of surveying, supplementing lists of Jewish households derived from Jewish organizations and other sources with some type of canvass of households of unknown status, at least in high density areas.

*Buffalo, 1938.* The first known use of an entirely geographic approach to studying a Jewish community is in the 1938 study of Buffalo, New York (Engelman 1943). The city was subdivided into what were effectively two strata by the expected density of Jewish population. Strata are simply mutually exclusive and exhaustive groups of elements that are sampled at different fractions (Groves et al. 2004a), and strata are most commonly used to ensure that enough cases are drawn from subgroups of interest to allow for meaningful analyses. In some circumstances, stratification can be used to reduce sampling error compared to a simple random sample. All households in high incidence areas were sampled, against a sample of households in low incidence areas. The exact procedure used in the second stratum is unclear. It seems as if every third or fourth home was visited and was asked whether any neighbors were Jewish, along with every store. How these questions were received at a time marked by antisemitism at home and abroad remains obscure. Underenumeration does seem to have been a problem, with a number of

known Jewish households (derived from lists of Jewish organizations) apparently wrongly classified as non-Jewish, along with some proportion of the low density stratum.

As I discussed at the beginning of the chapter, a survey bears a probabilistic relationship with the universe from which the sample was taken (pp. 28-31). A survey supplies the range within which the true value of the population could be found at a given level of confidence. The size of the confidence interval is also affected by the variance of values in the population and the sample size. Another factor applies, too: the design of the survey. Most surveys are not simple random samples in which every unit has the same probability of selection. For reasons of efficiency or expediency, some sampling units have a greater or lesser probability of selection. In order to avoid bias, households in non-simple random samples are assigned weights which are the inverse of the probability of selection. In the case of the Buffalo study, all households in high density areas were surveyed (and thus would have received weights of 1.0), against one in three or four in areas with fewer Jews (the probability of selection was 1/3 or 1/4 with weights respectively 3.0 or 4.0). It is not clear whether weights were assigned here. The lack of clarity regarding the sampling fraction of households in low density areas suggests this was not the case. This would have led to biased estimates of the characteristics of the Jewish population of Buffalo in areas where the population in the core areas differed from the periphery.

*Pittsburgh, 1938.* The first known use of a sample rather than a census occurred in the same year. The 1938 study of Pittsburgh (Taylor 1943) used a master list drawn from Jewish organizations and a list of Jewish children in public schools (how the status of children was determined is not specified) to determine the approximate incidence of the

Jewish community in different areas in the city. Blocks were then selected from wards with an estimated Jewish incidence of one percent or more "in proportion to the density of the Jewish population as shown by the master list" (p. 82). The selection of blocks within wards, however, was not random, as they were "selected for canvassing by inspection of the spot-map with the following principles in mind: to prevent the concentration of the entire sample within a given ward in a single block (some blocks contained more households than the allocated number for the entire ward), and to distribute the sample as widely as possible throughout the ward, taking into account differing economic levels" (p. 82). Every household in each block was to be interviewed.

This design was advanced for any population survey at the time, Jewish or not. Like others of its era, however, it was compromised by the undeveloped sampling methods employed. Purposive selection of blocks introduces a source of nonrandom error, particularly when the largest blocks were excluded. It is not unreasonable to think that large blocks may have contained more multifamily dwellings containing the less economically advantaged. Weights for the probability of selection do not appear to have been used, overrepresenting Jewish households in the higher density areas. Residents of these areas were presumably more likely to be immigrants and to have stronger connections to the Jewish community. Confidence intervals were not reported. In addition to the problems of sampling, the field period for the study was almost vanishingly small at only one week long. In practice, this must have meant that the status of many households could never be resolved.

In spite of these early uses of elements of survey methodology, widespread adoption was rather slow. This can be seen in Seligman and Swados's (1949) review of Jewish

population studies, which sent a questionnaire to all 228 communities affiliated with the Council of Jewish Federations and Welfare Funds asking whether "any formal population enumerations, complete or sample censuses, or counts of Jewish population been made in your community within the last ten years?" (p. 653). The replies and the authors' description show the slow development of the field, reporting the methods used to finding Jewish households without indicating whether a census or a survey was undertaken. In seven cases, however, cities thought to have Jewish populations between 1,000 and 90,000 reported having conducted studies based on "sampling" (p. 658). Unfortunately, the authors do not report which cities undertook such surveys. It is also unclear whether these studies sampled the known Jewish population, ascertained via traditional combination of lists, personal referrals, and "Jewish" names or included sampling households of unknown status, which would allow researchers to include unaffiliated Jews. A later update (Seligman 1951a) reported that a number of recently updated estimates were based on "surveys," although the way in which they were conducted is not specified.

*Nonuniversal Surveys*

The Pittsburgh survey stands out as being truly ahead of its time, incorporating both a sample and random selection from the entire population, rather than just known Jewish households. The bulk of early surveys, however, drew samples from lists of the Jewish population.[22] These were eventually expanded to a combination of lists and distinctive Jewish names, with the first known survey of this type taking place in Flint, Michigan in 1967 (Mayer 1969b).[23] As a relatively small community at a time before levels of intermarriage began to increase rapidly, it was possible that this sample was relatively effec-

tive. While this no doubt improved the level of coverage, unaffiliated households without "Jewish" names would not have been included. Progress in Jewish surveys, however, is not always even. A pure distinctive Jewish names survey was conducted as late as 1987 in New Haven (Cohen 1987). The next logical step was to use the list and distinctive Jewish names households as the kernel of a snowball sample, which asks people for the names of everyone they know with a given characteristic (i.e. being Jewish). This continues until the sample is saturated, when no new names are added. Surveys with this design were completed in Kansas City (United Jewish Council 1977), Tampa (Wheeler 1980), and Toledo, Ohio. An unfortunate side-effect of snowball samples is that it becomes impossible to calculate the probability of selection. In the case of a list and/or distinctive Jewish names sample, the size of the sampling universe (i.e. households known to Jewish organizations and households with "Jewish" names) is known, even if it does not include the entire conceptual universe (i.e. all Jewish households). A snowball sample expands this universe in unknown ways, preventing researchers from using inferential statistics.

*Area-probability Samples*

The geographic techniques used in Pittsburgh and Buffalo were the ancestors of area-probability samples, the regnant sampling technique in the postwar years. Area-probability designs divide the region of interest into subunits. For instance, a state might be divided into counties. A sample of the primary sampling units would be drawn, e.g., a sample of counties. A sample of households could then be taken within each primary sampling unit, or the primary sampling unit could be further subdivided into in secondary sampling units, from which a sample would be taken, and so on. The major advantage of

this procedure is that it allows for an unbiased sample with known probabilities of selection. It is unbiased because each stage of the sample is a random draw. Because the number of units and the size of the population from which they are drawn are known at each stage, the probability of selection can be calculated accurately. If we were to sample 10 counties from a state with 100 counties, each county would have a probability of selection of 10/100, or .1. If we sampled 100 households from a county with 100,000 households, each household would have a probability of selection of 100/100,000, or .001. The probability of selection of a household in this county would be .1 * .001, or .0001 and the weight assigned to this household would be 1/.0001, or 10,000. A household from a smaller county in the same study might have a within county probability of selection of 100/10,000, or .01. Consequently, its overall probability of selection would be .1 * .01, or .001, and would receive a weight of 1,000, reflecting its greater odds of being selected.

Like the Buffalo and Pittsburgh studies, area-probability samples can be made more efficient by assigning sampling units into different sampling strata (see p. 91). If we are interested in studying Jews, there is little point in treating Kings County in New York City as if it was the same as a county in South Dakota. Omitting Brooklyn would clearly have greater consequences for estimates than not including the later. To account for this, sampling units can be assigned into different strata prior to surveying. Kings County might be put in a high density stratum along with New York County, Rockland County, Queens County, and so on, while counties in South Dakota could be assigned to a very low density stratum. Samples of counties would then be drawn within each stratum following the procedure described above.

Where such designs become complicated is that multiple stages of selection introduce an additional source of sampling error. Selecting one area instead of another can have a significant impact on the estimates of a sample. Selecting our unnamed county in South Dakota while omitting Kings County, for example, would lead to significantly lower estimates of the Jewish population of the United States. Greater sampling error will occur when there is variation between primary sampling units regarding a particular statistic. Whether we include Brooklyn or a piece of South Dakota would not have much effect on estimates of gender composition because the ratio of men to women tends to be relatively constant. More precisely, the higher the rate of homogeneity is (how correlated households within a primary sampling unit are compared to households elsewhere on a particular variable), the greater the level of statistical uncertainty. Jewish households tend to be tightly clustered, leading to a high rate of homogeneity in clustered designs and ultimately wider confidence intervals. The confidence intervals of a clustered design are also influenced by the number of primary sampling units chosen. Using more primary sampling units will decrease the effect of intracluster homogeneity, at the price of greater costs.

*Washington, D.C., 1956.* That these prewar studies did not account for these factors is understandable as much of the methodology of area-probability designs was developed during the Second World War (Frankel and Stock 1942; Hansen and Hurwitz 1942; 1943; 1944; Hauser 1941; all cited in Converse 1987). The first known probability sample of a Jewish community was undertaken in Washington, D.C. in 1956 under the direction of Stanley K. Bigman. The methodology is described as follows:

A complete census, or enumeration, of this Jewish population would have been extremely expensive. It was not necessary, however. As in a large proportion of the studies conducted by the U.S. Census Bureau and similar agencies, it was possible to accomplish our purpose by selecting a sample carefully designed to reproduce in miniature the whole population we wished to study. This sample had to be so designed as to permit two things:

1. It had to be representative of *all* people—Jewish and Gentile—in the area. From this sample we would then be able to determine the percentage of Jews in the total population; and, multiplying this percentage by the total, we could then find out how many Jews composed the community.

2. At the same time, the sample chosen had to contain a large enough number of Jews (we wanted about 2,000 families) so that we could make a fairly intensive analysis of their characteristics. Previous information indicated that Jews were about *5%* of the total, however, which meant that we could not expect a reasonable-sized sample of the whole population to contain very many Jews.

What we did was to select two sets of samples, one to satisfy each of the above conditions; check them against one another to prevent duplication; and use appropriate statistical techniques to combine the two. For a representative sample of the whole population, we utilized a sample of all blocks in the area, and selected specified numbers of "dwelling units" (apartments and private houses) on each block. The sample of blocks was available from a previous study conducted by the writer and Reuben Cohen at the Bureau of Social Science Research. In each of 154 blocks, from 16 to 40 dwelling units were selected so as to constitute 1 of each 100 dwelling units in the area. This was supplemented by a sample of dwelling units constructed between January 1, 1954 (as of when the block sample had been constructed) and June 30, 1955.

To obtain a large number of Jewish families, we made use of a "master list" of known Jewish persons made available through the kindness of the United Jewish Appeal. This list of 29,600 names and addresses we arranged in order geographically while simultaneously removing duplications, business firms and business addresses, incomplete addresses, addresses outside the survey area and on military reservations, etc. The remaining 15,500 addresses, arranged in nine geographical areas, were sampled at high rates where there were few Jews (for example, every other address in Fairfax County, Virginia) and at low rates where Jews were numerous (e.g., every 20th address in Northwest D.C.). (Bigman 1957:161-162)

This study was advanced for its day. The first dual-frame sample was only conducted in 1944 and the first paper on the was published five years after the Washington survey (Hartley 1962). A dual-frame design combines two sources of data are combined with each other, in this case the list frame of households known to the United Jewish Appeal and the area-probability frame. The advantage of such designs is their efficiency in studying rare populations. A list or similar frame is inexpensive because it contains a high percentage of members of the rare population, requiring few unproductive interviews with ineligible households. However, such a sample is insufficient by itself because it does not cover the entire population and will give biased estimates of variables for which the population of the list frame differs from the rest of the population of interest. One would expect that Jews found on lists would, on the whole, be more affiliated and active than other Jews. The area-probability (or similar) frame, on the other hand, is universal (i.e. it covers the entire population), but expensive because many households must be screened in order to find members of the rare population. A dual-frame sample combines these approaches, providing a universal sample at reasonable cost. For this approach to work, the two frames must be unduplicated, with each household appearing on one frame and not the other. The probability of selection for each household is higher for households found on the list frame and lower for other households, but is known and can therefore be weighted for.

*Boston, 1965.* Due to its complexity and cost, area-probability methodology was little used in surveys of Jewish communities. The level of effort required can be seen in the logistics required for the 1965 survey of the Boston Jewish community, as described

by Floyd J. Fowler, Jr., former director of the University of Massachusetts at Boston Survey Research Center:

> In 1963, the Combined Jewish Philanthropies of Greater Boston decided that a major survey study of the Greater Boston Jewish community was necessary as part of their Long Range Planning effort. Morris Axelrod was induced to leave the Survey Research Center at the Universe of Michigan to direct the survey. In 1965, I joined that project as Assistant Director and had an opportunity to play a major role in the analysis of these data.
>
> The staff that was assembled for the Long Range Planning Project moved *in toto* to the Joint Center for Urban Studies of M.I.T. and Harvard University when the project was completed, forming the core of what is now the Survey Research Program. (Fowler 1977:v)

Area-probability samples of Jewish communities had a relatively short heyday. The final use of an area-probability sample as a regular part of sampling procedures was in Boston in 1975 (Fowler 1977), although the 1985 Boston study included a small area-probability sample in a specific area to test the comparability of random digit dialing methods (Israel 1987). In addition to these local studies of Jewish communities, the first national study of the American Jewish population used a dual-frame list and area-probability design.

*National Jewish Population Study, 1970-71*. A persistent theme in the writings of Jewish demographers had been the need for a national study that would use scientific methods. This need was finally addressed with the National Jewish Population Study (NJPS) 1970-71, conducted under the auspices of the Council of Jewish Federations and Welfare Funds. Also, in marked contrast to earlier studies, a wide variety of questions were asked, allowing are far broader analysis of Jewry than had been heretofore possible.

The NJPS 1970-71 remains the largest, most complicated, and probably most expensive survey of the American Jewish population ever undertaken. It was initially designed to include 10,000 to 12,000 Jewish households, an enormous sample (Chenkin 1969). Finally, a total of 7,550 interviews with Jewish households were conducted, still a huge number for a national survey and far larger than any project subsequently attempted by the Jewish community (Massarik and Chenkin 1973).

Like the Washington, D.C. and Boston studies, NJPS 1970-71 used a dual-frame sample including list and area-probability frames. However, covering an entire nation is more difficult than surveying a metropolis by orders of magnitude. The study divided the United States into 39 areas, representing 38 major Jewish communities and the rest of the country.[24] Within each area, the distinctive Jewish names method was used to determine the approximate incidence of Jewish population within each geographic area, apparently as a way to stratify based on Jewish population density (Massarik and Chenkin are not entirely clear). Sample blocks or small geographic units were then chosen within the strata defined by Jewish population incidence. At this stage, households that appeared on lists of Jewish households would have been removed to ensure equal probabilities of selection. Finally, where possible, the names associated with different housing units were assigned different interviewing rates based on the likelihood the inhabitants were Jewish. Households found on the list frame were also interviewed, also likely geographically clustered to minimize transportation costs. The administrative and statistical complexity of this design is the equal of any survey fielded in the United States.

One drawback of this approach was its statistical efficiency. I have previously discussed a number of factors that influence a survey's margin of error: variation of a

characteristic in the population, sample size, the proportion of a population covered by a sample, and the effect of using a multiple stage sample. One more factor can be added to this list. Broadly speaking, the greater the difference in the probability of selection of given units, the less efficient a survey will be. As some cases will have very large weights, whether a given case has one value or another can have large impacts on estimates. This variability leads to a high level of instability in estimates, and thus greater sampling error. The design of NJPS 1970-71 included many factors that led to wide variation in the probability of selection. People living in areas without a large Jewish community were far less likely to be selected. The same applied to people not found on Jewish organizational lists, people in tracts with few distinctive Jewish names, and people without distinctive Jewish names. All these factors would have led to considerable variation in probability of selection and considerable statistical inefficiency.

Every interview was conducted in person, except where respondents were concerned about their safety. Personal interviews remain the gold standard for public opinion surveys due to high response rates and perceived greater accuracy, and were the only method then available for high quality surveys. This shows in the response rate for NJPS 1970-71, which varied between 75 percent and 90 percent in different communities. Unfortunately, personal interviewing is also a gold standard of another kind. The average interview with a Jewish household took an hour to an hour and a half, not to mention the time spent screening non-Jewish households. The costs must have been enormous, but were not reported.

In spite the size of the enormous effort put into NJPS 1970-71, it was analyzed relatively little. Massarik and Chenkin's (1973) report in the *American Jewish Year Book*

did provide a number of tables on selected household characteristics. This was followed up by estimates of the U.S. Jewish population (Massarik 1974), estimates of the New York Jewish population and some of their household characteristics (Massarik 1975), a study of Jewish education (Massarik 1976), and a new typology of Jewish affiliation (Massarik 1977). In comparison to the large number of articles and papers generated by NJPS 1990, these analyses barely scratched the surface of the data. The most likely reason is simply computational power. Running statistical analyses required time on centralized mainframe computers, which was either in short supply in academic settings or expensive outside academe, as well as considerable facility in programming. Furthermore, the large size of the dataset would have significantly increased the time required for each run. Twenty years later, statistical programs were available for desktop computers (and, indeed, desktop computers were no longer a novelty), mainframe computers were dramatically faster, less programming was required, and the NJPS 1990 dataset was smaller. Another by-product of the computing revolution was that multivariate analysis, which requires a lot of processing power, was far easier to implement, with the effect that not only were there more papers, but some used sophisticated methods.

*The end of the line.* The relatively fast switchover to random digit dialing was a product of general factors in the social and technological environment rather than being peculiar to the Jewish community. As the provision for telephone interviews in NJPS 1970-71 attests, safety was a concern for interviewees. The same was true for interviewers, leading to increased levels of staff turnover (Steeh 1981). The efficiency of in-person interviews also declined, as level of refusals also increased noticeably at this time, while

respondents were less likely to be at home, along with greater numbers of secured multi-unit buildings, which were difficult to sample (Steeh 1981).

*Random Digit Dialed Samples*

Besides the increasingly difficult social environment for in-person interviewing and area-probability samples, the maturing telephone system offered a viable alternative that had previously been lacking. A key element in the switch to telephone interviewing was the expansion of telephone coverage, which stood at nearly 90 percent by 1980 (Frankel 2003). While telephone coverage in the Jewish community was no doubt much higher, there were several reasons Jewish surveys could not have changed over earlier. First, the size of the Jewish population is estimated by extrapolating from the proportion of Jews and non-Jews in the sample to the known total population of the area. If too many non-Jews had no telephone access, the proportion of Jews would be artificially high, leading to over-estimates of the size of the Jewish population. Second, random digit dialing methodology was not developed until well into the 1970s, so there was no existing set of techniques for Jewish survey researchers to use (Frankel 2003). Simply picking the area and exchange codes one is interested in and dialing numbers at random is extremely inefficient, with most numbers ending up being disconnected.

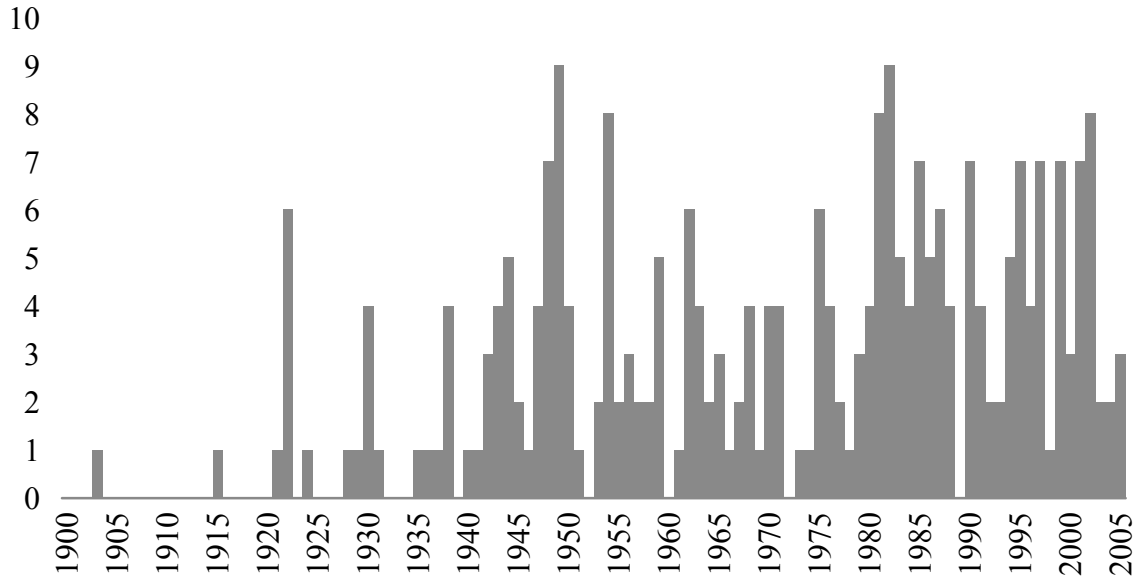
The first workable method for more efficient random digit dialing was developed by Joseph Waksberg (1978) based on techniques developed by Warren Mitofsky, and involved dialing a number from randomly selected 100 blocks (groups of telephone numbers that vary only in the last two digits: XXX-XXX-XXNN). If the first number was a residence, additional calls were made to the 100 block until a set number of interviews

were completed. If the first number was not a residence, another 100 block was sampled. The Waksberg-Mitofsky method was, however, relatively cumbersome and inefficient. It was largely superseded by list-assisted methods. While many variations exist, they share certain core features. Like the Waksberg-Mitofsky method, the pool of telephone numbers is divided into 100 banks. However, instead of relying on the results of the first call to the 100 bank to determine whether further calls should be made, listed residential telephone numbers are used as the basis for calling. The presence of one (or sometimes more) listed numbers makes a bank eligible, and numbers are then selected randomly within it (Brick et al. 1995; Casady and Lepkowski 1993). One might think that this would lead to serious bias, as unlisted numbers in 100 banks with no listed telephone numbers could be called, but this does not appear to be the case, as Brick et al. (1995) reported that only 3.7 percent of numbers were missed.

Besides these methodological issues, the ready availability of computers also helped the spread of random digit dialing (Frankel 2003). The interviewer reads each question off the computer and records the data, prompting the next question (the technique is called computer-assisted telephone interviewing). While seemingly trivial, this step bypassed the considerable time and labor required to turn paper interviews into electronic data. Computerized interviews could also be integrated with the software used to track the status of the sample and the autodialer that makes telephone calls and, in some cases, randomly selects numbers, further increasing ease and efficiency. The use of computers also made it possible to use complex skip patterns that could determine the next question the interviewer would ask on the basis of the respondent's previous answers, making it possible to ask detailed questions of targeted respondents.

*Adoption by Jewish community studies.* One of the consequences of more affordable random digit dialing methods may have been modest growth in the number of studies undertaken. Figure 2:6, below, plots the number of known studies of Jewish communities per annum. An apparent increase in the number of studies can be seen after 1980. It is also entirely possible that the apparent pattern is the result of other pressures within the Jewish community or an artifact of nonobservation of older studies prior to the establishment of the Mandell L. Berman Institute—North American Jewish Data Bank in the mid-1980s. It is not unreasonable to think that all these factors might be at work. Mandell (Bill) Berman, the eponymous donor of the Jewish Data Bank, was a major force in advocating for social scientific studies of Jewish communities. Nonobservation is also a serious concern. Had I not worked at the Jewish Data Bank, I would not be aware of some recent studies. Nevertheless, many of these studies would not have been undertaken or would not have used a universal sample covering Jewish and non-Jewish households had random digit dialing methods not existed.

Figure 2:6. Number of Jewish Community Studies by Year




*Note:* There is no comprehensive source of information on studies of Jewish communities. There figures are based on available data. It is likely that studies prior to 1950 are underestimated, particularly during the Second World War.

*Random digit dialing only with oversamples of low density areas.* As was the case with area-probability samples, some studies of Jewish communities were early adopters of new methods, while others lagged behind. The first known random digit dial survey of a Jewish community is the 1979 study of the Los Angeles Jewish community (Phillips 1986b), which used a pure random digit dialing approach (i.e. lists were not used). In order to understand areas with low density Jewish populations better, the study oversampled southern and eastern areas. In other words, proportionally more calls were made to the areas with the lowest Jewish population. Because these areas had a larger sample size than if all areas had been sampled at the same rate, the confidence intervals for estimates are correspondingly lowered. The cost of this approach is financial, because these areas

have fewer Jewish households, more interviews with non-Jewish households will be required, either decreasing the overall sample size or increase the cost of finding a given number of Jewish households. This was made possible by the relatively affordable nature of random digit dialing in this period, as was the fact that the study authors did not have to use a dual-frame sample in order to control costs. Oversampling low density areas is characteristic of Jewish communal studies prior to the mid-1990s.

Like the Los Angeles study, the 1981 Chicago survey (Jewish Federation of Metropolitan Chicago 1982) used only a random digit dialed sample and oversampled "certain areas," which presumably were low density. The same appears to be true for the 1985 study of Atlantic County, New Jersey (Tobin 1986a). These were joined in 1991 by the first valid study of Jewish New York (Horowitz 1993), which also oversampled low density areas.

*National Jewish Population Survey, 1990.* The best known study to use a random digit dialed sample with oversamples of low density areas was the National Jewish Population Survey (NJPS) of 1990 (Kosmin et al. 1991a), which oversampled areas with the least Jewish population, although the exact details of which areas were oversampled remain elusive.

The first move toward another national population survey in 1990 was made in October 1987, when the World Conference on Jewish Demography in Jerusalem recommended that a national survey of American Jewry be carried out (along with similar studies in other nations), which was endorsed by the Council of Jewish Federations' National Technical Advisory Committee on Jewish Population Studies. The following year, the Council of Jewish Federations agreed to conduct the survey (Kosmin et al.

1991a). Compared with NJPS 1970-71, NJPS 1990 was more ambitious in the questions it addressed but less convoluted in its methodology. In keeping with the Jewish community's recognition that NJPS 1990 would be a major resource for communal planning, it asked questions on a wide array of topics, including the sociodemographic characteristics of the Jewish population, intermarriage, Jewish education, philanthropy, observances of Jewish rituals and practices, synagogue membership, utilization of social services, volunteerism, attitudes to certain issues of Jewish concern, and so on (Waksberg 1991).

Methodologically, NJPS 1990 was much more straightforward than NJPS 1970-71, using a sample of about 2,500 random digit dialed telephone interviews with people who had a Jew or someone who was once a Jew in their households.[25] This was larger than initial discussions called for, which had pegged the sample size at 2,000 (Goldstein 1988). People were considered to be Jews for the purpose of the study if they identified as a Jew. This was a response to the perceived problems of NJPS 1970-71: poor quality address lists, budget overruns, supervision problems, and dissension in the research team (Kosmin 1992).

In order to achieve a large sample size with a very low density population (Jews constituted about two percent of all Americans in 1990), NJPS 1990 cut costs by not making its own calls to screen for Jewish households, instead buying time on EXCEL, an ongoing national omnibus telephone survey (one that asks questions on a variety of topics). In addition to being asked typical market research questions, EXCEL respondents were asked "Do you consider yourself Jewish?" "Were you raised Jewish?" and "Do you have a Jewish parent?" The respondent was also asked these questions regarding other household members (Kosmin et al. 1991b). This process began in April 1989 and took

about a year. Altogether, 5,146 households were identified as having a qualified Jewish (or formerly Jewish) member out of 125,813 that gave information about their religious preferences.

While this approach reduced screening expenses considerably, it was not without cost. Market research surveys generally have low response rates, and EXCEL was no exception, with an average weekly response rate of 47 percent, which was quite low for the time. This was a product of several factors. EXCEL was a bi-weekly survey, which allowed only about three days for contacting households. Households that were particularly difficult to contact would therefore be underrepresented in the sample. The other factor was that market research studies cannot bring the full panoply of ways to increase the response rate used by government and academic studies to bear: prenotification letters, incentives, the benefit of an authoritative sounding sponsor, refusal conversion, and so on. Thus, while only 2.3 percent of respondents refused the screening questions, nearly almost half the entire sample refused to participate in the survey.

Due the extraordinarily long field period for NJPS 1990, during which Jewish households were identified, qualified households were recontacted in order to collected detailed information about the status of every member of the household and the location and type of residence. The respondent was then told about the study and asked to participate in the main phase.

The main phase of interviewing took place from May to August 1990. This period was chosen as it was at about the same time as the U.S. Census and was during the summer when, it was hoped, college students would be more likely to be contactable. All households that qualified in Phase I, including those that failed Phase II, were contacted

in the main phase of interviewing, when the main questionnaire was administered. Of the 5,139 qualified households, 51 percent ($n$ = 2,639) were interviewed and appeared to qualify. A small number of completed interviews were discarded as the technical advisory committee felt that, on detailed consideration, they did not warrant inclusion in the study. Altogether, 2,441 interviews with households containing a person born, raised, or currently a Jew form the basis of NJPS 1990. Another 21 percent ($n$ = 1,054) of households failed to requalify due to changes in household composition (e.g., moves, divorce) and also an unexpectedly high number of people who were not born, raised, or currently Jewish by religion but said in Phase I that they considered themselves to be Jewish. This appeared to be a product of Christian beliefs. In addition, 13 percent of households refused to participate in stage 3 ($n$ = 670), while an additional 15 percent ($n$ = 776) were neither requalified nor disqualified for reasons not described by Kosmin et al. (1991b), but presumably included noncontacts and refusals to the second stage of the study.

Taking account of phases I and III, Kadushin et al. (2005) estimate that net response rate was 32 percent, which was very poor for its era. By comparison, the response rate to the contemporaneous New York study (Horowitz 1993), which was conducted by the same firm, was 58 percent despite being conducted in the most difficult environment for survey research in the nation. In other words, NJPS achieved a little more than half the response rate it should have. While the response rate is far from a perfect indicator of survey quality (see pp. 145-146), this raises serious questions about the survey's accuracy.

Given the pluralistic nature of the Jewish community, NJPS 1990 leadership felt it would be counterproductive to provide an official report and instead opted for the short

and factual *Highlights* document (Kosmin 1992). A *de facto* official report did, however, appear in the form of Sidney Goldstein's (1992) long article in the *American Jewish Year Book*, which provided the detailed review of the survey findings missing from the *Highlights* report. In addition, it was planned that about 20 books would be published by prominent scholars of contemporary Jewry. These works would analyze data from the survey on a wide variety of issues. As it turned out, only six were ever published: *Jews on the Move* by Sidney and Alice Goldstein (1995), which looked at population shifts; *Gender Equality and American Jews*, by Moshe and Harriet Hartman (1996); *Jewish Choices*, by Bernard Lazerwitz, J. Alan Winter, Arnold Dashefsky, and Ephraim Tabory (1998), which studied the denominations and religious practices of American Jews; *Jewish Life and American Culture*, by Sylvia Barack Fishman (2000); *The Next Generation*, by Ariela Keysar, Barry Kosmin and Jeffrey Scheckner (2000), which looked at children and adolescents; and *Jewish Baby Boomers*, by Chaim Waxman (2001).[26] There were two reasons for the smaller than expected total of books analyzing NJPS 1990: problems funding publication and reported foot dragging by some of the assigned authors. In spite of the limited success of the publication plan, NJPS 1990 was a tremendous success when it came to being analyzed in articles, with literally hundreds of analyses published in various journals and reports.

In addition to the academic side of NJPS 1990, the Council of Jewish Federations made a concerted effort to make its findings available to the Jewish community. In addition to the release of the data at the 1991 Council of Jewish Federations General Assembly and the publication of *Highlights*, programs about NJPS 1990 were broadcast to its affiliates via satellite, it sponsored a three day colloquium at the University of Judaism

for lay leaders and communal professionals, people associated with the preparation of NJPS 1990 went to communities throughout the country to explain the survey and describe its findings, and presentations were made at annual meetings of major Jewish organizations (Scheckner 1998).

In spite of the wide array of research conducted using NJPS 1990, the study is best remembered in the Jewish community for the much-publicized reports of a 52 percent rate of intermarriage for couples married between 1985 and 1990. These reports generated much heat, particularly when some journalists and social scientists (most notably Steven Cohen and Ira Sheskin) questioned the rationale behind the way the intermarriage rate was calculated and argued that it was in fact lower. This drew a vigorous response from the members of technical advisory committee (Goldstein 1995), who defended the integrity of the study. Most of the heat of the debate focused on who was considered a Jew for the purposes of calculating intermarriage. Cohen (1994) argued that it was unreasonable to calculate the rate counting as Jews people who had been raised as Jews but no longer considered themselves to be Jewish, leading to a higher rate than would otherwise be the case. While this was an important distinction, a new definition could have easily been calculated by scholars. More serious were Cohen's allegations that the sample's weights had been miscalculated. The details of his reweighting, however, were never published in a peer-reviewed journal. On the whole, though, NJPS 1990 was accepted by social scientists as methodologically sound.

*Random digit dialing only with equal probabilities of selection.* Like the Chicago study, the 1981 Denver survey (Phillips and Judd 1981) used only a random digit dialing frame. Interestingly, the sponsoring Federation trained and developed its own interview-

ers rather than contracting with an academic or commercial research center. It appears that all areas were sampled at the same rate, possibly a result of the relatively dispersed Jewish population of Denver. From this point on, random digit dialed studies become commonplace, with studies in Chicago and New York in the same year. The Hartford study (Abrahamson 1982) followed the Denver approach of using an equal probability of selection method, as did the 1982 study of Tidewater, Virginia (Ephross and Weiss 1982), the 1983 study of Philadelphia (Yancey and Goldstein 1984), the 1983 study of Phoenix (Jewish Federation of Greater Phoenix 1984), and the 1985 study of Houston (Smith and Phillips 1986). In a deviation from standard random digit dialing methods, Phillips (1984) randomly selected numbers from the Milwaukee telephone directory, biasing the sample in whatever ways listed households differed from unlisted ones.

After this point, we see the virtual disappearance of pure random digit dial studies, testimony to the increasing costs of random digit dialing. The exceptions to this rule are telling. Greater New York City, when surveyed in 1991, was hardly a low density area. The 1995 survey of South Palm Beach, Florida (Sheskin 1995) was originally to have used both random digit dialing and distinctive Jewish names surveys (see pp. 118-128) but for unexplained reasons, the distinctive Jewish names sample was never completed. The area was estimated to be 43 percent Jewish, extremely high density. Likewise, Sheskin's Broward County study (Sheskin 1997b) took place in area with an overall Jewish density approach the most Jewish counties in the New York area, at an estimated 19 percent, as did his study of West Palm Beach (Sheskin 1999a) which was estimated to be 15 percent. In addition to these local examples, two national studies conducted at the beginning of the present decade used national random digit dial samples with equal prob-

abilities of selection: the American Jewish Identity Survey (Mayer et al. 2002) and the Survey of Heritage and Religious Identification (Tobin and Groeneman 2003). The American Jewish Identity Survey replicated the methodology of NJPS 1990 by using ongoing market research surveys to screen for Jewish households, obtaining considerable cost savings at the price of a response rate of 18 percent. The Survey of Heritage and Religious Identification made its own calls, but reduced costs by conducting few interviews with Jewish households ($n$ = 259).

*Random digit dialing with oversamples of high density areas.* As the typical response to the high cost of surveying a rare population is to use a dual-frame design that includes an incomplete frame with a high proportion of members of the rare population, the alternative approach of oversampling areas with high incidence of the rare population is seldom used. As we shall see, however, the increasing cost of survey research led to surveys that use both dual-frame designs and oversampling areas. Other than NJPS 2000-01, the only other random digit dialing studies that used a variation of this design were the 2001 study of Bergen County, New Jersey (Sheskin 2001a) and the 2004 study of Miami (Sheskin 2004b), and the 2005 studies of Palm Beach (Sheskin 2005a; 2006). The Bergen study did not sample very low density areas at all. Sheskin estimates the level of undercoverage at five percent based on the distribution of telephone numbers on the Federation list, but it potentially may been somewhat higher if Jewish households unknown to the Federation were overrepresented in lower density areas. The Miami study initially made calls to all telephone exchanges, but calls were subsequently limited to exchanges covering somewhat under 95 percent of the Jewish population, and finally exchanges covering somewhat under 78 percent in the final replicates. The 2005 Palm

Beach studies oversampled areas thought to have higher concentrations of households with Jewish children and the City of Palm Beach in light of its importance for fundraising. It is not known how weights were calculated. Kosmin (2005) argued against this approach, calling for equal probability of selection samples. This approach would be extraordinarily costly and appears to assume that omnibus studies would be used as the primary means of locating Jewish households. Unfortunately, it is far from clear whether the problems of using omnibus surveys to screen for Jewish households would represent an improvement over the problems of oversampling high density areas. While the latter is associated with increased variance, the former is at greater risk of bias due to the particularly low response rate of such surveys.

*Random digit dialing and list frames.* As I discussed with respect to the Washington, DC study of 1956, a dual-frame design can be used to minimize screening costs. The random digit dialing sampling strategies used in dual-frame designs varied between the equal probability of selection method used in Atlanta and oversampling low density areas. The surveys of Washington, DC in 1983 (Waksberg, Greenblatt, and Tobin 1984), Tidewater, Virginia in 1988 (Ruchelman 1989), and the 1995 study of Erie, New York (Goldhaber Research Associates 1995), followed Atlanta in using random digit dialing with equal probability of selection in conjunction with a list sample.

The 1985 and 1995 studies of Boston (Israel 1987; 1997), on the other hand, oversampled low density areas, as did the Chicago survey of 1990 (Groeneman and Cue 1991).[27] The 1985 Boston study is also notable for having conducted parallel random digit dialing and area-probability samples in the same neighborhood of Boston (Allston-Brighton) in order to determine whether the different methods led to different results—

the results were comparable, indicating that random digit dialing was a viable alternative to area-probability designs.

By the later 1990s, costs had risen to the point that researchers began oversampling areas with the densest Jewish populations even when using a dual-frame approach, which itself provides considerable savings. The 1997 Los Angeles (Herman 1998) and Denver studies (Ukeles and Miller 1998) did so, although the latter actually combined three frames and is discussed below (p. 127). The central role of cost is spelled out by Ukeles and Miller:

> In order to ensure representation of all households within the survey area it was determined that random digit dialing (RDD) sampling techniques would be required. However, as is the case with sampling any low incidence population, a purely unrestricted approach where every household has an equal chance of selection would be prohibitively expensive. (p. A23)

Later dual-frame studies following this design were Palm Springs in 1998 (Ukeles, Miller, and Tobin 1999), Baltimore in 1999 (Ukeles and Miller 2001a; 2001b), Seattle in 2000 (Herman and Phillips 2001b; Herman and Phillips 2001c), New York in 2001 (Ukeles and Miller 2004b), Columbus in 2001 (Horner, Stewart, and Stec 2001), Phoenix in 2002 (Ukeles and Miller 2002), Pittsburgh in 2002 (Ukeles and Miller 2003a), San Diego in 2003 (Ukeles and Miller 2004a), Atlanta in 2004 (Ukeles and Miller 2004c), and San Francisco in 2004, with the addition of a list of Russian-speaking households (Phillips 2005).

An unusual adaptation of combining random digit dialing and lists was used in the 1990 survey of Columbus Jews (Mott and Mott 1990). A complete random digit dial

sample was conducted along with an entirely separate sample from a "master list" developed from 25 Jewish organizations. The two samples were not combined, which as I describe below, would lead to misestimates. Instead, the random digit dialing sample was used for most estimates, except for cases where comparison was made to the 1969 study, which was based on a list sample, and for certain services where the list sample was thought to be a proxy for the active Jewish community.

Additional variations on dual-frame methods exist. The 1990 study of Michigan (Cohen and Ukeles 1991) included a list of "high givers" to the Federation campaign besides the random digit dialing and Federation list samples. While the inclusion of major donors may seem suspicious, it was for good reason: to "assure the presence of a sufficient large number of Campaign donors for the analysis of philanthropic giving" (p. 7). This study was, however, not representative of the Jewish population of Detroit, as both the list and random digit dialing samples were restricted to the 45 telephone exchanges with the highest concentration of Jews. The 1986 study of Worcester (Tobin and Fishman 1987b) used list and random digit dial samples, but this was not conducted properly, leading to invalid estimates—the method used was identical to those used by Tobin and Ira Sheskin in distinctive Jewish name samples, which I examine next, simply substituting a list sample for a distinctive Jewish name sample.

*Random digit dialing and distinctive Jewish names.* One variation of the dual-frame methodology used in Atlanta and other places is to substitute households with distinctive Jewish names for the list frame. Unlike the use of distinctive Jewish names alone to estimate population size, a properly conducted dual-frame approach that combines a universal frame like random digit dialing with "Jewish" names is an unbiased

estimator because every household has a known probability of selection. While a household with a distinctive Jewish name is more likely to be contacted, all Jewish and non-Jewish households have some chance of being selected, provided they have landline telephone service.

The problem, as we shall see, is that extremely few dual-frame samples using distinctive Jewish names have been properly conducted. For such a study to work, the sample must be partitioned into two frames: a distinctive Jewish names frame (which covers all households with "Jewish" names) and a residual universal frame that excludes distinctive Jewish name households. This allows the researcher to keep the probability of selection straight. Each distinctive Jewish name household has a probability of selection equal to the number of Jewish name households sampled divided by the total number of Jewish names households. Cases in the random digit dialing sample (or some other universal sample) have a probability of selection equal to the number of random digit dialing cases divided by the number of elements in the universal frame less the total number of distinctive Jewish name households. The first study to use this approach, the 1981 survey of New York, was a harbinger of things to come.

Leo Tolstoy's (2004 [1877]) opening to *Anna Karenina*, "All happy families are alike; each unhappy family is unhappy in its own way," applies well to survey research. There are a limited numbers of ways to get a survey right, but countless opportunities for serious error. In many ways, a post mortem of the abject failures among surveys is more instructive than recapitulating the successes. The 1981 New York survey is a study in contrasts. Where the Los Angeles, Denver, and Chicago studies either oversampled low density areas or maintained a constant probability of selection, the Federation of Jewish

Philanthropies of New York (1984) continued its long tradition of abysmal methodology by limiting random digit dialed calls to the 40 telephone exchanges (i.e. XXX-NNN-XXXX) with the highest proportion of distinctive Jewish names. At most, these directories could yield 400,000 telephone numbers. In practice, the number of households contained therein was probably much lower, given that many telephone numbers go unused or are ineligible as they belong to businesses or government agencies. These calls were supplemented by a mail survey to a sample of households with distinctive Jewish names. It is unknown whether the Federation went to the trouble of removing duplicate cases. The study's authors estimated that 31 percent of Jews had distinctive Jewish names. The study itself estimates the Jewish population at 1.67 million. Assuming, for the sake of argument, that this figure is correct, at most the study could have covered only 31 percent of New York Jews (c. 520,000) plus some fraction of less than 400,000 households. In other words, at least half of New York's Jews—and probably many more—were systematically excluded from the sample. This would have been problematic enough if it were a random half being excluded, but clearly this was not the case. Those excluded lived in less Jewish neighborhoods and did not have identifiably Jewish surnames. It is hardly a leap of faith to assume that all estimates from the study were irreparably biased.

Because the study was not based on a universal sample, the Jewish population size could not be directly estimated by multiplying the proportion of Jewish households found by the survey by the known population of the study area. Instead, the population estimate is extrapolated from the numbers of households with distinctive Jewish names on cantilevers of guesswork and surmise. A contemporary study of Hartford Jewry, comparing estimates from a purely random digit dialed sample with the distinctive Jewish

names approach, concluded that: "It seems clear that [distinctive Jewish name] estimates cannot be brought to a reasonable level (i.e. even to "ballpark figure") by any kind of adjustments" (Abrahamson 1982).

The reason for this travesty appears to be the New York study's vast sample size of 4,505 Jewish households, which it boasted was "the largest ever single study of a Jewish community outside the state of Israel" (Federation of Jewish Philanthropies of New York 1984:71). (Large as it was, the sample size was still well below the number of interviews conducted for the National Jewish Population Study of 1970-71.) Survey research can be seen as a balancing act between sample quality, sample size, and cost. The Federation of Jewish Philanthropies traded quality and/or cost in order to maximize sample size. The benefit of a large sample is that it decreases the size of the confidence intervals around an estimated value. This is, of course, pointless when the estimate will be biased as a result of the sampling scheme. The fact that the largest Jewish community in the world, with the densest Jewish population in the United States, and presumably the greatest financial resources could not mount a valid study at a time when the cost of survey research was probably at an all-time low beggars all description.

The two main proponents of distinctive Jewish names have been Gary Tobin and Ira Sheskin. Tobin (1986b) first used this approach in the 1985 survey of Baltimore, combining a random digit frame that oversampled areas with few Jews with a distinctive Jewish names sample that was apparently used to supplement the random digit dialed survey in low density areas. In addition to the Baltimore survey, Tobin and Sheskin combined stratified random digit dialing and distinctive Jewish names in studies of Metro-West New Jersey (Meier, Greenfield, and Tobin 1988), South Broward County, Florida

(Tobin and Sheskin 1990), Sarasota-Manatee (Sheskin 1992; 2001c), Orlando (Sheskin 1993), Harrisburg, Pennsylvania (Sheskin 1994d), Miami (Sheskin 1994a), Pinellas County, Florida (Sheskin 1994b), Richmond (Sheskin 1994c), Delaware (Ukeles et al. 1995), Charlotte (Sheskin 1997c), Atlanta (Atlanta Jewish Federation 1996), Monmouth County, New Jersey, where the Ashkenazi names were supplemented with a list of Sephardic names (Sheskin 1997a), York County, Pennsylvania (Sheskin 1999b), Rochester (Sheskin 2000b), Hartford (Sheskin 2000a), Tidewater, Virginia (Sheskin 2002a), Westport-Weston-Wilton-Norwalk, Connecticut (Sheskin 2001b), Rhode Island (Sheskin 2003a), Tucson (Sheskin 2002c), Washington, DC (Sheskin 2003b), Atlantic County, New Jersey (Sheskin 2004a), and Minneapolis-St. Paul, with the addition of individuals with Russian Jewish first names (Sheskin 2004c).

Unfortunately, not one of these studies was carried out correctly. As Ira Sheskin has been a prolific user and ardent advocate for this, it is worth examining his argument in favor of it in detail.

> Everything else being equal, [Random Digit Dialing (RDD)] should produce the most random sample of Jewish households. … DJN [Distinctive Jewish Name] and List samples almost always produce Jewish identity profiles that are more Jewishly oriented than RDD samples. While weighting factors are applied to "correct" the DJN and List samples, they do not always adjust all of the biases introduced by DJN and List sampling.
>
> Different sampling methods may lead to differences in survey results. Lists of affiliated Jews usually include fewer intermarried households than do RDD samples. Thus, an intermarriage rate reported in Place A, using a combination of RDD and List samples, may be lower than one reported for Place B, using a combination of RDD and DJN samples, and for NJPS 2000-01 which used a RDD-only sample. While weighting factors should nullify these differences, not all researchers have provided enough information to discern if the weighting factors actually do so. While a full explanation of the types of bi-

> ases produced by DJN sampling compared to List sampling is beyond the scope of this paper, the main biases introduced by DJN sampling is that intermarried households in which the woman is Jewish are underrepresented, as are households who are not listed in the telephone directory (including recent in-migrants). The former bias almost always means that the DJN sample is older in age than the RDD sample. List samples are always biased toward households who have made themselves known to the Jewish community in one way or another, and particularly toward households who donate to the Jewish federation. Intermarried households also are somewhat less likely to be on the Jewish federation mailing list and those intermarried households who are on the mailing list are most likely a biased sample of intermarried households in the sense that they would tend to be more involved in the Jewish community. (Sheskin 2005b:164-166)

In other words, as both distinctive Jewish names and list frames are biased, they should be avoided wherever possible because weighting procedures are likely to be inadequate. Because distinctive Jewish names frames are less biased on important variables, they are superior. Sheskin then goes on to outline his weighting procedures, which are identical to Tobin's.

> In all the studies I have undertaken, the bias introduced by the use of the DJN samples is removed via a weighting procedure that compares the RDD and DJN samples. The RDD sample is compared to the DJN sample on a number of key variables: geographic area, age of the head of the household, household size, household structure, marital status, length of residence, household income, home ownership, Jewish identification (Orthodox, Conservative, Reconstructionist, Reform, Just Jewish), type of marriage (in-married, conversionary in-married, intermarried), synagogue membership, JCC membership, familiarity with the Jewish federation, visits to Israel, and whether the subject donated to the federation in the past year. Chi-square tests are used to discern instances in which the RDD and DJN samples differ significantly, and appropriate weighting factors are applied to correct the biases introduced by DJN sampling. (Sheskin 2005b:166-167)

Sheskin uses two separate, overlapping samples (one random digit dialing, one of distinctive Jewish names) in contrast to the universally accepted dual-frame approach. All cases on the distinctive Jewish names frame will also be found on the random digit dialing frame, giving households with distinctive Jewish names multiple chances of selection, consequently doubling their influence on the final estimate. To adjust for the bias introduced by distinctive Jewish names—though not the bias of overlapping samples—a naïve form of sample balancing is carried out in an effort to make the distinctive Jewish names sample resemble the random digit dialing sample.[28] Unfortunately, this cannot overcome one of the less desirable features of distinctive Jewish names samples, that they systematically fail to count Jewish women married to non-Jewish men. As such households will be almost entirely absent from a distinctive Jewish names sample, no amount of weighting can make up the difference. Intermarried Jewish women are also more likely to raise their children as Jews (Fishman 2004). These are not problems if a distinctive Jewish names frame is used as part of a properly conducted dual-frame survey.

The use of overlapping samples and weighting based solely on matching characteristics flies in the face of more than half a century of scientific sampling. The foundation of sampling theory is that every sampling unit has a known probability of selection, the inverse of which is the sampling weight. In this instance, however, the probability of selection is never computed. The weight for a distinctive Jewish names case in the random digit dialing sample remains unadjusted despite its greater probability of selection. When the adjusted distinctive Jewish names cases are analyzed with random digit dialing cases, whatever characteristics are overrepresented in the adjusted distinctive Jewish names sample will be overrepresented in the random digit dialing sample, leading to bias.

In addition, because data from two separate samples are combined in analyses, the correct variance cannot be calculated, rendering inferential statistics useless. In other words, no tests for statistical significance are possible and confidence intervals cannot be computed. Sheskin, nevertheless, misleadingly reports margins of error (e.g., Sheskin 2003a:2:2, 2:9-10).

Sheskin's treatment of distinctive Jewish names concludes by questioning the procedures of other researchers:

> The procedure used in these studies is not perfect, but it guards against the DJN sampling introducing any significant bias. The studies by Gary Tobin use a similar procedure, but most of the local studies completed by other researchers are not clear about the manner in which they dealt with this issue. Thus, some of the results of the local Jewish community studies may be affected by biases introduced by non-RDD sampling methods. (Sheskin 2005b:167)

The claim that other studies fail to correct for the bias introduced by list frames is false. Ukeles & Associates, Sheskin's nearest rival in surveying Jewish communities, use dual-frame samples weighted by probability of selection, which do not lead to bias.[29]

Sheskin's ill-informed advocacy of distinctive Jewish names samples unfortunately obscures their potential utility. The potential benefit of a distinctive Jewish names sample is not its lesser degree of bias, but its efficiency, which is a result of its cost and variance. Developing a distinctive Jewish names frame simply requires searching for households with listed telephone numbers with "Jewish" surnames, which can be conducted very efficiently with computerized records (see Appendix C for several examples of distinctive Jewish names frames).

*Random digit dialing, list, and distinctive Jewish names.* In theory, there is no need to stop at using a two sampling frames—one can combine three or more sampling frames to achieve still better cost-efficiency. Properly integrating three overlapping sampling frames is, however, a daunting task.

As with dual-frame distinctive Jewish names samples, the first implementation of a multiple-frame study was a failure. The 1984 Pittsburgh study (Schorr 1984a) followed the new method of combining list and random digit dialed samples. Schorr estimated the size of the Jewish population by counting the number of distinctive Jewish names found in the Jewish Community Federation of Pittsburgh's list and the telephone directory, and calculating the proportion of the telephone directory names also found in the Federation list. This assumed that the proportion of Jewish households known to the Federation is the same for households without distinctive Jewish names as the proportion with distinctive Jewish names, a problematic assumption. The interviews with households with distinctive Jewish names were then supplemented by samples from list cases without Jewish names, unless they resided in zip codes in one of three high density areas. Random digit dialed calls were made to numbers in these zip codes. To further complicate the situation, a "small sample" was found by "going through memberships list of synagogues, temples, and other Jewish organizations" to find "households not in the Federation file" (Schorr 1984a:3). It is unclear whether this was used in the three exchanges where random digit dialing took place. Schorr reports that weights were assigned. Examination of the dataset (Schorr 1984b), however, finds that the weights appear to be approximate at best.[30] As with the 1981 New York study, this was by no means a universal sample and was unlikely to return accurate estimates.

Sheskin and Tobin at times conducted multiple-frame samples, doing so in San Francisco (Tobin and Sassler 1988b), Las Vegas (Tobin 1995a), St. Louis (Tobin 1995b), Milwaukee (Sheskin 1996), Philadelphia (Ukeles Associates 1997), and Jacksonville, which omitted somewhat over five percent of the Jewish community from the random digit dialed sample (Sheskin 2002b).[31] Unfortunately, they used a similar approach to their dual-frame surveys, which led to inaccurate data.

The 1991 study of Southern New Jersey (Ukeles Associates 1992) combined list, random digit dialing, and distinctive Jewish name frames in a different fashion. A universal sample drawing upon list and random digit dialing was drawn for the core area of Jewish population (Cherry Hill), while a nonuniversal sample based on households with distinctive Jewish names and known to the Federation was used in noncore areas. Data from the former was therefore generalizable while the latter was not.

There are several instances of properly conducted multiple frame samples. One is the 1997 Denver study (Ukeles and Miller 1998), which drew on a Jewish community list (presumably that of the Federation), a distinctive Jewish names sample, and a residual random digit dialed sample. This differed from previous attempts by removing duplicate entries from across frames. The list was used as the basic frame, households with distinctive Jewish names were then added (except where they appeared on the list), and finally random digit dialing (except where the telephone number was found either on the list or distinctive Jewish names frame). Accordingly, households were not double (or triple) counted as they had been in previous studies. The other example appears to be the Cleveland survey of 1997 (Raff 1996), which combined a random digit dialed sample, an ex-

panded list frame based on multiple organizational lists, a distinctive Jewish name frame, and a list of households from the former Soviet Union.

## EVALUATING JEWISH POPULATION STUDIES

The history of Jewish population studies in the United States defies easy generalization. In most periods, some studies of Jewish populations used advanced methods while others employed techniques that had been long since discredited. As we have seen, the factors behind the wide variation in methods do not remain constant.

Prior to the Second World War, medium size communities were often at the leading edge of methods while large communities relied on proxy measures. Presumably, this was at least in part a result of the lack of progress made by the sample method in the social sciences at that time; completely enumerating large communities would have been practically impossible. In contrast, medium sized communities had sufficient resources while at the same time being in a position to know virtually the entire population.

While larger Jewish communities adopted more advanced methods, old techniques have persisted to the present day. Smaller communities continue to use variations of the master list strategy first implemented in the 1920, sometimes even attempting to conduct censuses rather than sampling. While smaller communities are likely to know a greater proportion of Jews than are large communities, it is still questionable whether this approach is effective given Jewish residential dispersion and assimilation.

In the post-war period, larger communities took the lead, with Washington, DC and Boston employing area-probability samples. Nevertheless, the largest community of

all—New York City—remained reprobate in its use of inferior methods until 1991. Jewish population studies were similarly at the forefront of random digit dialing surveys, which succeeded area-probability samples due to increases in the latter's cost and declines in its effectiveness.

From the 1980s onwards, there was a veritable explosion of studies using variations on random digit dialing, presumably driven by its relative affordability. The new generation of studies concentrated far more on explicitly Jewish matters than had their predecessors, testimony to the thorough acculturation of Jews who felt secure in their position in American life, as well as a sense of growing concern about the threat posed by assimilation to the continuation of a distinctive and cohesive Jewish community. The 1990 National Jewish Population Survey was the epitome of this transformation, with its much publicized estimate of a 52 percent current intermarriage rate and exclusive use of random digit dialing. The new methods, however, were often implemented poorly, most notably in the many random digit dialing/distinctive Jewish names studies carried out by Gary Tobin while at Brandeis University and Ira Sheskin of the University of Miami. While most of these studies estimated the overall size of the Jewish population in an unbiased fashion (even so the confidence intervals derived from a small random digit dial sample would be quite large), their estimates of community characteristics were suspect.

The 1990s were marked by an apparent sense of comfort with the methods in use, mirroring that of survey researchers more broadly. Jewish studies did not adopt or even test more advanced methods for studying rare populations, instead adapting to the growing cost of survey research by greater levels of reliance on dual-frame designs and oversampling higher density areas. The fit of the random digit dialing method with the social

and technological environment was decreasing for survey research in general and studies of Jews in particular was, however, declining. These problems, as we shall see, came to a head in the 2000s.

## ENDNOTES

[1] The question of when Judaism can be said to have begun is highly complicated and ultimately subjective. I follow Shaye Cohen's (1999) use of "Israelite" to refer to the Hebrew population of the Biblical era, in recognition of the tremendous differences in religious practice and the location of religious authority between the Hebrew population at that time and that of the Talmudic and following periods.

[2] Daniel Elazar's studies of Jewish communal organization and political tradition (Elazar 1983; Elazar and Cohen 1985) and the subsequent work of the Jerusalem Center for Public Affairs (e.g., Levine 1999), are prime examples of the dangers of overemphasizing continuities in Jewish history. Elazar's attempts to fit the organization of Jewish communal life across time and space to the Talmudic notion of the three *ketarim* (crowns) governing society—*malkhut* (kingship), *kehunah* (priesthood), and *torah* (religious judicature)—has led to a Jewish political science literature that contorts itself trying to shoehorn contemporary communal organization into an political schema extinct for well over a thousand years. The perpetual popularity among some historians of "prophetic tradition" as an explanation for the anomalous liberalism of American Jews (Fuchs 1956; Sorin 1997; Walzer 1995) in the face of overwhelming evidence that the most religious Jews are the least liberal (Cohen 1985; Cohen and Liebman 1997; Lazerwitz et al. 1998; Lerner, Nagai, and Rothman 1989; Levey 1996) is another case where Biblical parallels are misread as evidence for the appealing notion of a continuous chain of attitudes and behaviors that binds contemporary Jewry to Israelite society.

[3] DellaPergola (2001) uses information on Jews from censuses from Australia, Azerbaijan, Belarus, Brazil, Bulgaria, Canada, the Czech Republic, India, Ireland, Israel, Kazakhstan, Kyrgyzstan, Latvia, the former Yugoslav republic of Macedonia, New Zealand, Romania, Russia, South Africa, the Soviet Union, and Switzerland. The United Kingdom's 2001 census—unavailable to Del-

laPergola when his article went to press—also included an optional question on religion. National population registers with data on religion are present in Estonia, Finland, Israel, Latvia, Lithuania, Norway, and Switzerland. Some censuses consider Jewishness to be an ethnicity, sometimes in addition to a religion; see also Kosmin (2005) and Millman (1983).

[4] I am grateful to Antony Polonsky for assigning a passage from Blau (N.d.) as my German translation examination.

[5] Unless otherwise specified my discussion of the development of the U.S. Census is based on Anderson (1988). The speculation on the relative lack of importance of religion is, however, my own. Anderson only offers mute testimony, mentioning religion only with a series of other subjects introduced in supplementary enumerations in the 1850 census.

[6] Of the 60,630 members of families surveyed, 67 percent were born in the United States. Over half (55 percent) of all family members had mothers born in Germany, with the next largest contingent (21 percent) having mothers born in the United States, many of whom were presumably of German descent.

[7] While low in relative terms, the birth rate was still far higher than today. Mothers in the sample had an average of 4.66 children, though only 3.56 for those born in the United States (Billings 1890).

[8] Groups in favor of the proposal included the Population Association of America, the American Sociological Association, the American Statistical Association, the Catholic Church, the Catholic Press Association, the National Council of Churches of Christ in the U.S.A., and the Association of Statisticians of Religious Bodies (Good 1959).

[9] To save repetitive citation, unless otherwise noted, my discussion of surveys oriented to social welfare is based on Converse's (1987) extremely thorough study of the antecedents of contemporary surveys.

[10] A quota sample aims to reproduce the composition of a population with respect to salient factors. For example, a quota sample might try to have a mix of respondents that match the age, sex, and racial mix of the population being studied.

[11] I am grateful for the assistance of Jonathan Sarna, as well as Kevin Proffit of Hebrew Union College, Cincinnati.

[12] The dissertation is quite brief and provides little contextual information. There is no description of the methodology used and even the department the dissertation was written for remains obscure. The author's motivation for studying Jews is not described. I surmise Mr. Talamo was not Jewish from that faithful stand-by, the lack of a distinctive Jewish name, as well as references to "these people" (p. 8), which I take to indicate a degree of distance. Wirth's (1966) unflattering portrait of Jewish immigrant life, however, is a warning that kinship does not always lead to affective ties.

[13] Talamo's (1915) dissertation lacks a methodology section and provides no information on data gathering. However, detailed breakdowns of age, nativity, occupation, gender, as well as references to "the population we have 'covered'" (p. 8) and "our investigation of 3500 Jews of [sic] city" (Table II) lead me to believe a survey, whether by interview or questionnaire, was conducted. A Master of Arts seems too little reward for the tremendous effort that collecting and tabulating this data (no doubt by hand) would have required. The degree of sympathy shown for the Jewish community is considerable. The author goes out of his way to demonstrate that Jews are underrepresented among pawn brokers and do not own any of the money lending establishment, concluding that "In Worcester, at least, the Shylocks are not Jews" (p. 23). He similarly debunks the popular association between Jews and the liquor trade, and qualifies his report of the prevalence of Jews in peddling with the (prescient) assessment that "it is probable, though, that as time goes on they will enter a greater and greater number of industries and will not limit themselves to those which they have traditionally held" (pp. 24-25).

[14] The key informants interviewed included Jewish communal professionals ("professional Jewish social workers," p. 10), real estate dealers, school principals and superintendents, telephone, insurance, and train company executives, the assistant secretary of the chamber of commerce, the state census director, city planners, senior health administrators, and an official at the State Intelligence Bureau.

[15] Only brief mention is made to movement outside the Boston municipality: "The remaining two large Jewish centers, Roxbury and Dorchester, are also losing part of their Jewish population: the

well-to-do class, who are moving to districts outside of Boston City, to Brookline, Allston, the Newtons, etc." (Rosen 1921:18).

[16] Unfortunately, these works are generally inaccessible. The 1922 National Jewish Welfare Board reports are held at the library of the Cincinnati campus of the Hebrew Union College, while the 1923 Cleveland report only exists in microfilm at the Western Reserve History Society library and is unavailable for interlibrary loan, and the 1923 Detroit report at the Detroit Public Library.

[17] I am indebted to Jonathan Sarna for bringing this reference to my attention.

[18] A separate attempt to estimate the growth trajectory of the Detroit Jewish population compared the total number of telephone directory listing with the following surnames: Cohan, Cohane, Cohen, Cohn, Freidman, Friedman, Gold, Goldberg, Goldenberg, Levene, Levenson, Levey, Levi, Levin, Levine, Levinson, Levy, Rosen, Rosenberg, Rosenbaum, Rosenblatt, Rosenfield, Rosenthal, and Rosenzweig (Meyer 1943:112).

[19] Ritterband et al. (1988) provide a comprehensive overview of death-rate studies.

[20] I am thankful to Jonathan Sarna for providing these references.

[21] Unless otherwise cited, all information on the development of survey research as a field is sourced from Converse (1987).

[22] Studies known to use only lists were Los Angeles (Massarik 1953), Canton, Ohio (Jewish Community Federation of Canton 1955), Worcester, Massachusetts (Cohen 1961), Providence (Goldstein 1964), Cincinnati, Hollywood, St. Paul, Dallas (Maynard 1974), Houston (Schulman, Gottlieb, and Sheinberg 1976), Minneapolis (Geer, Griffel, and Lazarus 1981), Toledo, Ohio (Odesky 1994), Milwaukee (Sheskin 2000c), Houston (Herman and Phillips 2001a), Nashville (Yacoubian Research 2002), and Youngstown, Ohio (Hart 2003).

[23] Other list-distinctive Jewish name surveys took place in Cleveland (Jewish Community Federation 1982), Miami (Sheskin 1983), Manchester, New Hampshire, Richmond (Jewish Community Federation of Richmond 1984), Kansas City (Tobin, Levy, and Asher 1986), Rochester (Tobin and Fishman 1987a), New Orleans (Tobin and Sassler 1988a), San Antonio (Proust 1990), St.

Paul (Tobin 1993), Minneapolis (Project Research Inc., Tobin, and Berger 1994), Akron (Wise 1999), and Cleveland (Immerman and Jacobs 2004).

[24] Communities are at times rather loosely defined: Tucson and Las Vegas are grouped together. Massarik and Chenkin (1973) term these areas "strata," which is a little misleading as they are closer to self-representing clusters.

[25] The Alaska and Hawaii samples came from lists as the omnibus survey used to screen for Jewish households was not conducted in these states (Kosmin et al. 1991b).

[26] A seventh book in the State University of New York Press Series on American Jewish Society in the 1990s, *The Conservative Movement in Judaism*, by Daniel Elazar and Rela Mintz Geffen (2000), does not use NJPS 1990 at all.

[27] The 1995 Boston study (Israel 1997) chose to oversample Allston-Brighton. This is was a rather strange choice given that area has a comparatively dense Jewish population composed of Russian Jewish migrants, college students living independently, and young professionals. This may be a case where the rationale does not match the description. A more likely reason for this choice is that this was the most significant low density Jewish community. An oversample of Roxbury and Dorchester (former areas of Jewish settlement, now nearly devoid of Jews) would have been virtually pointless as there would have been next to no Jews to count.

[28] The resemblance between Sheskin's weighting adjustment of a distinctive Jewish names sample to match a random digit dialing sample and sample balancing (also known as rim weighting) is largely superficial. Sample balancing is a form of poststratification weighting that adjusts a single sample that has been previously weighted for probability of selection to known totals on key variables (see Chapter 5). A sample of United States residents might, for instance, be matched to Current Population Survey estimates of age, race, gender, and level of education to prevent undercoverage, for instance, of Hispanic-Americans. Traditional sample balancing (Deming 1943; Deming and Stephan 1940) iteratively adjusts the sample weights until the weighted distribution of the sample on $k$ categorical target variables matches that of the target values to a specified degree of precision. By contrast, Sheskin's procedure is used in lieu of weights for probability of selection for the distinctive Jewish names sample which, as I have discussed, is only possible in the context of a dual-frame sample. This is roughly equivalent to the

first iteration of rim weighting, as values adjustments are not repeated until convergence occurs. Accordingly, the later a variable is entered, the greater effect it will have on the final weights. The distribution of early variables will almost invariably differ from the target values and may potentially be more biased than prior to weighting.

[29] Ukeles Associates studies do not, however, follow standard dual-frame methodology derived from Hartley (1962), treating the frames as independent, incomplete samples. While this procedure does not introduce bias, it does increase variance.

[30] Distinctive Jewish name households drawn from the Federation's list without listed telephone numbers receive a weight of .8, cases from the other lists receive a weight of .7, a small number of cases (presumably from a reported oversample) receive a weight of .6, while the balance have a weight of 1.0.

[31] Gary Tobin was part of the research team for the Philadelphia study.

# Chapter 3

## Contemporary Challenges

The definitive moment for survey research in the present decade occurred far into the night of November 2, 2004, when the national exit poll conducted for the National Election Pool, a consortium of news organizations, called John Kerry the next president of the United States, with a national margin of 51 percent to 48 percent over the incumbent George W. Bush. The final tally was in fact precisely the opposite. While this survey has been cited as evidence that the election was somehow fixed, the reality is more mundane. The primary reason for the discrepant results was that people who had voted for the president were simply less likely to agree to be interviewed than were those who voted for John Kerry (Edison Media Research and Mitofsky Election System 2005). While this chapter will focus on telephone surveys rather than the in-person interviews that constituted the bulk of the exit poll data, the field is increasingly threatened by discrepancies between the respondents who are interviewed and the universe they are meant to represent.

Simply put, telephone survey research is in crisis, with its ability to provide adequate samples in serious doubt. The American Statistical Association convened a special summit in early 2006 to address these concerns, but no panacea has emerged. In this chapter, I examine the nature and extent of the present crisis in a variety of areas: the rapid decline in survey response rates, growing concerns about the proportion of the population covered by standard telephone sampling frames, and increasing evidence of differential response rates between various groups in the population. I then move to a detailed analysis of the problems faced by the National Jewish Population Survey 2000-01 as a case study in the difficulty of surveying Jewish populations in the contemporary environment. Before discussing either, however, it is important to consider the problems inherent in understanding the adequacy of research methods.

## THE EPISTEMOLOGY OF SURVEY RESEARCH METHODS

I previously made an analogy between Kuhn's (1962) conception of scientific progress and the punctuated evolution of survey methods. The analogy, however, is not a perfect one, as the nature of the change differs. For Kuhn, a paradigm shift occurs when observations diverge from predictions derived from existing theory. In the case of survey methodology, rather than a decline in the goodness of fit between theory and observations, observations will increasingly diverge from expectations as a method becomes increasingly ill-adapted to its environment. Determining whether misfit has occurred, however, is considerably more difficult than finding anomalous observations. The divergence from expectations could be due to changes in the characteristic of interest in the population (in

which case the method is not at fault), or might be an artifact of a maladapted method, or perhaps even a combination of the two. Where there is external knowledge about the population of interest, identifying maladaptation and adjusting for it may be possible, depending on the extent to which demographic characteristics (a common measure of fit between sample and survey universe) can be assumed to be an adequate proxy measure. A study of lawyers might, for instance, identify bias—systematic differences between a sample and a population—by comparing the age, race, and gender of the sample to U.S. Census or the Current Population Survey estimates for lawyers. Even if the sample can be demographically matched, it is possible that the sample will differ from the population in other meaningful ways. Senior partners, for instance, might be underrepresented, as could lawyers from small firms.

Identifying bias where surveys are the sole source of information on the population of interest—as is the case with American Jews—is more complicated. In such cases, the process of identifying bias is a form of validity assessment, albeit one focused on sampling strategy rather than an item or scale. Where one would compare a putative measure of a construct to other items believed to be associated with the construct, validating methodology compares salient aspects of the survey and its data to other evidence. In such cases, identification of maladaptation or bias is necessarily contingent, relying on the convergence of multiple lines of reasoning. Forms of validation can be divided into those than can be done *post hoc* and do not alter the study design, and *ex ante facto* tests which are built into a survey.

*Post Hoc Validation*

*Post hoc* tests do not rely on particular features of the study design and may be used as part of a secondary analysis. While this flexibility is valuable, it is not without cost. These methods generally draw much of their explanatory power from comparable surveys. If there are few such surveys, the paucity of data will make it difficult to reason *ex post facto*.

*Comparison to contemporaneous surveys*. Where multiple surveys contain information on the population of interest, comparisons can be made on overlapping items; the greater the level of agreement, the better the evidence of validity. When data from multiple contemporaneous surveys are available, meta-analysis is highly desirable as a means of increasing the accuracy of estimates. This type of evidence is most powerful when comparable surveys are contemporaneous and contain similar items, but differ with regard to methodology. If too many features are changed, determining the source of variation will be extremely difficult, although the point at which too few elements are held constant is a subjective determination.

*Comparison to prior surveys*. Where data on the population of interest is available from previous surveys, comparisons can be made between past and present. More precisely, comparisons are made between extrapolations from earlier data and the survey of interest. The persuasiveness of this evidence is dependent on a variety of factors. Projections are most accurate when based on a long, frequent, and regular series of data points. Where possible, the accuracy of predictive modeling should be assessed for the immediately previous time-point in the series. Variations in methodology significantly diminish confidence in the comparability of contemporary data to projections based on earlier

studies. This type of evidence can be confounded where the research environment changes gradually, leading to a steady increase in the level of systematic error that is wrongly attributed to changes in the population of interest. In such cases, projections based on earlier data may be quite accurate but misidentify the cause of variation. Given that most social phenomena vary over time and may undergo rapid change, such evidence must be assessed cautiously, as the departure from the trend-line may be true variation in the characteristics of the population.

*Comparison of screening data to demographics*. While the true characteristics of the population of interest may be unknown, the demographics of the screening sample may be compared to estimates of the population at large derived from the Census or high quality surveys like the Current Population Survey. While variations between survey and external data are amenable to weighting, large differences are an indicator of potential problems in the sample. Concern should be greatest where the differences are larger than those of comparable studies, whether earlier or contemporary.

*Correlation with social indicators*. Many rare or hidden populations participate in distinctive activities or consume particular goods and services. Where information on such social indicators is obtainable, the data may be compared to survey estimates for parallel time periods.[1] Unfortunately, the relationship between membership in a rare or hidden population and identifiable behavior often varies over time. Identifiable behavior may decrease, as is the case with ethnic languages, the use of which tend to diminish with length of residence in the destination country. However, identifiable behavior may also increase as social acceptability changes—witness the bruited ethnic revival of the 1960s (Glazer and Moynihan 1970; Novak 1971); but see Gans (1979). Finally, the population

at large may increase or decrease its participation in distinctive activities or consumption, like the increasing popularity of kosher products with non-Jewish consumers, who feel they are of higher quality. In each case, the correlation may be confounded. Social indicators are probably best used as indicators of growth or decline rather than magnitude and should only be used in combination with evidence of the relationship between the measure and the hidden population.

*Peripheral indicators of data quality*. Traditional indicators of data quality like response and other outcome rates are useful indicator of potential problems with a sample. As with screening data and demographics, however, this evidence is most meaningful in comparison to other surveys. Outcomes rates that are poor in comparison with other studies are an indicator of potential problems.

*Variation between screener and main interview*. In cases where the respondent is selected from household members, as opposed to interviewing the initial informant who first picks up the telephone, attrition may be greater among some groups of respondents. Men, for instance, may be more likely to refuse to participate in the main interview. In addition to being methodological problems in their own right, such variations may indicate that response rates to the screening phase differ for certain groups. If male respondents are less likely to agree to participate, it is likely that initial informants who are male will be less likely to agree to be interviewed in the first stage, leading to bias. While variations between the screener and main interview are listed as *post hoc* tests because the screener dataset is a part of the design that primarily serves another purpose, its utility depends on the specifics of the study. The more comprehensive the range of items found in the screener, the more useful a screener is in identifying potential bias. As a practical

matter, screener datasets are far less likely to be publicly available than are datasets from the main study.

*Ex Ante Facto Validation*

Where *post hoc* validation requires external sources of data, *ex ante facto* tests must be built into the study design, which in all but exceptional circumstances limits their utility. Because the designer controls the treatments, limiting extraneous sources of variation, *ex ante facto* tests are more powerful than their counterparts, in much the same fashion as an experimental design rules out alternative explanations more effectively than quasi-experimental studies. The lack of external comparisons, however, can only provide relative evidence of validity (i.e. one method works better than another), being blind to biases that equally affect the methods being tested.

*Methodological experiments*. Experiments assign cases to a variety of treatment conditions, which allows the analyst to compare the outcomes of a particular set of treatments corresponding to one method to the outcomes of treatments analogous to another methodology.[2] However, an experiment alone says little with regard to assessing methodological fit. Unless the experiments are performed serially as part of a longitudinal design, however, they are unable to determine whether the effectiveness of one method has changed respective to another over time. Using experiments in a study itself, as opposed to a pilot, raises additional issues as the portion of the sample that received the "better" treatment systematically differs from the rest. While postestimation weights can be used to match the remainder of the sample based on salient variables, it runs much the same

risk as does weighting to demographic targets as the weighting may not correct for other, unobserved biases.

*Nonresponse testing*. The second form of *ex ante facto* testing is to build nonresponse estimation into the study design. While performed *ex post facto*, planning will usually be done in advance of the start of interviewing. While postestimation weighting on demographic variables can be seen as a form of nonresponse modeling and does not require pre-planning, it applies only where census or other high quality data include the applicable variables. In other cases, estimates of the characteristics of nonrespondents are derived from extrapolations from the hardest to reach cases, measured in calls required before interviewing in the case of noncontacts or soft refusals converted in the case of refusal. This method is subject to a recursive form of criticism that insists that any respondent, no matter how difficult to interview, systematically differs from nonrespondents (Lin and Schaeffer 1995). A partial answer to such objections is to convert additional noncontacts and refusers by offering a significant monetary incentive in return for providing some information. Strictly speaking, this does not answer the criticism as some portion of the population will never be interviewed (e.g., nonresponse to the mandatory U.S. Census is believed to be most common among groups with lower socioeconomic status), but an imperfect nonresponse model may be preferable to no nonresponse model at all.

### *Bias in Methodological Evaluation*

As a practical matter, most methodological evaluation occurs in response to an unexpected finding. Unexpected findings, in turn, are most likely to occur where there is a sudden change in the research environment, although suddenness is relative to the fre-

quency of surveys—a study that recurs every ten years is far more likely to see dramatic shifts, whether due to misfit or population characteristics, than will a study repeated each year. Limiting methodological evaluation to such cases, however, may systematically fail to detect gradual decreases in methodological fit and the increasing levels of bias that accompany them because the slow and relatively even changes in estimates are not eye-catching. Similarly, an increase in bias and a roughly equal but opposite change in population characteristics will give a false picture of stability that does not draw attention to itself.

*Persuasive Methodological Validation*

Methodological evaluation is a particularly contingent and nuanced form of social scientific investigation as it rests on the accumulation of a variety of evidence, none of which would be persuasive by itself. An unfortunate side-effect of contingency is that the persuasiveness of claims of bias (or the reverse) rests on the extent to which the reader believes the author's argument is a reasonable interpretation of the facts. As with Shadish, Cook, and Campbell's (2001:40-42) contention that causal inference is more persuasive where threats to validity are explicitly considered as part of the argument, even if they cannot be directly tested. In keeping with this suggestion, evaluation of survey methodology should use a relatively "thick" argument that is explicit regarding the assumptions underlying the data presented and considers the evidence for the alternative finding.

## RESPONSE RATES

Having examined the difficult task of identifying bias in surveys, I will turn my attention to the contemporary survey research environment. As I discussed previously (p. 141), peripheral indicators of data quality, like response rates, are often used to evaluate the quality of surveys. A response rate is defined as "the number of complete interviews with reporting units divided by the number of eligible reporting units in the sample" (Frankel 1983). In other words, a response rate is an estimate of how successful a survey was at reaching the population of its sampling frame. It is generally assumed that the higher a response rate is, the more likely a survey reflects the characteristics of the target population (Battaglia et al. 2006). This, however, only holds true as far as nonresponse bias goes—the extent to the survey's estimates differ from the true mean as a result of systematic differences in response rates between groups of respondents.

However, coverage bias—differences between the observed value of the sample and the true value of the population that are the result of certain types of units being systematically underrepresented on the sampling frame—is unrelated to the sampling frame. Thus, a response rate is only a partial indicator of potential bias. One might, for example, obtain a sample of Americans by interviewing every a person in a diner. While this might yield a 100 percent response rate, the sample would still be unrepresentative due to an inadequate sampling frame. Conversely, a poor response rate will only result in bias if those who do not respond differ systematically from those that do.

In most cases even very good response rates are generally far from 100 percent, leaving response rates as a measure of comparative inadequacy rather than a gold standard of survey quality. For these reasons, survey methodologists increasingly look to

evaluate evidence for the various types of survey error rather than focusing solely on response rates. Nevertheless, it remains true that reaching more respondents is generally a good thing, so we will look first at trends in response rates.

*Declines in Response Rates*

It has become increasingly difficult to interview people for survey research. In part, this is a continuation of long term trends. Steeh (1981) documents a steady increase in refusal rates and other types of nonresponse from 1952 to 1979 to the University of Michigan's Survey of Consumer Attitudes and National Election Studies. The response rate of the Survey of Consumer Attitudes, a high quality monthly survey that uses monetary incentives and makes an unlimited number of contact attempts during the month-long field period, continued to fall, dropping from about 72 percent in 1979 to 60 percent in 1996, despite increased effort, reflected in growth of the average number of calls required to complete an interview rising from 3.9 in 1979 to 7.9 in 1996 (Curtin, Presser, and Singer 2005). From 1996, the response rate to the Survey of Consumer Attitudes fell more steeply again at 1.5 percent per year (compared to 0.7 percent per year between 1979 and 1996), standing at 48 percent in 2003 (Curtin et al. 2005).

The Survey of Consumer Attitudes hardly stands alone. The world's largest telephone survey, the Behavioral Risk Factor Surveillance System, an annual series of cross-sectional surveys conducted by state health departments in the United States with the assistance of the Centers for Disease Control and Prevention, similarly saw an annual average decline in the median state response rate of 1.9 percent from 1995 to 2003 (Battaglia et al. 2006; National Center for Chronic Disease Prevention and Health Promo-

tion 2006). The same rate of decline was true over the period 1995 to 2004 for the National Immunization Survey, also sponsored by the Centers for Disease Control (Battaglia et al. 2006). A similar drop is observed by the Pew Research Center for The People & The Press (2004). Even surveys conducted by the U.S. Census Bureau have seen declines (Atrostic et al. 2001). Rates for market research, never very good, have fallen to dismal levels.[3] The Council for Marketing and Opinion Research's (2004) Cooperation Tracking System, sponsored by the Marketing Research Association, reported overall telephone response rates of 18 percent, but only 9 percent for random digit dialing samples, both representing falls from earlier years; updated figures are no longer posted. While the largest falls have been observed for telephone surveys, response rates have also fallen for face-to-face interviews, the gold standard of survey research. The National Health Interview Survey, conducted by the Census Bureau for the Centers for Disease Control fell from 95.5 percent in 1990 to 86.9 percent in 2004 (Bates and Piani 2005). The factors behind the decline in response rates can be divided into proximate and ultimate causes, which I examine next.

*Proximate Causes*

Nonresponse to surveys is a product of two factors: noncontacts and refusals. A noncontact occurs when an interviewer is unable to determine the status of a sampling unit. In telephone surveys, this can occur when a telephone number is permanently busy, always reaches an answering machine, is never picked up, or other similar outcomes. Refusals occur when a sampling unit is reached but declines to participate in the study. Both noncontacts and refusals have contributed to the dramatic decline in response rates in recent

years. Households have become more difficult to reach, for reasons I will examine under ultimate causes (Battaglia et al. 2006; Curtin et al. 2005:93; Piekarski, Kaplan, and Prestegaard 1999). Increased difficulty in contacting households was primarily responsible for the decline in response rates to the Survey of Consumer Attitudes between 1979 and 1996 (Curtin et al. 2005), a change from the period of 1952 to 1979, where increased levels of refusal rather than difficulty making contact led to falls in response rates. After 1996, refusal rates once more became the driving force behind the falling response rate (Curtin et al. 2005). Other surveys have also experienced increases in the level of refusals (Atrostic et al. 2001; Battaglia et al. 2006). Knowing that surveys have seen increases in refusal and noncontact rates, however, tells us relatively little. The more interesting question is why these changes have occurred, which I turn my attention to now.

*Ultimate Causes*

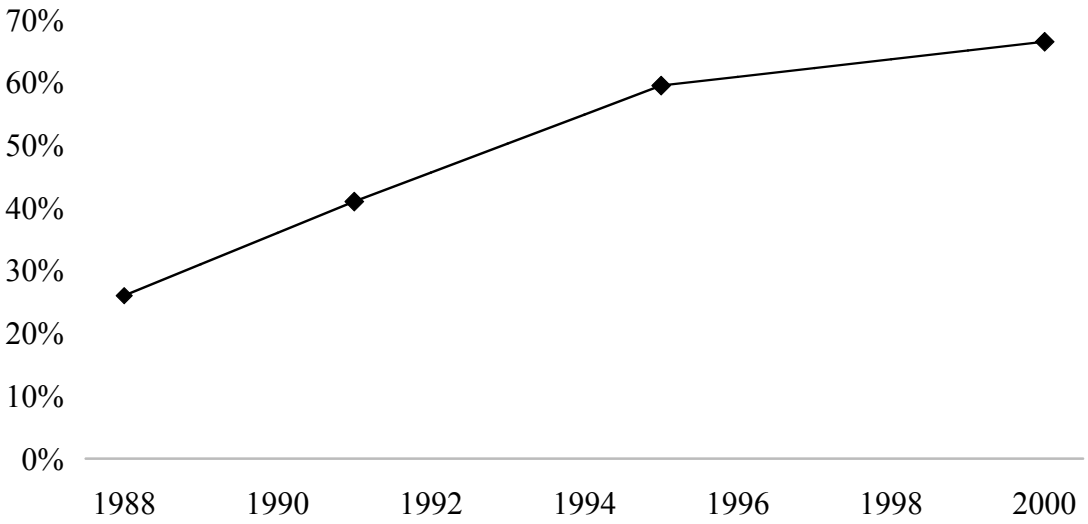
The ultimate causes behind changes in refusal and contact rates are the product of both societal and technological forces. The very fiber of the American telecommunication system has been changing, having profound impacts on telephone surveys. In part driven by forces associated with technological change, such as the growth of telemarketing in the late 1990s, Americans have in turn changed their behavior, leading to further changes in the technological landscape. Below I examine the nature and impact of these changes. This treatment relies on argument by analogy, assuming that owners of devices that can be used for call screening do in fact employ them in order to avoid surveys, and that increased ownership necessarily results in increased nonresponse. While this is entirely reasonable, a key question is whether the specific variations in response rates predicted

from ownership of these devices are in fact borne out by other types of evidence. I attempt to synthesize multiple lines of evidence regarding nonresponse at the end of this section (pp. 172-176).

*Answering machines.* The massive changes in American telephony over the past decade or so can be seen in the methodological literature on answering machines. In the early 1990s, answering machines were just emerging as a potential barrier to telephone surveys. The consensus of three *Public Opinion Quarterly* articles devoted to the issue (Oldendick and Link 1994; Piazza 1993; Tuckel and Feinberg 1991) was that answering machines constituted a potential rather than actual threat to surveys, and in some ways were potentially beneficial as households with answering machines were more likely to be amenable once reached than other types of noncontactable households (Xu, Bates, and Schweitzer 1993). A little over a decade later, critical attention had largely shifted from answering machines to newer technologies.

Nevertheless, ownership of answering machines has increased steadily since 1988, with more than 50 percent of households possessing one by 1995 (Figure 3:1). Use of answering machines for screening, however, appears to be relatively stable, with the proportion of answering machine households sometimes or always screening for calls practically unchanged from 35.2 percent in 1995 to 35.8 percent in 2000 (Tuckel and O'Neill 2001b). The increased ownership of answering machines, however, means that the proportion of telephone households using answering machines for call screening increased slightly from 21.0 percent to 23.9 percent in this period.

Figure 3:1. Ownership of Answering Machines




*Source:* Tuckel and Feinberg (1991); Tuckel and O'Neill (2001a)

*Note:* Markers indicate points of observation.

While I will examine nonresponse error subsequently, answering machine owners are disproportionately younger, better educated, come from higher income families, live in large size cities or their suburbs, and be found in the Mid-Atlantic or Pacific regions (Tuckel and O'Neill 2001b).

*Caller identification.* In contrast to the steady growth in telephone answering devices, subscription to caller identification (caller ID) services that display the number of a caller increased from 10.2 percent of households in 1995 to 45.0 percent in 2000 (Tuckel and O'Neill 2001b). It has no doubt increased considerably since then, but no updated statistics are available. Like answering machines, use of caller ID is not distributed evenly among all Americans, most commonly being used by younger Americans, the more educated, African-Americans, households with children, living in medium-size cities, and

residence in the South (Callegaro, McCutcheon, and Ludwig 2006; Link and Oldendick 1999; Pew Research Center for The People & The Press 2004; Tuckel and O'Neill 2001b). The pattern of those who use caller ID for screening is generally quite similar: the young, African-Americans and Latinos, single adult households and households with children, people in medium and large cities and in the suburbs surrounding large metropolitan areas, and residence in the South and Mid-Atlantic (Link and Oldendick 1999; Pew Research Center for The People & The Press 2004; Tuckel and O'Neill 2001b). The one major difference is that both low and very high levels of income are associated with frequent screening (Tuckel and O'Neill 2001b).

*Other technologies.* In addition to telephone answering devices and caller ID, other technologies have also emerged as obstacles to telephone surveying. Anonymous call rejection blocks callers that do not display identification, as some research organizations do (Battaglia et al. 2006). Call blocking technologies like the TeleZapper use a brief inaudible tone to mislead autodialers into classifying a telephone number as disconnected (Battaglia et al. 2006).

*Other factors.* As I previously alluded to, survey methodologists view the growth of telemarketing as a major contributor to declines in the contact rate and increased levels of refusals (Battaglia et al. 2006; Frankel 2003). It was initially hoped that the Federal Do Not Call list, which places severe restrictions on telemarketing while allowing most forms of survey research, would reduce the rate of decline in response rates or even reverse the trend. This has not happened and it has been argued that the Do Not Call list has in fact made people less likely to participate in survey research (Steeh and Piekarski

2006). Alongside the growing volume of telemarketing, numbers of telephone surveys fielded have also grown, further trying people's patience.

Researchers have also claimed that Americans spend an increasing amount of time away from home, making it harder for survey researchers to make contact and presumably leading people to guard their precious spare time more carefully (Battaglia et al. 2006; Frankel 2003).

*Variation in Response Rates*

Response rates are a threat to survey validity only to the extent that different groups respond at different rates (see pp. 31-32). If respondents and nonrespondents are identical with respect to a given variable, no bias will result. Comparing the response rates of given areas with the characteristics of those areas can shed some light on possible biases, although this data is relatively weak as areas are heterogeneous. However, authoritative data on the characteristics of areas is readily available from the U.S. Census and government surveys, in contrast to data on nonrespondents themselves. I will examine individual-level data, which is not without problems of its own, later this chapter. It is also important to bear in mind that, unless otherwise specified, these relationships do not take into account other factors. Low response rates associated with low levels of income may not actually be a product of low income, for instance, but a spurious correlation resulting from the association with large migrant populations with both low income and response rates.

*Age.* Results differ with respect to age. Kennickell (1998) found response rates lower in areas with older populations, having controlled for other factors. Battaglia et al.

(2006) report response rates are highest in areas with more 45-54 year-olds, while Huddy et al. (2006) arrived at the puzzling conclusion, as they themselves admit, that areas with more 18-24 year-old males had higher response rates.

*Household composition.* Battaglia et al. (2006) report that areas with more children have higher response rates.

*Race.* There is general agreement that response rates are lower in areas with greater black and Hispanic populations and higher in areas with more whites (Battaglia et al. 2006; Huddy et al. 2006; Kennickell 1998; Triplett and Abi-Habib 2005). Disagreement exists on the effect of Asian populations, with Battaglia et al. (2006) and Triplett and Abi-Habib (2005) finding higher response rates and Huddy et al. (2006) concluding the opposite.

*Education.* Areas with higher proportions of college graduates appear to have slightly higher response rates and lower refusal rates (Huddy et al. 2006; Kennickell 1998), although Triplett and Abi-Habib (2005) come to the opposite conclusion.

*Employment.* Findings with respect to employment are consistent and make sense. Areas with higher proportions of people working at home (and presumably able to be reached on the telephone) have higher response rates (Huddy et al. 2006), while those with more working men and longer average commuting times have lower overall response rates (Kennickell 1998), while Triplett and Abi-Habib (2005) found response rates were only marginally lower in areas with higher employment rates.

*Socioeconomic status.* The limited data on socioeconomic status suggests that response rates may be lower in areas with higher rents and property values (Battaglia et al. 2006; Huddy et al. 2006; Kennickell 1998). In addition, Huddy et al. (2006) suggest that

response rates may be lower in areas with more vacant houses, indicating that the highest response rates may be achieved in areas with modest property values.

*Region.* Accounts of the effect of region differ, with Battaglia et al. (2006) reporting lower response rates on the East Coast and Huddy et al. (2006) finding lower response rates on the West Coast, although the effect disappeared once the migrant population of the West Coast was controlled for.

*Urbanicity.* There is a broad consensus that response rates are lower in urban areas and higher in rural areas (Battaglia et al. 2006; Huddy et al. 2006; Losch et al. 2002; Steeh 1981; Triplett and Abi-Habib 2005).

*Evaluating variation in response rates.* Inference concerning the characteristics of nonresponders by comparing the aggregate response rates of areas with their aggregate characteristics does not lead to strong conclusions. There is a disconcertingly high rate of disagreement between studies. When combined with the fact that relationships between possible causes are not controlled for, the data become very problematic. There is consensus, however, that urban areas have the lowest response rates, as do areas with high socioeconomic status and large working populations. The studies also agree that areas with larger black and Hispanic populations have lower response rates. How this integrates with the finding that areas with higher socioeconomic status (which black and Hispanic areas usually are not) also have low response rates is unclear.

NONRESPONSE

The evidence provided by correlating the aggregated characteristics of an area with the response rate of its inhabitants is quite weak and sometimes contradictory. One reason for this may be the tendency of response rates to conflate two separate processes: how difficult an individual is to contact and the likelihood a person will cooperate if contacted. To this point, nonresponse bias has been explored via indirect means by examining the aggregate characteristics of areas with their aggregate response rate, which makes it difficult to identify individual factors. At this point, I shift from *post hoc* methods for understanding bias that rely on the analogy between external data to *ex ante facto* techniques that extrapolate from the characteristics of difficult to reach cases to those that could never be reached.

As was discussed previously (pp. 137-144), this method is not without potential problems of its own, as difficult to reach cases may not, in fact, resemble those that proved impossible to reach. This argument is most forcefully made by Lin and Schaeffer (1995), who used data on child support payments from a survey of fathers who were delinquent in paying child support to examine the extent to which difficult to reach cases resembled those that proved impossible. In this instance, it was possible to collect data about true nonrespondents by interviewing the mothers whose children the delinquents failed to support. In general, one would look askance at general conclusions derived from a study of such a particular population, but the rarity of having data on nonrespondents means that we must rely on unusual samples. Lin and Schaeffer concluded that the child support payments of the most difficult to reach cases in some instances looked less like those fathers who never responded than did cases that were easier to interview, resulting

in conclusions that were not simply inaccurate but actually wrong. While Lin and Schaeffer's finding is nearly universally cited as a caveat, attempts to convert nonrespondents persist, as do analyses of their characteristics. How much faith one puts in *ex ante facto* evidence is a matter of judgment. The unspoken view of survey researchers appears to be that while Lin and Schaeffer's finding was incontrovertible with respect to the population they studied, more evidence is required before abandoning such a potentially useful line of analysis. I think it is a reasonable one, and will consequently present data from *ex ante facto* studies.

### *Noncontacts and Refusals*

Nonresponse actually consists of two separate phenomena with potentially different etiologies. Eligible cases may be excluded from a survey because they proved impossible to contact, while others may have been contactable but refused to participate. While these have the same result—bias if nonrespondent cases differ systematically from those that respond—the causal factors may be different and potentially even pull in disparate directions. It is conceivable that some types of units might be easy to contact, but highly likely to refuse, while others are difficult to reach but readily agree. Accordingly, the following discussion distinguishes between these forms of nonresponse.

### *Characteristics Associated with Nonresponse*

As with coverage and *post hoc* analyses of the correlates of variation in response rates, this section will examine the literature regarding factors associated with nonresponse. As

with the previous section, these relationships do not take into account other factors, unless otherwise specified.

*Age.* The evidence from comparing census data to response rates was contradictory and unfortunately the same applies to data from individuals. Currivan (2005) reports that older people are less likely to initially refuse to participate in surveys, while Curtin et al. (2006) find precisely the opposite. Supporting Curtin et al., Biemer and Link (2006) find that older adults are much more likely to be early cooperators. On the other hand, Tuckel and O'Neill (2001b) report that attitudes about surveys are most negative among people aged 60 and above. The one point on which there appears to be consensus is the difficulty of contacting young people (Keeter et al. 2000b), although as Fleeman (2006) notes it is unclear whether this is a result of problems with the survey frame (young people are less likely to be on the telephone survey frame at all), or whether they are more likely to refuse. The most likely explanation for these apparently contradictory data is that older individuals are more likely to be contactable but also more likely to refuse to participate (Curtin, Presser, and Singer 2000).

*Household composition.* As was the case with findings based on comparison with census data, one person households appear to be the least likely to respond, being the most difficult to reach (Keeter et al. 2000b) and the most negatively disposed to surveys (Tuckel and O'Neill 2001b). Having children in the household was not, however, reported to affect the likelihood of refusals (Currivan 2005).

*Race.* Nonwhites are more likely to be difficult to contact than white respondents (Biemer and Link 2006; Curtin et al. 2000). The picture on cooperation is a little more difficult to assess. On the one hand Curtin et al. (2000) conclude that are nonwhites are

more cooperative, while their more recent paper (Curtin et al. 2006) found no differences, as did Currivan (2005), and while Tuckel and O'Neill (2001b) found Hispanics held the most negative attitudes to surveys. Finally, Keeter et al. (2000a) reported that respondents to a more rigorous survey design were more likely to be white, which goes against the finding that whites are easier to contact and no more likely to refuse.

*Education.* The effect of education is little clearer in studies of nonrespondents than analyses based on census data. The more highly educated do appear to be more difficult to contact (Curtin et al. 2000; Keeter et al. 2000b). Refusals, however, remain an open question. Currivan (2005) found that the least educated were more likely to initially refuse, while Curtin et al. (2000) came to the opposite conclusion. Overall, Keeter et al. (2000a) found that a more rigorous design included fewer high school graduates, suggesting that the highly educated tend to be underrepresented on surveys.

*Income.* The impact of income generally appears to match that of education. More contact attempts were required to reach people with higher incomes (Curtin et al. 2000). The effect of income on refusals, like education, is confused. While Tuckel and O'Neill (2001b) reported that the most negative attitudes to surveys were found among people with higher incomes, experimental studies generally find no connection between refusals and income (Currivan 2005; Curtin et al. 2000; Curtin et al. 2006). As with education, Keeter et al. (2000a) found that respondents to a more rigorous survey were less likely to report lacking money to buy clothes, food, and medical care and more likely to have higher income, although this result could be explained solely by the greater difficulty of contacting higher income households.

*Employment status.* Households with employed adults were, not surprisingly, more difficult to contact (Keeter et al. 2000b). Employed respondents were also more likely to refuse (Currivan 2005).

*Urbanicity.* In keeping with findings from census data that response rates in urban and suburban areas are much lower, respondents from urban and suburban areas appear to hold more negative attitudes to surveys (Bates and Piani 2005; Tuckel and O'Neill 2001b).

*Region.* The Northeastern United States is typically seen as an especially difficult place to conduct surveys. Empirical data largely bear this out. Interview rates for the National Health Interview Survey were lowest in the Northeast and Mid-Atlantic, although interview rates in Boston, while below the national average, were better than in New York and Philadelphia, which represented the nadir of cooperation. Attitudes to surveys in the Northeast census region (which includes New York) were also the most negative in the country (Tuckel and O'Neill 2001b).

### *Impact of Nonresponse*

Considering ever-declining response rates and observed variation in response propensities for different groups, evidence of bias has been surprisingly difficult to find. Serious studies of this issue have found minimal variation in survey findings when comparing data collected under less rigorous conditions, which is presumably more likely to be biased, than data collected with greater methodological rigor (Biemer and Link 2006; Curtin et al. 2000; Keeter et al. 2000b; Montaquila et al. 2006). These findings have been treated with a degree of skepticism, even by their authors. A persistent criticism of this

type of design is that it assumes that the most difficult to reach cases in some ways resemble those which proved impossible to reach. The problem, of course, is how to reach the unreachable. Lin and Schaeffer (1995) demonstrated that such concerns were not idle ones by examining a population for which data was available on nonrespondents. In this analysis, more difficult reach respondents were actually less like the nonrespondents than were earlier cooperators. While a convincing demonstration of the problems associated with extrapolation, the special nature of the sample—which allowed the study in the first place—makes it difficult to generalize from.

Where does this leave us? The evidence of minimal bias is encouraging, but not convincing. These studies generally focus on attitudinal items that presumably have a fairly even distribution throughout the population. The known variations in response rates associated with demographic factors suggest that bias for these items would be greater. These studies also target broad samples and do not necessarily apply to surveys of special populations. Finally, lack of bias in the past is no guarantee that it will not be present in the future. It is important to recognize that there are no global solutions and that each survey must be evaluated on its own terms by carefully examining multiple types of data for evidence of bias.

## COVERAGE

In addition to nonresponse error, bias may be the product of incomplete coverage of the target population, or coverage error (see pp. 25-28). One can achieve a response rate of 100 percent but still have a biased sample if the sampling frame does not entirely cover

the target population. Understanding bias therefore requires examining coverage error (i.e. what characteristics are systematically over- or underrepresented in a sample because of an inadequate sampling frame). As we shall see, contemporary telephone surveys are troubled in this respect, with accumulating evidence of coverage error. The details of the coverage of telephone sampling frames are quite technical but absolutely vital to understand.

*Nontelephone Households*

Axiomatically, households without telephones cannot be interviewed in a telephone survey, leading to coverage bias to the extent that nontelephone households do not resemble households with telephone service.

*Extent.* Most national longitudinal estimates of nontelephone households have changed little in recent years, although estimates of prevalence vary considerably between sources. Blumberg, Luke, and Cynamon (2003; 2006a) estimate the proportion of adults without telephones at 1.6 percent to 1.7 percent in 2003 and between 1.6 percent and 1.9 percent in 2006 using data from the National Health Interview Survey. Meekins (2006), by contrast, estimates undercoverage at 5.4 percent using the Consumer Expenditure Interview Survey and 4.8 percent using the Current Population Survey.

*Characteristics.* These households are, unsurprisingly, very likely to have lower levels of education and income (Blumberg et al. 2003). Blacks and Hispanics were also overrepresented among nontelephone households (Blumberg et al. 2006b). Nontelephone households are also associated with smaller household size and residence in the South (Blumberg et al. 2003; Meekins 2006). One person households were also strongly asso-

ciated with having no telephone service (Blumberg et al. 2003; Meekins 2006). The extent to which these factors are independent on each other remains unknown as neither Blumberg et al. nor Meekins present a multivariate analysis. Similar shortcomings pervade analyses of the characteristics of cell phone only households, which are discussed next.

*Cell Phone Only Households*

Cellular telephones represent a major challenge to telephone survey research. The Telephone Consumer Protection Act, which was passed by Congress in 1991, authorized the Federal Communications Commission to formulate rules on calls to cell phones (Dautch 2005).[4] Unlike other federal communication rules, such as the Do Not Call list, the restriction on the use of autodialers for calling cell phones includes survey research. This decision was informed by concerns about safety, appropriateness, and the cost of the incoming call to consumers. In 1992, the Federal Communications Commission restricted the use of automatic dialing devices. These rules were subsequently tightened in 2003, effectively prohibiting the use of autodialers and predictive dialers, which are used in virtually all telephone surveys to increase interviewer productivity (less time is spent determining what number to call) and maximize quality control. Surveying cell phones was estimated by one researcher to require 40 percent more interviewer time than calling landlines.[5]

Federal regulations are not, however, the only source of problems for surveys using telephone research. Cell phone samples are also less efficient than landline samples. A high proportion of cell phone users are minors who cannot be surveyed without paren-

tal consent (Jay and DiCamillo 2006a; Pew Research Center for The People & The Press 2006). Cell phones are also on only intermittently, increasing the number of calls that must be made in order to complete an interview (Jay and DiCamillo 2006a), although the proportion of cell phone owners who keep their phone always on was estimated to have increased from 33 percent in 2003 to 42 percent in 2005 (Tuckel and O'Neill 2006). Including those who kept their telephone on most of the time, coverage increased from 58 percent in 2003 to 68 percent in 2005 (Tuckel and O'Neill 2006).[6] Intermittent coverage is, however, hardly a unique problem. Few people can always be reached on their home phone and cell phones in fact appear to be easier to contact than landlines (Pew Research Center for The People & The Press 2006). Cell phones are also difficult to geographically classify, with 32 percent not being located in the same county as their cellular rate center, where the phone was purchased, resulting in a relatively high rate of ineligibility for samples that cover a specific area (Fleeman and Estersohn 2006). In general, cell phones have lower response rates than landlines and people reached on cell phones are less cooperative (Pew Research Center for The People & The Press 2006; Steeh and Piekarski 2006).

The relative inefficiency of cell phones and the special dialing procedures required add up to much higher costs. The Pew Research Center for the People & the Press (2006) put the additional cost of calling a sample of cellular phones at 140 percent more than calling landlines, including the cost of a $10 incentive to reimburse respondents for the incoming call.

Cell phones also present considerable challenges to sampling. A cellular phone is primarily associated with an individual, while most landline phones belong to a house-

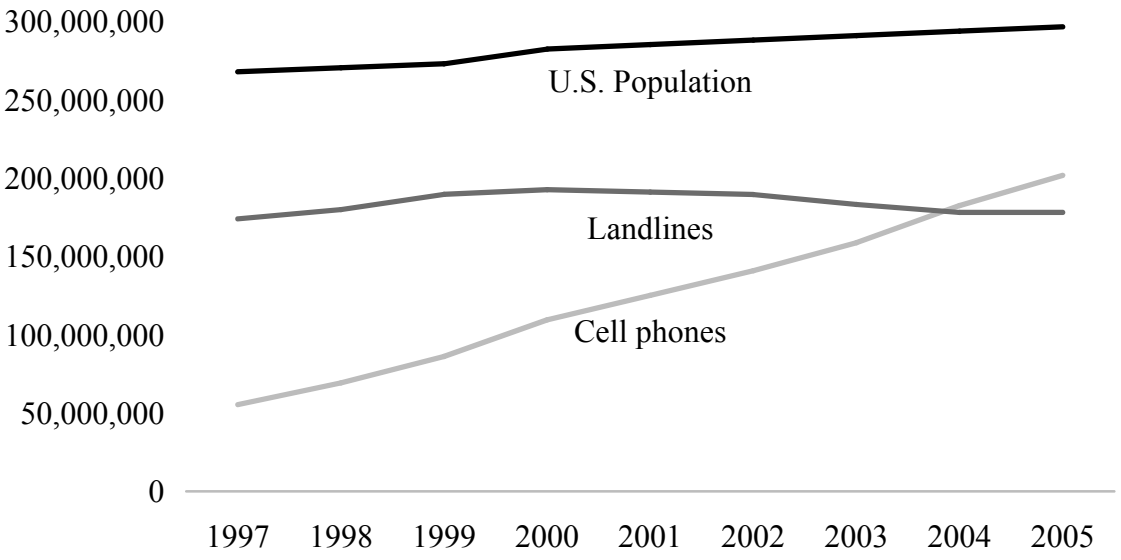
hold (Kuusela, Vehovar, and Callegaro 2006). The probability of selection of household would be the sum of all landline and cellular telephones belonging to the household. However, a sampling and weighting scheme for cellular and landline telephones has yet to be developed. Simply determining the number of telephones in a household is a major challenge. Federal surveys use 15 or so questions to determine the number of telephones possessed by household members, clearly far too many items for most surveys to ask. Matters get even more complicated, however, as the Pew Research Center for the People & the Press (2006) survey found that 16 percent of cellular telephone owners reported that another adult regularly uses their cell phone, while 19 percent reported regularly using more than one cellular telephone, making the probability of selection very difficult to determine.

The fact that some cellular phone users keep their phones on more than others mentioned previously also complicates matters, as those who keep their phones on the most also have the highest probability of being contacted. Bias will occur if these individuals systematically differ from those who keep their phones on less frequently. Unfortunately, this appears to be the case. Men are more likely to be contactable than women, as are younger people, the less educated, individuals who rent their residence, full-time employees, and blue-collar workers (Tuckel and O'Neill 2006).

In the light of these challenges, it is not surprising that the overwhelming majority of random digit dialed surveys do not include cell phones. Those with cell phone frames have been exploratory in nature, and have not been primarily focused on issues besides cell phone coverage. However, the difficulties presented by cellular telephones are in-

creasingly salient as the number of cellular telephones continues to expand while landline coverage peaked about 2000 and has contracted since (Figure 3:2).

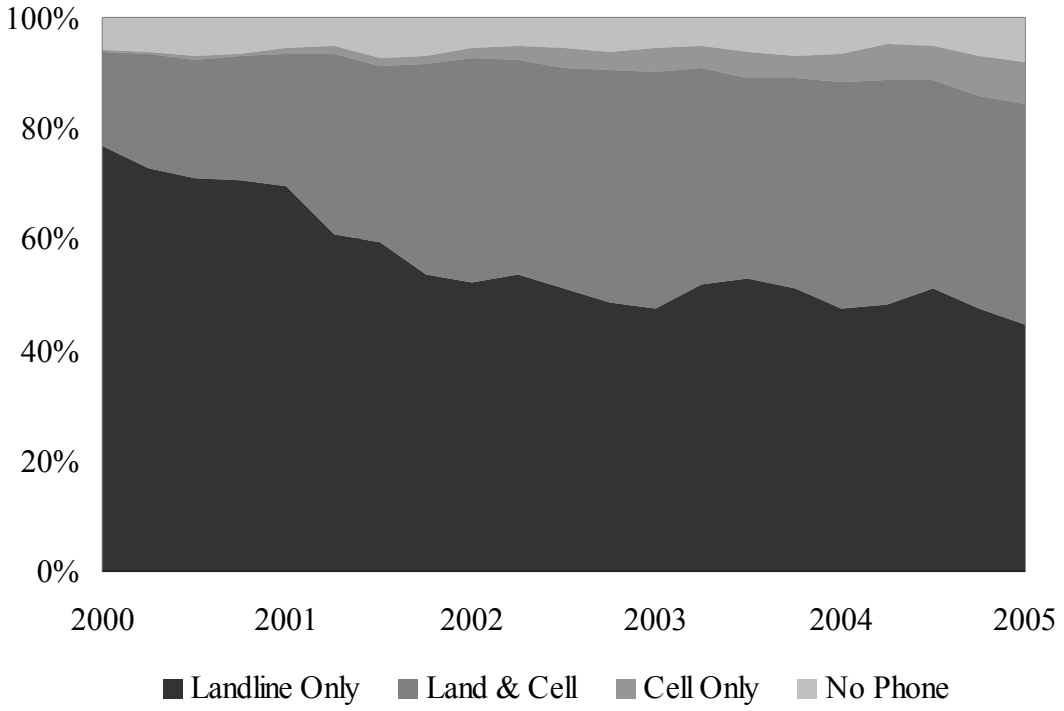
Figure 3:2. Subscription to Cellular and Landline Telephones




*Sources:* International Telecommunication Union (2002a; 2002b; 2003a; 2003b; 2004a; 2004b; 2005a; 2005b; 2006a; 2006b).

While some of the contraction in landline usage is likely due to decreasing need for second lines for fax machines and dial-up internet connections, a major factor is that some households have abandoned landlines altogether. Meekins (2006) estimated that 7.8 percent of households only had a cell phone in 2005 from consumer expenditure reports. Blumberg et al. (2006a) came to a slightly higher estimate of 10.4 percent of households (7.7 percent of adults in July-December 2005) using data from the National Health Interview Survey. Figure 3:3 (adapted from Meekins 2006), below, shows the growth of cell phone only households.

Figure 3:3. Telephone Coverage, 2000-05




*Note:* Figure adapted from Meekins (2006).

It is likely, however, that cell phone only households are less common in New England than the nation as a whole (Blumberg et al. 2003; 2006a). Counterbalancing this trend is the association between metropolitan areas and cell phone only households (Blumberg et al. 2003; 2006a).

*Characteristics.* As is the case with nontelephone households, noncoverage will only result in bias if the groups that are not covered differ systematically from those that are. The answer in this case is far from reassuring.

Age: The literature consistently reports that younger individuals are the most likely to be uncontactable by landline (Blumberg et al. 2003; 2006a; Fleeman and Estersohn

2006; Jay and DiCamillo 2006a; 2006b; Pew Research Center for The People & The Press 2006; Tuckel and O'Neill 2006).

Household composition: Smaller households are more likely to only have cell phones (Blumberg et al. 2003; Jay and DiCamillo 2006a; Meekins 2006). Living alone or especially with roommates is highly correlated with being accessible only via a cellular telephone (Blumberg et al. 2003; 2006a; Jay and DiCamillo 2006a; Meekins 2006). There is some uncertainty about the effect of children, with national studies reporting that households with children are more likely to have a landline telephone (Blumberg et al. 2006a; Meekins 2006), while Jay and DiCamillo's (2006a) sample of recent cell phone only households in California arrives at the opposite conclusion.

Socioeconomic status: There appears to be a strong socioeconomic component to the incidence of cell phone only households, with less educated individuals being the most likely to only have a cell phone (Blumberg et al. 2003; Jay and DiCamillo 2006a; Pew Research Center for The People & The Press 2006; Tuckel and O'Neill 2006). The same holds true for income, with the least educated the most likely to only have access to a cell phone (Blumberg et al. 2003; 2006a; Jay and DiCamillo 2006a; 2006b). Not surprisingly, renters are also more likely to only have a cell phone (Blumberg et al. 2003; 2006a; Jay and DiCamillo 2006a; 2006b; Meekins 2006; Pew Research Center for The People & The Press 2006; Tuckel and O'Neill 2006).

Occupation: Students are by far the most likely to only have cell phones, followed by full-time workers (Blumberg et al. 2006a; Jay and DiCamillo 2006a; Meekins 2006).

Race: Hispanics are the most likely to only possess a cell phone, followed by African-Americans (Blumberg et al. 2003; 2006a; Pew Research Center for The People & The Press 2006).

While the degree of agreement between sources is reassuring, the lack of multivariate analysis is problematic as the effects of some factors are likely the product of others. It appears that age and socioeconomic status are key factors, with both the young and the socioeconomically disadvantaged being the most likely to use cellular telephones exclusively.

*Zero Listed Phone Banks*

The following sections are among the most technical in this work, focusing on the minutiae of random digit dialing sampling frames. In this case, the devil really is in the details, and it is necessary to examine sampling frames in some detail in order to understand potential coverage bias.

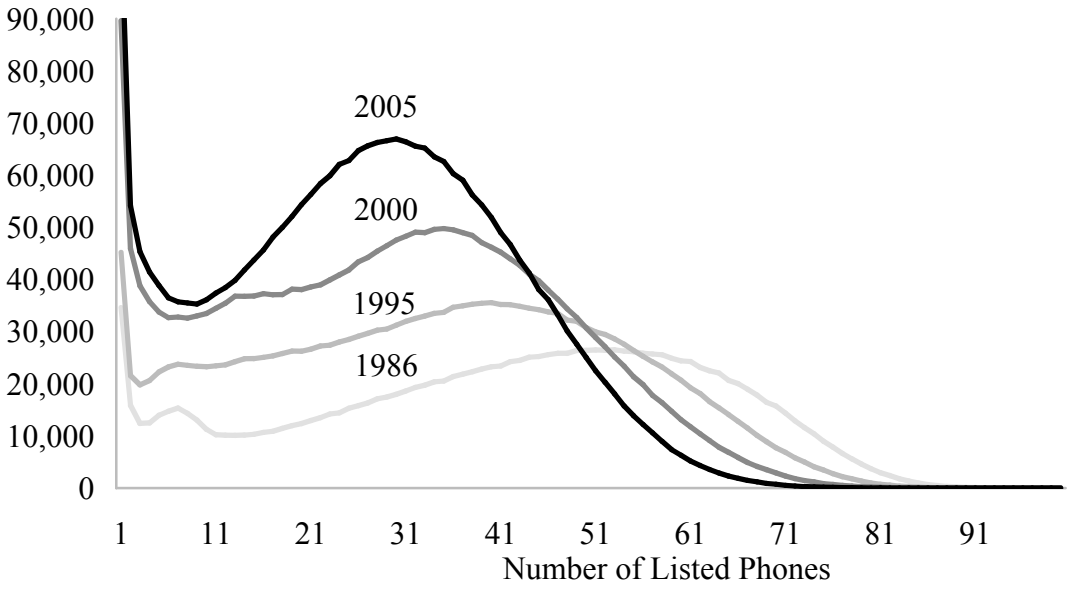
True random digit dialing, in the sense of selecting telephone numbers to call completely at random, is almost never attempted. For one thing, most surveys have geographic constraints. Even a survey of the entire United States will want to exclude area codes associated with Canada (which shares the same international prefix as the United States). For another, certain codes simply do not exist—no area or exchange code begins with a zero or a one. Finally, cell phone numbers must be purged from calls made by autodialers as a matter of law.

In order to increase the efficiency with which working residential telephone numbers are reached, several approaches were developed in the late 1970s (Frankel 2003;

Tucker, Lepkowski, and Piekarski 2002). Only list-assisted random digit dialing will be concern us, as the Waksberg-Mitofsky method (see p. 104) proved cumbersome and fell into disuse (Tucker et al. 2002). List assisted random digit dialing takes advantage of the concentration of telephone numbers in certain clusters of numbers, e.g., a given exchange code (XXX-NNN-XXXX) may not be used, by selecting only 100 blocks (XXX-XXX-XXNN) with one or more numbers listed in the telephone directory (Brick et al. 1995; Casady and Lepkowski 1993).[7]

One of the hallmarks of United States telephony of the past decade is the explosion of telephone numbers. Growing demand for telephone numbers—fed by technologies like fax machines, dial-up internet access, and cellular telephones, as well as "shadow" numbers brought into being when a person keeps their telephone number while switching carriers—led to the expansion of the number of available area and exchange codes (Piekarski et al. 1999; Steeh and Piekarski 2006; Tucker et al. 2002). One result of this growth has been an increase in the number of 100 banks with zero listed numbers and overall decline in the number of listed numbers per bank (see Figure 3:4), decreasing the efficiency of list-assisted designs (Piekarski et al. 1999; Tucker et al. 2002).

Figure 3:4. Prevalence of Listed Numbers in 100 Blocks




*Note:* Figure slightly adapted from Steeh (2006).

The efficiency of list-assisted designs, however, says nothing about their level of coverage. A consistent concern with list-assisted designs is that unlisted phone numbers that reside in zero listed phone banks will go unsampled and lead to biased results if they differ in meaningful ways from listed and unlisted numbers in listed phone banks. There is some reason to be sanguine on this score, at least. Brick et al. (1995) found little difference between the characteristics of households from zero listed and one plus listed phone banks. The level of undercoverage also appears to be low, with Brick et al.'s (1995) estimate of 3.7 percent closely matched by Tucker et al.'s (2002) estimate of 3.5 percent, suggesting that the systematic differences would have to be very large in order to introduce meaningful bias.

*Business Numbers*

In order to increase efficiency, most random digit dial frames are scrubbed of listed business telephone numbers. Should some of these numbers in fact be used for nonbusiness purposes, undercoverage may result. Merkle et al. (2006) report that a considerable number were in fact home or home office numbers used for personal or personal and business use. These households were more likely to be middle aged, have higher incomes, and be more politically conservative, but did not differ with respect to education, race, sex, region, or political party identification. The extent to which these limited differences could lead to bias was further hampered by the small amount of undercoverage, amounting to only three percent of households.

*Telephone Number Portability*

A potentially more serious threat may result from telephone number portability. Since 1996, individuals who wanted to switch carriers or residencies could keep their telephone number (Steeh and Piekarski 2006). This was implemented by assigning a shadow number at the new carrier to which calls to the telephone number at the former carrier are redirected to. The pace of number portability increased in 2003-04 with the introduction of transfers of numbers between cellular telephones and between cell phones and landlines (Steeh and Piekarski 2006). It has been widely recognized that portability is degrading the link between geography and the telecommunications system, and will eventually necessitate drastic changes to telephone survey operations. Cell phones, which can move with their owners, increased the rate of degradation. More recently, voice over internet protocol services like Vonage have accelerated the pace of change, offering mul-

tiple virtual phone numbers in widely separated telephone geography (Steeh and Piekarski 2006).

An unanticipated effect of number portability has been suggested by Eckman and Smith (2006), who report that some random digit dial calls may be made to shadow numbers. If true, this raises the disturbing possibility that the most mobile households will also be the most likely to be contacted and that the increased probability of selection (both the active number and its shadow) will not be accounted for by existing weighting procedures, which rely on the respondents report about the number of landlines used by the household. At present, there are no data to estimate the extent of this putative problem.

## DEVELOPING A COHERENT PICTURE

Bias in telephone surveys is by no means easy to understand. Not only are there several forms of bias, but the various methods used to understand bias are problematic themselves. One advantage, however, is that *post hoc* and *ex ante facto* methods for examining bias have different weaknesses. It seems reasonable to believe that where both lines of evidence converge there is a decent likelihood that a given characteristic really is associated with nonresponse bias. The evidence of coverage error is generally less problematic as it draws upon census data or high quality in person interviews. In this section, I will attempt to develop a synthetic portrait of biases caused by key factors.

*Age.* Age is widely implicated in mechanisms associated with bias. Younger people are more likely to be absent from standard random digit dialing frames due to

their overrepresentation among individuals with cell phones only. To the extent that young adults are more likely to move than other age groups, it is reasonable to think that they will be underrepresented on geographically constrained samples. Young adults living in college dormitories are also likely to be absent from traditional random digit dialing frames. There is also reason to believe that even young adults with landline telephones are less likely to be contactable, as they are more likely to own answering machines, have caller ID, and use caller ID to screen calls. The evidence from studies of the characteristics of nonrespondents also suggests that young adults are difficult to contact. The balance of the evidence suggests that older adults are more likely to be contactable. Virtually any telephone based survey appears to be at serious risk of bias against young adults. The evidence for the relationship between age and refusal rates, however, is contradictory and unclear.

*Household composition.* One person households appear to be particularly likely to be underestimated in telephone surveys. Single person households are at significantly greater risk of not having access to a telephone at all, while smaller households are more likely to use cell phones only. Households with children may be more likely to have landline telephones. Single adult households and, conversely, households with children appear to be overrepresented among owners of answering machines and users of caller ID, as well as call screeners. Even single person households that are contactable are less likely to be reached, and may be more likely to refuse. Like young adults, telephone surveys are almost certainly biased against single person households. Some lesser degree of bias against households with children may also exist due to screening practices.

*Race.* Associated as it is with socioeconomic status, race is linked to bias. Blacks and Hispanics are overrepresented among nontelephone households and also among people who use only a cell phone. Possession of caller ID, though apparently not answering machines, is greater among African-Americans than other groups, and is used particularly frequently for screening calls by African-Americans and Latinos. Not surprisingly, response rates are lower in areas with black and Hispanic populations. Studies of nonresponse have found that nonwhites are more difficult to contact. Evidence regarding cooperation when contacted is mixed. As with age, bias with respect to race is almost a given for any telephone survey. The situation will be particularly acute when Spanish-language interviewers are not available.

*Education.* Data on the effect of education are somewhat contradictory. On the one hand, higher levels of education are associated with increased likelihood of owning an answering machine and using caller ID, although apparently not with call screening. Among households that could be contacted, higher education was associated with greater difficulty of contact. On the other hand, households without telephone access and those that use cell phones only are far more likely to have lower levels of education. In addition, the preponderance of evidence suggests that households in areas with higher average levels of education are generally easier to contact and less likely to refuse, although studies of refusal conversion come to contradictory findings regarding education. Education appears to be an instance where coverage error and nonresponse error pull in different directions. Less educated households are less likely to be found on standard telephone frames, but those that have telephones can be more easily contacted. In such a case it is difficult to see which bias would predominate.

*Socioeconomic status.* Naturally, low income is associated with lack of access to a telephone. Low income is also associated with having only a cell phone. As a commodity, one might expect that ownership of answering machines and caller ID would be associated with socioeconomic status, but there no data on this point. The effect of income on call screening is unusual, with the highest levels of screening found among households with low and very high levels of income. There is some evidence that a similar distribution may apply to response rates. Analyses of difficult to reach cases find greater difficulty of contact among high income households. Refusal rates do not appear to be related to income. As with education, there appear to be offsetting biases, though there is no guarantee that they are of equal magnitude.

*Employment.* Employment, with the exception of people who work from home, appears to be associated with greater difficulty of contact and lower response rates. There is some suggestion that people who are employed may be more likely to refuse. It does not appear likely that this is a major source of bias in surveys, unless one is trying to estimate unemployment rates and similar statistics.

*Urbanicity.* In keeping with findings from census data that response rates in urban and suburban areas are much lower, respondents from urban and suburban areas appear to hold more negative attitudes to surveys (Bates and Piani 2005; Tuckel and O'Neill 2001b).

*Region.* The Northeastern United States is typically seen as an especially difficult place to conduct surveys. Empirical data largely bear this out. Interview rates for the National Health Interview Survey were lowest in the Northeast and Mid-Atlantic. Atti-

tudes to surveys in the Northeast census region (which includes New York) were also the most negative in the country (Tuckel and O'Neill 2001b).

## COST

The cost of a telephone survey is driven in large measure by its productivity. The more rapidly interviews are completed, the less a survey will cost. It is no surprise, then, that falling response rates have significantly reduced interviewer productivity. This effect is, however, enhanced by other consequences of falling response rates. Increased effort has been required to prevent steeper declines in response rate. These efforts have taken the form of adding more callbacks in order to reach difficult to contact households, as well as efforts at refusal conversion, all of which require more interviewer hours. In addition, other techniques for increasing response rates are often employed, including prenotification letters with relatively small preincentives (typically between $2 and $10, with $5 the apparent sweet-spot). As these incentives are paid regardless of whether an interview was completed, they add significantly to the cost of surveying. In addition, high quality surveys may offer refusers substantial payments for completing the survey, typically beginning at $10, which—in some designs—may be increased if a refuser continues to hold out. The present situation in some ways resembles the mid to late 1970s, where declines in productivity increased the costs of in-person interviews to the point that many surveys switched to telephone surveys.

These costs can be addressed in three potentially unpalatable ways: accepting the increased cost of interviewing and increasing research budgets accordingly, reducing the

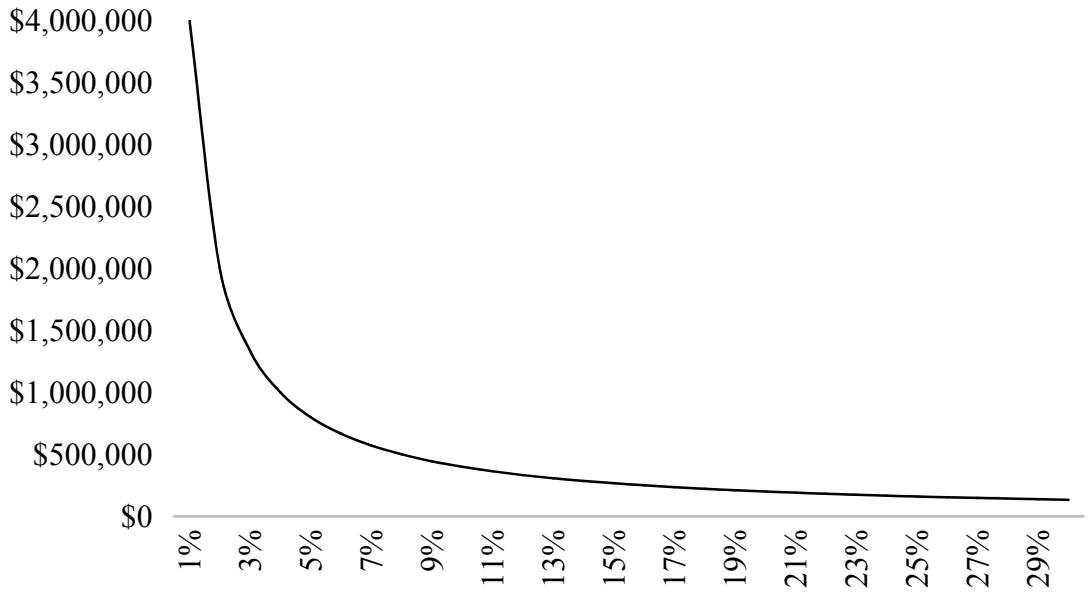
sample size, or compromising on some aspect of quality. The first lies outside the sphere of survey methodology, except to make the obvious point that all budgets are finite and research funds are unlikely to be permitted to increase too much as a proportion of the expenditure of the supporting organization. Even if a given survey is fully funded, there may be an opportunity cost as other studies are tabled. The second alternative—reducing sample size—is generally self-defeating, as most high quality designs are calibrated to have a sample size that is sufficient to produce key estimates with a desired degree of confidence and no more. While some compromise is possible, at a certain point estimates will have such broad confidence intervals that they are effectively useless. This leaves compromising on quality, and survey methodology in many ways can be seen as the science of finding the least harmful compromise. For example, while increased numbers of callbacks are near universal, studies may exclude not only zero listed phone banks but those with only one or two listed numbers, trading off nonresponse error against coverage error. I will systematically explore some of these trade-offs in the following chapter.

Delineating the impact of cost is difficult as survey research centers and corporations are understandably reluctant to discuss their costs in public and sponsoring organizations are unlikely to want to explain the ways in which their research agendas have been compromised by high research costs. Nonetheless, the challenge posed by increasing costs to survey research is no less threatening than the effect of the survey research environment on the quality of surveys.

The impact of increased costs is particularly severe for studies of rare populations. In order to identify members of a rare population, one has to contact all households in order to find them. (There are ways of minimizing the amount of screening required,

some of which I discussed in the previous chapter, but the essential problem nevertheless remains.) Consequently, the number of interviews required to find the rare population is inversely proportional to its incidence, the proportion of the total population with the characteristic of interest. Finding 1,000 members of a rare population with 10 percent incidence would require 10,000 screening interviews on average. While screening interviews can be very short, the problem is that time spent conducting interviews is a relatively small proportion of interviewing costs, with much more of an interviewer's time spent on fruitless calls. Thus, the less common the population, the greater the costs are. To illustrate this relationship, Figure 3:5, below, shows the cost of screening for $n$ = 1,000 rare population elements at different levels of incidence, assuming each completed screening interview costs 40 dollars (a relatively realistic estimate).

Figure 3:5. Screening Cost for n=1,000 Elements of a Rare Population by Incidence



As the reader can readily appreciate, the cost of screening for a rare population quickly becomes prohibitive. To make the scenario a little worse, 1,000 cases might be an adequate sample for estimating overall means, but is less than ideal if one wanted, for instance, to compare the means of men and women on some characteristic of interest, and is still less adequate if one wanted to make finer distinctions.

## CONTEMPORARY SURVEYS OF JEWISH POPULATIONS

Surveys of Jewish populations share the challenges of other contemporary surveys: declining response rates, nonresponse error, and coverage error. In addition, like other surveys of rare populations, studies of Jewish communities suffer the additional cost of working with a rare population. In addition to these general disadvantages, however, Jewish surveys face particular problems. One is simply determining how best to screen for Jewish households. Another is the possibility of bias within the Jewish population—that some Jews may be systematically less likely to be found on the sampling frame or to respond to a survey than others. I address screening for Jewish households first and then examine a particular survey of a Jewish population in detail—the National Jewish Population Survey (NJPS) 2000-01.

### *Screening for Jewish Households*

As it is impossible to know in advance which households are Jewish, even from a listed sample (people may have moved or may never have been Jewish in the first place), it is necessary to "screen" households for the presence of Jews by asking a series of questions

to determine whether there are any Jewish inhabitants. Question wording, however, may impact cooperation rates, refusals and Jewish denial.

*Introduction.* The introduction to a study sets its tone, attempts to pique the initial informant's interest, and encourages them to respond. There is an interesting divergence, however, in the approach used by local and national Jewish surveys in the United States. Local studies generally immediately identify the study as sponsored by Jewish organizations about Jews. In the Ukeles & Associates study of New York, the introduction was simply:

> Hello, I am calling on behalf of the New York Jewish Population Survey, an important study sponsored by UJA-Federation of New York. (Ukeles and Miller 2003b)

For Pittsburgh, Ukeles & Associates used slightly different (though still very direct) introductions for the list and random digit dialing samples. For list households this was:

> Hello, my name is ____. I'm calling from ICR, an independent research firm in Media, Pennsylvania. We are doing a study of the Jewish population in Pittsburgh to supplement the information in the U.S. Census. The study is sponsored by the United Jewish Federation of Pittsburgh, which wants to hear from you about your views and experiences. You may have heard or seen a radio or TV spot about the study. We are NOT asking for money. We are NOT selling anything. The survey is anonymous and confidential. Your phone number was chosen randomly by a computer. (Ukeles and Miller 2003a)

The random digit dialing version was as follows:

> Hello, my name is ____. I'm calling from ICR, an independent market research firm in Media Pennsylvania. We are doing a study to add to the information collected by the U.S.

> Census. The study is sponsored by the United Jewish Federation of Pittsburgh, but we need some information about everyone who lives in the Pittsburgh area, whether Jewish or not. You may have heard or seen a radio or TV spot about the study. We are NOT asking for money. We are NOT selling anything. The survey is anonymous and confidential. Your phone number was chosen randomly by a computer. (Ukeles and Miller 2003a)

The introductions used by Ira Sheskin—principal investigator for many studies of medium-sized Jewish communities—are equally direct:

> Hello, my name is ____. I am calling on behalf of a population study for the Jewish community sponsored by the Jewish Federation of Rhode Island. We are NOT asking for contributions. We ARE randomly interviewing households with one or more Jewish persons. (Sheskin 2003a)

The assumption behind all these direct approaches is that Jews will be more likely to respond to a request by a Jewish organization. The biases introduced by such a procedure are not clear. In general, most polling organizations would prefer not to mention a survey's sponsor.

For this reason, national studies of Jewish populations have used less direct methods. A less direct approach is to identify the study as focusing on religion and ethnicity but does not mention its Jewish sponsorship. For example, NJPS asked:

> Hello, I'm _____ from Audits and Surveys Worldwide, a public opinion research company in New York City. I would like to ask you a few questions that take less than three minutes. If your household is selected by the computer for a longer interview, we will mail the person who completes it a check for $25 to show our appreciation. This national survey is about important social issues such as religion and ethnicity. We are NOT selling

> anything and we are NOT asking for any donation. All your answers are confidential. (United Jewish Communities 2003g)

If the person was Jewish by religion in answer to the first question, a second introduction was given:

> The survey we are conducting is the National Jewish Population Survey. It is being sponsored by all the Jewish Federations in the United States. Five thousand Jews across the country are being interviewed. Our objective is to understand the characteristics and needs of the Jews in America. All answers to this survey are confidential. For this brief, preliminary interview, I'd now like to ask a few questions about your background and about any other adults in the household. (United Jewish Communities 2003g)

If the person was not Jewish by religion in answer to the first question, the second introduction did not mention the Jewish purpose of the survey:

> Thank you. We are speaking with 4,000 people of all religious backgrounds across the country, as well as people who have no religious affiliation at all. The survey we are conducting is the National Survey of Religion and Ethnicity. Our objective is to understand the characteristics of the American population. All answers to this survey are confidential. For this brief, preliminary interview, I'd now like to ask a few questions about your background and about any other adults in the household. (United Jewish Communities 2003g)

The most neutral approach does not even mention potentially controversial topics like ethnicity and religion, and was used by Survey of Heritage and Religious Identification:

> Hello, my name is _____. I'm calling on behalf of researchers at the University of Michigan and other organizations. Your household has been selected for an important study of families and their cultural heritage. (Tobin and Groeneman 2003)

Determining which approach is most effective is potentially confounded by variation in other elements of the survey. Nevertheless, there is reason to believe the direct approach is more effective. Response rates to local studies generally exceed those of national studies. In addition, a test of the two approaches in the Boston Jewish Community Study found the direct approach yielded a significantly higher response rate (see p. 251).

These findings comport with social exchange theories of survey participation. The norm of reciprocity dictates that individuals should "be more willing to comply with a request to the extent that the compliance constitutes the repayment of a perceived gift, favor, or concession" (Groves, Cialdini, and Couper 1992:480). Reciprocity successfully predicts the greater effect of preincentives over postincentives or no incentive (Cantor, O'Hare, and O'Connor 2006a; Church 1993; Curtin et al. 2006; Fahimi et al. 2006b; Groves, Singer, and Corning 2000; James and Bolstein 1990; 1992; Singer et al. 1999; Singer, Van Hoewyk, and Maher 2000; Traub, Recco, and Lindner 2006; Trussell and Lavrakas 2004; Warriner et al. 1996; Willimack et al. 1995), prenotification about a survey over not being contacted in advance (Camburn et al. 1995; de Leeuw et al. 2006; Goldstein and Jennings 2002; Hembroff et al. 2005; Link and Mokdad 2005; Mann 2005; Mann, Lynn, and Peterson 2006; Traugott, Groves, and Lepkowski 1987), and prenotification letters that offer detailed information about survey procedures (de Leeuw et al. 2006).[8]

It seems reasonable to assume that surveys that are relatively coy about their sponsorship and goals should perform poorly as such introductions in effect propose an asymmetric transaction where the requestor offers no information but expects the respondent to be forthcoming. By not mentioning the sponsor, indirect screeners forgo the opportunity to increase the perceived legitimacy of the survey (Groves et al. 1992). Of course, this cuts both ways, as Jewish and non-Jewish households may not regard the sponsor (or even the survey topic) as legitimate.

*Screening questions.* These approaches to screening for Jews persist in the questions asked of respondents to identify a household as Jewish or not. The direct approach represented by the New York 2002 study (Ukeles and Miller 2004b) immediately asks whether the initial informant considers her- or himself Jewish. This has the advantage of speed—and attendant cost savings—while using a broad definition of Jewishness, albeit one that excludes Christian and Messianic Jews (who are subsequently identified and removed) and people raised as Jews or of Jewish parents. The choice of target population was a substantive decision that preserved the maximum number of cases for analysis of the currently Jewish population at the price of precluding analyses of the impact of Jewish background. Other community studies ask about childhood identity and parentage either sequentially (Phillips 2005) or simultaneously (Sheskin 2003a), maximizing efficiency while using a broader definition. The 1995 Boston study used this approach, supplemented with questions regarding religious identity (Colten, Roman, and Fowler 1995).

National studies are more guarded, asking first about religion before asking specific questions regarding Jewish background (Mayer et al. 2002; United Jewish Communities 2003g). NJPS 2000-01, for instance, asked first about current religion before turning

to questions on Jewish identity. A still more cautious approach used in the National Survey of Heritage and Religious Identification (Tobin and Groeneman 2003) spent an extremely long time building rapport before asking questions about ethno-cultural and religious background, taking great care to conceal the study's particular interest in Jews by always asking about a variety of groups.[9] NJPS 1990 (Kosmin et al. 1991a) was the most cautious—and limiting—asking only about religious identity. Table 3:1, below, shows the topic and sequence of questions asked in a variety of surveys.

Table 3:1. Screening Questions in Jewish Population Studies

| | Religion | | | Identity | | | Parents | |
|---|---|---|---|---|---|---|---|---|
| | Born | Raised | Now | Born | Raised | Now | Relig. | I.D. |
| NJPS 1990 | 2 | 3 | 1 | | | | | - |
| Boston 1995 | | | 4 | | 2 | 1 | 5 | 3 |
| NJPS 2000-01 | | | 1 | | 3 | 4 | | 2 |
| AJIS 2001 | | | 1 | | 3 | 4 | | 2 |
| HARI 2002 | | 3 | 4 | | | 1 | 5 | 2 |
| Rhode Island 2002 | | | | 1 | 1 | 1 | | |
| New York 2002 | | | | | | 1 | | |
| San Francisco 2004 | | | 1 | | 2 | 1 | | 3 |

*Sources*: National Jewish Population Survey (NJPS) 1990—Kosmin et al. (1991a); Boston 1995—Colten et al. (1995); National Jewish Population Survey (NJPS)—United Jewish Communities (2003g); American Jewish Identity Survey (AJIS)—Mayer et al. (2002); Survey of Heritage and Religious Identification (HARI)—Tobin and Groeneman (2003); Rhode Island 2002—Sheskin (2003a); New York 2002—Ukeles and Miller (2004b); San Francisco 2004—Phillips (2005).

*Note:* Numbers indicate sequence in which questions asked.

Given the impact of reciprocity, presenting a survey as a general study of religion and ethnicity and asking questions clearly directed at identifying Jewish households can be seen as a breach of trust, violating the implicit contract between interviewer and inter-

viewee regarding the topic of the survey. For some Jewish households, fears of antisemitism may increase the probability of terminating at that point.

Both approaches to screening, however, face similar problems in that they place the question on religious preference at the very beginning of the interview, which may turn some people off (Schulman 2003). The usual practice is to ask potentially sensitive questions like religion after the interviewer has gained some rapport with the respondent. Little can be done to alleviate the problem, however, as each additional question asked in the screener significantly increases the cost of a study.

*Defining Jewish identity.* Jewish identity has never been a simple construct, with *halakhah* (Jewish jurisprudence) continually challenged by ambiguous cases that required adjudication. Even so, Jewish identity has become far more complicated in recent decade, driven by intermarriage and religious pluralism. The ancient definition of Jewish identity as a person born of a Jewish other or converted under appropriate auspices was redefined by the Reform movement in response to intermarriage.[10] Henceforth, the Central Conference of American Rabbis (1983), declared that only children of two Jewish parents would be automatically recognized as Jewish, while the identity of children of intermarriages was decided in the following fashion:

> The Central Conference of American Rabbis declares that the child of one Jewish parent is under the presumption of Jewish descent. This presumption of the Jewish status of the offspring of any mixed marriage is to be established through appropriate and timely public and formal acts of identification with the Jewish faith and people. The performance of these mitzvot serves to commit those who participate in them, both parent and child, to Jewish life.
>
> Depending on circumstances, mitzvot leading toward a positive and exclusive Jewish identity will include entry into the covenant, acquisition of a Hebrew name, Torah

> study, Bar/Bat Mitzvah, and Kabbalat Torah (Confirmation). For those beyond childhood claiming Jewish identity, other public acts or declarations may be added or substituted after consultation with their rabbi. (Central Conference of American Rabbis 1983)

Conversion under appropriate auspices, of course, remained a valid way of becoming a Jew.

Studies of Jewish populations, however, have avoided using either definition for eminently practical reasons. Other than dissatisfying those in one camp or the other, the number of people who would fall under either definition of a Jew cannot be determined by a survey. The traditional definition, of course, would require knowing the precise status of every generation in the matrilineal line. Even if a shorter line of descent were used, it would of course rely on respondents' subjective assessment of their mothers' Jewishness. The Central Conference of American Rabbis definition, not unnaturally for a rabbinic organization, leaves considerable room for interpretation. As a practical matter, though, Jewish opinion hews exactly to neither definition, generally extending some type of recognition to the children of intermarriages while excluding adults who were raised as Jews but identify with another religion, particularly Christianity or Islam. Turning necessity into a virtue, Jewish population studies have adopted a "sociological" definition of Jewish identity that is explicitly draws upon the respondent's subjective identity, typically defining current Jews as those who identify themselves as current Jews, by religion or for ethnic/secular reasons, unless they identify with Christianity or Islam. As a practical matter, many studies include a "fudge factor" of interviews with some other connection to Jewish identity such as being raised as a Jew or having a Jewish parent. Such an ap-

proach allows for a much wider range of possible analyses, including the study of former Jews, although at the cost of a smaller sample size of current Jews.

*National Jewish Population Survey 2000-01*

Having begun this dissertation by outlining the development and controversies of NJPS 2000-01, I will now look at the challenges faced by the survey and its results as a well-documented case study of a contemporary survey of a Jewish population.

*Sampling frame.* The method by which a survey selects sampling units is its sampling frame (see p. 25). Many studies of rare populations use dual-frame methods that combine a sampling frame with a high proportion of members of the rare population (e.g., lists of relevant organizations) but incomplete coverage with a frame with universal coverage, but low incidence of members of the rare population. This approach is effective because it provides universal coverage while reducing costs. Cost reduction is vitally important as the number of units that must be screened (i.e. interviewed in order to determine their status) increases tremendously as the incidence of the rare population declines (see Figure 3:5, p. 178). As the proportion of irrelevant units that must be screened increases, ever larger portions of the budget must be spent on screening costs, reducing the size of the sample of the rare population. NJPS did not, however, use such a design. Instead, the sole sampling frame was a random digit dialing sample of the United States. This decision had major cost implications as survey costs increase dramatically below five percent or so incidence.

In order to control costs, the effective incidence of the Jewish population was increased by dividing the United States into seven strata based on location, metropolitan

status, and Jewish incidence (United Jewish Communities 2003h:43-46). Stratification is a standard procedure (and one that can be used in combination with dual-frame designs) that can enhance accuracy and increase cost-effectiveness if properly applied (see p. 91). The maximum sampling ratio (the extent to which a given stratum is over- or under-sampled) for NJPS was set to a relatively conservative 3:1 (relative to the lowest density stratum), which minimizes design effects at the expense of increasing cost.[11] The way in which sampling ratios were assigned is unclear, and does not appear to have been optimized either for variance or cost and variance. Optimization is treated at greater length in Chapter 4.

For reasons that are not specified, strata one, two, and four—representing high and medium density counties in the New York consolidated metropolitan statistical area and high density metropolitan statistical areas outside the New York region (Miami-Fort Lauderdale and West Palm Beach in Florida)—are treated separately despite having the same sampling ratio. Similarly, strata three and five—low density counties in the New York consolidated metropolitan statistical area and medium density metropolitan statistical areas elsewhere in the nation—have nearly identical sampling ratios of 2:1 and 1.9:1. Finally, stratum six—low density metropolitan statistical areas—with a sampling ratio of 1.1:1, is practically identical to stratum seven, the rest of the United States. Large numbers of strata will tend to increase design effects (Cochran 1961). The sampling ratios were changed mid-survey due to lower cooperation rates in higher incidence strata, a procedure that significantly increases the difficulty of correctly calculating sampling variance and developing weights. It is unclear whether the change in sampling ratios was correctly accounted for.

The sampling frame was likely the overwhelming influence in the ballooning costs of NJPS. Out of an estimated total of 181,397 completed screeners, only an estimated 9,913 were with households eligible for NJPS, or 5.5 percent.[12] The effective incidence of Jewish households is only three or so times the actual very low incidence of Jewish households, making the survey an extremely expensive proposition.

*Screener.* To determine a household's status for the survey a series of questions were asked about the religious and ethnic identities of the inhabitants. In keeping with the previous study, NJPS 1990, the screening questions began by asking about the current religious identification of household members. The previous study had, however, screened for respondents during an omnibus market research survey. By the time religion was mentioned, some rapport had been established between the interviewer and respondent, reflected by the trivial proportion of respondents who refused to answer the question. In contrast, the subsequent incarnation of NJPS called households directly. While the decision to eschew an omnibus survey was well founded given the very poor response rates for market research, it had significant implications for screening. Where NJPS 1990 had asked the questions in an ongoing interview, the way NJPS 2000-01 immediately characterized the survey as being about religion and ethnicity and asked about religious identification may well have been disconcerting to some respondents and led to systematic drop-out by people who were most sensitive to these topics. Immediately asking about Jewish parentage following an introduction that, in many cases, did not mention the Jewish purpose of the study may have raised further concerns among respondents.

This approach stands in contrast to studies of local Jewish communities, which usually introduce the study's Jewish purpose and ask first about Jewish identity. As I will

discuss later, a test of the indirect approach used by national studies of American Jewry against the more direct approach used by local studies found the local approach was associated with significantly higher response rates (see p. 251).

*Target population.* Another difference between NJPS and many local population studies was the definition of an eligible household. NJPS cast its net broadly, including any household containing an adult with a Jewish parent, who was raised as a Jew, or who considered him- or herself to be a Jew.[13] The advantage of this approach was that it permitted analysts to define Jewish identity in multiple ways and allowed for analysis of former Jews. The disadvantage was that scarce resources were spent conducting interviews with households that are generally of secondary interest although, as I shall discuss presently, these costs are minor compared to those of sampling.

*Survey instrument.* All might have been well had NJPS simply included any household with any adult who had a Jewish parent, was raised Jewish, or currently identified as a Jew. Instead, an extremely complicated scheme of sample allocation codes was developed that determine what interview a case received. This scheme is shown in Table 3:2.

Table 3:2. NJPS 2000-01 Jewishness Classification

| Sample Allocation Code | Current Religion | Jewish Mother or Father | Raised Jewish | Consider Self Jewish | Initial Classification | Reclassification | n |
|---|---|---|---|---|---|---|---|
| 1 | Jewish | - | - | - | Jewish | Jewish | 3,767 |
| 2 | Jewish and other | Yes | - | Yes | Jewish | Jewish | 14 |
| 3 | Jewish and other | Yes | - | No | PJB | Jewish-connected | 5 |
| 4 | Jewish and other | No | Yes | Yes | Jewish | Jewish | 1 |
| 5 | Jewish and other | No | Yes | No | PJB | Jewish-connected | 0 |
| 6 | Jewish and other | No | No | Yes | Jewish | Jewish | 2 |
| 7 | Jewish and other | No | No | No | PJB | Non-Jewish | 20 |
| 8a | Other (compatible) | Yes | - | Yes | Jewish | Jewish-connected | 72 |
| 8b | Other (incompatible) | Yes | - | Yes | Jewish | Non-Jewish | 253 |
| 9a | Other (compatible) | Yes | - | No | PJB | Jewish-connected | 66 |
| 9b | Other (incompatible) | Yes | - | No | PJB | Non-Jewish | 319 |
| 10a | Other (compatible) | No | Yes | Yes | Jewish | Jewish-connected | 1 |
| 10b | Other (incompatible) | No | Yes | Yes | Jewish | Non-Jewish | 11 |
| 11a | Other (compatible) | No | Yes | No | PJB | Jewish-connected | 2 |
| 11b | Other (incompatible) | No | Yes | No | PJB | Non-Jewish | 22 |
| 12 | Other | No | No | Yes | NSRE | Non-Jewish | 36 |
| 13 | Other | No | No | No | NSRE | Non-Jewish | 3,404 |
| 14 | None | Yes | - | Yes | Jewish | Jewish | 360 |
| 15 | None | Yes | - | No | PJB | Jewish-connected | 220 |
| 16 | None | No | Yes | Yes | Jewish | Jewish | 3 |
| 17 | None | No | Yes | No | PJB | Jewish-connected | 10 |
| 18 | None | No | No | Yes | NSRE | Non-Jewish | 2 |
| 19 | None | No | No | No | NSRE | Non-Jewish | 585 |
| Total | | | | | | | 9,175 |

*Notes:* PJB is "Persons of Jewish Background." NSRE is "National Survey of Religion and Ethnicity," the companion survey to NJPS for non-Jewish households.

The table should be read from left to right as a kind of cladogram (a branching diagram used to show evolutionary relationships between species and their ancestors), with cases being differentiated at each successive question. The sample allocation codes designated with an "a" or "b" are anachronisms, introduced after the survey for reasons that will be explained later. Each number initially formed a single sample allocation code.

The initial classification determined the type of interview a household received.[14] "Jewish" cases received the full NJPS interview, except for a few questions reserved exclusively for the next category: persons of Jewish background. Persons of Jewish background received a bowdlerized interview the skipped many questions about current

identity and, in a few cases, were asked a different version of a question than were Jews. (Only a portion of persons of Jewish background households were initially selected for interviews.) The final interview type, the National Survey of Religion and Ethnicity (NSRE), was administered to a random subsample of households without ties to the Jewish community to provide a point of comparison.

The most distinctive feature of the NJPS interviewing scheme was its inordinate reliance on respondents' self-classification to determine which questions a person received. One effect of this choice was that was that salient questions were not asked in some circumstances. A person with no religion but Jewish parents, for instance, would only be asked detailed questions about their current identity if they said they were currently Jewish. The dilemma, of course, is that identity is labile and contextual. People, particularly young adults (Caplovitz and Sherrow 1977), may resist being "labeled" while still identifying as Jews in substantive ways. Failing to ask these questions precluded empirical analysis of the ways in these individual differed from those who affirmed Jewish identity. In other cases, relying on current identity to determine the questions asked may have led to inappropriate questions being asked. A person with a Jewish parent who was raised as a Christian, was exclusively Christian by religion, but considered her- or himself to be Jewish, for instance, would be asked the full suite of questions on Jewish ritual observance. Of course, asking possibly irrelevant questions creates far fewer problems than excluding items *prima facie*.

The effect of the distinction between Jews and persons of Jewish background was magnified by the decision to ask only current Jews certain questions about their background. For instance, while both Jews and persons of Jewish background were asked

whether they belonged to a Jewish youth group while growing up, only current Jews were asked if they had attended a Jewish summer camp or detailed questions about Israel travel. Throwing away the "failures" renders these questions useless. It is impossible to compare the effect of summer camps without looking the lives of all those who attended, not only those who remained Jewish. Any analysis on such a censored sample would, for example, likely underestimate the effect of camps. Sadly, this was not the only illogicality. Questions on conversion from Judaism were only asked of persons of Jewish background. For example, someone who had Jewish parents and was raised Jewish but was exclusively a Christian at the time of the survey would only be asked about conversion to another religion if they did not say they identified as a Jew. Once again, a potentially useful question was rendered pointless. Questions about wedding officiants, too, were only asked of people who currently identified as Jews, despite the fact nonidentifiers may have been Jewish at the time of marriage. Other items not related to Jewish identity like geographic mobility were also skipped for persons of Jewish background, precluding potentially useful lines of analysis. Finally, in a few instances, Jews and persons of Jewish background were asked variations of the same question for no apparent reason. Jews, for instance, were asked "Last Christmas, did you have a Christmas tree?" while persons of Jewish background were asked "How often do you have a Christmas tree in your home?" This is a distinction without a difference, except that the questions are no longer directly comparable.

It is unclear why the decision was made to place such reliance on the question about current identity. The most likely reason was concern about cost, given the general effort to shorten the interview with persons of Jewish background. If so, it was misguided

as the additional cost was minuscule compared to the expenses of sampling. A total of 664 households containing persons of Jewish background but not Jews were interviewed, for the sake of the argument I inflate this to 800 to account for those not offered a full interview in the earlier replicates. If I assume that the additional cost of interviewer time is $25 per interview, only $20,000 would be added to a $6,000,000 study, or 0.3 percent. The $25 figure is wholly reasonable considering these households were already identified and, in most cases, interviewed at some length. All that was required was probably another 10 minutes of the interviewer's time. While this would add to the respondent burden, the total would be no more than that of the Jewish households.

I do not mean to advocate for an undifferentiated survey instrument that asks the same questions of everybody. In addition to variations in interviews based on the distinction between Jews and persons of Jewish background distinction, NJPS made extensive use of skip patterns, complex logical conditions that determine whether a respondent should be asked a question based on their answers to previous questions. For example, persons of Jewish background (and, alas, only persons of Jewish background) were asked if they had converted to another religion only if they were not Jewish but had been raised as a Jew, whether wholly or in part. Indeed, the impression one gets when reading the instrument (whether true or false) is that the distinction between Jews and people of Jewish background was overlaid on generally well considered skip patterns, with the resulting interaction between the systems causing most of the damage.

*Comparability with NJPS 1990.* After the survey was completed, it was realized that the classification of people into Jews, persons of Jewish background, and non-Jews was fundamentally different than that of NJPS 1990. Table 3:3 shows the items used.

Table 3:3. NJPS 1990 and 2000-01 Screening Questions

| Construct | NJPS/NSRI 1990 | NJPS/NSRE 2000-01 |
|---|---|---|
| Current religion | What is your current religion? | What is your religion, if any? |
| Religion born | In what religion were you born? | - |
| Religion raised | In what religion were you raised? | Were you raised Jewish? |
| Parental religion | Which of your parents were or are Jewish?* | Was your mother born Jewish?†<br>Was your father born Jewish? † |
| Self-identification | - | Do you consider yourself Jewish for any reason? |

*Source:* Kadushin et al. (2005).

*Notes:* * Only asked of a third of the sample. † In the screener, this was asked as "Do you have a Jewish mother or a Jewish father?"

People were classified as Jews in NJPS 1990 if they were currently Jewish by religion or if they were Jews of no religion, which was defined as people who were either born or raised Jewish by religion and either had no religion or identified with a nonmonotheistic religion. It had no equivalent to the Jewish-connected category. By contrast, the NJPS 2000-01 definition counted people who identified with another monotheistic religion as Jews while not counting some people with no religion as Jews.

In order to conform as closely as possible to the definition used in NJPS 1990, sample allocation codes were reclassified into Jews, Jewish-connected (cases that would have been classified as Jews in NJPS 1990 but did not receive the full interview in NJPS 2000-01), and non-Jews (see Table 3:2). In some cases the reclassifications were dramatic. People with a Jewish parent who identified with another monotheistic religion but said they considered themselves Jewish were originally interviewed as Jews but were reclassified as non-Jews.

Despite the reclassification, the questions asked in 2000-01 were fundamentally different than those asked in 1990: there was no equivalent to the religion born question

and religion raised, though asked, was not a part of the classification scheme. These differences contributed to ambiguity over whether the Jewish population has increased, decreased, or stayed about the same since 1990.

Another concern is that the reclassification again interacted with the distinction between Jews and persons of Jewish background in unfortunate ways. Some statistics are available for the "Jewish" population only, while others were calculated for the Jewish and Jewish-connected population. The statistics that were only available for the smaller population were marked with an asterisk in the NJPS report (Kotler-Berkowitz et al. 2004b), requiring mental gymnastics on the part of the reader to comprehend exactly what they mean. As an example, denominational market share in NJPS 1990 was calculated using the answers of all respondents, while NJPS 2000-01 only asked the question of people who currently identified as Jews. Not only were the resulting estimates incompatible, but excluding the unknown category likely overestimated the market share of traditionalist denominations (Table 3:4).

Table 3:4. Current Denominational Identification

| Denomination | Without Unknowns | With Unknowns |
|---|---|---|
| Orthodox/Traditional | 11% | 9% |
| Conservative/Reconstructionist | 28% | 24% |
| Reform | 35% | 30% |
| Just Jewish/Secular | 25% | 22% |
| Other | 1% | 1% |
| Unknown | - | 14% |

*Source:* Adapted from Kadushin et al. (2005)

At least one newspaper article misreported this statistic as a result of these errors (Cohen 2004). While current denomination cannot be imputed, other statistics are more amenable. If we assume that no person of Jewish background observed *kashrut* (Jewish dietary laws) at home, the reported level of practice dropped from 21 percent with an asterisk to 17 percent. Analysis of the dataset was further complicated by the absence of codes for why a value was missing.

Beyond the effects of the distinction between Jews and people of Jewish background, the wording of questions was sometimes changed between NJPS 1990 and 2000-01, making comparison difficult. For example, NJPS 1990 asked respondents "During Passover does your household attend a Seder in your home or somewhere else all the time, usually, sometimes, or never?" while the 2000-01 question was "Last Passover, did you hold or attend a Seder?" The tension between retaining a less than perfect item to maintain comparability and changing to a better item while losing comparability is a common one with no perfect solution. The differences between the introduction and screening questions in NJPS 1990 and 2000-01 create enough difficulty in making direct comparisons that other questions of comparability are effectively rendered moot.

*Skip pattern programming errors.* I have previously mentioned the complex skip patterns. NJPS, like all modern telephone surveys, was conducted using computer-assisted telephone interviewing technology where interviewers ask questions as prompted by a computer program and record the responses via computer. This procedure reduces data entry errors and makes possible highly complicated skip patterns. An unfortunate side-effect of computer assisted telephone interviewing systems, however, is that testing for programming errors is extremely difficult and amounts to error reduction rather than

error elimination (Tarnai and Moore 2004). The NJPS experienced more than its share of errors. Questions intended for unmarried partners were not asked of boyfriends/ girlfriends and fiancés/fiancées. Answers requiring a year did not include 2001 as a valid option. The already problematic question on conversion to another religion was further harmed when 80 qualified respondents were excluded for reasons not adequately explained either in the *User Guide* or *Study Documentation* (United Jewish Communities 2003h; 2003j). Some questions about children were not asked for children aged three to seven while others omitted to ask for years of Jewish education. An item on providing for Jewish charities in one's will suffered problems that were not adequately explained in the *Study Documentation* and omitted in the *User Guide*. Questions on the occupation and earnings of spouses and partners of nonworking respondents were mistakenly omitted. Respondents who were 62 were incorrectly excluded from a question on social security payments, as were spouses and partners aged 62 to 64. Questions on children at college were asked only if respondents with spouses/partners were both aged between 35 and 75 rather than if either the respondent or the spouse/partner fell in this age range.

*Other lost data.* Further complicating the analysis, the survey contractor (Roper-ASW) lost about two thirds of the screening data for households that completed the entire screener but refused to participate in the main interview ("screener incompletes") (Schulman 2003:12-13; United Jewish Communities 2003h). For those parts of NJPS for which we have data on refusers, the response rate for the main interview was 61 percent for persons reached who were identified as Jews, 55 percent for persons of Jewish background, and 41 percent for non-Jews. The loss of the screening data had serious implications, as the weighting scheme (discussed below) had to include estimates for the missing

data, raising additional possibilities for error. To the extent that refusers were different than those who completed the interview, they had to be taken account of in the weighting scheme. In fact, the long-interview refusers were more likely to be in households that included both Jews and non-Jews rather than households composed entirely of Jews (Phillips, Kadushin and Saxe, 2004). Such differential response rates are consistent with studies that find that those less interested in the subject of a survey are less likely to respond (Groves et al. 1992; Groves, Presser, and Dipko 2004b; Groves et al. 2000).

*Weighting*. So far, discussion of survey weights has been limited to design weights, which adjust for the probability of selection. For instance, a unit that had a probability of selection of 100/100,000 (i.e. 100 units were selected from a population of 100,000) would receive a weight of 100,000/100, or 1,000. A second type of weights—poststratification weights—adjust for the fact that different types of people may have responded at different rates, and the sample may not accurately represent the population, and are developed empirically after design weights have been applied. For instance, if a survey found that 60 percent of its respondents are women, far above the actual population rate, females would be "downweighted" while males would be "upweighted."

Because some NJPS strata were sampled at different ratios, the first layer of the design weights compensated for unequal sampling ratios. The second type of design weight accounted for the fact that the more telephone numbers a household had, the greater its chance of being sampled. Typically, respondents are asked about how many telephone lines in their household receive voice messages. The design weight in this case was the reciprocal of the number of phone lines (2 phone lines = 1/2 = a weight of 0.5).[15] Unfortunately, if a household was selected for a longer interview, NJPS did not ask the

question about the number of telephone lines in the screening interview. Because some respondents declined the long interview, these critical weights were missing for about 40 percent of the Jewish sample. To correct for this problem, study analysts sorted completed screening interviews by subsample (Jewish, persons of Jewish background, and non-Jewish) and by household size and applied the average weights of these cases to the other screening interviews (United Jewish Communities 2003i). Although there may have been no reasonable alternative, this procedure made these weights potentially unreliable.

The third type of design weights applied to the longer interviews for which Jews, persons of Jewish background, and non-Jews were selected at different rates. The exact proportion of persons of Jewish background selected varied by replicate.[16] (A replicate is a random draw from the telephone numbers that constitute the sample. The purpose of replicates is to ensure that when a study is conducted over a long period of time, persons later called do not systematically differ from those called earlier.) Persons of Jewish background had to be upweighted to compensate for the fact that only some were offered the full interview while others were missing by design. There were 22 such replicates in the NJPS.

A fourth type of design weight applied to individuals. Thus far, the focus has been on households, not individuals. Although household composition is important, the survey also included individual opinion items. The more individuals in the household over 18, the less likely a particular individual was to be interviewed.[17] The design weights are directly proportional to the number of individuals in the household. As noted above, a major problem for interpretation of NJPS is that *all* screening data for people who refused the longer interviews were lost for the first 15 replicates. In order to know how many

people of different religions there are, one needs screening interviews for both those who were offered a longer interview and those who were not. In the case of non-Jews, this was not a serious problem, since very few were offered a long interview. All Jews were offered a long interview, so about 73 percent of the respondents who refused were missing. The study authors chose to estimate the number of missing screeners on the basis of replicates 16 through 22 (which represented an estimated 30 percent of the total number of screening interviews), and used those estimates to arrive at estimates of the size of the population of Jews (and persons of Jewish background, and non-Jews), adding further uncertainty to the weighting scheme.

The problem regarding poststratified estimates is made even more critical for NJPS because of the high percentage of nonrespondents. This issue represents a "bird in hand" dilemma: To what extent are people who respond different in systematic ways from those who do not respond? Poststratification weighting of NJPS was intended to adjust both for differential sampling by stratum, as well as for differential response rates by region and household size, and the number of persons in the target population in the United States. The screening questions did not ask, as do most sample surveys, about household income, gender, age, or level of education of the informant. Typically, sample surveys adjust the sample to conform to the distribution of these characteristics in the population. One assumes that the bird in hand is the same as the one in the bush, not only on these characteristics, but on others as well. Although there was no alternative to the assumption that non-respondents were identical to respondents with the same characteristics, the likelihood that they are different increases with a very low response rate. NJPS weighted non-Jewish respondents to the longer interview by age and sex, first subtracting

respondents who were Jews or persons of Jewish background. For Jews, adjustment by age and sex were impossible because there was no outside yardstick against which to weight Jews.

There were several obvious problems with the poststratification weighting of NJPS. Part of the problem was caused by the imputation of a large number of missing screener cases and the lack of information about telephone lines. NJPS used a complex respondent selection procedure: in Jewish households only Jews were eligible for selection, while in Persons of Jewish Background households only Persons of Jewish Background were eligible for selection. The weight assigned to the respondent was the reciprocal of the number of eligible adults in the household. However, the change in the definition of Jewishness sometimes altered the status of households or individuals, potentially invalidating the weights. Finally, by not using the full data in the screening interviews for replicates 16 through 22, the current weights do not take account of the fact that refusers had more non-Jews in their households.

Finally, some useful information available for households that were screened but refused was not used to develop weights. Households with both Jews and non-Jews/ persons of Jewish background were more likely to drop out than all-Jewish households. Poststratification reweighting that accounts for the screener data makes clear the importance of these issues (see Appendix E). This analysis, for example, produces different figures for intermarriage rates (the total rate of intermarriage is estimated to be 37 percent as compared to 31 percent when using unadjusted weights). This should not be surprising, since households with adults of different religions were more likely not to complete

the long interview. But the reweighting did not dramatically affect other measures; in most cases, the differences were no more than a few percent.

*Bias.* It is difficult to determine whether American Jews are likely to be over- or underestimated on telephone surveys as there are countervailing sources of bias. American Jews are overwhelmingly white and whites in general are overrepresented on telephone frames, due to the overrepresentation of blacks and Hispanics among nontelephone households and similarly among people with cell phones only, as well as the greater difficulty of contacting nonwhite households that do have landline telephones. As the age distribution of the Jewish community is skewed toward the elderly due to a low birthrate, it is possible that Jews are overrepresented as a result of a relative dearth of young adults, who are relatively unlikely to be counted. On the other hand, high levels of education and income, both of which are associated with American Jews, are linked to greater difficulty of contact and possibly higher rates of refusal. Thus, among the white population, Jews are likely to be underrepresented to some extent. For national surveys of the Jewish population, like NJPS 2000-01, an additional element that may lead to underenumeration is the fact that urban and suburban areas where Jews are disproportionately located consistently have lower response rates than do rural areas. Finally, compared to the national population, Jews are disproportionately located in the Northeast, which has the lowest response rates. As there is a lack of reliable data on the relative impact of these biases, it is impossible to determine from reasoning from the characteristics of the Jewish community whether it is likely to be under- or overrepresented. It is clear, however, that a level of uncertainty greater than that produced by sampling error alone surrounds estimates of the size of the Jewish population.

While we cannot predict the direction of bias from the characteristics of the American Jewish community, a number of studies have specifically addressed this issue. Schulman's (2003) review of NJPS for the study's sponsor contains estimates of the proportion of American adults who are Jewish from other sources. In comparison to the 1.6 percent estimated by NJPS, Gallup polls estimated 2.0 percent, while the American National Election Studies and the General Social Survey concluded 1.8 percent of American adults were Jewish. Conversely, the American Religious Identification Survey/ American Jewish Identity Survey, which was based on an omnibus survey (actually, two), estimated the statistic at 1.4 percent. While based on a limited number of data points, this supports the notion that Jews were underrepresented on NJPS, although the extent to which Jews may be underrepresented on all surveys remains unknown.

Other evidence that alone might be insufficient also points in this direction. A *post hoc* comparison of the proportion of cooperators and refusers with distinctive Jewish names, found that refusers were significantly more likely to have a "Jewish" name (0.37%) than were cooperators (0.16%), suggesting that Jews were less likely to respond, resulting in underestimates of the Jewish population (United Jewish Communities 2003a).[18] Unfortunately, no data on the likelihood of contact of Jewish and non-Jewish households was provided. Distinctive Jewish names are a problematic source for population estimates and this figure should not be used to estimate the magnitude of the effect. Nevertheless, it strongly suggests that the size of the Jewish population was underestimated. Tobin and Groeneman (2003) attempted a similar study comparing outcome rates of random digit dialed samples with samples drawn from Federation lists in four metropolitan areas (Houston, Miami, Minneapolis, and San Jose). Unfortunately, the study was

flawed. List samples invariably in higher contact rates than random digit dialing, making comparison impossible. All things being equal, one would also expect that people from list samples would be more cooperative. Even so, members of the list sample refused (out of those that were successfully contacted) at the same rate as did people from the random digit dialing sample. Kotler-Berkowitz et al. (2004b:31) recognize these problems in the study report, noting that "many researchers believe that the methodologies of survey research may yield undercounts of the Jewish population."

It has also been argued that certain groups in the Jewish population (e.g., Orthodox, Israeli, Russian, intermarried, and unaffiliated Jews) are less likely to participate than are other Jews, leading to biased estimates of the characteristics of the Jewish population. While the latter two have not been the study of formal study, the first three groups have been tested by United Jewish Communities and Tobin and Groeneman.

Complaints from within the Orthodox community arise from time to time that survey estimates of the number of Orthodox Jews are too low. United Jewish Communities (2003k) accordingly investigated, finding that no significant differences in the refusal rate of the Orthodox sample from a Conservative/Reform sample, while an "unaffiliated" sample was more cooperative. Being found on the lists of Federations, however, the "unaffiliated" sample, even if they had not contributed for two years and were not known to belong to a synagogue (many of which do not share their membership lists with Federations), was clearly not composed of entirely marginal Jews. Tobin and Groeneman (2003) came to a similar conclusion, finding that the Orthodox were more likely to be contactable (which the United Jewish Communities study did not address) and about as likely to cooperate. There does not appear to be any support for the idea that estimates of Ortho-

dox Jews are biased. Surveys of Jewish communities, with the exceptions of those that screen for Jewish households using omnibus surveys like NJPS 1990 and the American Jewish Identity Survey of 2001, invariably do not make telephone calls on the Jewish Sabbath and holidays. This eliminates what might otherwise have been a source of bias against Orthodox and other observant Jewish households that do not use telephones at these times.

Estimates of the number of Israelis in the United States have at times been put as high as 500,000, compared to survey estimates of 100,000 or less, leading to suggestions that Israelis were grossly undersampled. While undertaking no additional research, United Jewish Communities (2003b) thoroughly reviewed the existing literature, including estimates derived from the census and government immigration records to show that the NJPS estimate of 70,000 to 93,000 was roughly what it should have been. A more direct test by Tobin and Groeneman (2003) came to the same conclusion, finding no significant differences between Israelis and "mainstream" Jews regarding contact or cooperation (these cases came from Federation lists), although as these Israelis were found on various lists, there is some possibility that they may have been easier to reach than Israelis in general. It is possible that some of the discrepancy between guesstimates of the number of Israelis and survey estimates is due to the existence of a population of illegal migrants who have overstayed their visas. This putative group would be a classic example of a "hidden population" that is difficult to reach by standard surveys. The significance of this population, should it exist, is questionable. Given the small size of the legal Israeli migrant population, the illegal population would have to be very large indeed in order to

comport with the guesstimates. Substantively, any illegal Israeli population is likely to be transient in nature, probably composed heavily of young adults after military service.

Estimates of Jews of Russian origin have also been challenged as too low. United Jewish Communities (2003c) reconciled estimates from the Hebrew Immigrant Aid Society with its own figures by accounting for non-Jews in these estimates, although its procedures were problematic—namely, relying on the guesstimates of two unnamed former officers of the society. This defense also left itself open to charges that the number of Jews from the former Soviet Union that migrated independently of the Hebrew Immigrant Aid Society was too low. Tobin and Groeneman (2003) found that Russian Jews were about as likely as "mainstream" Jews to be contacted (although as interviewing was in English only, it seems likely that contact rates were in reality lower) but were less likely to be cooperative. The Russian households were apparently derived from Hebrew Immigrant Aid Society lists as well as "known" (the report does not specify how) Russian émigrés. Russian Jews do, then, appear to be underrepresented on surveys, especially those that do not conduct interviews in Russian. NJPS interviewers were, however, trained to attempt to determine if a non-English speaker was speaking Russian and had Russian-language interviewers available to call back. It is likely that these efforts reduced but did not entirely eliminate bias against Russian Jewish households.

Although not part of the series of methodological studies commissioned by United Jewish Communities, estimates of intermarriage have seen more than their fair share of controversy. The NJPS sample, as weighted, was in fact biased against ethno-religiously mixed households, presumably mostly intermarriage (Kadushin et al. 2005). This was a result of disproportionate drop-out of ethno-religiously mixed households

after screening questions had been completed but before the main interview. Figure 3:6, below, shows the distribution of household ethno-religious composition by whether a main interview was completed or not: "complete" indicates that the main interview took place while "incomplete" households dropped out after screening.

Figure 3:6. Comparing Complete and Incomplete Household Types

| | *Jewish* | | *Persons of Jewish Background* | | *Non-Jewish* | |
|---|---|---|---|---|---|---|
| *Household type* | *Complete* | *Incomplete* | *Complete* | *Incomplete* | *Complete* | *Incomplete* |
| Jew only | 70.8% | 52.3% | | | | |
| Jew + Jewish-connected | 0.7% | 2.0% | | | | |
| Jew + Jewish-connected + non-Jew | 0.4% | 0.9% | | | | |
| Jew + non-Jew | 16.5% | 32.0% | | | | |
| Jewish-connected only | 0.9% | 0.3% | 18.9% | 7.2% | | 0.1% |
| Jewish-connected + non-Jew | 1.2% | 0.9% | 30.6% | 27.3% | | |
| Non-Jew only | 9.5% | 11.5% | 50.5% | 65.5% | 100% | 99.9% |
| Total | 100% | 100% | 100% | 100% | 100% | 100% |

*Source:* National Jewish Population Survey 2000-01

Jewish households (i.e. those containing an adult who was currently Jewish) containing Jews and non-Jews were about twice as prevalent among incompletes as completes. Once adjusted for, the overall intermarriage rate was estimated at 37 percent, not 31 percent as reported. Other estimates changed little (see Appendix E for details).

A key question is whether this phenomenon also applied to participation in the screener. Were intermarried households more likely to refuse the screener? It has long been observed that those who find a survey to be salient are the most likely to respond (e.g., Heberlein and Baumgartner 1978). Recently, this has been expanded in the form of leverage-salience theory (Groves et al. 2000), which holds that different types of appeals have different levels of leverage for a given respondent, which survey researchers can

exploit by varying the salience these attributes are given during the survey request. It is likely that being Jewish is on the whole less salient to intermarried than inmarried Jews, if for no other reason than intermarried Jews have less Jewish social and human capital (Phillips and Fishman forthcoming). However, another factor is surely at work, too. Intermarried Jews and their spouses often feel rejected by the Jewish community, weakening their ties and likely undermining compliance with a request to complete a Jewish survey.

Could these factors have been at work in the screener? The introduction to the survey (p. 181) did not mention the Jewish community at all and hardly seemed calculated to leverage anyone, let alone Jews. The first question similarly asked only for the religion of the initial informant, the person who picked up the telephone. So far there is little to suggest that intermarried households might be more likely to decline to participate. Things begin to change at this point, though. If the respondent was Jewish by religion, an introduction calculated to leverage Jews was read (see p. 182). Two sentences in particular seem likely to cause offence to someone with the most intimate of ties to non-Jews: "Five thousand Jews across the country are being interviewed. Our objective is to understand the characteristics and needs of the Jews in America" (United Jewish Communities 2003g). No mention was made of people who may be living with Jews. The use of the definite article ("the Jews"), in particular, seems calculated to reinforce the purely Jewish concerns of the survey. If the informant was not Jewish by religion (most probably the non-Jewish spouse, although a considerable proportion of intermarried Jews are secular), the survey was introduced as the National Survey of Religion and Ethnicity and makes no mention of the Jewish purpose of the study (p. 182). However, after asking a

few questions about the relationship of other household members to the informant and their religious preference, the tone suddenly shifted, with the interviewer saying (United Jewish Communities 2003g): "Part of this national study is to determine the extent to which Americans of different backgrounds also have Jewish origins. So I would like to ask a few questions about that." While not as jarring as the introduction for Jewish informants, it might be taken as a violation of the implicit contract between interviewer and interviewee: the interviewer will ask questions relevant to the survey topic and the interviewee will respond. It is possible that non-Jewish spouses in intermarriages may have felt marginalized and dropped out at that point. Unfortunately, one can only speculate as the screening data do not include a record of partial screener interviews.

Unlike the other putatively problematic categories, there are no data—good or bad—to determine whether unaffiliated Jews were more or less likely to respond to the survey. By their very nature, it is not possible to get a list of truly unaffiliated Jews to use for *ex ante facto* explorations of nonresponse, and the screener did not include items that could be used to determine whether unaffiliated Jews were more likely to drop out prior to the main interview. Still, to the extent that the introduction for the survey emphasized Jewish concerns, which may have had little salience for unaffiliated Jews, there may have been some bias against less attached Jews.

Beyond the potentially problematic categories, certain estimates in NJPS appear to be biased. The prime example is a report on Jewish college students (United Jewish Communities 2004b). The 18-24 year-old age group, for which separate estimates are presented and which supplies the majority of college students, is apparently biased with respect to gender and attendance at college. Respondents in this age category were 56

percent female rather than the nearly even split in the population, adjusted by the original respondent weights. College students among this age group were much more likely to live at home than shown in other studies of college students. This is understandable as dormitories, for the most part, are not part of a random digit dialing frame and college students are notoriously difficult to contact for survey research. College students and other young adults are unlikely to be at home from 6pm to 9pm and increasingly use cell phones exclusively, which are not included on random digit dialing frames. The college students most likely to be reached are, therefore, those who live with their parents at home. The Orthodox are overrepresented among those living at home, which may be the cause of high levels of Jewish behavior observed in respondents under 30 years of age and apparent population bulge of Orthodox young adults (United Jewish Communities 2004a; 2004b).

*Response rate.* While the response rate to a study is far from the only measure of quality, it remains true that the lower the response rate, the greater the potential for bias. The overall response rate for screener questions was about 28 percent (United Jewish Communities 2003h). This is significantly lower than those achieved by community studies carried out by Ukeles & Associates in the present decade, despite the fact that many of these were conducted in the Northeast, where telephone surveying is notoriously difficult.[19] Identifying exactly why this is the case is difficult.

It might be thought that the practice of not making calls on the Jewish Sabbath depressed the response rate. This is certainly one factor, as 55 to 60 percent of the population is at home on Saturdays between 10am and 4pm, though Sundays are even better (Maitland 2006). While not making calls on Saturday may globally decrease Jewish re-

sponse rates, this clearly cannot be the whole story in the case of NJPS, as the Ukeles & Associates studies which achieved higher response rates than NJPS also followed this practice.

As suggested earlier, indirect screeners appear to depress response rates. The Survey of Heritage and Religious Identification (Tobin and Groeneman 2003), which used a very indirect screener, achieved a practically identical response rate of 29 percent. Another contributing factor may be that NJPS used only random digit dialing. List samples, such as those used in combination with random digit dialing in the Ukeles & Associates studies, traditionally have higher response rates.

Another likely reason for the low response rate is simply that NJPS took few steps to increase the response rate. No prenotification letters were sent to households for which an address could be determined explaining the survey, a step which typically significantly increases response rates (Camburn et al. 1995; de Leeuw et al. 2006; Traugott et al. 1987). As there was no prenotification letter, there could be no preincentive, which significantly increases response rates further still (Cantor et al. 2006a; Church 1993; Curtin et al. 2006; Singer et al. 2000). A postincentive of $25 was tested in the seventh replicate that was fielded from October 18, 2000, nearly two months after the survey began. As the results of the test were apparently positive, the postincentive was implemented for replicates 14 and beyond, beginning December 19, almost four months after calling began. Postincentives are generally less effective than preincentives, although the amount offered was large enough to have some positive effect (Cantor et al. 2006b; Fahimi et al. 2006b); as I discuss later, an $18 postincentive was found to have no discernable difference from a $2 preincentive (see p. 254). The fact that the incentive was reserved for

cases where the household was "selected by the computer for a longer interview" (United Jewish Communities 2003g:1), as interviewees were told, may have limited its effectiveness, however.

Other than for the week of October 14-21, refusal conversion incentives were not offered to households that had refused in the screener. These have been shown to be effective at converting refusals (Fahimi et al. 2006b), although Curtin et al. (2006) find refusal conversion incentives to be effective only when used to increase the response rate of subpopulations known to be underrepresented. Refusals and midinterview break-offs of known Jewish households were offered $50 on callback from October 14, 2000, but these had no effect on the screener response rate. The number of refusal conversion callbacks is not clear.

Letters were sent to households that expressed concerns about the legitimacy of the study *and* agreed to provide a mailing address, but this may well have been too little, too late. (The number of households sent such letters is not documented.) The letter received by households of unknown Jewish status was also vague. De Leeuw et al.'s (2006) meta-analysis of contact letters found that a "tit-for-tat" strategy when surveyors offered a copy of the results or explained the sampling process was most effective, suggesting that lack of clarity about a study's objectives will do little to increase cooperation.

Besides prenotification and incentives, the level of effort put into contacting each household was mediocre. The number of contact attempts was capped at eight for budgetary reasons (it was originally sixteen), high for a market research survey but low for government or academic research.

So far, I have discussed only the screener response rate. Once identified, households with Jews and persons of Jewish background were solicited to participate in the longer interview. However, since many people refused to participate in the longer interview (which asked detailed questions about Jewish identity), the final response rate for Jews and persons of Jewish background was less than 20 percent (Schulman 2003). The drop-out was in large measure produced by the two-stage nature of the sampling design. On reaching a household, the interviewer asked whoever picked up the phone (the initial informant), provided they were 18 years of age or older, about the adults in the household. The household roster information was then used to determine which adults were eligible to serve as a respondent (see note 14 of this chapter for a full description of sampling procedures). The respondent then was randomly selected from among eligible adults. This step was necessary as the initial informant was not a random draw from the household. Older adults, women, and people not in full-time employment are the most likely to be at home and available to answer the telephone (Maitland 2006). Even if all eligible adults are at home, some may be more likely to answer than others. In order to collect detailed information about an individual, including their attitudes and opinions, it is necessary to make a random selection from all adults, as otherwise the data will be systematically biased. Local surveys of Jewish communities typically avoid this problem by asking most questions only about the household roster or household level behavior such as belonging to a synagogue.

The problem with randomly selecting the respondent is that it harms the response rate. In some cases, the selected respondent would not have been able to respond due to health problems. In others, the respondent may have been unavailable, requiring addi-

tional contact attempts that may or may not have succeeded. The selected respondents may also have refused to participate in the survey. The informant may also have acted as a gatekeeper. As only Jewish respondents were eligible, non-Jewish spouses and partners may have felt slighted (the higher drop-out rate of ethno-religiously mixed households described on pp. 208-211 may well be a product of this). When the initial respondent was a woman and the interviewer asked to be transferred to her husband or partner, she may have felt that the interviewer really wanted to speak to the "man of the house" and the talk of random selection was only subterfuge.

Even when a respondent begins the long interview, there is no guarantee she will complete it. In NJPS, the description about the length of survey offered to respondents who asked was deceptive, which may have increased the number of break-offs. It was described as being between 15 and 35 minutes for most people, when the mean length was 44 minutes with a standard deviation of 11 minutes (United Jewish Communities 2003h).

*Reporting estimates.* A final concern with NJPS is the way in which estimates were reported, invariably without confidence intervals and often including exact figures for population size, which was known to be likely biased. This was unfortunate, as it gave readers an unwarranted impression of precision, when compared to the contingent and probabilistic nature of survey research. A representative example comes from the special report on Nazi victims (Kotler-Berkowitz, Blass, and Neuman 2004a:5):

> NJPS data are weighted to produce population estimates. The 146 NJPS respondents who were identified as victims represent a total victim population of 122,000 people, including 87,000 who lived in a country under Nazi control and 35,000 flight cases. Among the 87,000 who lived in a country under Nazi control, 21,000 (24%) were in concentration

> camps and 16,000 (18%) were in labor camps. The 122,000 victims represent 7% of all U.S. Jews over the age of 55.

Kadushin et al. (2005:11-12) reword the passage as follows:

> NJPS data were weighted to produce estimates of the characteristics of a given population. The 146 NJPS respondents who were identified as victims represent between 6% and 9% of American Jews over the age of 55 at the 95% confidence interval, including 107 respondents who lived under Nazi control (4% to 7%) and 39 who left a country that had fallen under Nazi control (1% to 3% of American Jews over the age of 55). Among those who lived in a country under Nazi control, between 15% and 32% were in concentration camps and 10% to 26% were in labor camps.

In retrospect, this may have been a little extreme. The midpoint of the estimate can be given, while the number of respondents can be omitted, as the confidence intervals convey similar information, only more precisely.

*Evaluating NJPS 2000-01.* The difficulties encountered by NJPS were both a product of the environment and a result of decisions made in the survey design. Telephone survey research faces unprecedented challenges from the perspective of validity and cost. Surveys of Jews, unfortunately, have greater exposure to these problems than do many other surveys. From the perspective of cost, as a survey of Jews must contact many households for every Jewish household interviewed, even small increases in cost per interview can lead to major increases in total cost. Jewish surveys are also particularly sensitive to the biases and declining response rates of the telephone survey research environment as there are practically no external sources of data to which estimates can be validated and, if necessary, corrected.

While these challenges were already considerable, their impact was exacerbated by the survey design. The research team chose not to use well-known and effective ways to increase response rates nor did they build in ways to understand the characteristics of nonrespondents. Design decisions also introduced additional errors. The research team's failure to foresee that the interaction of a new definition of Jewish identity that made rather large assumptions about the identities and behavior of people with a Jewish background with a survey instrument that foreclosed large areas of inquiry based on the definition led to the illogical exclusion of certain groups from key questions. (Some members of the National Technical Advisory Committee did, in fact, appreciate these problems, but their concerns were overridden by the research director.) The result was an extremely confusing set of estimates, some of which pertained only to a more strongly identified population of 4.3 million and others to a broader population of 5.2 million. Additional problems were caused by an incompetent survey research firm that lost critical data and had a large number of errors in its computer-assisted telephone interviewing system, and a poorly structured contract that left United Jewish Communities to pay for the work required to ameliorate these problems, increasing the already stratospheric cost of the study.

While NJPS had more than its fair share, all surveys contain flaws. It is critical to understand the limitations introduced by flaws, provide a full explanation, and present analyses that either avoid problematic areas or provide appropriate qualifications. Here, too, decisions made by those responsible for the study heightened preexisting problems. While today there is an extraordinary amount of methodological information available on NJPS, this has largely been the result of outside pressures. Much of this material was

generated after the withdrawal of the survey's findings in 2002 following external pressure. Had there been a serious internal analysis of the effects of the problems initially, this unbecoming spectacle could likely have been avoided. Despite the considerable body of material available on the study's shortcomings, topical reports by United Jewish Communities still contain misleading information. The report on Nazi victims (Kotler-Berkowitz et al. 2004a), for instance, provides point estimates that give an unwarranted impression of certainty given the wide confidence intervals in the underlying data. More problematically still, reports on college students (United Jewish Communities 2004b) and young adults (United Jewish Communities 2004a) are based on data that are known to be biased.

While NJPS has significant drawbacks, particularly as a source of accurate point estimates of the characteristics of American Jews, it remains an incredibly useful as a way of understanding the relationships between key factors in Jewish life. Multivariate analyses of NJPS data have taken a while to appear, but are now becoming available and will add significantly to our knowledge of contemporary American Jewish life.

## ENDNOTES

[1] The use of social indicators to estimate the characteristics of rare or hidden populations was suggested by Christopher Winship in early planning meetings for the Boston Jewish Population Study.

[2] The effect of various assignment strategies is beyond the scope of this discussion. See Shadish, Cook, and Campbell (2001) for a detailed exploration of this topic.

[3] In Canada, response rates to marketing surveys fell from 17 percent in 1999 to 12 percent in 2002, while the response rate to omnibus studies has declined from 19 percent in 1997 to 13 percent in 2002 (Allen et al. 2003).

[4] Unless otherwise stated, all information on federal restrictions on cell phone sampling comes from Dautch (2005).

[5] I cannot cite the source of the 40 percent estimate, as it was proprietary information provided informally and not for attribution.

[6] The Pew Research Center for the People & the Press (2006) provides similar estimates, with 91 percent of their cell phone only sample reporting keeping their phone always on while those with a cellular telephone and a landline kept their cell phone on 73 percent of the time.

[7] More sophisticated implementations of list-assisted random digit dialing frames exist (Tucker et al. 2002).

[8] Some anomalous data points do exist, however. Fecso, Broach, and Grigorian (2005) experimented with using a brochure as an incentive and achieved lower response rates and lower survey quality. Singer et al. (2000) found that prenotification letters alone are no more effective than no prenotification, as did Curtin et al. (2005). The widespread replication of the finding that prenotification letters are effective across survey modes and studies suggests that these are anomalies rather than counterfactual data.

[9] The Survey of Heritage and Religious Identification instrument is not strictly a screener, as every individual contacted was asked the full battery of questions. The questions do, however, classify respondents into Jews, non-Jews and various intermediate states. Tobin and Groeneman (2003) suggested that a similar approach could be taken by future studies of Jewish communities. However, the time and expense involved was only possible given the study's goal of estimating the Jewish and Jewish-related population of the United States. Had detailed questions on the characteristics, behaviors and attitudes of the Jewish population been asked, the expense would likely have been astronomical and many respondents would have terminated mid-interview due to its extreme length.

[10] As Cohen (1999) shows, however, Israelite identity was not transmitted matrilineally, testifying to the evolving nature of Jewish identity.

[11] A design effect is a measure of the sampling variance of a complex sample compared to a simple random sample of the same size and characteristics (Groves et al. 2004a). A complex sample with a design effect of 2.0 is half as efficient as a simple random sample of the same size. It would have to have twice as many cases in order to have the sample level of precision as the simple random sample.

[12] This figure is calculated from United Jewish Communities (2003j:13) reports of the number of completed screeners and the estimated number of "screener incompletes" for replicates 1-15, which were lost by RoperASW.

[13] People who currently identified as Jews were not included in NJPS unless they had a Jewish parent, were raised as Jews, or identified as Jewish by religion (whether exclusively or not).

[14] The respondent was only selected from among the most Jewish adults. If a household included Jewish, persons of Jewish background, and non-Jewish adults, the respondent would only be selected from the Jewish adults. If a household contained only persons of Jewish background and non-Jewish adults, the respondent would only be drawn from adult persons of Jewish background.

[15] No matter how carefully the question on telephone lines is phrased, some respondents will misinterpret it to mean the number of extensions they have for the same number. In NJPS, up to 13 numbers were reported in the screening interviews; up to 6 or more in the longer interviews (the number was truncated at 6 and above). It was decided to give weights for one, two or three or more lines. Some surveys decide to count one or two or more lines, choosing not to believe more than that.

[16] The proportion of persons of Jewish background selected for these interviews is not entirely clear from the documentation. It appears that in the first replicate (round of interviews), 50 percent of persons of Jewish background were selected, and in replicates 2 through 15, 33 percent were selected. In replicates 16 through 22, all were selected.

[17] The actual rules for selecting adults within households as respondents were rather complicated. In a house with one or more Jewish adults, people who were currently Jewish could be selected as the respondent. For households with no Jewish adults, but with one or more adult persons of Jewish background, only persons of Jewish background could be selected. Non-Jewish adults were only eligible to be respondents in a household where there were no adults who were Jewish or persons of Jewish background. Respondents are weighted for the number of eligible adults in the household. In other words, in a household with two Jews, a person of Jewish background and a non-Jew (all adults), only the two Jews could have been selected as respondents, so the respondent receives a weight of about 2.0, not 4.0 (the respondent weighting scheme also took into account some characteristics of the respondent, like age and sex).

[18] Names were found by address matching based on telephone numbers. A list of 31 distinctive Jewish names was used. It is not known which names these included.

[19] I exclude studies conducted by Ira Sheskin because the response rates reported are implausibly high and insufficient data is available to understand the context of these reports. As an example, Sheskin (2003a) reported a screener response rate of 93.0 percent, and this after only four contact attempts and no refusal conversion. One reason for this is that Sheskin used a nonstandard formula that only counts refusals against the response rate. (See Appendix M for details on the calculation of response rates.) Attempts to calculate a response rate following American Association for Public Opinion Research (2006) formulae are made difficult by Sheskin's uninformative and nonstandard final dispositions. Even so, the response rates are still extraordinarily high, calculated to be 78.9 percent (AAPOR RR3). The final dispositions reported are only for the first 10,000 numbers out of a total of 27,500, a nonstandard practice that may inflate the response rate by including only the most intensively worked numbers. One problem is that answering machines are grouped with no answer or permanently busy numbers. When a household can be inferred from an answering machine, it should be counted as known eligible, while telephone numbers that are never answered or are permanently busy should be counted as being of unknown eligibility. However, this cannot be the entire explanation, as treating all such cases as known eligible only decreases the response rate to 70.8 percent. Further, there are a number of glaring omissions from the table of dispositions. There is no apparent category for unresolved cases where inconclusive contact was made, such as a request for a callback. Given a maximum of four contact attempts were made (a very low number), this should be particularly large. No households are

reported as having language barriers, which seems unlikely, particularly given that Sheskin reports no Spanish-language interviewers. No out of area cases are reported, which again is unlikely given number portability. Similarly, one would expect to find a number of cases where the telephone was answered by a minor, but none are listed.

# Chapter 4

## Sampling Frames

In 1936, *Literary Digest* magazine famously predicted that Alf Landon, the Republican candidate for president, would beat Franklin Delano Roosevelt by a landslide, 55 percent of ballots cast to 41 percent (Converse 1987; Squire 1988).[1] There were strong reasons to believe the *Literary Digest*. It proudly proclaimed the "uncanny accuracy" of its polls, and had predicted the winners of the three previous presidential elections. It did not lack for sample size, either, with well over 2 million responses from more than 10 million straw votes sent out. As many as 400 clerks were required just to sort and count the ballots. Nevertheless, when Election Day came, the nation endorsed Roosevelt 61 percent to 37 percent. The *Literary Digest* had underestimated Roosevelt's vote by 33 percent and overestimated Landon's support by 49 percent, a staggering error. Presciently, a young man by the name of George Gallup had predicted in his syndicated column not only that Roosevelt would win (although he, too, underestimated Roosevelt's support) but performed the pollster's equivalent of Babe Ruth's called shot by accurately predicting the final estimate of the *Literary Digest* poll. What happened was that the sources the *Literary Digest* used—its subscribers, people with telephones, automobile owners, and regis-

tered voters—were not representative of the American population. In the terminology of survey research, the target population (i.e. Americans who would vote in the election) did not match the sampling universe, the units eligible to participate (i.e. *Literary Digest* reading, telephone subscribing, automobile owning, registered voters). The aforementioned sources the poll drew on to gather addresses to send ballots constituted the sampling frame, the set of materials used to identify elements of the target population (Groves et al. 2004a:68). The units found on the sampling frame constitute the sampling universe. ("Units" and "elements" are used in preference to "people" or "households" because surveys encompass many types of sampling units—businesses, farms, even fish.)

The preceding chapters outlined sampling frames—some common, others obscure—in some depth. The question I will address in this chapter is: Given the range of choices, what is the best sampling frame to adopt? The iron law of survey research is that it is not possible to have a large, high quality sample at reasonable cost. Survey methodology is the art of finding the best compromise between these considerations. The iron law, however, focuses on practical considerations: cost, measures to improve survey quality, and sample size. The central purpose of surveys has so far escaped definition.

The job of a survey, then, can be defined as providing the most accurate understanding of the characteristics of subdivisions of a population, including subpopulations and elements. The central characteristic on which all else is based is relative size. Had the *Literary Digest* the capability in a time before computers and even punch-card sorters, it could have examined (however inaccurately) the voting preferences of the Republican and Democratic subpopulations (which could also be termed the prevalence of these populations). The estimate of the absolute size of a subpopulation is also a function of the

relative size of a subpopulation, which is multiplied by the absolute size of the population. Survey estimates of the size of the American Jewish population, for instance, are the product of the (weighted) proportion of Jews in the survey and the size of the American population. (Characteristically, the size of the survey universe—the population of the United States—is calculated from external estimates, in this case from the Bureau of the Census.) Likewise, relative size is the source of averages. Mean age is the sum of the ages of every person in the population, divided by the total size of the sample.

The expected characteristics of individual cases are also derived from relative sizes. Instead of predicting the final vote, the *Literary Digest* could have expressed the same information in terms of elements of the population: the probability of an individual American to vote one way or the other (.55 for Landon, .41 for Roosevelt, .04 for Lemke of the Union Party of Father Charles Coughlin, radio priest and supporter of Fascism, and Dr. Francis Townsend, advocate of old age pensions). Finally, analytic statistics, such as regression coefficients, ultimately yield expected probabilities or values for a case under a specific set of conditions.

## SURVEY METHODOLOGY

Having examined the purpose of a survey, the next step is to look at the theoretical underpinnings of survey methodology. Naturally, the central question is what separates a successful survey from an unsuccessful one. The answer is accuracy. A perfect survey would give the same answer as a complete enumeration of the characteristics of interest for every member of the target population. For the purposes of this discussion, I will set

aside issues related to measurement to focus on sampling accuracy.[2] Errors related to sampling fall into three broad categories: coverage error, sampling error, and nonresponse error. Coverage error occurs when the sampling frame does not cover all elements of the target population ("undercoverage"), as was the case with the *Literary Digest* poll, and/or covers elements that are not part of the target population ("ineligible units") (Groves et al. 2004a), and will be addressed later in the chapter. Nonresponse error occurs when the sample differs systematically from the sampling universe; that is, where the selected units that participated in the survey differ from selected units that did not participate. Bias from nonresponse will be addressed in later chapters. First, I will address sampling error.[3]

*Sampling Error*

The theory of sampling error is central to understanding sampling frames and designing effective sampling schemes (see pp. 28-31). The best survey, after all, is the most accurate, and sampling error is, as I will discuss next, a predictable source of error. The design of sampling scheme can have dramatic impacts on sampling error, which will be examined by looking first at the design of the Boston Jewish Community Study and then developing an optimal design.

*Cost*

A survey's *raison d'être* may be accuracy, but its *élan vital* is its budget. Much as economics may be the process of satisfying unlimited wants with limited means, survey methodology can be defined as the science of providing maximal accuracy with limited

means. Surveying Jews, tautologically, involves finding Jews to survey. As Jews in the United States constitute but a fraction of the population, the odds that a given household will contain a Jew are low (Schulman 2003). Without knowing advance whether a household contains a Jew, it is necessary to contact all households in order to find a Jewish respondent. As Jews (or any other rare population) become scarcer, the number of households that must be screened rises exponentially (see Figure 3:5).

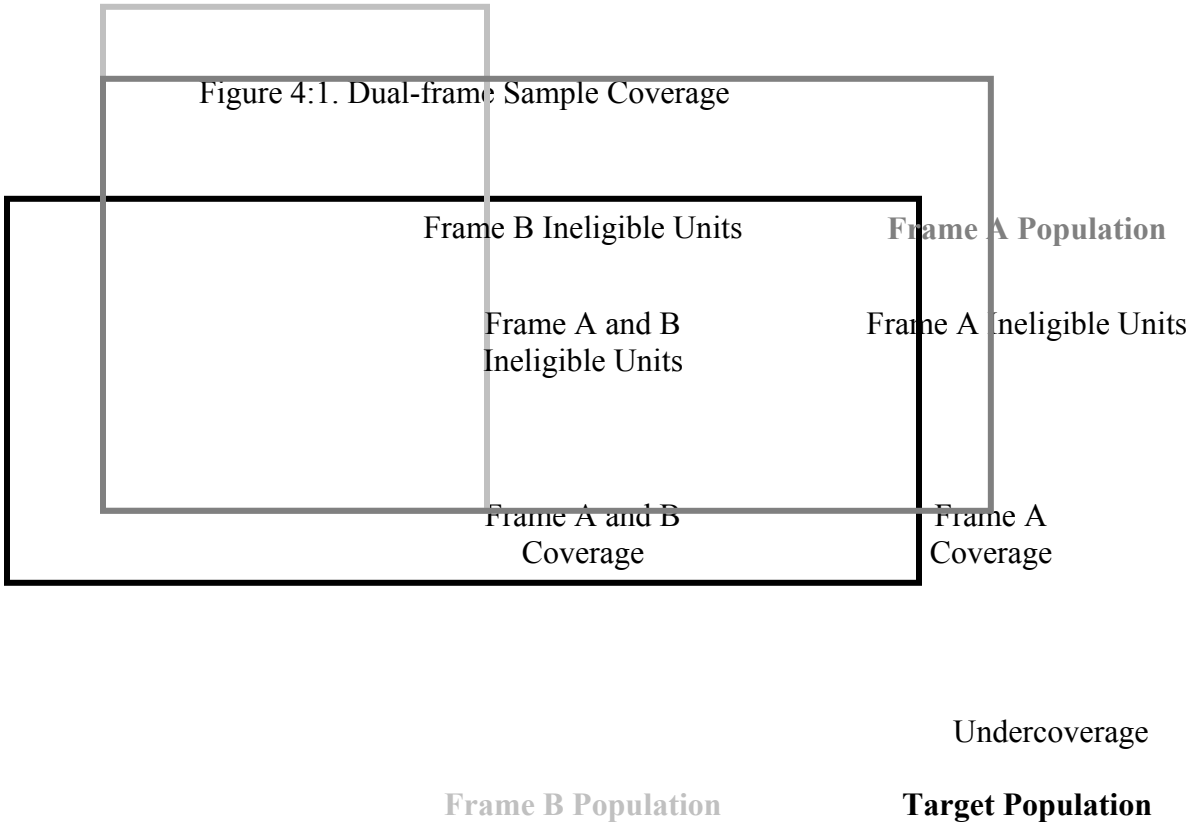
*Minimizing Cost and Sampling Error*

These problems are not, of course, unique to the Jewish population or particularly recent. A characteristic technique is to use a list containing a high proportion of units from the rare population as a supplement to a frame that covers the entirety of the sampling universe, which were covered in Chapter 2.[4] (See Figure 4:1 for a diagrammatic representation of a dual-frame sample.) The first use of such a dual-frame sample, according to Hartley (1962), was a 1949 Bureau of the Census survey of retail stores described by Bershad (1953), dating practically to the beginning of scientific sampling in the United States (Converse 1987). Another, entirely compatible, way of increasing efficiency is to divide the population to be studied into exhaustive and mutually exclusive strata (typically, but by no means always, geographic) and select the sample in the most efficient manner for a given variable (Kish 1988; Neyman 1934). Having discussed these central pillars, I will now examine the sampling scheme used for the Boston Jewish Community Study.

## SAMPLING SCHEME

The objective of the 2005 Boston Jewish Community Study was to obtain a representative sample of households in the Greater Boston area with sufficient cases to allow for analysis of subpopulations of interest within the study budget.[5] (The astute reader will note that no mention is made of Jewish households for the simple reason that, as no perfect list of the Jewish population is available, all households must be included in order to screen for Jews.) A simple random sample using random digit dialing alone would have been prohibitively expensive, as we have seen, with approximately 90 percent of interviews would have been with ineligible households (see discussion p. 262).[6] Alternately, a sample that drew purely from lists of Jewish organizations would have been far less expensive given the high proportion of Jewish households on organizational lists, but would likely exclude large sectors of the Jewish community, particularly the less institutionally engaged and, most problematically, would have been unable to determine the extent to which the listed Jewish community differed from the unlisted. Accordingly, a combination of these approaches was thought to be most desirable (see discussion, p. 262). Figure 4:1, below, illustrates a dual-frame design (frame sizes are not drawn to scale).

Figure 4:1. Dual-frame Sample Coverage




*Note:* Adapted from Groves *et al.* (2004a)

The target population is represented by the black rectangle, which is partially covered by the grey rectangles representing the two sampling frames. The universal sampling frame, Frame A (dark grey), only covers a portion of the target population (i.e. undercoverage), while including some ineligible cases (those outside of the black rectangle). Frame B (light grey), the smaller sampling frame, is a subset of frame A (i.e. all eligible cases found in Frame B are also found in Frame A, while the converse is not true). Naturally, some ineligible cases are found on both Frames A and B, although Frame B has ineligible cases unique to itself. Because Frame B contains a high concentration of cases from the rare population, screening costs are minimized, while Frame A ensures that cases not found in Frame B are still eligible for inclusion, avoiding noncoverage error (except for cases in the target population found on neither frame).

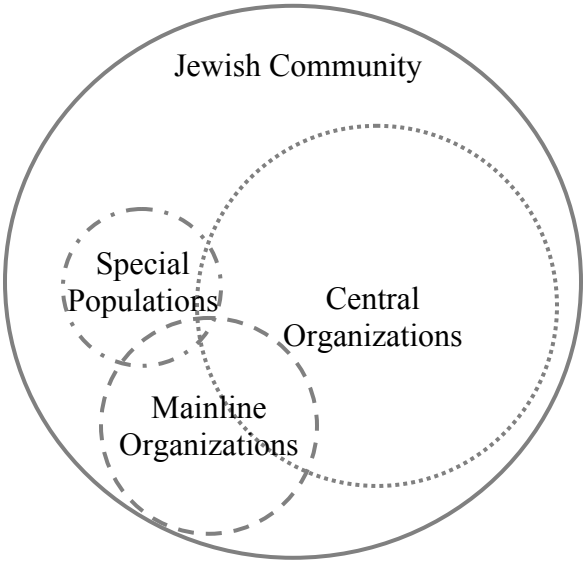
In discussing stratification and dual-frame samples, I have implicitly touched upon the fact that households selected from different strata and/or frames will have different probabilities of selection. Without correction, the households most likely to be selected would clearly be overrepresented in the sample, together with any characteristics that differentiate them from other households, leading to biased data.

Attention will now turn to the specifics of each sampling frame: lists from Jewish organizations for the incomplete coverage, high incidence frame (Frame B) and random digit dialing for the complete coverage, low incidence frame (Frame A), as well multiplicity sampling, another approach altogether.

*List Frame*

As was previously discussed in Chapter 2, lists of Jews are perhaps the oldest method for understanding the size and characteristics of Jewish populations, with the Biblical description of a tribal census in Num. 1:2-3 being one of the earliest explanations anywhere of a list-based method for understanding any population.[7] Most studies of American Jewish communities use a list from a central Jewish organization (typically a Federation) or several at most when a survey covered the territory of more than one Federation.[8] In view of the difficulty and cost involved in implementing a random digit dialing frame, Combined Jewish Philanthropies and the Cohen Center for Modern Jewish Studies agreed to construct as large a list frame as possible, assuming that increased coverage would reduce the likelihood of bias and significantly decrease survey costs. The assumptions underpinning the design are shown in Figure 4:2, below.

Figure 4:2. Conceptual Diagram of Multiple List Frame Approach




It was assumed that a significantly greater proportion of the Jewish community was on some organizational list than on the Federation list alone, mainline organizations like synagogues and cultural groups would have significant but far from total overlap with members of large, central organizations, and that Jewish organizations catering to special populations would have relatively limited overlap with either mainline or central organizations, further increasing the coverage of an expanded list frame.

*List development.* While no formal protocol was developed to guide the collection of lists, the way in which organizations were selected to be approached to contribute their lists was consistent with Figure 4:2. The initial list was from Combined Jewish Philanthropies, which included both former and current donors. The central cluster of lists was constituted on lists from local and national organizations with many members in the Boston area.[9] The mainline cluster was drawn from synagogues and organizations thought to

have relatively heterogeneous communities. Subsequently, a general call to provide lists was sent via the Synagogue Council of Massachusetts. In late 2004, due to concern regarding the relatively few synagogue lists received to that point, special efforts were taken to obtain lists of members of Conservative and Reform congregations. The special populations cluster was defined as groups believed to be particularly difficult to sample and/or underrepresented on the Combined Jewish Philanthropies list as priorities for list-gathering: elderly, poor, young adults, ultra-Orthodox, gays and lesbians, Russians, Israelis, intermarried families, less-affiliated, and residents of western suburbs. Naturally, some organizations approached to provide lists did not do so. Altogether, 85 organizational lists were collected.

The process of list collection was managed by Mark Niedergang, a consultant to Combined Jewish Philanthropies. Typically, a senior professional at the organization was called and asked to make the organization's database of members and/or contacts available for the study. In addition to the verbal description provided over the telephone, a description of the study was emailed, including the benefits of participation to the organization and the Jewish community and details on the type of information required. To allay common concerns, letters from Combined Jewish Philanthropies and the Cohen Center for Modern Jewish Studies containing assurances that the list would be used for solely for the Boston Jewish Community Study and not for any future surveys or fundraising efforts were appended to the email (see Appendix F).

During this period, Combined Jewish Philanthropies staff, members of the community study committee, and the director of the Cohen Center briefed a number of meetings of Jewish professionals and lay leaders about the study's method and purpose, in

part with the aim of increasing levels of cooperation. In many cases, obtaining the list required multiple contacts before permission was given (which often required approval from the organization's board) and repeated follow-ups afterward in order to obtain the list. Some lists were never sent to the Cohen Center in spite of months of calls from Cohen Center and Combined Jewish Philanthropies staff.

A major consideration in the development of the final list sample was that the data contain no duplicate listings for a given household. Otherwise, certain households (most likely those on more than one list), would have had higher probabilities of selection, biasing the sample. The lists were standardized, entered into a database, deduplicated, sent for address standardization and updating, and deduplicated again until no certain duplicates remained (Table 4:1). (A full description can be found in Appendix G.)

Table 4:1. Number of Households on List Frame at Various Stages of Development

| Stage | Number of households |
|---|---|
| Before deduplication | 327,548 |
| First round of deduplication | 143,972 |
| Record updating and second round of deduplication | 129,741 |
| Removal of cases with bad information | 85,939 |
| Third round of deduplication | 76,093 |
| Estimated number of households in study area on list frame | 52,517 |
| Estimated number of households on list frame containing an adult who was currently Jewish, raised as a Jew, or had a Jewish parent | 37,362 |
| Estimated number of households on list frame containing a currently Jewish adult | 36,489 |

*Estimates of population characteristics.* To determine whether the data matched *a priori* understandings of the distribution of the Boston Jewish community, Jewish incidence (i.e. the proportion of households that were Jewish in a given area) was calculated

as the proportion of listed households in each city and town of the study area divided by Census 2000 estimates of occupied household units:

$$density = \frac{N_{list}}{N_{census}} \quad \textbf{(4.1)}$$

These findings generally (see Appendix H) matched the results of the 1995 study, appearing to slightly overestimate incidence, which is expected given that these estimates were calculated prior to the second and third rounds of deduplication. The main belt of Jewish population was a chain of contiguous communities declining in density as one moved westward along I-90 from the centers of Newton and Brookline. Except for communities with lower socioeconomic status, like Waltham and Watertown, the Jewish population spreads outwards from this band. Another line of communities follows Rt-2a northwards from Cambridge. The final area of population concentration lies between Sharon and I-128. Figure 4:3, below, shows the estimated incidence of Jewish households in the study area.

Figure 4:3. Estimated Proportion of Jewish Households by Town

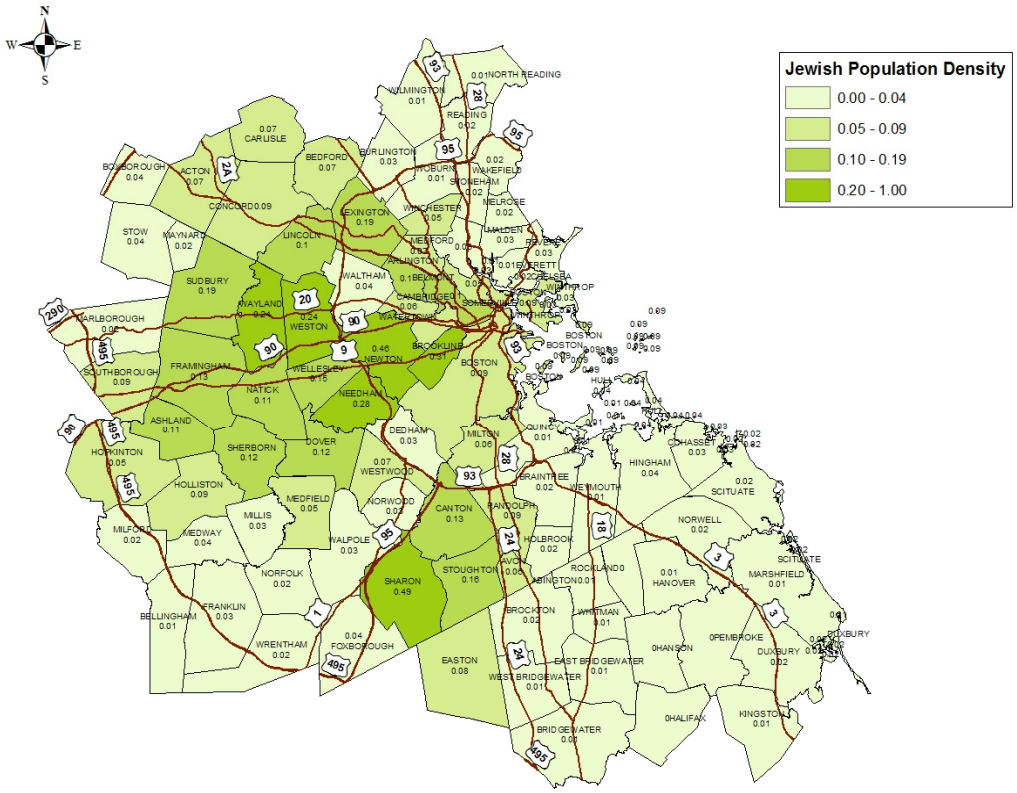
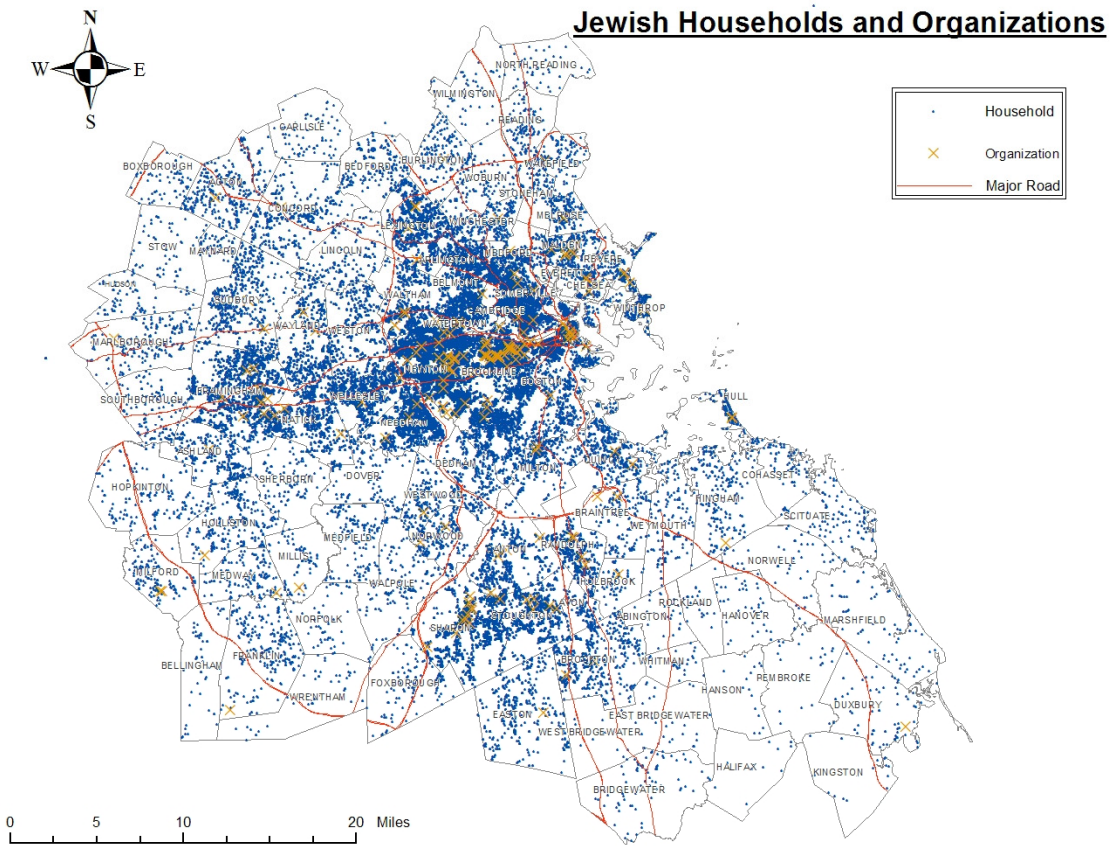



Figure 4:4 shows the location of each household on the final list database together with the locations of Jewish organizations, synagogues, and retailers (marked by X's).

Figure 4:4. Location of List Households




*List coverage.* As one of the aims of the list sample was to expand the proportion of the Jewish community covered by the list frame, it is important to look at the proportion of the population covered by the list frame, as well as the proportion of the list frame that was eligible, which are displayed in Table 4:2, below.

Table 4:2. List Frame Coverage

| | CJP only | | All lists | |
|---|---|---|---|---|
| | Broad Population[a] | Narrow Population[b] | Broad Population[a] | Narrow Population[b] |
| Percentage of all eligible households[c] | 20% | 22% | 32% | 37% |
| Percentage of households eligible for full survey[d] | 87% | 85% | 71% | 70% |

*Notes:*

[a] Includes all households containing an adult who is currently Jewish, was raised as a Jew, or has a Jewish parent.

[b] Includes all households containing an adult who is currently Jewish.

[c] Proportion of all Jewish households (under either the broad or narrow definition) covered by the list frame (CJP only or all 85 lists).

[d] Proportion of cases on the list frame (CJP only or all 85 lists) that are eligible under either the broad or narrow definition).

The traditional list frame, using only cases from the Combined Jewish Philanthropies list, covered an estimated 22 percent of households containing current Jewish adults. The expanded list frame, using all 85 lists, increased coverage considerably to 37 percent of the population. This was far lower than presurvey estimates, based on the final size of the list frame: 76,093 households (see Table 4:1). Had all these households existed, been within the study area, and been Jewish, they would have constituted 65 percent of all Jewish households. This was not the case, as there were only estimated to be 52,517 list households, including those that did not contain any adult raised as a Jew, was currently Jewish, or had a Jewish parent, for a 31 percent rate of ineligibility. The reason for the discrepancy was that many of these households had bad contact information despite the

effort to update telephone and address data through the U.S. Postal Service Delivery Sequence File, remove cases with a high probability of bad information, and lookup and verify telephone numbers (see Appendix G for details).

Other households were out of the study area despite our efforts to remove ineligible cases. Street addresses were not always accurate and telephone numbers could not be used as a basis for removing ineligible cases due to telephone number portability, which has removed the direct connection that previously existed between geography and telephone area and exchange codes.

A small number were ineligible for other reasons, including businesses (again, despite efforts to remove ineligible cases), a fax or modem line, reported having no adult household member, or various other reasons.

The other reason for the low level of coverage compared was that the list frame contained an unexpectedly high proportion of non-Jewish households: 29 percent of list households were estimated to have no adult who was currently Jewish, raised as a Jew, or had a Jewish parent. The proportion of non-Jewish households for a Combined Jewish Philanthropies only frame was much lower, with only 13 percent ineligible. The discrepancy is a product of the differing bases of organizational lists discussed previously (see note 9 of this chapter). Certain organizations contributed a disproportionate number of non-Jewish cases. The list of a representative organization, for instance, appeared to combine donors (presumably mostly Jewish) and a broad-reaching mailing list that included many influential figures. All commonwealth legislators were included, for instance. While they were removed, it is likely that many similar cases were not detected.) Another was a cultural organization that drew on people, Jewish or not, interested its

focus. Yet another was a social service organization with a diverse clientele. The coverage of the list frame will be discussed in greater depth subsequently.

*List sample.* Once the list frame had been developed, a random sample of numbers were drawn from each sampling stratum at a disproportionate ratio of 2:3:4:1 in keeping with the suggestion by Christopher Winship, co-chair of the community study committee and a mathematical sociologist, in order to increase the number of cases available in strata with low random digit dialing sampling ratios.

*Random Digit Dialing Frame*

The confluence of relatively low incidence and increased cost per interview has dramatically changed the economics of Jewish survey research. Where it was possible as late as 1995 to use an economically inefficient sampling scheme that oversampled low incidence areas, yielding better information about these populations, most major Jewish population studies conducted after 1998 have oversampled high incidence areas.[10] The plan adopted for the Boston study was an outgrowth of these designs, taking advantage of the high level of coverage provided by the list frame to exclude very low incidence areas from the random digit dialing scheme, sampling high, medium, and low incidence strata at a 4:2:1 ratio.

*Stratification.* In order to determine which households would be assigned to which strata, we used the estimates of the proportion of Jewish households in towns and cities from the list database (see Appendix H). Ideally, these data would be have been cross-referenced with an external source. Unfortunately, no city by city estimates exist.

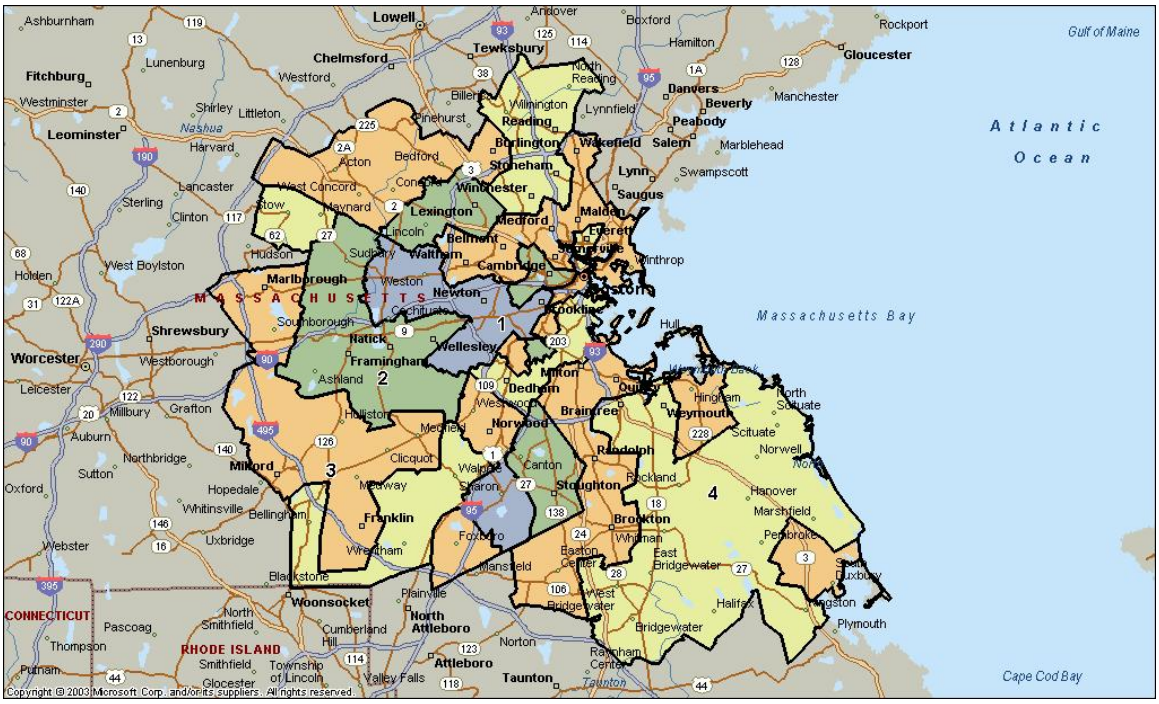
The 1995 data had very small cell sizes for most communities and correspondingly wide confidence intervals, and therefore could not be used for this purpose.

Communities were assigned to strata using the following criteria:

| | |
|---|---|
| High density | 20% and higher |
| Medium density | 10% to 19% |
| Low density | 5% to 9% or less than 5% and adjacent to a high incidence area or contains a synagogue |
| Unsampled | Residual |

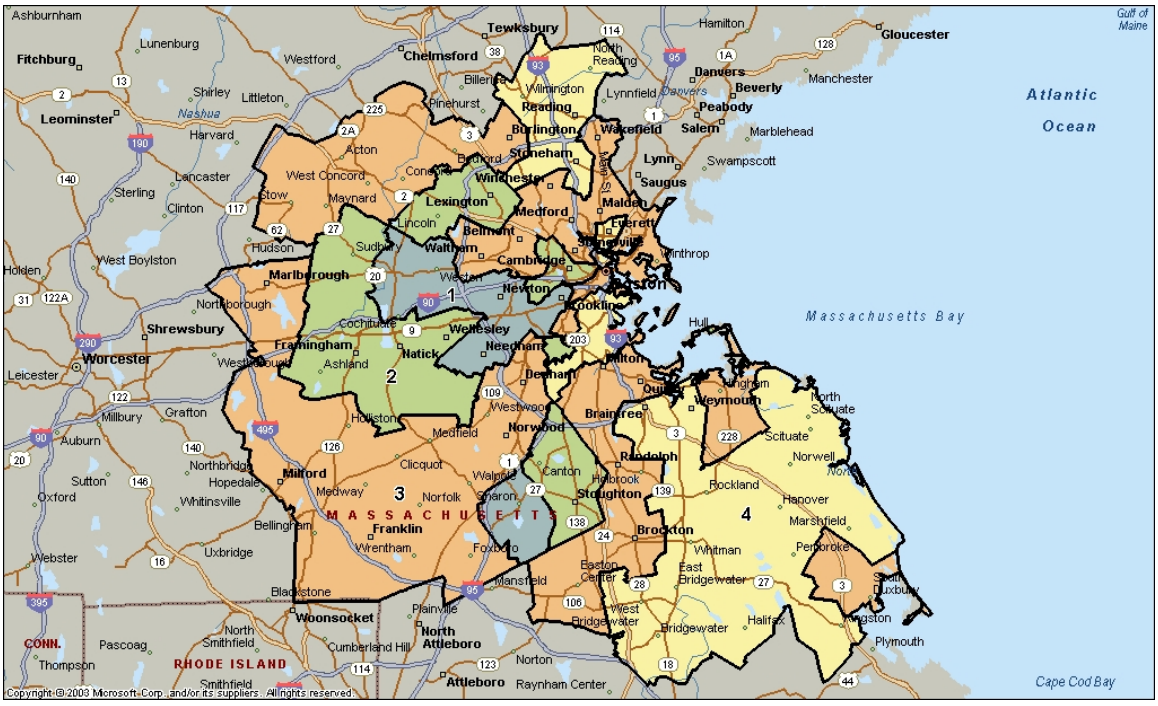
The additional criteria for communities estimated to have less than 5 percent incidence (below which survey costs increase dramatically) were included as a safeguard against underestimates of relatively new communities, such as those on I-495 south of I-90. Synagogue listings used the Massachusetts Council of Synagogues directory. The original strata are presented in Figure 4:5, below, provided by Schulman, Ronca & Bucuvalas, Inc. (See Appendix H for a listing of towns and cities by strata.)

Figure 4:5. Pilot Test Random Digit Dialing Strata




Following the pilot study, the random digit dialing sampling scheme for the survey was revised in consultation with CJP to add unsampled areas to the low density stratum, specifically all towns in the study area west of Sharon, due to the likelihood of underestimates of Jewish population density in areas of rapid Jewish population growth (see Figure 4:6).

Figure 4:6. Final Random Digit Dialing Strata




The random digit dialing design was implemented by selecting all telephone exchanges (XXX-NNN-XXXX) with estimated incidence derived from directory listings of greater than one percent in any sampled strata. Within selected exchanges, phone banks (XXX-XXX-NXXX) with incidence in multiple strata it were assigned to the stratum with the plurality of listings. Within sampled phone banks, hundred blocks (XXX-XXX-XXNN) with at least three listed residential numbers were selected (see discussion of list-assisted methods on pp. 104-105). The advantage of a list-assisted methodology, which uses directory listings as a guide to the distribution of the universe of telephone numbers, is that fewer calls are made to nonworking numbers, reducing overall costs with little bias. In retrospect, this erred on the side of efficiency rather than coverage. A one plus

listed design would have reduced efficiency but minimized problems associated with differences between listed and unlisted households.

*Random digit dialing sample.* Once the random digit dialing frame was developed, telephone numbers were randomly selected within hundred banks within strata in keeping with the sampling ratio (e.g., a telephone number in a phone bank in Stratum 1 were four times as likely to be selected as a number in a phone bank in Stratum 3). As sampling within hundred banks is random, both listed and unlisted telephone numbers were eligible for selection, except of course unlisted numbers from hundred blocks with zero to two listed numbers. The resulting sample of telephone numbers drawn from the frame was then scrubbed of known business and mobile phone listings. Finally, all telephone numbers found on the list frame were removed from the random digit dialing frame, although their identifying information was retained for calculating probabilities of selection.

*Multiplicity Sample*

Given the high cost associated with sampling Jewish households, it was decided that the Boston Jewish Community Study would investigate alternative approaches to sampling that would find Jewish households more efficiently. A particularly promising class of techniques uses the social network of the respondent as the basis for sampling.[11] This takes advantage of the fact that members of a rare or hidden population are more likely to know other members of a rare or hidden population than a respondent chosen at random from the population. A Jew is likely to know more Jews than a non-Jew. If it were possi-

ble to use Jews to find other Jews while maintaining a clearly defined sampling frame, the savings in time and money could be enormous.

The forebear of social network methods is the snowball sample (Coleman 1958; Goodman 1961), designed to collect information about social networks rather than as a basis for population samples. In its original form, the snowball asks respondents for a list of people ("ties" or "links") with a specified characteristic, which is often ambiguously defined. The people listed are then contacted and themselves asked for list of ties in a chain-referral design, the size of the sample snowballing with each additional wave of referrals, until the point of saturation is reached when no new cases are added. The ambiguous definition of a tie results in an undefined sampling universe, making generalization and statistical inference impossible. More problematically still, snowball samples will oversample individuals with many ties, creating bias unless corrected for.

*Link-tracing samples.* The snowball sample has, however, been adapted for probability sampling. Link-tracing designs mirror snowball sampling by drawing an initial ("seed") sample, often of the rare population, asking respondents for links with people sharing the characteristic of interest.[12] Estimating the properties of a population from a link-tracing design is, however, still in its relative infancy (Thompson and Collins 2002). The most sophisticated of these uses the sample to discern characteristics of the social network, which in turn are used to estimate the characteristics of the population (Salganik and Heckathorn 2004). This procedure, however, requires three assumptions that are difficult to satisfy. First, the network must be dense enough that every member of the population of interest can be connected to every other member. Second, the nature of the tie between the respondent ("ego") and her alters must be clear (and concise) enough that

the person can accurately estimate the number of ties ("in-degree"). This presents serious constraints when a fuzzy concept such as friendship is used. Third, the Salganik and Heckathorn procedures requires that respondent-driven sampling be used, where members of the rare population do the recruiting by putting putative respondents in touch with researchers (Heckathorn 1997), with the additional provisos that ego selects randomly from her alters and that only one referral be made.

The problems of using the Salganik and Heckathorn procedures for studies of Jewish populations are clear. American Jews are becoming less densely connected, as the increased rate of intermarriage has shown. Jewish identity is unclear, with many potential answers to the "who is a Jew" question. Further, highly-connected individuals will find it practically impossible to estimate their in-degree, the number of Jews they know. Indeed, Salganik recommended following alternate approaches because of the estimation problem in an email to Charles Kadushin on the utility of respondent-driven sampling for the Boston study.

Initial designs for the Boston Jewish Community Study called for a unique adaptation of network sampling. Leonard Saxe conceived of a sample drawn from attendees at Passover Seders (as it is the common form among American Jews, I use the English plural rather than the Hebrew) attended by respondents to the random digit dialing and/or list seed, drawing on the fact that the Seder is the most observed Jewish ritual (Sklare and Greenblum 1979) and is a product of the social network of its hosts. Unlike asking Jewish friends or acquaintances, the guest list of a Seder is not subject to vagaries of interpretation. In theory, those who attended Seders would be asked for the name and contact information of their host, while hosts would be asked for the name and contact information

of a randomly selected dinner guest. As the survey was scheduled to be fielded soon after Passover, it was thought that respondents would be able to remember the details. The fact that attendance at a Seder is not universal would not have been problematic as the sample would have been obtained together with a random digit dialing or a dual-frame sample that would "catch" people who did not attend a Seder, or attended one outside the study area. The notion, however, foundered on how respondents would randomly select a guest. It have been too burdensome (not to mention invasive) for the respondent to list every attendee. Normal within-household selection techniques like the next (or last) birthday method, themselves hardly unproblematic (Gaziano 2005), would be too difficult given the weak ties between many hosts and attendees. Selecting the person seated to the right or left of the respondent, which was mooted as an alternative, would have introduced bias as these tend to be positions of honor. Other alternatives were either nonrandom or overly burdensome.

*Multiplicity samples.* While link-tracing designs proved problematic, another sampling technique has been used in similar cases. Multiplicity samples were initially developed for a study of rare diseases where the same patient appeared in more than one hospital (Birnbaum and Sirken 1965). To avoid bias, some way had to be found to account for the greater probability of selection for such patients. These techniques were quickly expanded to allow for population estimates based on reports by the respondent on people linked to them (Sirken 1970; 1972a; 1972b; Sirken and Levy 1974; Sudman, Sirken, and Cowan 1988). While resembling link-tracing designs, multiplicity samples are distinguished by the unambiguous nature of the enumerated relationships, typically one or more categories of first-order relatives (parents, siblings and children). The clear na-

ture of ties allows for estimates of population size and characteristics with far fewer assumptions and greater certainty than link-tracing samples. The utility of multiplicity samples for studying Jewish populations, where family is so closely tied to identity, is self-evident, and indeed were first suggested for Jewish communities more than 30 years ago, but not implemented (Sirken and Goldstein 1973).

Given the potential of multiplicity samples to efficiently identify Jewish households without introducing bias, it was decided that a multiplicity sample be included in the Boston Jewish Community Study.[13] A series of questions in the main instrument determined whether the respondent had any living first order relatives aged 18 or older living in the Greater Boston area (see Appendix J). A multiplicity household was randomly selected from among adult first-order relatives in the Greater Boston area by the computer-assisted telephone interviewing software and the respondent was asked to provide contact information at the end of the interview, offering an honorarium of $18 to the selected relative on behalf of the respondent. Cases where contact information was provided were added to the following replicate. As such cases were also eligible for selection on the list and random digit dialing frames, the multiplicity element of the survey constituted a separate, nonexclusive sample (Casady and Sirken 1980; Sirken 1979). As is described subsequently, however, too few respondents were willing to provide contact information for their relatives and the experiment was terminated.

*Distinctive Jewish Names Frame*

The Boston Jewish Community Study did not include a distinctive Jewish names frame (see pp. 74-78 and pp. 118-128). Nevertheless, I will describe the construction of a dis-

tinctive Jewish names frame here as it is a commonly used approach to sampling Jewish populations, it was discussed with respect to the Boston study design, and because the results of a distinctive Jewish names frame will be simulated later this chapter.

In order to test the effectiveness of a distinctive Jewish names frame, I simulated two variations using all households with surname information. This included the list frame as well as random digit dialing records that could be reverse address matched. Including the list records introduces some degree of error, as it is likely that some list households have unlisted numbers and would not be part of an independent distinctive Jewish names frame. As I do not have information on which list households were listed in the telephone directory, the degree of error is unknown. It is reasonable to assume, however, that the simulations mildly overestimate the level of coverage and the level of Jewish attachment and behavior. It is unlikely, though, that this error will significantly affect estimates of efficiency. The first realization of a distinctive Jewish names frame uses Sheskin's (2003a) list of Jewish surnames, which is very similar to Kohs's (American Jewish Historical Society 1946); both are listed in Appendix C. The second realization uses ethnic classifications from AccuData, a data provider, which appear to include first and middle names in addition to surnames in the classification process.[14] The AccuData frame is of interest because of its different origin and purposes. Whereas the traditional distinctive Jewish names frame focuses on selecting surnames with a high incidence of Jews, the ethnic name frame must classify every name into an ethnic category. It includes a category for Israeli names, which are absent from traditional distinctive Jewish names frames. The methodology used to construct the frame is proprietary but appears to use a complex analytic routine that analyzes elements within names. Surnames that do not

appear to be Jewish, such as Beneway and Benjoya, are classified as Israeli, likely due to the presence of "Ben" at the beginning of the name. First names also appear to be considered by the algorithm underlying the ethnic name frame. This translates into higher levels of coverage but lower Jewish incidence (see also Table 4:7).[15]

## ELIGIBILITY

As I previously discussed, national and local studies of Jewish populations diverge on how Jewish households should be screened for, including the wording used to introduce the survey and the questions used to determine the Jewish status of the household. In the absence of unambiguous information on the effectiveness of different wording for the introduction and screening questions, a number of variations were tested. In each case, response rate was used as the criterion for evaluation, which measures its effectiveness among both Jews and non-Jews. Ideally, response rates for Jewish and non-Jewish households would have been examined, but as there was no valid, preexisting source of data about the ethno-religious status of the household, it was only possible to compare the overall response rate of each alternative. A significant, albeit unavoidable, drawback to this approach was that the probability of response of non-Jewish households was the main determinant of the response rate, given their unquestioned numerical superiority.

The pilot survey tested two alternative introductions with random digit dialing cases. The first followed the pattern used in local studies, directly identifying the sponsor and subject matter:

> Hello, my name is _____. I am calling from SRBI [Schulman, Ronca & Bucuvalas, Inc.]. We are NOT asking for money. We are NOT selling anything. We are doing a study of Greater Boston on behalf of Combined Jewish Philanthropies of Boston, as we do every 10 years.

In addition, the first substantive questions asked about the Jewish status of the informant and other adults in the household (currently Jewish, raised Jewish, or has a Jewish parent). The alternative followed the model used in NJPS, which did not reveal the Jewish sponsorship and purpose of the study:

> Hello, my name is _____. I am calling from SRBI, a national survey research organization. You may have seen our polls in *Time* magazine. We are doing a study of the cultural heritage of families in the Greater Boston area. All your answers are completely confidential and will not be identified with you in any way. This survey will take approximately 20 minutes. Participating in this survey is entirely voluntary and you may ask us to stop at any time.

In contrast to the direct approach, the first substantive questions asked about the religious status of the informant and other household adults. Jewish status was only asked at the end of the screener interview. The more direct approach proved more successful, achieving a response rate of 13.1 percent compared to 9.0 percent for the indirect introduction ($\alpha = .05$, $\beta = .16$, two tailed). Based on these results, it was decided that all introductions would directly identify the survey's sponsorship and purpose.

The operational definition used for what constitutes a Jewish household has a major impact on results. In keeping with other contemporary Jewish community studies, the Boston Jewish Community Study used a "sociological" approach that asked for people's subjective identity rather than religious affiliation, a necessary step given that Jewish

identity is not an entirely religious construct. The other major choice faced by studies of Jewish populations is whether to survey households containing people who were raised as Jews or had a Jewish parent. Whether to do so or not is a question of whether it is more important for the study objectives to maximize the number of currently Jewish households or be able to analyze households with more peripheral ties to Jewish identity. In the case of the Boston study, Combined Jewish Philanthropies preferred the latter alternative. Specifically, any household that answered yes to one or more of the following questions was included in the sample:[16]

> Does any adult in your household consider themselves to be Jewish?
> Were any adults in your household raised as Jews?
> Does any adult in your household have a Jewish parent?

## FIELD PROCEDURES

In light of the difficult survey research environment the survey faced, it was decided that the Boston Jewish Community Study would be methodologically rigorous, essentially increasing quality at the price of significantly increased costs. The sample size of the study was not affected by this particular trade-off, as it remained at the level initially planned, itself an increase over the 1995 study.

### *Prenotification*

As I described in relation to NJPS 2000-01, it is well established that advance notification increases response rates. Accordingly, all households for which an address was available

received a letter describing the study and asking the recipient to cooperate. As there was no evidence as to whether prenotification letters for Jewish surveys were more effective when the request came from the sponsoring organization or a research contractor, two alternatives were tested. One set of letters was on Combined Jewish Philanthropies letterhead and mentioned the Jewish purpose of the study (and the introduction to the telephone call explicitly mentioned the organization) while another half were on the letterhead of the survey research firm, Schulman, Ronca & Bucuvalas, Inc. (SRBI) and referred to the study's topic as "in a very important research study about religion and public life of people of all faiths and backgrounds in the Boston area" (see Appendix K for the full text). The telephone introduction described the Jewish purpose of the study but did not mention Combined Jewish Philanthropies by name.

At the time the decision was made to identify the study explicitly with Combined Jewish Philanthropies (June 6), Federation cases had a response rate of 15.4 percent, compared to 13.4 percent for SRBI. In hindsight, the experiment was terminated too early ($\alpha = .05$, $\beta = .85$, two-tailed), given the small sample size (n=1,068) and relatively minor difference between response rates. Attempts to retrospectively use the larger Combined Jewish Philanthropies sample to examine the decision are complicated by the longer period the other cases were in the field, which would tend to increase response rate. Restricting the analysis to Combined Jewish Philanthropies cases that were fielded at the same time as SRBI, the response rates equalized over time, with response rates of 47 percent for SRBI cases and 48 percent for Federation cases, while the unweighted proportion of Jewish households was 21 percent for SRBI cases and 22 percent for Combined Jewish Philanthropies cases.

*Incentives*

A small prepaid monetary incentive has been consistently shown to have significantly increase response rates. By contrast, payment in return for participation has generally been found to have little effect on response rates, whether in the form of direct payment, lotteries or donations to charity. However, the cost structure of studying a rare population such as the Jewish community differs from those of more broadly focused efforts, as relatively few cases are eligible for the main instrument, causing a considerable increase in the amount of preincentive payments. It was therefore decided to offer a large postincentive of $18 for respondents who completed the entire instrument to a randomly selected half of cases for which a mailing address was available, while including a $2 preincentive in the other of cases with an address.[17] Among random digit dialing cases for which no address was available, a randomly selected draw of 50 percent of cases was offered an $18 postincentive while other households were not offered an incentive. Multiplicity cases were, as discussed previously, always assigned an $18 incentive. The use of prenotification and a preincentive where address information is available is, to the best of my knowledge, a first for Jewish population studies. Somewhat surprisingly, given the greater efficacy of small preincentives in the literature, the results were practically indistinguishable, with a response rate of 42 percent for households offered a preincentive against 39 percent for those offered a postincentive. The unweighted proportion of Jewish households identified was also remarkably consistent, with 48 percent where a preincentive was offered and 49 percent for postincentives. Among cases for which no address was available, an $18 postincentive was only marginally more effective than no incentive at all, with response rates of 37 percent and 33 percent respectively.

*Callbacks*

As it has become increasingly difficult to contact households, high quality surveys must make multiple phone numbers in order to maximize response rates. Accordingly, it was determined that up to 20 contact attempts were made initially. These continued until the household was screened or refused to complete the interview or hit 20 attempts (in rare cases, more than 20 attempts were made when a respondent asked to complete an interview at a later time). In addition, list households that could not be contacted by telephone (and for which prenotification letters had not been returned) were mailed an invitation to complete a web survey.

*Refusal Conversion*

Refusals to participate in surveys have also become more common. Accordingly, it was determined that households that had refused to participate in the screener interview would be recontacted. Households for which an address was available were mailed a letter promising a payment of $18 if the main interview was completed (see Appendix L). Up to 20 contact attempts were made for refusal conversion (multiple attempts were in order to make contact—if a household refused for a second time, they were not contacted again).

*Russian Households*

Cohen Center staff examined the names associated with households that could not be interviewed due to language difficulties and determined that a high proportion had Russian names. Patterns of residence of Russian names also closely mirrored the perceived

distribution of migrants from the former Soviet Union, being heavily clustered in the quadrangle bordered by Chestnut Hill Avenue, Commonwealth Avenue, Washington Street, and Beacon Street. Accordingly, it was decided to implement a Russian-language survey. Russian name households from the list and random digit dialing frames were sent a prenotification letter based on the English text with a $2 preincentive. Russian-language households were subsequently contacted by a Russian-speaking Cohen Center staff member. The instrument used excluded questions that Cohen Center staff observed had led to midinterview terminations by respondents born in the former Soviet Union, such as what religion the respondent was raised. As the interviewer used a paper instrument rather than the computer assisted telephone interviewing system, it was necessary to reorganize the instrument and, in some cases, simplify skip patterns. Respondents were highly cooperative and were generally delighted to be interviewed, often requiring considerable patience and tact on the part of the interviewer in order to bring the survey to a conclusion.

## RESULTS

As I have argued above, the key feature of a survey is its accuracy, mediated by cost. I will briefly present the results of the survey, providing key indicators of quality by sampling frame.

*Outcome Rates*

While response rates are no longer seen as a sufficient indicator of survey quality, they are a basic requirement for a properly reported survey (American Association for Public Opinion Research 2005). Substantively, response rates are also an appropriate place to begin, as they speak to both survey quality and efficiency. All things being equal, a survey with a high response rate has less potential for bias than a survey with a low response rate, while a low response rate implies more interviewer hours and higher costs. Outcome rates for each sampling frame are shown in Table 4:3 below (see Appendix M for a more detailed presentation).[18]

Table 4:3. Screener Outcome Rates by Sampling Frame

| | Frame | | | |
|---|---|---|---|---|
| Outcome rate | RDD | List | Multiplicity | Total |
| Response rate (RR3) | 29.8% | 41.1% | 68.2% | 34.1% |
| Cooperation rate (COOP1) | 47.4% | 57.9% | 77.3% | 51.6% |
| Refusal rate (REF2) | 21.5% | 20.1% | 20.1% | 21.1% |
| Contact rate (CON2) | 62.8% | 67.0% | 88.0% | 66.0% |

The outcome rates for the list frame and multiplicity sample testify to their much greater efficiency. Calls to list and multiplicity households were much more likely to yield a completed screener (generating a higher response rate) than were calls to random digit dialing households. In some measure, this was due to the fact that an address was available for the overwhelming majority of list and multiplicity households, while many random digit dialing households could not be matched to an address and thus did not receive a prenotification letter. It also reflects the fact that households found on lists are more cooperative. As I will discuss later in this chapter, it is also possible that Jewish

households (which were more common in the list and multiplicity frames) found the survey more salient and responded at a higher rate. List and multiplicity households were also far more cooperative. (The difference between the response rate and the noncontact rate is that the response rate estimates the number of eligible households among telephone numbers with unknown eligibility, while the contact rate is based only on known households.) Refusals were slightly less frequent among list and multiplicity households. The wonderful response to the multiplicity sample was, however, overshadowed by the extremely poor performance of its sampling frame. Only 30 percent or so of respondents with eligible first order relatives provided their contact information—far too few for the sample to be viable—and no new multiplicity cases were added.

*Eligibility*

As we have seen, the proportion of a sample belonging to the subpopulation of interest has a large impact on the economics of the sample, with higher levels of incidence costing considerably less than lower incidence. Table 4:4, below, shows the incidence of Jewish households within frame and stratum.

Table 4:4. Unweighted Percentage of Jewish Households by Frame and Stratum

| Stratum | RDD | List | Total |
|---|---|---|---|
| 1 | 31.3% | 86.9% | 44.4% |
| 2 | 16.2% | 78.5% | 47.5% |
| 3 | 8.6% | 71.2% | 50.9% |
| 4 | - | 61.6% | 61.6% |
| Total | 23.1% | 75.9% | 47.7% |

As expected, lower incidence strata in the random digit dialing frame in fact have lower proportions of Jews, confirming the economically motivated decision to focus the bulk of random digit dialing calling in higher incidence areas (although I explore the optimal stratification later this chapter). Unexpectedly, the same pattern prevailed among list cases, with the higher incidence areas again containing more Jewish households. It is not clear why this should be the case. One possibility is that the non-Jewish households in the list frame were distributed differently to the Jewish cases, conforming to the general geographic dispersion of the Greater Boston area rather than the Jewish community's distinctive pattern of settlement. Given that the majority of non-Jewish households in the list frame were contributed by large organizations that do not focus on a particular set of communities, this is a plausible turn of events. The paucity of Jewish households in low density areas would magnify the effects of such a process.

*Sampling Error*

Due to the complex sample design and the large variation in probability of selection, the level of sampling error was larger than was anticipated. As I discuss in Chapter 5, to prevent cases with large weights having too great an influence on estimates, it was necessary to compress the weights. One side effect of this procedure was to increase bias on variables which correlated with the size of weights. As sampling error is a product both of the sample design and the distribution of the particular variable of interest, the do-it-yourself tables of sampling errors found in some reports are misleading. Accordingly, I present sampling error for two key variables: Jewish population size and the intermarriage rate (80% confidence intervals are presented as they appear in the study report).

Table 4:5. Estimates of Jewish Population Size

| | 80% Confidence Intervals | | 95% Confidence Intervals | |
|---|---|---|---|---|
| | Original Weight | Compressed Weight | Original Weight | Compressed Weight |
| Upper Bound | 210,688 | 201,689 | 220,914 | 207,082 |
| Midpoint | 191,378 | 191,504 | 191,378 | 191,504 |
| Lower Bound | 172,068 | 181,319 | 161,842 | 175,926 |
| Design Effect | 7.3 | 3.5 | 7.3 | 3.5 |

Table 4:6. Estimates of Intermarriage Rate

| | 80% Confidence Intervals | | 95% Confidence Intervals | |
|---|---|---|---|---|
| | Original Weight | Compressed Weight | Original Weight | Compressed Weight |
| Upper Bound | 42% | 37% | 47% | 41% |
| Midpoint | 33% | 29% | 33% | 29% |
| Lower Bound | 25% | 23% | 22% | 20% |
| Design Effect | 24 | 15 | 24 | 15 |

As the reader can appreciate, the confidence intervals and design effects are very large and a more efficient design would clearly have been preferable.

## OPTIMIZATION

Having estimated the sampling error of the survey, we now turn to the central question of the chapter: How could it have been done better? Would we have been better off using only a random digit dialing sample? Did the list frame work? Would a distinctive Jewish names sample have been more effective? Should more interviews have been made in low density areas? While every Jewish community is unique and has special features that will influence sampling, the specific lessons of the Boston Jewish Community Study can be a basis for generalization.

While this exercise is unique for a survey of a Jewish population, cost-variance optimization is an established, though underdeveloped, field in survey methodology. A typical problem faced by survey statisticians is to obtain the most accurate estimates of a given statistic for a fixed cost or to find the least expensive design to obtain estimates of prespecified accuracy. Thus, either cost or variance is fixed and a solution is found for the minimum value of the variable parameter.

There is, however, a major shortcoming in the literature (which I make no pretense of filling): it is only possible to easily optimize for a single statistic. That is, while there are optimum designs for estimating the Jewish intermarriage rate, the size of the Jewish population, or the type of education received by Jewish children, there is presently no method for determining the optimal design for these three statistics jointly. While various authors have proposed procedures for optimization problems (Bethel 1985; Chromy 1989; Chromy 1987; Kish 1974; 1988; Kokan 1963; Kokan and Kahn 1967; Poggio 1983; Valliant and Gentle 1994), Groves et al. (2004a:118) comment that "there have been some serious studies of how to improve sample designs for multipurpose samples…but practical tools have yet to be developed." The same applies for optimizing survey cost. Accordingly, I work within these limitations. In practical terms, the absence of these techniques matters little because Jewish incidence is the only characteristic that is estimable in advance.

Given the limited budgets available for surveys of Jewish populations, including the Boston Jewish Community Study, this analysis will minimize variance within the constraint of fixed costs. These will be set at of the approximate cost of fielding the survey (less expenses borne by the Cohen Center), which was $350,000. In addition, I will

introduce an additional cost not present in the original design, developing the list frame. The cost of developing the list frame is added to provide appropriate estimates of the size of the list frame. Working with multiple lists consumes time and money, which must be paid for by cuts elsewhere in a survey. Failing to account for this will lead to unreasonably optimistic estimates of its effectiveness. I could have increased the budget by an arbitrary amount to "pay" for the additional expenses, but did not. Because the cost of list development is fixed with respect to the variable costs of a survey, this choice will impact simulations of sample size and standard error, but not the calculation of the optimal survey design, which is my main concern. As the discussion of survey costs is vital yet extremely technical, I relegate it to Appendix N.

I add an additional requirement to my simulations, undertaking an unbiased sample of very low density areas. The Boston Jewish Community Study did not include a random digit dialing sample in these areas, and with good reason. The costs per Jewish household would have been excessive and negatively impacted the effectiveness of the sample.[19] The results of simulations show that this decision was reasonable and contributed little bias due to the tiny number of Jewish households in this stratum. Nevertheless, an optimal design should eschew bias, so I have added this requirement. Including very low density areas also serves the purpose of this analysis by allowing examination of a more expansive scenario.

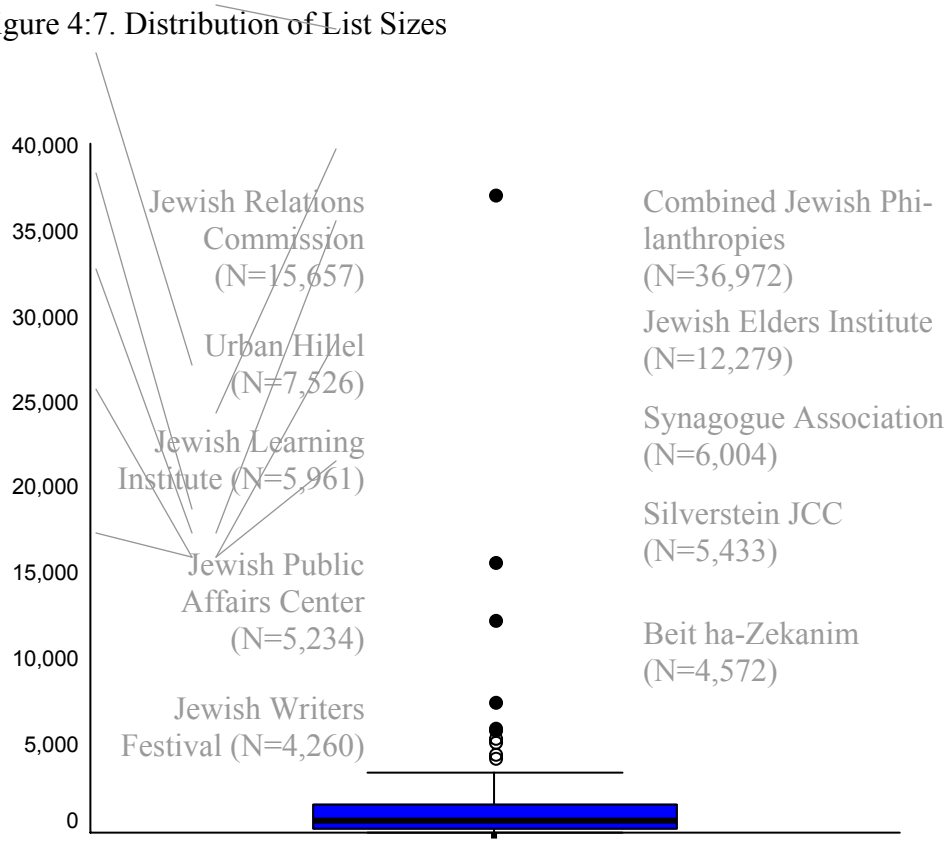
*Optimizing the List Frame*

Previous studies of dual-frame methods have understandably treated the nature of the list frame as fixed, given the number of other variables that play a role in optimization, and

also the prominence of federal and medical surveys which often draw upon a single large list. In many situations, however, more than one list may be used. The National Survey of Veterans, for instance, used two administrative lists (Choudhry et al. 2002). While the National Survey of Veterans, like other surveys drawing upon multiple lists, merged the lists and removed duplicate entries to ensure each list entry had an equal probability of selection, it would have been possible to use only one list. Among populations where no peak body exists to agglomerate records (which I will refer to as "decentralized" populations), many lists with varying degrees of overlap may exist. The Detroit Arab American Study, for instance, used 13 lists from "community, religious, business, educational and social organizations that serve the Detroit Arab- or Chaldean-American populations" from an unspecified number of potential donors (Heeringa and Adams 2004:3). As we have seen in Chapter 2, early Jewish community studies often used scores of lists. Given that many dual-frame studies are able to draw upon a variety of lists, what are the costs of multiple lists and how do they interact with the universal frame?

The situation faced when developing the Boston list sample was likely not atypical for studies of decentralized populations. Organizational lists in the Boston Jewish community, shown in Figure 4:7, follow a power-law distribution or scale-free topology characterized by few large and many small nodes, with node size rapidly decaying (Barabási and Albert 1999; Faloutsos, Faloutsos, and Faloutsos 1999). In organizational terms, this can be thought of as a few large mainstream organizations, followed by smaller ones with increasingly limited constituencies (due to both specific agendas and local foci), tailed by semiformal groups with relative handfuls of members.

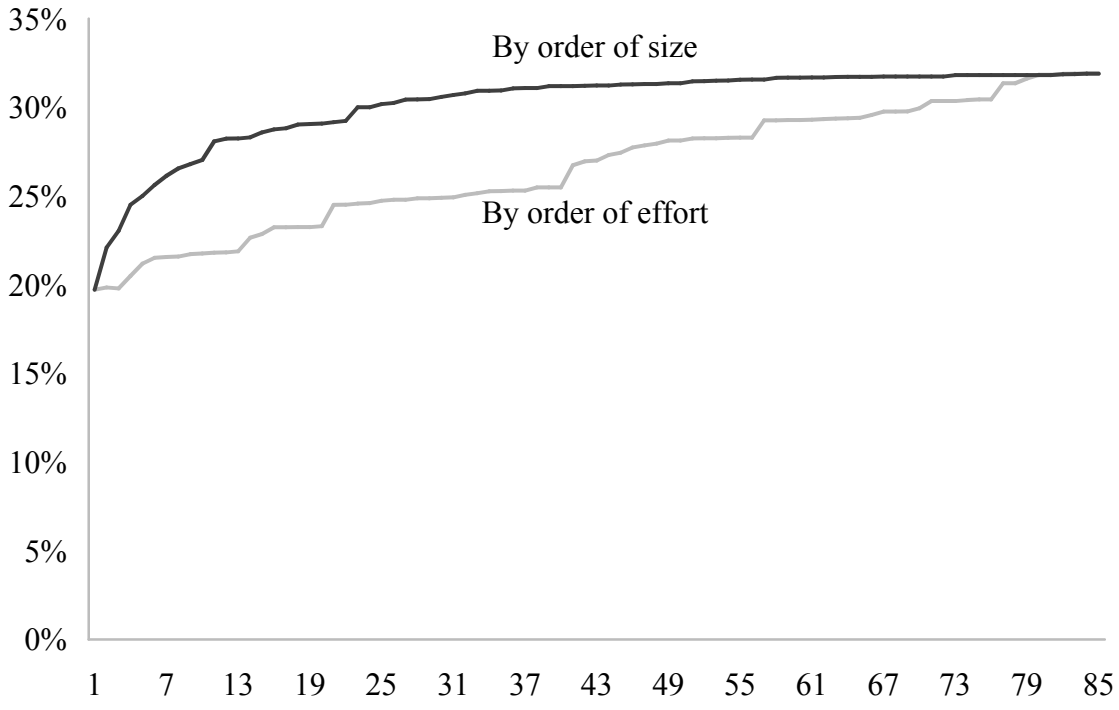
Figure 4:7. Distribution of List Sizes




As I discuss in the following section, the cost of developing a list frame is far from trivial, so an efficient list building strategy is essential. Two systematic strategies can be articulated. One is to concentrate on minimizing the cost of list acquisition (or maximizing the number of lists collected) by concentrating on the most readily available lists. The difficulty of obtaining a given list, however, can only be guessed at in advance. An alternative is to focus on potentially available lists by their size, which proved to be relatively easily available prior to sampling and is usually a desirable attribute.

In order to simulate these approaches, in Figure 4:8 I compare the estimated proportion of the Jewish population covered by a specified number of lists ordered by both size and level of effort required (operationalized as the order in which lists were received).

Figure 4:8. Percentage of Jewish Population in List Frame by Number of Lists Used




A size-based strategy leads to rapid saturation, where additional lists add few if any new cases. If lists are added by the order in which they are received, a different picture prevails. Rather than the smooth asymptotic curve of the size-based strategy, an effort-based strategy appears as a step-like function where individual steps are associated with the addition of key lists. Surprisingly, size appears to be at best weakly associated with individual steps, as smaller organizations like Suburban Hillel ($N$ = 3,501), Yeshiva School ($N$ = 1,562), and Congregation Sim Shalom ($N$ = 477) are notable contributors, with the Yeshiva School and Suburban Hillel even adding appreciable steps in the size-based distribution. Neither, as it happens, was entirely unexpected. The Yeshiva School draws heavily upon recent migrants who might be expected to be weakly attached to mainline organizations, while Suburban Hillel was thought to have a large number of

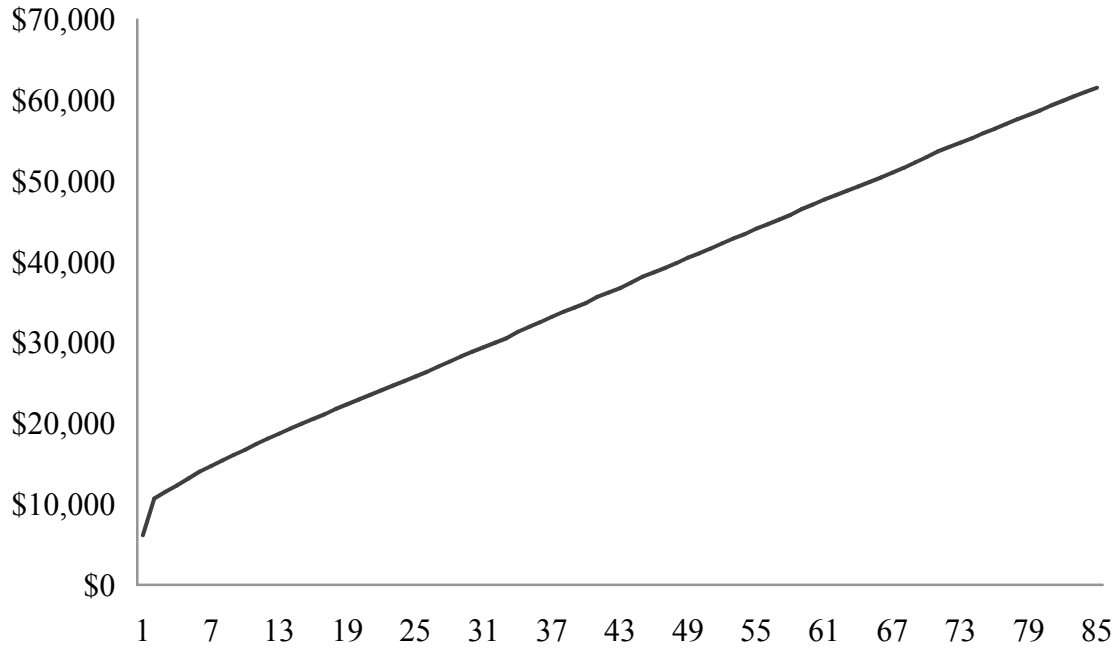
otherwise unaffiliated Jews by virtue of offering high holiday services without synagogue membership. This finding suggests a third approach to selecting lists would be to pick the lists expected to add the most new cases. (This best describes the inductive approach used to develop the list frame.) As attractive as this notion may be, it depends on how successfully one can pick "winners." Despite of decades of experience with the Boston Jewish community and variety of perspectives among researchers and the lay and professional leadership of Combined Jewish Philanthropies, many lists that were thought to have quite distinct populations in fact did not.

While the network of organizational affiliation will vary among different populations, some general conclusions can be derived. While the rapidity of the decay in size of organizational is not a constant, scale-free network topologies appear to be the rule rather than the exception. This suggests that focusing on the largest nodes, rather than following the course of least resistance, adds additional cases faster, even though large lists may have a high degree of overlap in membership. Absent preexisting, predigested data on overlaps in membership between candidates for the list frame, picking a winner does little better in survey research than at the racetrack.

For a list-building strategy to be cost-effective, the rate at which marginal cases are added must exceed the rate at which costs increase. Figure 4:9 shows the costs of developing the list frame by the number of lists used. There is a near linear increase in the cost per percent covered, indicating that the most efficient design would have been to use the single largest list only. The cost of developing a list frame is, however, relatively small compared to the costs of actually surveying. It might be the case, then, that the

greater cost of developing a large list frame is more than offset by the other benefits from a larger list frame, to which we turn to now.

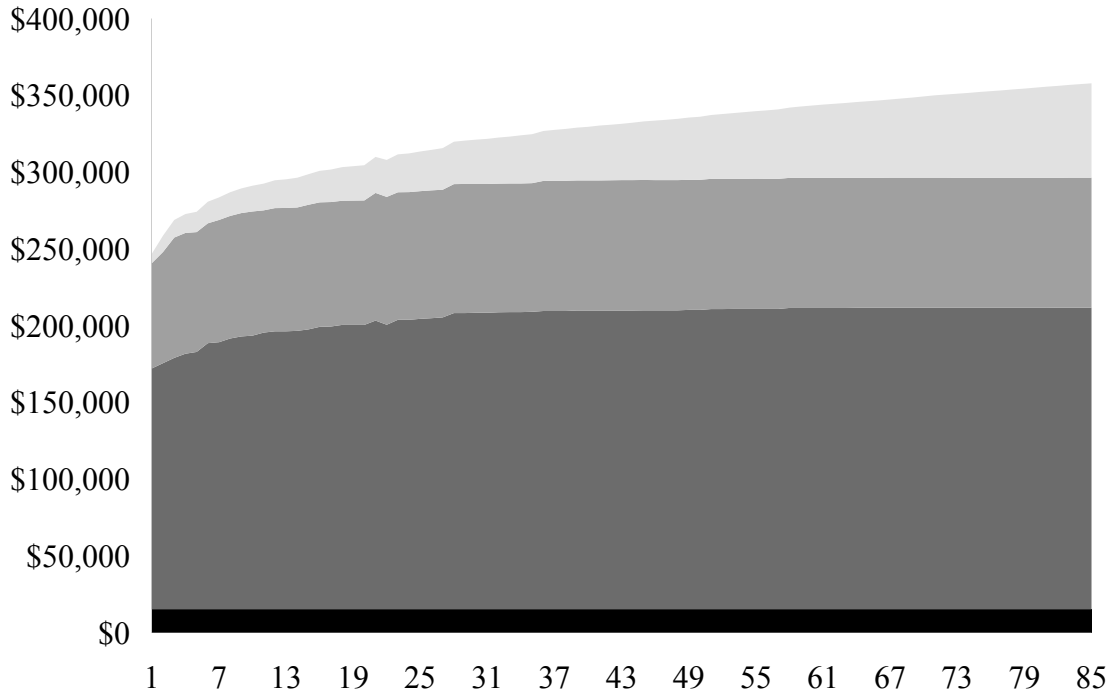
Figure 4:9. Cost of Developing List Frame by Number of Lists Sorted by Size




What effect does the size of the list frame have on survey costs? In general, one would suppose that the greater the proportion of the rare population covered by the list frame, the less random digit dialing would have to be relied upon. Figure 4:10, below, shows the estimated cost of surveying by the number of organizations in the list frame (added in order of size), calculated using the formula in Eq. 4.2, retaining the balance between list and random digit dialing cases of the original survey design.[20]

$$C = c_0 + c_R n_R + c_L n_L + c_\varepsilon \quad \textbf{(4.2)}$$

Figure 4:10. Cost of Surveying by Number of Lists in List Frame

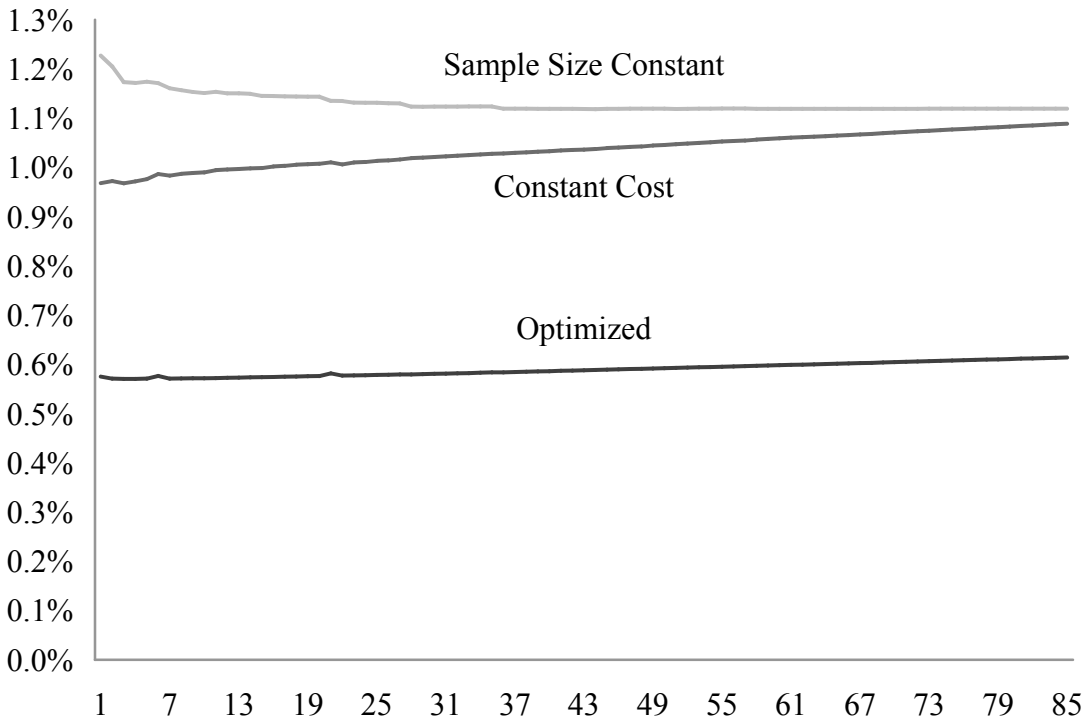



Increasing the coverage of the list frame, as it turns out, raises the costs of the random digit dialing frame sample somewhat. When the costs of list collection are included, greater list coverage is strongly associated with higher cost. The list frame removes cases that would otherwise have been found on the random digit dialing frame, decreasing the effective incidence of the rare population, and increasing the number of screening interviews required to find a sample of a specified size of the rare population. As Figure 3:5 showed, relatively small declines in incidence can lead to large increases in cost. The impact of the size of the list frame on random digit dialing survey cost, in this instance, is relatively minor, as the range of list coverage was fairly narrow (19.7 percent for 1 list and 31.9 percent for 85 lists) and the incidence of the Jewish population was relatively high (approximately 10 percent). Where the rare population is less common,

maximum coverage of the list frame is greater, or there is greater variation in list coverage, the effects will be larger. This raises the interesting possibility that one can be so successful at developing a list frame that a universal frame would be unaffordable.

However, cost is only one part of efficiency. What effect do different levels of list coverage have on variance? Figure 4:11, below, shows the size of the 95 percent confidence interval for estimates of the incidence of the rare population by different numbers of lists under the original sampling design.

Figure 4:11. 95% Confidence Intervals for Jewish Incidence by Number of Lists




Keeping sample size constant, a higher level of list coverage is associated with a decrease in variance. This, however, tells us little, as cost is always a consideration. A

more realistic approach, however, is to calculate the variance under a constant cost of $350,000. In this instance, the greater effectiveness of designs with larger list frames is vitiated by their greater cost per sampling unit.

*Dual-frame Optimization*

So far, I have not undertaken any cost-variance optimization. Optimizing a stratified, dual-frame design involves both determining the optimum allocation between frames and the optimum allocation of the sample between strata for a selected statistic. In this case, the statistic that will be used is the proportion of Jewish households in the survey universe. I first determine the optimum allocation between list and random digit dialing frames within each stratum for each variation in list frame following Choudhry (1989) and then optimized sample allocation between strata following Groves (1989). The procedures used may be found in Appendix O.

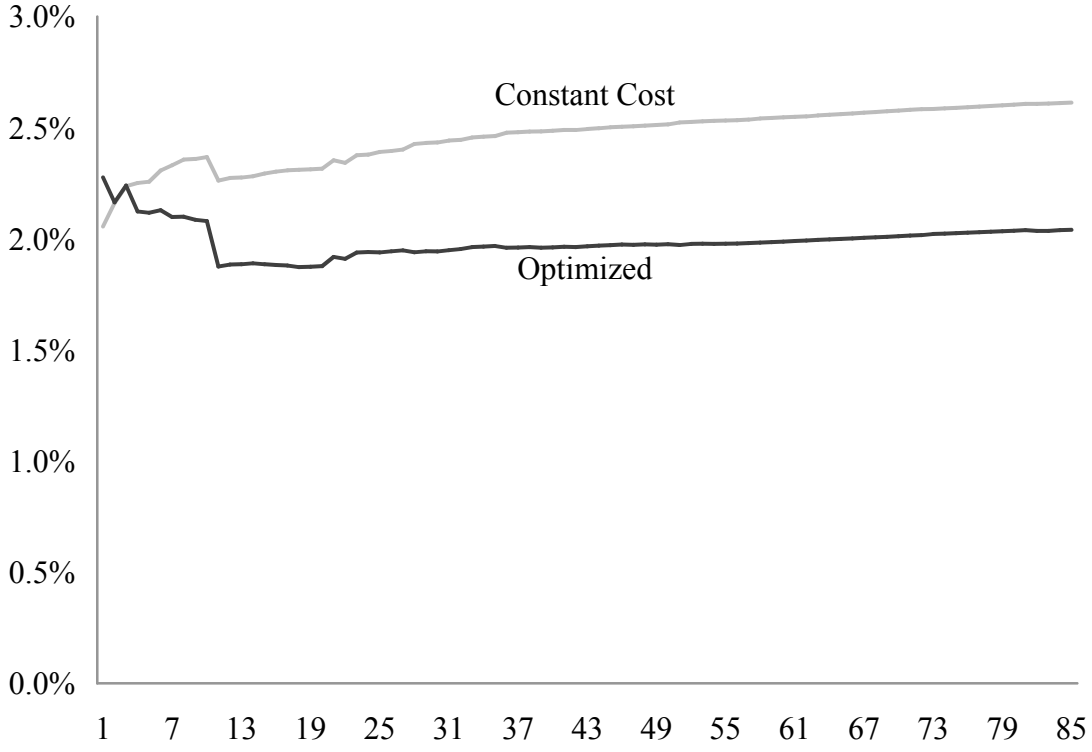
The results are shown in Figure 4:11, above. As can be seen, following this design would have significantly improved estimates of the size of the rare population. As the optimization accounts for costs, very little variation in variance occurs (the three and four list solutions had the lowest variance).

Optimization is, however, difficult as it requires relatively accurate estimates of the incidence of the rare population and survey costs prior to taking the field. These problems are more acute for rare populations for the simple fact that a study of a rare population is usually funded because existing data is poor. External data regarding American Jews is particularly difficult to come by as the U.S. Census does not collect data on religious groups. As I discuss next, it is possible to optimize for measures of community

characteristics, but accurate data here is even less common than for incidence. In either case, optimization requires choosing between the goals of measuring the population or understanding its characteristics

Estimating the proportion of the population that is Jewish is, however, only one goal for Jewish population studies. Understanding the characteristics of the community is at least as important. In this case, obtaining sufficient numbers of non-Jewish households to estimate the proportion of the population that is Jewish is irrelevant. Instead, the number of Jewish households is the key determinant of variance. I use the Jewish intermarriage rate to simulate the effect of different list designs, as it has been the most frequently cited statistic about American Jews—other, perhaps, than population size—since the National Jewish Population Survey of 1990 reported a current intermarriage rate of 52 percent. As only currently married Jewish households are included in the calculations, confidence intervals will be particularly broad. Figure 4:12, below, shows the 95 percent confidence intervals for the intermarriage rate. The optimized values are those used for estimating Jewish population incidence.

Figure 4:12. 95% Confidence Intervals for Intermarriage Rate by Number of Lists




Compared to the variance of estimates of the incidence of the rare population, the trends observed for optimized and unoptimized samples differ considerably. The variance of the design based on *a priori* assumptions increases with list coverage—the increased sample size of the list frame is counterbalanced by the increased cost of random digit dialing, which reduces the overall sample size. The design optimized for estimates of Jewish incidence, on the other hand, reduces variance as list coverage increases due to its more efficient use of the available resources. Notably, the minimum variance for the optimized design is achieved at the point at which the list frame becomes saturated, with additional lists adding few, if any, new cases. After this point, increased coverage is off-set by the smaller sample size available due to higher per case costs.

*Alternative Sampling Frames*

Having examined the construction of a multiple list frame in detail, I will now examine the effectiveness of a distinctive Jewish names frame. I examine two operationalizations of a distinctive Jewish name frame. The first ("ethnic names") uses ethnic classification from AccuData, a data provider, which maximizes coverage at the expense of a higher proportion of non-Jewish households (see pp. 249-250). The second ("trad. DJN") is a list of Jewish names used for population studies (Sheskin 2003a), which maximizes Jewish incidence within the distinctive Jewish names frame at the expense of coverage (see pp. 118-128). Table 4:7, below, shows the characteristics of these frames.

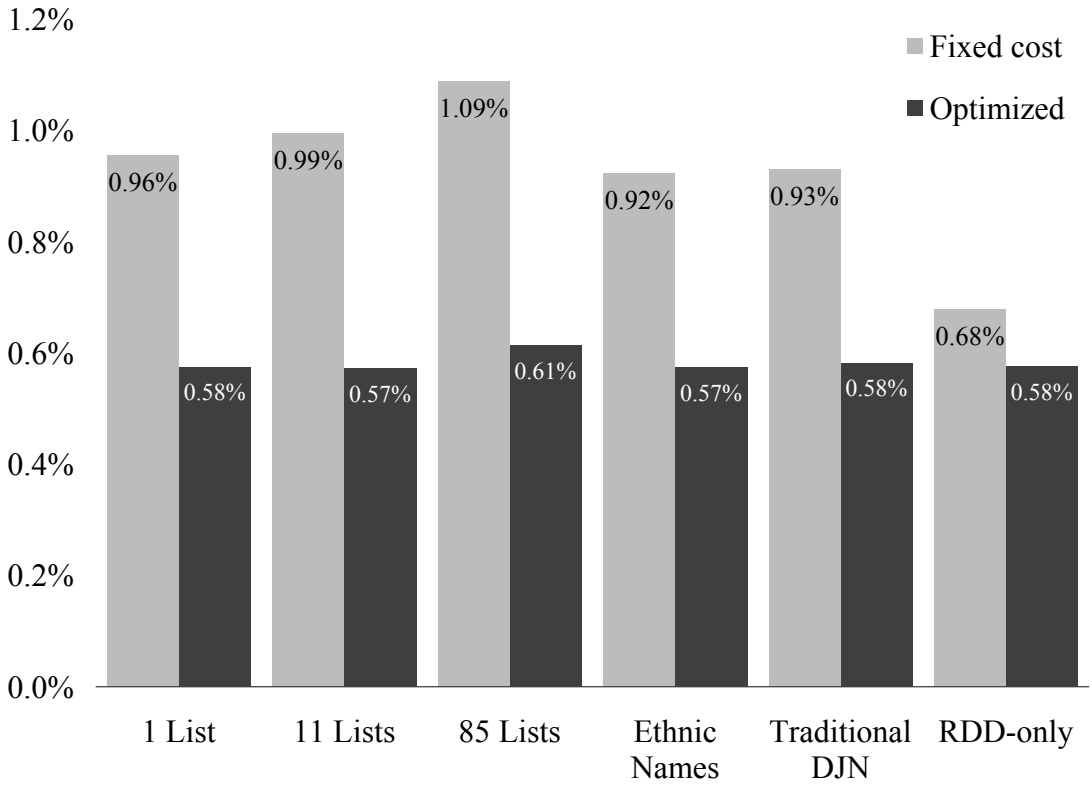
Table 4:7. Characteristics of Alternate Non-Universal Sampling Frames

| | CJP List Only | 11 Lists | 85 Lists | Trad. DJN | Ethnic Names |
|---|---|---|---|---|---|
| *Coverage characteristics* | | | | | |
| Jewish incidence in frame | 86.7% | 75.3% | 71.2% | 91.8% | 69.8% |
| Coverage of Jewish pop. | 19.5% | 28.2% | 31.9% | 12.4% | 27.1% |
| *Ethnic characteristics* | | | | | |
| Pct. intermarried | 20.2% | 22.5% | 23.1% | 26.8% | 29.1% |
| Pct. synagogue member | 67.0% | 58.2% | 58.5% | 51.9% | 47.0% |
| Pct. don't know Hebrew | 22.3% | 23.4% | 22.9% | 28.3% | 30.3% |
| Pct. always attend Seder | 80.5% | 75.6% | 75.3% | 70.4% | 55.9% |

As can be seen, both realizations of the distinctive Jewish names frame have lower levels of ethnic behavior than do either a Federation-only or 85 list frame, consistent with Sheskin's (2005b) assessment. The question, though, is whether a distinctive Jewish names frame is more cost-effective, given that both list and Jewish names frames are unbiased estimators when combined with a universal frame.

I examine the variance for six alternate sample designs, covering virtually all designs used in Jewish population studies in the United States. The Combined Jewish Philanthropies list and 85 list frames have been described in depth previously. In addition, I add an optimal 11 list frame in light of my findings above. Traditional distinctive Jewish names and ethnic name frames are also included, as is a random digit dialing-only design. The random digit dialing-only sample can be seen as a kind of null hypothesis, testing whether dual-frame designs are any use at all. Figure 4:13, below, shows the size of the 95 percent confidence intervals for the percentage of the population that is Jewish. As in the previous figures, the unoptimized estimates follow the original assumptions regarding sample design.

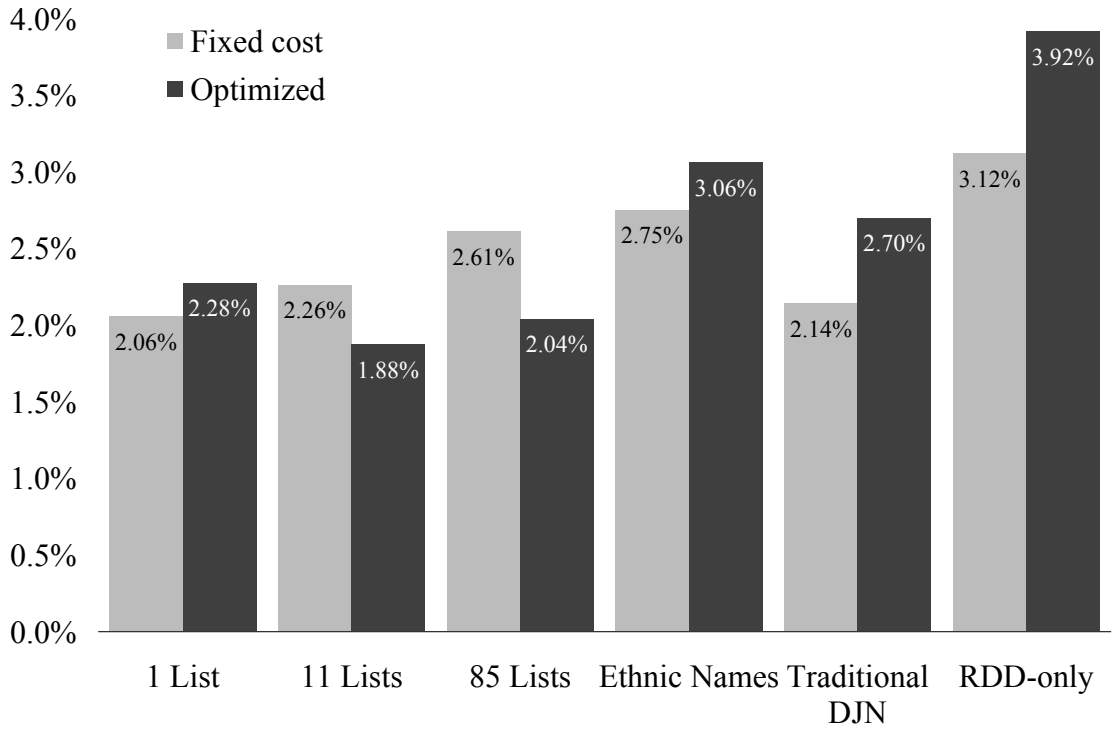
Figure 4:13. 95 Percent Confidence Intervals for Jewish Incidence by Sample Design



Looking first at the variance of estimates of Jewish population incidence using the *a priori* sample design, both distinctive Jewish names frames achieved lower variance than any list frame, testament to the lower cost of building the sampling frame, which translated into a larger sample size. (Note that there is little difference between the least expensive one list design and the distinctive Jewish names frames.) The random digit dialing-only frame achieved lower variance than did any of the dual-frame designs because it did not maximize the number of Jewish households at the expense of non-Jewish households (both, of course, being needed to estimate the proportion of the population that is Jewish). However, once the design is optimized, there is little variation in the confidence intervals between designs. A properly implemented distinctive Jewish names frame is no less and no more efficient that list frames at estimating the proportion of the population that is Jewish.

Turning to variance for estimates of Jewish intermarriage (Figure 4:14, below), the various designs based on *a priori* expectations display a similar pattern of variance to that seen for Jewish incidence.

Figure 4:14. 95% Confidence Intervals for Intermarriage Rate by Sample Design




Looking first at the estimates based on the original sample design, the Combined Jewish Philanthropies list and traditional distinctive Jewish names frames have the lowest variance, a result of lower random digit dialing costs due to the smaller proportion of the Jewish community covered and the lesser cost of building the list frame. Not surprisingly, the random digit dialing-only frame does the worst because it is the most expensive, translating into fewer Jewish households and greater variance. However, when each design is optimized to minimize the variance for population incidence, the 11 list frame achieves the lowest variance, reflecting its useful combination of relatively high levels of coverage and Jewish incidence at reasonable cost. The traditional distinctive Jewish names frame is too narrowly based, inflating variance by drawing a large number of cases

from a small proportion of the Jewish population. The ethnic names frame, by contrast, covers a relatively large section of the Jewish community, but is quite inefficient as it includes a high percentage of non-Jewish households. Looking beyond the relative performance of sampling frames, a design optimized to minimize variance for estimates of rare population incidence improves the accuracy of broadly based list frames. The higher variance of optimized designs with Jewish name frames requires careful consideration of the balance between the competing priorities of estimating the characteristics of the Jewish population and its size.

## COVERAGE

As I have previously touched upon, sampling variance is one property of sampling frames, while coverage error is another. While sampling variability is a predictable source of error that does not introduce systematic bias, coverage error—the difference between the target population and the population of the sampling frame—can introduce serious bias. As Figure 4:1 shows, a sampling frame can create error by including ineligible units or excluding eligible elements of the target population. As the screener for the study included an extensive—perhaps even exhaustive—set of questions to determine eligibility, overcoverage is only a salient with respect to cost and should not be a source of bias. Undercoverage, on the other hand, is a present and serious source of bias. As this chapter focuses on the properties of sampling frames, the third sampling-related source of error, nonresponse, will be addressed subsequently as is a result of the implementation of the frame, not an intrinsic property.

*Population Definitions*

Before addressing sources of coverage error, a brief review of the target and frame population is in order.

*Target population.* The target population for the Boston Jewish Community Study consisted of all households containing an adult who had a Jewish (or half-Jewish) parent, was raised as a Jew (or as a half-Jew), or currently considers himself to be Jewish (or half-Jewish) in the towns and cities constituting the study area (see Appendix H).[21] This definition was sufficiently broad to support virtually any analysis of Jewish life and allowed for a nearly infinite number of definitions of Jewish identity.

*Frame population.* In contrast to the simply defined target population, the frame population was far more specific. As the characteristics of the target population will structure the rest of this section, I will present it in outline form, with areas of potential divergence between the target and frame populations italicized.

1. *Households* in the
2. *Area served by Combined Jewish Philanthropies*, containing an
3. *Adult*, with a
4. *Landline telephone* in a
    a. *Non-zero listed exchange* with *greater than one percent incidence* in *communities expected to have significant Jewish population*, or
    b. Found on *lists provided by Jewish organizations*.

*Households*

The primary sampling unit of the Boston Jewish Community Study was the household. That is, all persons living in a single dwelling, whether related or not. Households, however, systematically exclude certain elements of the population that live in other kinds of residence. The largest populations excluded from the study are those of college students and residents of nursing homes and other care facilities.

Table 4:8. Group Quarters Population of Boston and Brockton MSAs

| Type of group quarters | Population |
|---|---|
| Nursing homes and other care facilities | 35,364 |
| College dormitories | 63,650 |
| Correctional institutions | 15,901 |
| Military quarters | 360 |
| Other noninstitutional group quarters | 15,966 |
| All group quarters | 218,057 |
| Total population | 3,662,288 |

*Source:* U.S. Bureau of the Census (2002b)

*Nursing homes and other care facilities*. At any given time, a number of individuals live in nursing homes, hospitals and hospices for the chronically ill, mental hospitals, and juvenile institutions. About one percent of the population of the Boston and Brockton Metropolitan Statistical Areas (MSAs) were in such facilities in 2000, or approximately 35,000 people. While the all age groups are represented in this population, it is clearly tilted toward the elderly. As the Jewish community is relatively elderly, the lower bound for this population can be estimated by assuming that the proportion of Jews in these facilities matches the Jewish incidence in the study area, for an estimated Jewish population of 2,300 people.

*College dormitories.* Hillel, the foundation for Jewish campus life, estimates the population of Jewish undergraduates at major universities of the Boston area as approximately 10,000 (see Table 4:9). There may be another 1,000 or so at other institutions of higher education. While the Hillel estimates have no scientific basis, Sales and Saxe (2006) found them to be generally accurate when compared with universal samples of the student population at several universities. Assuming that 25 percent of undergraduates live off-campus and there are about 11,000 Jewish undergraduates, about 7,750 will live in group quarters and be unenumerated.

Table 4:9. Estimated Jewish Undergraduate Population at Major Universities

| College | Jewish Undergraduates | Total Undergraduates | Jewish % |
|---|---|---|---|
| Harvard University | 2,000 | 6,658 | 30.0% |
| Boston College | 200 | 6,000 | 3.3% |
| Boston University | 3,000 | 20,000 | 15% |
| Northeastern University | 1,000 | 12,500 | 8% |
| Brandeis University | 1,900 | 3,081 | 61.7% |
| Massachusetts Institute of Technology | 360 | 4,220 | 8.5% |
| Tufts University | 1,500 | 4,755 | 31.5% |
| Total | 9,960 | 57,214 | 17.4% |

*Source:* Hillel (2006)

*Prisoners.* The population of correction facilities in the Boston and Brockton metropolitan statistical areas is 15,901. If Jews were represented in proportion to their share of the total population in the study area, a little over 1,000 inmates would be Jewish. Given that Jews are predominantly white and of higher socioeconomic status that population norms, it is unlikely Jews are evenly represented. Adjusting by the national level of under-representation of whites in the inmate population, I estimate the population of

whites in Boston-area correctional facilities as about 9,500.[22] Of these, about 800 would be Jewish, not accounting for differences in socioeconomic status, which would further reduce this total.[23] The unenumerated Jewish population in the study area is therefore unlikely to be particularly high.

*Military quarters.* The Boston area has very few military facilities with barracks. With only 360 such residents in total enumerated in the 2000 census, the Jewish population would be marginal.

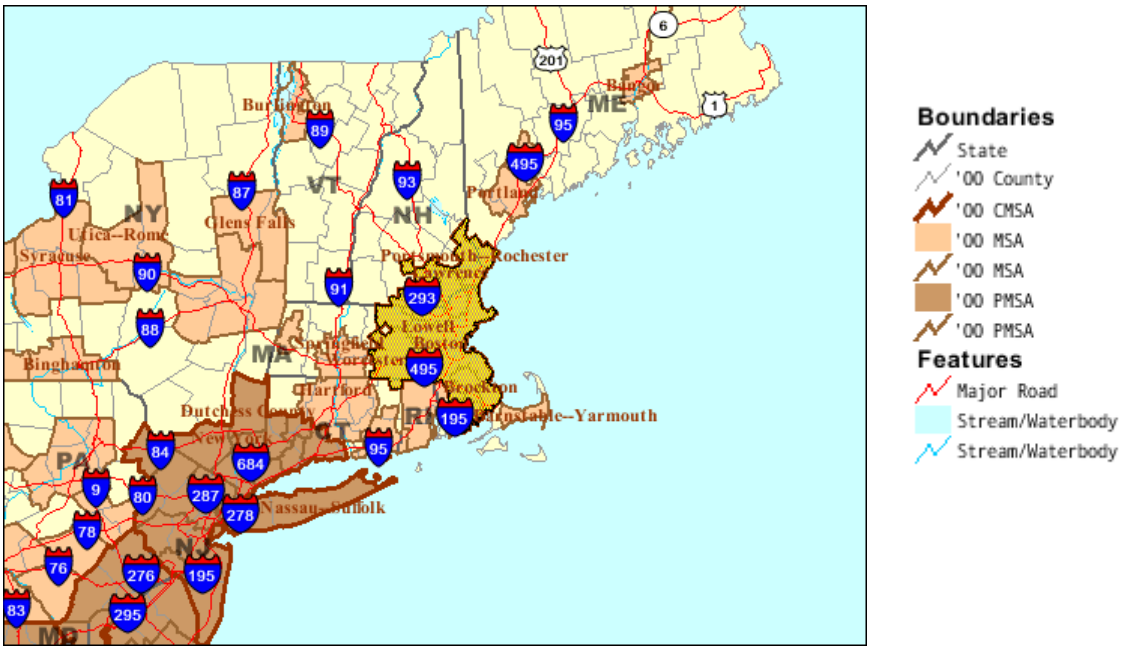
*Other group quarters.* The census estimates a population in other group quarters as 15,966 in the Boston and Brockton metropolitan statistical areas. Assuming Jews are represented in proportion to their share of the population of the study area, there would be about 1,050 Jews in other quarters.

*Homeless.* The City of Boston's annual homeless census estimated a population of 6,365 individuals without housing (Speizer et al. 2006). While the population of the City of Boston represents only a portion of the study area, it is likely that its share of the homeless population is far larger. Lacking data on the homeless population elsewhere in the study area, I will assume that the total homeless population amounts to 10,000 people. Were Jews to be represented according to their share of the population at large, this would amount to 650 individuals. As I have commented elsewhere in this section, the Jewish community is likely to differ considerably from the general population. In the case of the homeless population, this is absolutely unknown.

*Area Served by Combined Jewish Philanthropies*

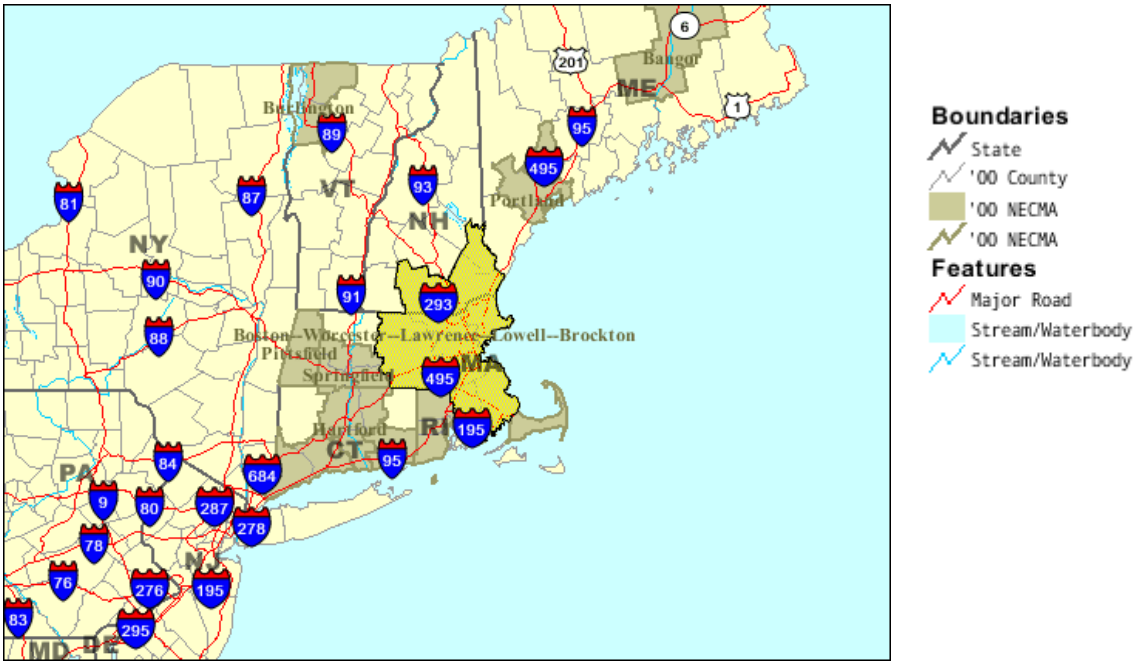
While geographic considerations do not constitute undercoverage as the study does not purport to provide information on these areas, as a practical matter it is difficult to keep in mind exactly which areas that might reasonably be thought of as belonging to the Boston area are covered and which are not. Depending on one's conception of the geographic area denoted by the concept "Greater Boston," the area covered by the study may be more or less comprehensive than expected. A brief examination of the U.S. census geographic units associated with the Boston area shows both the plasticity of the notion of "Greater Boston" and the fact it is more extensive than the area covered by the survey. Figure 4:15 shows the boundaries of the Boston-Worcester-Lawrence Consolidated Metropolitan Statistical Area (CMSA), which touches parts of all but two New England states, encompassing portions of Massachusetts, New Hampshire, Maine, and Connecticut. Figure 4:16 shows the boundaries of the somewhat more focused Boston-Worcester-Lawrence-Lowell-Brockton New England County Metropolitan Area (NECMA), which still, however, encompasses Fall River and Worcester. Figure 4:17 and Figure 4:18 respectively show the Boston and Brockton Metropolitan Statistical Areas (MSA) which, to my mind at least, provide the closest official approximation of the Boston area.

Figure 4:15. Boston-Worcester-Lawrence CMSA Boundaries



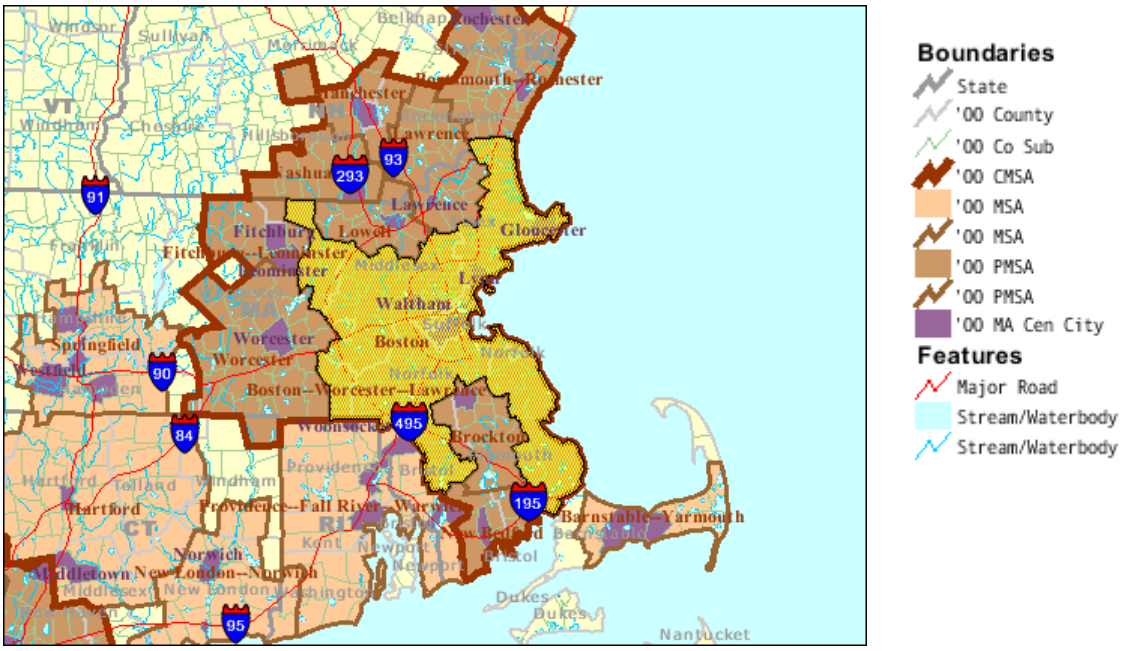

*Source:* U.S. Census Bureau

Figure 4:16. Boston-Worcester-Lawrence-Lowell-Brockton NECMA



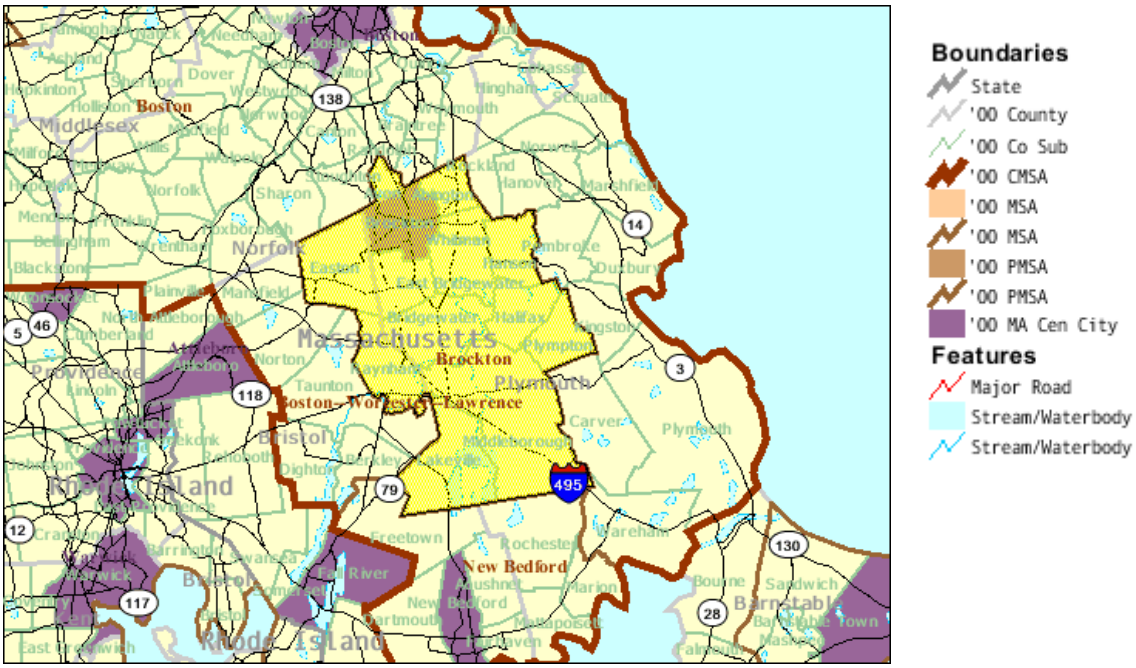

*Source:* U.S. Census Bureau

Figure 4:17. Boston MSA Boundaries




*Source:* U.S. Census Bureau

Figure 4:18. Brockton MSA Boundaries




*Source:* U.S. Census Bureau

By comparison, the study covers a slightly truncated area (Figure 4:6), broadly including towns and cities found inside I-495. The towns and cities included are those used in the 1995 study, with the exception of towns covered by the Jewish Federation of the North Shore, which did not participate in 2005.[24] This in turn was a product of previous decisions, dictated largely by the service areas of the various Jewish Federations that participated in the study.[25] In fact, consideration was given to expanding coverage. A meeting was held with the Jewish Federations representing the North Shore, Worcester, and Merrimack Valley to consider the possibility of conducting parallel surveys, although these did not occur.[26] Informal consideration was also given to including the areas covered by the Jewish Federations of Springfield and Cape Cod and the Islands.

Of course, the most obvious source of undercoverage is the exclusion of towns and cities in the very low density stratum from the random digit dialing frame. While we cannot determine the total population of this stratum with any accuracy, it is possible to estimate its size based on the number of list cases. As Table 4:10 shows, there was an extremely close relationship between the estimated incidence of Jewish households derived from the ratio of list households to total households and the survey estimates of the incidence of Jewish individuals in each stratum where a random digit dialed sample was conducted. It is not unreasonable to assume that a similar result would be obtained for stratum four.

Table 4:10. List and Survey Based Estimates of Jewish Population Incidence

| Stratum | List based estimated | Survey based estimate | List to Survey Ratio |
|---|---|---|---|
| 1 | 33.0% | 30.6% | 0.927 |
| 2 | 12.5% | 13.3% | 1.064 |
| 3 | 4.5% | 4.2% | 0.933 |
| 4 | 1.2% | - | - |

The lower bound of the uncovered population in the very low density stratum is estimated to be around 7,700 Jews (the estimated 1.2 percent incidence multiplied by .9 by the estimated stratum population of 777,111, less the estimated 1,650 or so Jews estimated to be found on the list frame) The lower limit for this population is assumed to be 6,700 Jews (90% of the mid-range projection). The upper limit of the population is estimated to be about 8,600 (110% of the middle estimate). This suggests the likely undercoverage is less than five percent, a relatively small fraction. As the Jewish households in the very low density stratum were thought to be younger, poorer, and less involved in Jewish life, it is likely that their characteristics differed significantly from those in the sampled strata. Even so, the most extreme differences would only have a small impact on overall estimates. To use an extreme example, if we assume that the overall rate of attending a Seder was 80 percent among the rest of the Jewish population and not a single Jew in the unenumerated part of the very low density stratum ever went to a Seder, the overall estimate would only decline by three percent. In reality, no differences this large would be observed, so the possible bias would be quite small indeed.

*Landline Telephones*

As I discussed in Chapter 3, the United States telephone system is undergoing rapid change which has serious implications for telephone surveys, especially those with a random digit dialing component. Any individuals who cannot be reached by telephone are effectively invisible to telephone surveys. To the extent that such individuals differ systematically from those with telephone coverage, survey results will be biased. I consider a number of possible sources of coverage error: nontelephone households, cell phone only households, and numbers in zero listed phone banks.

*Nontelephone households*. Households with no access to a telephone are the most obvious exclusions from telephone surveys. The 2000 census found that only 0.76 percent of households in the Boston and Brockton metropolitan statistical areas lacked telephone service. While it is possible to adjust for noncoverage bias for nontelephone households using information on temporary telephone service interruption (Fahimi et al. 2006a; Frankel et al. 2003; Keeter 1995), the survey did not include the necessary questions as it was unlikely a significant number of households would not be covered. Were the 0.76 percent ratio to hold true in the study area, less than 9,000 households would have no phone service. Considering the Jewish population's socioeconomic status, it is highly likely nontelephone households were a negligible source of coverage error. In addition, the overall level of undercoverage was too small to have much impact on estimates of Jewish incidence once age, race, gender, and education were adjusted for during the weighting process.

*Cell phone only households*. As I discussed in Chapter 2, households without cellular phones represent major challenges to telephone survey research. A particularly

troubling trait of cell phone only households is the very strong relationship with age. Younger people are far more likely to be contactable on a cell phone than their elders. Unfortunately, undercoverage is not simply a matter of finding too few young adults. The Pew Research Center for the People & the Press (2006) reports that young adults with cell phones only were far less likely to be married than those with landlines, less likely to live with a parent, and less likely to own their own home. This mirrors the finding of Kadushin et al. (2005) that the National Jewish Population Study 2000-01 appeared to draw a biased sample of young adults who were more likely to be Orthodox and were extremely unlikely to live with roommates. Regrettably, the Boston Jewish Community Study appears to have replicated these results, as an astonishingly large proportion of young adults are reported to be Orthodox compared to previous cohorts (Table 4:11) and a suspiciously high rate of marriage for 18-25 year olds of 33 percent is observed.

Table 4:11. Estimated Percentage Orthodox by Age Group

| Age | Percentage Orthodox |
|---|---|
| 18-25 | 21% |
| 26-30 | 5% |
| 31-35 | 2% |
| 36-40 | 4% |
| 41-45 | 3% |
| 46-50 | 6% |
| 51-55 | 9% |
| 56-60 | 3% |
| 61-64 | 2% |
| 65+ | 3% |

*Source:* Boston Jewish Community Survey

While virtually any statistic computed for young adults will be suspect, there is little that can be done to correct for the apparent oversample of Orthodox households as there is no external set of estimates that can be used as targets for corrective weighting.

*Business numbers.* In order to increase efficiency, the random digit dial frame of the Boston Jewish Community Study was scrubbed of listed business telephone numbers, including the Boston Jewish Community Study. Should some of these numbers in fact be used for nonbusiness purposes, undercoverage may result. While Merkle et al. (2006) report that a considerable number of listed business numbers were in fact home or home office numbers used for personal or personal and business use, little bias was likely to result.

*Determining which exchanges to call.* In addition to the usual dilemmas faced by random digit dialing designs, those with geographic restrictions must make further choices regarding how to implement the boundaries in a telephone survey. The Boston Jewish Community Study included any telephone exchange that had one percent or more listed numbers in the study area. This represents about the lowest possible threshold for inclusion in the survey, as any lesser criterion would have virtually opened up any telephone exchange in the country. A design optimized for efficiency would have used a higher standard, such as that a majority of telephone numbers be found in the study area. Nevertheless, some households in the Boston area would have had otherwise valid numbers that did not meet this requirement. Interestingly, the extent to which shadow numbers from switches between phone carriers pose a problem for computing probabilities of selection will prove a remedy for this type of noncoverage. Were shadow numbers indeed open to sampling, these households would have been called. Otherwise, the sample will have

disproportionately missed recent migrants to the area who retained their previous telephone numbers. Once again, insufficient exists to make a judgment about the seriousness of this problem.

## OTHER BIASES

It has at times been suggested that Israeli and Russian Jewish households are particularly difficult to contact, leading to underestimates of the size of these populations. However, the balance of evidence suggests that only the Russian population is likely to be underenumerated (Tobin and Groeneman 2003; United Jewish Communities 2003a; 2003k). The Boston Jewish Community Study attempted to guard against the possibility by making special contact efforts (a Russian-language contact letter and were recontacted by a Russian-speaking member of the Cohen Center staff). How well did these efforts work? Unlike many other data about American Jews, there is at least some external data on the number of Israelis and Russian Jews. The U.S. Census provides estimates of members of foreign language households as well the place in which people are born. Like the proxy measures previously discussed, however, these estimates are troubled by the usual paradox of not knowing what proportion of people with these characteristics are Jewish and what proportion of people of interest (i.e. Russian Jews and Israelis) have these characteristics. In the case of people born in Israel, it is likely that the vast majority of migrants are Jewish. However, not all Israelis are born in Israel. As the Boston Jewish Community Study only ascertained place of birth, this presents little problem. The situation with migrants from the Former Soviet Union is somewhat more difficult to ascertain, as some

non-Jews have certainly migrated to the United States. Nevertheless, anecdotal information on the composition of the Russian community in Boston suggests that the majority are either Jewish or have some tie to Jewish identity (a Jewish spouse or parent, for instance). In any case, the total number of migrants from the Former Soviet Union provides an upper limit on the likely size of this population.

Looking first at Israelis, this population was estimated to contain approximately 3,000 Jewish adults (the birthplace of children was not asked). This compares well with the census estimate of 2,764 Israeli born individuals (children and adults) in towns and cities of the study area in 2000. The adult Hebrew speaking population (which would presumably include Israelis born in other countries) was estimated by the census at 4,103. As with other studies, there is little evidence that Israelis are underenumerated.

The Russian population is another story. It was estimated that there were 7,000 Russian adults in the study area. The number of people born in Russia, Ukraine, or Belarus was estimated to be 17,493 by the census. This probably understates the size of the population born in the Former Soviet Union, as the census does not report the size of very small groups—thus individuals born in other nations that were once part of the Soviet Union are not counted. The adult Russian speaking population (which presumably includes migrants from elsewhere in the Former Soviet Union due to the USSR's Russophone policies) was estimated by the census to be a broadly comparable 18,376. At most, about 11,000 Russian Jewish adults were likely to have been missed. In practice, there must be some non-Jewish households from the Soviet Union. It seems reasonable to estimate this at 25 percent of the population. Estimates from the Office of Refugee and Immigrant Health (2000), however, placed 50,000 or so Jews in the Greater Boston area

(some of which were in areas not covered by the survey). The sources of these estimates are difficult to determine, and appear to rely on numbers of refugee migrants from the Former Soviet Union, supplemented by guesstimates of the number of family and other migrants. It is not clear whether these estimates are adjusted for mortality or in- and out-migration. As some of the agencies that provide information have contracts with the commonwealth government to provide services, there is at least an incentive to provide optimistic estimates. Given the uncertain and apparently problematic methodology used to arrive at these estimates, it is difficult to use them as a basis for determining the extent of underenumeration. Accordingly, the size of the Russian Jewish population, then, was probably underestimated by 50 percent, based on census data.

## IMPROVING SAMPLING FRAMES FOR JEWISH POPULATION STUDIES

I have examined two aspects of sampling frames used in surveys of American Jewish communities. Dual-frame surveys are well established methods of studying rare populations at reasonable cost. The coverage and characteristics of the list frame are, however, usually treated as givens. No single authoritative list exists for Jewish communities in the United States, leaving researchers with the task of assembling a frame from partial, overlapping lists. The cost of collecting, processing, and deduplicating lists, while small in comparison to interviewing costs, is still considerable. Unless the marginal proportion of cases added by each additional list exceeds the marginal increase in cost, using multiple lists will not be cost effective. Unfortunately, marginal coverage is very difficult to estimate in advance as it depends on the overlap in membership between lists. In the case of

Boston Jewry—and, I suspect, many other Jewish communities—list sizes follow a power-law distribution, with few large lists and many relatively small ones available for collection. In spite of the relatively high degree of overlap between core lists, focusing on the largest lists appears to be a more efficient approach to list collection than either collecting the easiest to acquire lists or attempting to cherry pick lists with low levels of overlap.

The proportion of the population covered by the list frame is another reason for caution when developing a list frame. High levels of coverage have the potential to decrease the effective incidence of the Jewish population in the random digit dialing or other universal frame to the point that it ceases to be economically feasible. While far short of achieving universal coverage, a larger list frame was associated with broader confidence intervals.

*A posteriori* cost-variance optimization produced considerably less variance than did an *a priori* design. While accurately estimating the many variables involved in optimization in advance is extremely challenging, it is an important step that should not be foregone. These results suggest following the conservative course of using a relatively small multiple list frame drawing on larger organizations.

In the special case of ethnic populations, distinctive ethnic surnames may be viable alternatives to a list frame, although the list of surnames chosen will have a very significant impact on the coverage of the surname frame and the extent to which it is composed of members of the ethnic group. As assembling a distinctive Jewish names frame is relatively simple and inexpensive, it may be worthwhile to develop in the planning stages of a survey to compare to the expected yield of a list frame. In the case of

Boston Jewry, neither the traditional distinctive Jewish names frame nor a commercial ethnic surnames frame proved more efficient than a moderately sized list frame. A list frame again appears to be the best option.

## ENDNOTES

This chapter includes material from presentations by the author at the inaugural conference of the Steinhardt Social Research Institute (November 3, 2005), the annual meeting of the Association for Jewish Studies (December 20, 2005), the annual meeting of the Eastern Sociological Society (February 26, 2006), and the annual conference of the American Association for Public Opinion Research (May 21, 2006), as well the unpublished *Expanded Codebook* delivered to Combined Jewish Philanthropies of Greater Boston on February 1, 2006. Needless to say, it has benefited greatly from the advice and assistance offered by my colleagues for which I am extremely grateful, particularly Charles Kadushin, Leonard Saxe, Christopher Winship, and Graham Wright.

[1] The discussion of the 1936 elections is based on Converse (1987) and Squire (1988). I will not cite the source for every factual statement to avoid repetition, but the reader should be aware that the data come from these sources, not independent research.

[2] There are many sources of measurement error. The idealized description of an accurate survey omits the fact that many characteristics of interest (e.g., intelligence) are latent variables never directly observed and are inferred from proxy measures, thus raising questions of validity. Many variables are also constantly changing, so a hypothetical measurement of perfect accuracy at any time might differ from one taken at a different time for the same unit.

[3] Groves et al. (2004a) divide sampling error into sampling bias, which occurs when units in the sampling frame have either no chance of selection or unequal probabilities of selection (the text is unclear), and sampling variance. This distinction is not terribly helpful. Cases with zero probability of selection are typically excluded from the sampling frame and thus fall under the rubric of coverage error. Unequal probabilities of selection are, of course, a characteristic of complex sample designs and are only actualized as error through either increases in sampling variability

(which Groves et al. treat separately) or through improper weighting, in which case adjustment error occurs.

[4] A universal frame is a common but unnecessary element of a multiple-frame design. All that is needed is a degree of overlap between the frames (Hartley 1974).

[5] Depending on one's understanding of the area of Greater Boston, the towns and cities included in the study may be more or less than expected. Specifically, only communities that were part of the 1995 Combined Jewish Philanthropies Demographic Study were sampled (Israel 1997), less towns and cities under the purview of the Jewish Federation of the North Shore. The area covered in the 1995 study was larger than 1985 (Israel 1987), adding Franklin, Holliston, Hopkinton, Hudson, Marlboro, Maynard, Medway, Southboro, and Stow, all of which lie outside the Boston Standard Metropolitan Statistical Area.

[6] An area-probability sample could have been used as the universal frame instead of random digit dialing, but its cost was prohibitive.

[7] Social scientific studies of European Jewish populations, which preceded their American counterparts, used lists of a very different sort. Government data on religious identification (which was required) was used to estimate the size of the Jewish population and the rate of intermarriage, among other statistics, at least for those registered as members of the Jewish faith.

[8] Examples of studies using Jewish Federation lists for separate areas include the 1975 Boston study, which included used the Combined Jewish Philanthropies and Brockton Federation lists (Fowler 1977), the 1985 Boston study, which included lists from the Framingham and North Shore Federations (Israel 1987), the 1995 Boston study, which included the Combined Jewish Philanthropies and the Jewish Federation of the North Shore (Israel 1997). A number of surveys of Jewish populations have drawn on multiple lists, including the 1965 Boston study, which drew on "a list of addresses compiled by Combined Jewish Philanthropies made up of members of Jewish organizations, synagogues, contributors and other similar sources where there was a high probability of occupancy by a Jewish household" (Axelrod, Fowler, and Gurin 1967:5) and the 2001 Houston study, which drew solely on lists from 36 Jewish agencies in Houston (Herman and Phillips 2001a).

[9] I use "membership" loosely when describing the relationship of individuals listed to the organizations providing the lists. Many lists included both current and former members. Other organizations, such as Combined Jewish Philanthropies itself, did not have members in the strict sense of the term, but rather current and former donors. Other lists contained subscribers to periodicals, purchasers, signatories to petitions, attendees at activities, and people who signed up to receive information about the organization's activities. Still other lists included cases seemingly added by the organization *en bloc*, such as all Massachusetts legislators (these were removed, but presumably others were not detected).

[10] The National Jewish Population Survey of 2000-01 (Kotler-Berkowitz et al. 2004b) oversampled areas expected to have high Jewish incidence, as did the Jewish Community Study of New York of 2002 (Ukeles and Miller 2004b) and the Pittsburgh Jewish Community Study of 2002 (Ukeles, Miller, and Kulp 2002). Among national studies, the American Jewish Identity Survey of 2001 (Mayer et al. 2002) did not oversample high incidence areas as its screening questions were integrated into two weekly omnibus surveys, providing considerably lower screening costs at the price of the particularly low response rate associated with such an approach; see, however, Kosmin's (2005) argument in favor of not oversampling on Jewish incidence. While its technical documentation is unclear, the Survey of Heritage and Religious Identification of 2002 (Tobin and Groeneman 2003) did not appear to oversample on Jewish incidence.

[11] The discussion of network samples in Salganik and Heckathorn (2004) and Thompson and Collins (2002) were extremely useful and provides much of the basis for this presentation.

[12] When the characteristic is particularly sensitive one, like illegal drug use, friendship or some other criterion may be substituted for directly asking about the characteristic, on the assumption that friends of the respondent will be similar.

[13] The multiplicity element of the sampling scheme was in fact a separate sample rather than a sampling frame, as households could be found on either or both the multiplicity and the list-random digit dialing sample.

[14] Cases with the same surname are at times classified in multiple ethnic categories, indicating that first and middle names also formed part of the criteria used to determine a name's ethnicity.

The limited number of multiple classifications, however, indicates that the primary method of allocation is based on surnames.

[15] The difference in coverage between traditional distinctive Jewish names frames and the commercial ethnic name frame is astonishing. While the traditional distinctive Jewish names frames found in Appendix I cover a little more than 100 names, the commercial name frame classifies more than 1,000 surnames as European-Jewish or Israeli, though some of these are presumably classified on the basis of first or middle names rather than surnames. The surnames of cases classified as Jewish are not listed in Appendix I due to the proprietary nature of the data.

[16] Strictly speaking, the initial informant was first asked about themselves. If they did not answer yes and there was another adult in the household, the informant was asked about the other adult. If there were two or more other adults in the household, the informant was asked about "any other adult in your household."

[17] The incentive of $18 was chosen as the numeric equivalent of the word *chai* (חי), meaning "life" but freighted with positive associations to a far greater extent than the English equivalent. The symbolism is widely known and regularly used as a suggested donation by Jewish charities and in other financial transactions, such as gifts.

[18] Screener rates are presented because they are the basis of estimates of Jewish population incidence and size. A non-Jewish household contributes nothing to analyses within the Jewish population but is essential to calculations of size. Likewise, Jewish households that completed the screener but refused the extended interview are nevertheless included in estimates of population incidence. Calculating outcome rates for rare populations is a significantly more difficult proposition. See Appendix F for additional outcome rates.

[19] In effect, the decision was made to increase bias so that variance could be reduced, in the form of a larger sample.

[20] Costs are calculated using a variation of the actual design. A random digit dialing sample has been added to the fourth (very low density) stratum, with sampling ratios of 8:4:2:1 compared to the upper three strata. I use the costs and estimated final dispositions of the most effective treatment for three groups of cases: list cases, listed random digit dialing, and unlisted random digit dialing. It is assumed that listed households transferred to the random digit dialing frame when

fewer than 85 lists are used will behave similarly to random digit dialing cases within frame and stratum. Household cases are assigned to the listed and unlisted random digit dialing frames proportional to the observed distribution of cases from that stratum. I investigated the possibility that Jewish households would be more or less likely to be found on the unlisted random digit dialing frame, but found no significant differences. It is possible, though, that list households may differ in their distribution between listed and unlisted random digit dialing frames to non-list households. The characteristics of the random digit dialing frame within the fourth stratum are extrapolated from the characteristics of random digit dialing samples in the higher density strata and from the observed relationship between random digit dialing and listed cases in the preceding strata.

[21] The option for "half-Jew" was not read to respondents but was allowed. Respondents were not asked to clarify what they meant to say. I use the term "half-Jew" reluctantly as it certainly carries derogatory overtones. Despite its potential for offense, the term has been adopted by some children of intermarried parents who were not raised as Jews. The term has been assailed as meaningless by those who view Jewishness as a binary identity that can only exist in the "on" or "off" position. I disagree with this position on practical and theoretical grounds (Phillips and Chertok 2004; Phillips and Fishman forthcoming; Phillips and Kelner forthcoming). From a practical perspective, being "half-Jewish" is a living, grassroots description for certain types of Jewish identity that would be foolish to ignore. Theoretically, being "half-Jewish" captures an identity that it is clearly not entirely Jewish, but not divorced from it, either, paralleling the trend observed by sociologists of religion and ethnicity trend toward plural and syncretistic identities.

[22] This figure is derived from Census 2000 estimates of the racial composition of the study area and the level of racial over- and under-representation, calculated by dividing Harrison and Beck's (2006) report on the race of inmates by American Community Survey estimates of the racial composition of the United States population (U.S. Bureau of the Census 2005).

[23] This estimate is derived from the estimated white population of the study area (U.S. Bureau of the Census 2002) divided by the estimated proportion of Jews among whites in the study area.

[24] The towns constituting the North Shore in the 1995 study are as follows: Beverly, Boxford, Danvers, Hamilton, Lynn, Lynnfield, Manchester, Marblehead, Middleton, Nahant, Peabody, Salem, Saugus, Swampscott, Topsfield, and Wenham.

[25] Combined Jewish Philanthropies merged with Jewish Federations covering the Brockton and Framingham areas.

[26] The Merrimack Valley Federation covers towns to the northwest such as Andover, Haverhill, Lawrence, Lowell, North Chelmsford, and Westford.

# Numbering the Jews: Evaluating and Improving Surveys of American Jews

Volume II of II

A Dissertation

Presented to

The Faculty of the Graduate School of Arts and Sciences

Brandeis University

Near Eastern and Judaic Studies and Sociology

Sylvia Barack Fishman, Advisor

Shulamit Reinharz, Advisor

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of the Requirements for the Degree
Doctor of Philosophy

by

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# Chapter 5

## Nonresponse Analysis and Weighting

Every year in late spring, the American Association for Public Opinion Research holds its annual conference. One of the minor events in the lead up to the conference is the announcement of the winner of the contest to come up with the slogan for that year's t-shirts. The winners are usually witty and sometimes profound aphorisms. One that was both was the slogan "If you don't like the answers, just weight." Weighting is a critical part of virtually every survey. True simple random samples, in which every person has the same probability of being selected, are very rare. Complex sample designs which vary the probabilities of selection are used for reasons of economic efficiency and/or to ensure adequate representation of subgroups. Without weights, the cases with the highest probability of selection will be overrepresented in survey estimates, biasing statistics in which these cases differ from the rest of the sample. To counteract this, design weights are applied, which are the inverse of the probability of selection. To use an example I gave in a previous chapter, a case with a probability of selection of 100/100,000 would receive a weight of 100,000/100 or 1,000. (In reality, calculating the probability of selection is much more complicated.) Design weights, however, are only part of the story. Response

and coverage error are realities. Some people, as we saw in the Chapter 3, are less likely to participate in surveys than others, and sample frames themselves may not adequately represent the target population. Failing to correct for this would, like probability of selection, lead to biased estimates of those variables on which the easier to contact groups differ from other cases. Poststratification weights are used to make these corrections. However, even more than design weights, poststratification weights involve inference and surmise in calculating what the "real" population would have looked like. Different assumptions can lead to very different results, as certain cases are accorded greater levels of influence. Not surprisingly, poststratification weighting can be highly contentious.

Developing poststratification weights for studies of Jewish communities is more difficult than usual, given the dearth of external sources of information on the characteristics of American Jews, let alone those in a particular locale. Given this, it is not surprising that poststratification weighting is uncommon in studies of American Jewish communities. Ira Sheskin, as we have seen, uses problematic sampling and weighting techniques. The other major researchers, Ukeles and Associates, use properly constructed dual-frame samples, but relatively inefficient weighting techniques. Neither report using poststratification weights. Indeed, neither Sheskin nor Ukeles and Associates report undertaking the detailed analyses of their samples that are a necessary precursor to poststratification weighting.

In this chapter, I present the weighting scheme for the Boston Jewish Community Study. The analysis of nonresponse is a vital component of this process, as it is necessary to extrapolate from the cases that were surveyed to those that were not. The text, however, is arranged around the development of weights rather than analysis of nonresponse.

While this may be awkward for some readers, there is a very good reason for this. Non-response analysis and weighting go hand in hand. In order to correctly analyze non-response, design weights are required. It therefore makes sense, I believe, to follow the process of developing weights and analyzing nonresponse in the order it actually occurs.

While this chapter necessarily focuses on the specifics of the Boston Jewish Community Study, it has broader implications. The stratified dual-frame design used in the Boston study is commonly used for studies of Jewish communities. Beyond this, while all American Jewish communities have a unique sociodemographic composition, the substance of the nonresponse analyses may well apply to other Jewish communities.

In lieu of repeated citations, unless otherwise specified, the weighting procedures outlined below use those of the 2001 National Survey of Veterans (Choudhry et al. 2002) as a model, as do descriptions and notation, due to its dual-frame design and particularly clear and careful documentation.

## LIST SAMPLE WEIGHTS

The initial steps in weighting a dual-frame design take place separately for the random digit dialing and list frames. For the time being, each is treated as if it were a self-contained sample and only later joined. List cases are examined first. The initial step is to calculate the design weight. As the list frame was stratified, with households from different strata having different probabilities of selection, the design weights for each stratum will differ (see Table 5:1 for frame and sample size).

Table 5:1. List Sample and Population Sizes by Stratum

| | Stratum | | | | |
|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | Total |
| Total listed households | 26,381 | 22,241 | 27,190 | 3,813 | 79,625 |
| Sample size (total drawn) | 1,181 | 2,219 | 2,771 | 548 | 6,719 |

The weight assigned to each household within a given list stratum (denoted by *s*) is the reciprocal of the odds of selection:

$$listwt1_{si} = \frac{N_s}{n_s}; (i \in s) \qquad \textbf{(5.1)}$$

Recall that *N* is the size of a population, while *n* is the sample size. The notation *i* refers to a single case. Thus, the list weight for the *i*th case in the *s*th stratum is the list population of the stratum divided by the number of list cases sampled in that stratum.

*Adjustment for Unknown Eligibility*

While it would be desirable to know the exact status of every case in the stratum, this is not possible. Some telephones, for instance, were never answered. While some of these might have been actual households, others may have been businesses or otherwise ineligible. Absent better information about these households, it is assumed that they have the same distribution of eligible and ineligible units as the cases for which status is known. In order to adjust the base list weights for cases where eligibility could not be determined, households are divided into four response status categories:

1. Eligible households—Households in the study area that completed a screener interview, regardless of whether it was eligible for the Jewish instrument or not. (Denoted as *R*.)
2. Ineligible—List cases that were ineligible for the study. (Denoted as *O*.)
3. Eligible nonrespondents—Known residential households where no interview was completed. (Denoted as *N*.)
4. Eligibility unknown—List cases of unknown eligibility. (Denoted as *U*.)

The proportion of cases of unknown eligibility is likely affected both by stratum (e.g., differential access to answering machines and voicemail) and experimental treatment (i.e. more effective treatments should be associated with lower rates of refusal). In addition, treatment was assigned randomly across rather than within strata (i.e. some strata may have had more "successful" experimental treatments than others, potentially conflating the effect of impact of each factor. An adjustment (*listadj*1) is made to distribute cases of unknown eligibility (*U*) proportionally across cases of known eligibility (*R*,*O*,*N*) within stratum (*s*) and treatment (*t*), a total of eight (4 x 2) cells:

$$listadj1_i = \frac{\sum_{i\in R} listwt1_i + \sum_{i\in O} listwt1_i + \sum_{i\in N} listwt1_i + \sum_{i\in U} listwt1_i}{\sum_{i\in R} listwt1_i + \sum_{i\in O} listwt1_i + \sum_{i\in N} listwt1_i} \quad \textbf{(5.2)}$$

Accordingly,

$$listwt2_i = listwt1_i listadj1_i, \text{ where } i \notin U$$
$$listwt2_i = 0, \text{ where } i \in U \quad (5.3)$$

As adjustments are calculated within strata and treatment, this procedure retains the relative probability of selection between treatments and strata.

*Poststratification Adjustments*

Calculating poststratification weights is complicated by the contingent nature of our knowledge of the sample universe—we do not have information external to that which was collected in the course of developing the list frame and implementing the survey.

*Adjustment by community characteristics.* Although no information is available concerning the characteristics of nonrespondent households, it is possible to use data about the characteristics of communities in which respondents lived to reduce the biases associated with nonresponse. It might be expected, for instance, that households in areas with large numbers of non-English speakers would have a lower probability of response. Were this not adjusted for, the sample would over-represent English speakers together with whatever characteristics differentiate them from non-English speakers.

Accordingly, data was obtained from Summary Files 1 and 3 of the 2000 Census of Population and Housing (U.S. Bureau of the Census 2001; 2002) concerning characteristics of communities in strata 1-3.[1] Where a street address was available, cases were

matched with block group data. For cases where no street address was available, data at the town or city level was used.

However, as I discussed in Chapter 3, nonresponse actually conflates two distinct phenomena: noncontacts and refusals. It is likely that the characteristics of households that refuse may differ from those that could not be contacted. When understanding nonresponse and correcting for it, it is important to treat these separately. The situation, however, is a little more complicated still. Because we made up to 20 contact attempts, the status of a case changed from one contact attempt to the next. Case 1, for instance, may have answered the survey on the first contact attempt, while Case 2 took five contact attempts before refusing.

In order to account for the multiple attempts and multiple outcomes, following Kennickell (1998), nonresponse is modeled as a discrete time-series multinomial logistic regression. Multinomial logistic regression is a technique used for analyzing variables with three or more discrete outcomes. A classic application of multinomial logit (logit is used interchangeably with logistic) is predicting whether a person will drive to work, catch a bus, or catch a train. A person can only commute on one of these at a time. In the case of the Boston study, there were $J$=3 nominal outcomes: noncontact, refusal, and completion of the screener interview.

However, as mentioned above, there is a temporal dimension, too. Each contact attempt represented a fresh chance for a case to refuse or be interviewed. This process is cumulative. While a given case may only have a small chance of being contacted, as more attempts are made, the odds of contact will rise. There is, however, another quirk. Once a case refuses or is screened, it has reached its final status. Strangely enough, this

resembles studies of mortality. Health risks accumulate across time periods, with some cases "failing" (i.e. dying). In this instance, cases failed by being interviewed or refusing. In order to implement this analysis, the dataset was transformed from one row per case to one row per contact per case. The statistic of interest in this case, was the probability of outcome *m*, completion of the screener. This is $\Pr(y_i = m \mid \mathbf{x}_i)$, the conditional probability that case *i* will fail in manner *m* at time *t*, otherwise known as the conditional hazard function, $h(t)$:

$$h_i(t) = \Pr(y_i = m \mid \mathbf{x}_i) = \frac{\exp(t, \mathbf{x}_i \boldsymbol{\beta}_m)}{1 + \sum_{j=2}^{J} \exp(t, \mathbf{x}_i \boldsymbol{\beta}_j)} \tag{5.4}$$

The resulting model is shown below in Table 5:2.

Table 5:2. Multinomial Logit Regression of List Household Response

| Variable | Refusal | Survey |
|---|---|---|
| Call 2 | 1.36* | 0.949 |
| | (-2.1) | (-0.65) |
| Call 3 | 1.81*** | 0.824* |
| | (-4.01) | (-2.21) |
| Call 4 | 2.35*** | 0.775** |
| | (-5.87) | (-2.62) |
| Call 5 | 2.56*** | 0.802* |
| | (-6.44) | (-2.25) |
| Call 6 | 2.02*** | 0.682*** |
| | (-4.43) | (-3.53) |
| Call 7 | 2.08*** | 0.746** |
| | (-4.57) | (-2.61) |
| Call 8 | 2.41*** | 0.807 |
| | (-5.37) | (-1.87) |
| Call 9 | 2.19*** | 0.738* |
| | (-4.34) | (-2.43) |

| Variable | Refusal | Survey |
|---|---|---|
| Call 10 | 2.56*** | 0.675** |
| | (-5.14) | (-2.72) |
| Call 11 | 2.17*** | 0.634** |
| | (-4.01) | (-2.87) |
| Call 12 | 2.36*** | 0.918 |
| | (-4.35) | (-0.56) |
| Call 13 | 1.96*** | 0.632** |
| | (-3.18) | (-2.69) |
| Call 14 | 1.47 | 0.582** |
| | (-1.58) | (-2.69) |
| Call 15 | 1.99*** | 0.502*** |
| | (-3.29) | (-3.33) |
| Call 16 | 2.84*** | 0.505** |
| | (-4.94) | (-3.07) |
| Call 17 | 1.77* | 0.443*** |
| | (-2.18) | (-3.58) |
| Call 18 | 1.62 | 0.512** |
| | (-1.61) | (-2.89) |
| Call 19 | 2.31*** | 0.616* |
| | (-3.19) | (-2.10) |
| Call 20 | 3.14*** | 3.89*** |
| | (4.15) | -10.91 |
| Percent Jewish households | .9932 | 0.996 |
| | (-2.87) | (-1.87) |
| Average number of rooms | 0.822 | 1.43*** |
| | (-1.66) | -3.84 |
| Number of rooms$^2$ | 1.01 | 0.971*** |
| | (-1.23) | (-3.78) |
| Average household size | 1.49*** | 1.00 |
| | (3.2) | (-0.02) |

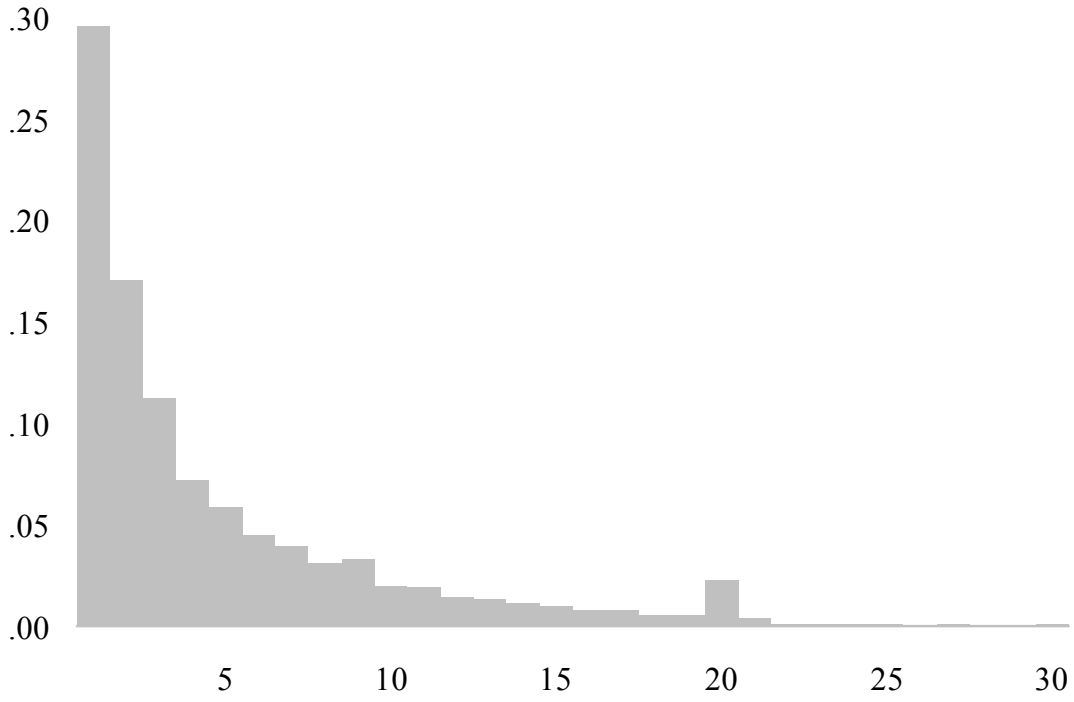
* $p \leq .05$; ** $p \leq .01$; *** $p \leq .001$

*Notes:* Coefficients are odds ratios. Numbers in parentheses are T-values.

The coefficients in Table 5:2 are odds ratios. An odds ratio is the change in the probability of a given outcome associated with a single unit change in the independent variable. To use a simple example, if we were only considering whether a person would buy an ice cream by the temperature measured in degrees Celsius, an odds ratio of 1.1 would mean that for every one degree increase in temperature, a person would be 1.1

times as likely or 10 percent more likely to buy an ice cream. Conversely, if the odds ratio for rain was 0.6, it would mean that a person on a rainy day would be only 60 percent as likely, or 40 percent less likely, to buy an ice cream as on day without rain. Returning to Table 5:2, the odds ratios of less than one for successive contact attempts for completing an interview mean that additional contact attempts were less and less likely to result in an interview. However, as the probability of completing an interview is cumulative, the overall odds of completing an interview increase as more contact attempts are made. This can be seen in Figure 5:1, below, which shows the proportion of completed screeners by contact attempt (note that this includes cases from both the list and random digit dialing frames).

Figure 5:1. Proportion of Completed Screeners by Contact Attempt



The only other factor that significantly impacted the odds of a list case being screened was the socioeconomic status of the area in which the household was located, measured by the average number of rooms. Two coefficients are used, the average number of rooms and the average number of rooms squared. This technique (called a quadratic term) allows for nonlinear relationships in statistical models. A well-known nonlinear relationship is the inverse J-curve effect of education on income. Initially, education increases salary, but at very high levels (a Ph.D., for instance) marginal years of education are associated with a decrease in expected salary. A positive coefficient for average rooms and a negative coefficient for average rooms squared are indicative of an inverse J-curve effect. In other words, the list households most likely to respond are those in the middle, not those in areas with the highest or lowest socioeconomic status. Interestingly, the results of this analysis mirror the findings of other studies of the impact of socioeconomic status on nonresponse (see Chapter 4). Larger household size was associated with increased risk of refusal.

Having developed a model of nonresponse, the next step is to turn this into a poststratification weight. The statistic used for this purpose is the survivor function $S(t)$, the estimated probability of a case "surviving" via noncontact or refusal; in other words, the probability of not being surveyed. This is expressed in terms of the conditional hazard function:

$$S_i(t) = \prod_{t=0}^{T-1} \left(1 - h_i(t)\right) \tag{5.5}$$

The resulting weight is calculated by multiplying the design weight by the survival function:

$$\begin{aligned} &listwt3_i = listwt2_i S_i(t), \text{ where } i \in R \\ &listwt3_i = 0, \text{ where } i \notin R \end{aligned} \tag{5.6}$$

This means that cases that are easier to contact have a lower chance of survival and hence receive a lower weight.

*Adjustment to Number of Eligible Cases*. An additional adjustment is made to ensure that the cell count within stratum and treatment weighted by *listwt*3 sums to estimated number of eligible cases (those where *i* is an element of *R* or *N*), ensuring that the relative size of populations within geographic units are maintained while allowing the weight of cases within sample and stratum to vary by probability of response:

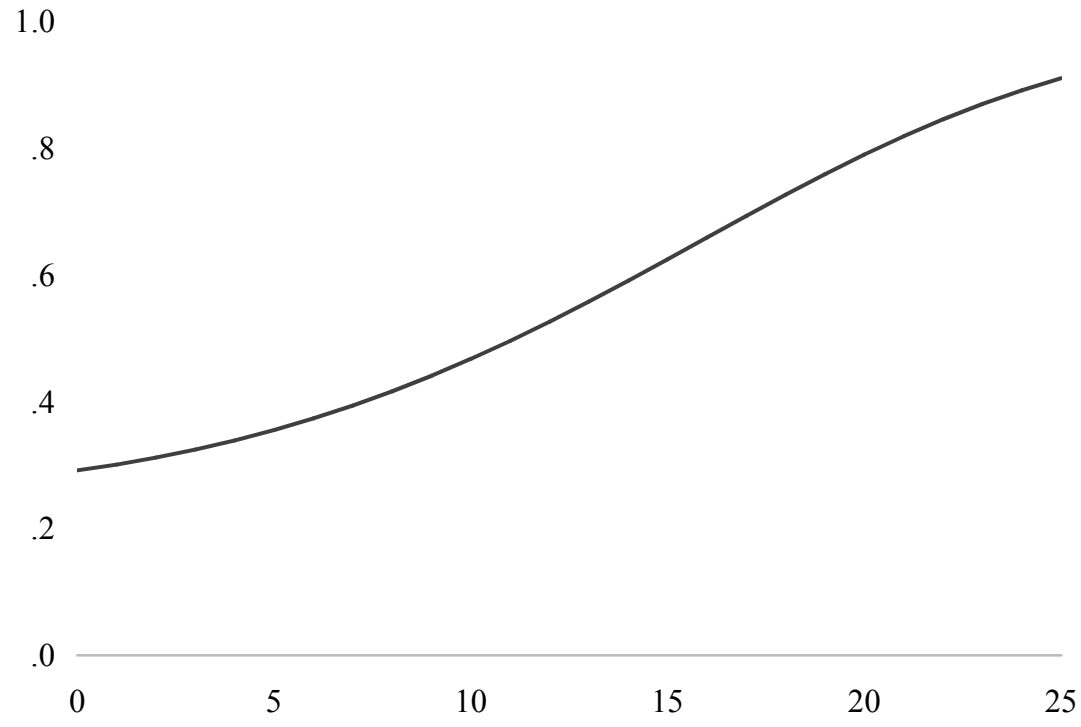
$$\begin{aligned} &listadj4_i = \frac{\sum_{i \in R} listwt2_i + \sum_{i \in N} listwt2_i}{\sum_{i \in R} listwt3_i}, \text{ where } i \in R \\ &listwt4_i = 0, \text{ where } i \notin R \end{aligned} \tag{5.7}$$

The weight is calculated as:

$$\begin{aligned} &listwt5_i = listwt4_i listadj4_i, \text{ where } i \in R \\ &listwt5_i = 0, \text{ where } i \notin R \end{aligned} \tag{5.8}$$

*Adjustment for organizational ties.* There is, however, yet another factor at work. The more organizational lists that a household in the list frame was found on, the greater the probability they would respond. This is not a function of the number of lists, because multiple entries were deduplicated, but of the household itself. Households that are members of multiple Jewish organizations are, not surprisingly, more likely to find being Jewish important in their lives and therefore are more likely to find the survey salient and to respond. Figure 5:2, below, shows this relationship.

Figure 5:2. Expected Probability of Response by Number of Organizational Ties



Accordingly, the weights must be adjusted to reduce overrepresentation of highly connected households. The number of eligible households within stratum ($s$) and number of organizational ties[2] ($o$) is calculated as:

$$\hat{n}_{so} = \sum_{i \in R} listwt2_{so} + \sum_{i \in N} listwt2_{so} \tag{5.9}$$

The poststratification adjustment for each cell within stratum and number of organizational ties is:

$$listadj5_i = \frac{\sum_{i \in R} listwt2_{soi} + \sum_{i \in N} listwt2_{soi}}{\sum_{i \in R} listwt5_{soi}} \tag{5.10}$$

The final list weight is:

$$\begin{aligned} &listwt6_i = listwt5_i\, listadj5_i, \text{ where } i \in R \\ &listwt6_i = 0, \text{ where } i \notin R \end{aligned} \tag{5.11}$$

RANDOM DIGIT DIALING SAMPLE WEIGHTS

The following discussion uses the conventional terminology used for dual-frame samples (Fuller and Burmeister 1972; Hartley 1962; 1974; Lund 1968). The list and random digit dialing frames are referred to using the subscripts $L$ and $R$. As the list and random digit

dialing frames overlap, the sample is divided into three domains, *l*, *r*, and *lr*, respectively comprising of cases found exclusively in the list frame, only within the random digit dialing frame, and in both frames (the "overlap domain"). Thus, $y = y_L + y_R$, $y_R = y_r + y_{lr}$, $y = y_l + y_{lr} + y_r$, and so on.

Once again, we begin by calculating the design weights. Table 5:3, below, shows the composition of the random digit dialing domain.

Table 5:3. RDD Domain

| | Stratum | | | |
|---|---|---|---|---|
| | 1 | 2 | 3 | Total |
| Total Active Blocks (min. 3 listed HH) * 100 | 222,100 | 448,100 | 1,406,100 | 2,076,300 |
| RDD sample size (total drawn) | 100,000 | 50,000 | 25,000 | 175,000 |
| Records removed from RDD sample: | | | | |
| - Business-flagged phone numbers | 4,432 | 2,308 | 1,171 | 7,911 |
| - Phone from list frame | 10,707 | 2,072 | 394 | 13,173 |
| - Phone outside study area | 5 | 5 | 73 | 83 |
| Estimated eligible numbers | 212,245 | 427,398 | 1,336,132 | 1,982,439 |
| RDD-only sample size | 84,856 | 45,615 | 23,362 | 153,833 |
| RDD-only records released to field (reps 1-100) | 16,991 | 9,127 | 4,679 | 30,797 |

The size of the list overlap domain is estimated as the number of list cases from random digit dialing replicates 1 to 100, representing the number of telephone numbers that would have been part of the random digit dialing domain had the sample exclusively used random digit dialing. A replicate is a self-contained sample. Replicates are a way of controlling the number of cases in the field at any one time, helping ensure that each case can be contacted (and if need be, recontacted) in a timely fashion given the number of interviewing staff available. However, the 2,612 cases in this category were only released to the field if they were selected from the list frame (i.e. list replicates 1 to 75), a total of 179 cases meeting both criteria.

Table 5:4. Overlap Domain

| | Stratum | | | |
|---|---|---|---|---|
| | 1 | 2 | 3 | Total |
| RDD-list sample size (total drawn) | 10,707 | 2,072 | 394 | 13,173 |
| List sample records in RDD reps 1-100 | 2,125 | 416 | 71 | 2,612 |
| RDD-list sample records released to field[3] | 93 | 71 | 15 | 179 |

The base weight (*rddwt*1) for cases in the random digit dialing frame is calculated as the reciprocal of the probability of selection, where *i* is the *i*th household:

$$rddwt1_{rsi} = \frac{t_{rs}\left(\frac{100B_{Rs}}{t_{Rs}}\right)}{n_{rs}} \tag{5.12}$$

where:

$B_{Rs}$ Number of 100 blocks (XXX-XXX-XXNN) with more than two listed residential telephone numbers within exchange codes (XXX-NNN-XXXX) with greater than one percent listed incidence within stratum. Where an exchange code has greater than one percent incidence in multiple strata, it was assigned to the stratum with the highest incidence within the exchange code.

$t_{Rs}$ Number of telephone numbers sampled within the *s*th stratum

$t_{rs}$ Number of sampled RDD-only telephone numbers less out-of-frame elements

$n_{Rs}$ Number of RDD-only telephone numbers released to field less out of frame elements

A slight modification of Eq. 5.12 was required for overlap cases to account for the higher probability of selection:

$$rddwt1_{lrsi} = \frac{t_{lrs}\left(\frac{100B_{Rs}}{t_{Rs}}\right)}{n_{lrs}} \quad \textbf{(5.13)}$$

where:

$t_{lrs}$ Number of sampled RDD-list telephone numbers less out-of-frame elements

$n_{lrs}$ Number of RDD-list telephone numbers released to field less out of frame elements

*Adjustment for Unknown Eligibility*

As was the case with the list weights, the base random digit dialing weights were divided into four response status categories:

1. Eligible households—Households in the study area that completed a screener interview, regardless of whether it was eligible for the Jewish instrument or not.
2. Ineligible—Cases that were ineligible for the study.
3. Eligible nonrespondents—Likely residential households where no interview was completed.
4. Eligibility unknown—Cases of unknown eligibility.

An adjustment (*rddadj*1) is made to distribute cases of unknown eligibility (category 4) proportionally across cases of known eligibility (categories 1-3) within stratum (*s*) and treatment (*t*), a total of 24 (3 x 8) cells:

$$rddadj1_{sti} = \frac{\sum_{i\in R} rddwt1_{sti} + \sum_{i\in O} rddwt1_{sti} + \sum_{i\in N} rddwt1_{sti} + \sum_{i\in U} rddwt1_{sti}}{\sum_{i\in R} rddwt1_{sti} + \sum_{i\in O} rddwt1_{sti} + \sum_{i\in N} rddwt1_{sti}} \tag{5.14}$$

As all list overlap cases were in treatments 5 or 6, to which RDD-only cases could not belong, separate calculations for overlap cases were not required. Accordingly,

$$\begin{aligned} &rddwt2_i = rddwt1_i\, rddadj1_i, \text{ where } i \notin U \\ &rddwt2_i = 0, \text{ where } i \in U \end{aligned} \tag{5.15}$$

As adjustments are calculated within strata and treatment, this procedure retains the relative probability of selection between treatments and strata.

*Adjustment for Probability of Response*

As with the list sample, the hazard rate was modeled as a function of the characteristics of the community within which a household was situated:

$$h_i(t) = \Pr(y_i = m \mid \mathbf{x}_i) = \frac{\exp(t, \mathbf{x}_i \boldsymbol{\beta}_m)}{1 + \sum_{j=2}^{J} \exp(t, \mathbf{x}_i \boldsymbol{\beta}_j)} \tag{5.16}$$

The final model is shown below in Table 5:5.

Table 5:5. Multinomial Logit Regression of RDD Household Response

| Variable | Refusal | Screened |
|---|---|---|
| Call 2 | 0.522*** | 1.079 |
| | (-3.52) | (0.53) |
| Call 3 | 0.85 | 0.998 |
| | (-0.89) | (-0.01) |
| Call 4 | 1.431 | 0.803 |
| | (-1.78) | (-1.05) |
| Call 5 | 1.139 | 0.650** |
| | (-0.72) | (-2.69) |
| Call 6 | 1.212 | 0.699 |
| | (-1.10) | (-1.44) |
| Call 7 | 0.803 | 0.610** |
| | (-1.15) | (-2.83) |
| Call 8 | 0.862 | 0.590 |
| | (-0.76) | (-1.69) |
| Call 9 | 0.930 | 0.563** |
| | (-0.36) | (-2.92) |
| Call 10 | 0.570* | 0.361*** |
| | (-2.45) | (-4.51) |
| Call 11 | 0.931 | 0.320*** |

| Variable | Refusal | Screened |
|---|---|---|
| | (-0.20) | (-4.84) |
| Call 12 | 0.538* | 0.250*** |
| | (-2.50) | (-4.58) |
| Call 13 | 0.666 | 0.339*** |
| | (-1.61) | (-4.08) |
| Call 14 | 0.754 | 0.282*** |
| | (-1.12) | (-4.15) |
| Call 15 | 0.733 | 0.366*** |
| | (-1.15) | (-3.65) |
| Call 16 | 0.490* | 0.376*** |
| | (-2.40) | (-3.27) |
| Call 17 | 1.036 | 0.294*** |
| | (-0.07) | (-3.88) |
| Call 18 | 0.66 | 0.138*** |
| | (-1.47) | (-4.22) |
| Call 19 | 0.637 | 0.202*** |
| | (-1.52) | (-3.96) |
| Call 20 | 0.72 | 0.573* |
| | (-1.15) | (-2.40) |
| Percent Jewish households | 0.972* | 0.958*** |
| | (-2.44) | (-4.42) |
| Percent Jewish households$^2$ | 1.000* | 1.001*** |
| | (-1.96) | (4.53) |
| Percent less than high school | 0.185** | 0.499 |
| | (-2.81) | (-1.27) |
| Percent employed | 1.015 | 1.068** |
| | (-1.09) | (2.96) |
| Median age | 1.027** | 0.997 |
| | (-2.78) | (-0.26) |

*Notes*: Coefficients are odds ratios. Numbers in parentheses are t-values.

* $p \leq .05$; ** $p \leq .01$; *** $p \leq .001$

As with list cases, additional contact attempts have a declining rate of effectiveness. When we consider other variables, however, a different picture appears. The proportion of Jewish households is a significant predictor of survey response, with something of a J-curve effect. Households from neighborhoods with greater proportions of Jews are more likely to respond. This may indicate that the screening approach used resonated more with Jewish than non-Jewish households. Surprisingly, areas with a high-

er level of employment were more likely to be screened. Households from areas with low levels of education were significantly less likely to refuse than were households from other areas. On the other hand, households from areas with an older population are more likely to refuse.

The estimated probability of survival for the *i*th household was:

$$S_i(t) = \prod_{t=0}^{T-1} (1 - h(t)) \tag{5.17}$$

The resulting weight was calculated as:

$$\begin{aligned} &rddwt3_i = rddwt2_i S(t)_i, \text{ where } i \in R \\ &rddwt3_i = 0, \text{ where } i \notin R \end{aligned} \tag{5.18}$$

*Adjustment to Match Stratum and Treatment Cell Size*

Because different cases received different experimental treatments that may have impacted their probability of response, it was necessary to adjust the cases within different treatment back to their original proportions. Accordingly, cell counts weighted by *rddwt*3 within stratum and treatment were adjusted to match those where *i* was an element of *N* or *R* weighted by *rddwt*2:

$$rddadj4_{sti} = \frac{\sum_{i \in R} rddwt2_{sti} + \sum_{i \in N} rddwt2_{sti}}{\sum_{i \in R} rddwt3_{sti}}, \text{ where } i \in R \tag{5.19}$$

Thus:

$$rddwt4_i = rddwt4_i\, rddadj4_i \text{, where } i \in R$$
$$rddwt4_i = 0 \text{, where } i \notin R \tag{5.20}$$

*Adjustment for Multiple Telephone Households*

An additional type of adjustment is needed for the random digit dialing frame, as households with multiple telephone lines (*t*) have a greater probability of selection. Both the random digit dialing and overlap domains are adjusted for the number of voice lines, which was capped at three:

$$rddwt5_i = \frac{rddwt4_i}{t_i} \tag{5.21}$$

*Adjustment to External Population Estimates*

Another way in which the random digit dialing frame weights differ is that the weights must be adjusted to match the estimated size of the population in the study area. Accordingly, the sum of the weights within each stratum (*s*) were adjusted to match the estimated population of households ( $\hat{N}_h$ ) in 2005 within stratum (see Appendix I for details on the procedure by which the population of households was estimated):

$$rddadj6_{si} = \frac{\hat{N}_{hs,2005}}{\sum rddwt5_{si}} \text{ where } s \leq 3 \tag{5.22}$$

Thus:

$$rddwt6_i = rddwt5_i rddadj6_i \text{, where } i \in R \qquad (5.23)$$
$$rddwt4_i = 0 \text{, where } i \notin R$$

COMBINED WEIGHTS

In contrast to most recent surveys of American Jewish populations, I follow Hartley's (Hartley 1962) method of calculating composite weights in multiple frame surveys, which takes into account the design effects of each frame (in this case the list frame *L* and the overlap domain *lr*) to provide the most efficient estimates for a given variable or variables (different ratios between the list and random digit dialing frames are most efficient for different variables). This is calculated as:

$$hhwt1_i = listwt4_i \lambda \text{ if } i \in l$$
$$hhwt1_i = rddwt5_i (1-\lambda) \text{ if } i \in lr \qquad (5.24)$$
$$hhwt1_i = rddwt5_i \text{ if } i \in r$$

The parameter λ is estimated as:

$$\lambda = \frac{\sum_i \lambda_i \left( \frac{n_{lr}}{deff_{y_{lr}}} + \frac{n_L}{deff_{y_L}} \right)}{\sum_i \left( \frac{n_{lr}}{deff_{y_{lr}}} + \frac{n_L}{deff_{y_L}} \right)} \tag{5.25}$$

where $y$ is a variable of interest, $deff_y$ is the design effect of the $y$th estimated proportion in $L$ or $lr$, and:

$$\lambda_y = \frac{\sigma^2_{y_{lr}}}{\sigma^2_{y_L} + \sigma^2_{y_{lr}}} \tag{5.26}$$

where $\sigma^2_y$ is the variance of a proportion. The variables of interest were used in the calculation of $\lambda$ are listed in Table 5:6, below. They were chosen to include a wide range of topics which were seen as important by Combined Jewish Philanthropies.

Table 5:6. Variables Used in the Calculation of Composite Weights

| Variable | Description |
|---|---|
| *rjew1* | Respondent raised Jewish |
| *rjew1r* | Respondent raised Jewish by religion |
| *cjew1* | Respondent currently Jewish |
| *cjew1r* | Respondent currently Jewish by religion |
| *momjew* | Respondent's mother Jewish |
| *dadjew* | Respondent's father Jewish |
| *jewproud* | I am proud to be a Jew (strongly agree) |
| *jewguide* | Look to Judaism for guidance (strongly agree) |

| Variable | Description |
|---|---|
| *jewimp* | Importance of being Jewish (extremely important) |
| *jdenrs1* | Respondent Jewish denomination raised (Conservative) |
| *rsdenom* | Respondent Jewish denomination (Conservative) |
| *jrjewedu* | Respondent received Jewish education grades 1-8 |
| *hsjewedu* | Respondent received Jewish education grades 9-12 |
| *welcome* | Jewish community very welcoming to intermarried families |
| *schl2* | Focal child currently attends Jewish day school |
| *ythgpnow* | Focal child current belongs to Jewish youth group |
| *fchjcamp* | Focal child ever attended Jewish camp |
| *isattach* | Respondent has very strong attachment to Israel |
| *isevents* | Respondent follows events in Israel very closely |
| *belsynog* | Household belongs to synagogue |
| *jbelong* | Respondent strongly agrees sense of belonging to the Jewish people |
| *jcare* | Respondent strongly agrees important to care for other Jews |
| *jseeself* | Respondent strongly agrees that being Jewish is important to the way sees self |
| *atndsrvc* | Respondent attends Jewish services weekly or more |
| *sedernw* | Respondent always holds or attends a Passover seder |
| *shabcnnw* | Respondent always lights shabbat candles |
| *volwrkj* | Respondent performs volunteer work for Jewish organizations over past year |
| *givecjp* | Household donated to CJP over past year |

## ADJUSTMENT TO CENSUS PROJECTIONS

Although the Census of Population and Housing does not obtain data on religious or ethnic identity that could provide weighting targets, by virtue of its probability sample, the Boston Jewish Community Study is a universal sample of towns and cities in strata 1 to 3. Accordingly, survey estimates of the characteristics of the population in these communities—Jewish and non-Jewish—should match census estimates. While there were no major inconsistencies between survey and census estimates that would indicate a high degree of bias, estimates of the age, racial, gender, and educational composition of the study area nevertheless remain. These differences were adjusted for via raking ratio esti-

mation, which iteratively adjusts sample weights until the target marginal frequencies of the specified variables are reached (Deming 1943; Deming and Stephan 1940).

Due to limitations of Census data, which do not provide tabulations of the number households with persons with a specific characteristic (e.g., the number of households with three female residents), it proved impossible to adjust for socioeconomic characteristics at the household level. The dataset was transformed from the household level (where each household is a single case) to the individual level (where every household member is a case), and weighting adjustments were calculated at the individual level.

A four dimensional raking procedure was used with the dimensions being age in 11 categories (0-9 years of age, 10-17, 18-21, 22-24, 25-29, 30-39, 40-49, 50-59, 60-64, 65-69, and age 70 and above), gender in two categories (male and female), race in four categories (white, black, Hispanic, and other), and education in three categories (less than high school, high school, and greater than high school).[4] Rim targets were calculated using 2000 Census data for towns and cities in Strata 1 to 3. The indices *a* (for age), *r* (for race), *g* (for gender), and *e* (for education) denote the dimensions, each of which contains the aforementioned number of categories.

The initial calculation uses *hhwt*1 (referred to as $W_i^{(0)}$ for convenience) and applies a sequence of weight adjustments as follows:

$$
\begin{aligned}
W_i^{(1,1)} &= \left( \frac{p_a}{\widetilde{p}_a^{(0)}} \right) W_i^{(0)}; i \in a \\
W_i^{(1,2)} &= \left( \frac{p_g}{\widetilde{p}_a^{(1,1)}} \right) W_i^{(1,1)}; i \in g \\
W_i^{(1,3)} &= \left( \frac{p_r}{\widetilde{p}_a^{(1,2)}} \right) W_i^{(1,2)}; i \in r \\
W_i^{(1,4)} &= \left( \frac{p_e}{\widetilde{p}_a^{(1,3)}} \right) W_i^{(1,3)}; i \in e
\end{aligned}
\tag{5.27}
$$

Note that $\widetilde{p}^{(0)}$ is an estimate of the cell proportion using the initial weights $W_i^{(0)}$. It is calculated as the weighted sum of cells within a category of a dimension contained within all dimensions.

$$
\widetilde{p}_a^{(0)} = \frac{\sum_{1}^{(2*4*3)} n_{agre}}{n} \tag{5.28}
$$

In this instance, this the estimated proportion of cases where *a*=1 (i.e. age 0-9) is the number of cases where *a*=1 within the four dimensions of age (*a*), gender (*g*), race (*r*), and education (*e*) divided by the sample size. In other words, this would be the total number of cases where age is zero to nine, divided by the total number of cases.

Using a simple example where the only dimensions are age (columns) and gender (rows), the expected proportion of cases would be equal to:

$$\frac{\sum_{g=1}^{2} n_{ag}}{n}$$

which is just the number of males aged zero to nine plus the number of females zero to nine divided by the number of all cases of either gender and any age.

Returning to Equation 5.28, the weight assigned to each category within each dimension adjusts the proportion of cases in the sample with a given characteristic of a dimension (e.g., less than high school education) to the expected value derived from Census data. Successive steps will, however, affect the weighted distribution of values for other dimensions. Accordingly, successive iterations ($t$) of through all dimensions are required.

$$\begin{aligned}
W_i^{(t,1)} &= \left( \frac{p_a}{\tilde{p}_a^{(t-1,4)}} \right) W_i^{(t-1,4)}; i \in a \\
W_i^{(t,2)} &= \left( \frac{p_g}{\tilde{p}_a^{(t,1)}} \right) W_i^{(t,1)}; i \in g \\
W_i^{(t,3)} &= \left( \frac{p_r}{\tilde{p}_a^{(t,2)}} \right) W_i^{(t,2)}; i \in r \\
W_i^{(t,4)} &= \left( \frac{p_e}{\tilde{p}_a^{(t,3)}} \right) W_i^{(t,3)}; i \in e
\end{aligned} \tag{5.29}$$

The iterative process is completed when the absolute relative difference was less than .01 percent for all marginal population proportions.

$$
\begin{aligned}
&\left| p_a - \left( \left( \frac{\sum_{1}^{24} n_{agre}}{n} \right) W_i^{(t,3)} \right) \right| \leq .01 \\
&\left| p_g - \left( \left( \frac{\sum_{1}^{132} n_{agre}}{n} \right) W_i^{(t,3)} \right) \right| \leq .01 \\
&\left| p_r - \left( \left( \frac{\sum_{1}^{66} n_{agre}}{n} \right) W_i^{(t,3)} \right) \right| \leq .01 \\
&\left| p_e - \left( \left( \frac{\sum_{1}^{88} n_{agre}}{n} \right) W_i^{(t,3)} \right) \right| \leq .01
\end{aligned}
\tag{5.30}
$$

Recall that the data at this point are still at the individual level, while the desired weight is at the household level. In order to transform the weights, the dataset was re-structured into the household level, with the household weight (*hhwt*2) calculated as the mean of the weights of household members ($i = 1,2,\ldots,I$).

$$
hhwt2_i = \frac{\sum_{i=1}^{I} W_i^{(t,3)}}{I} \tag{5.31}
$$

While the resulting weights no longer project the precise marginal proportions for categories within the dimensions of age, gender, race, and education, they are significantly closer than the unadjusted proportions.

NONRESPONSE WEIGHTS

While the weighting adjustments *listadj*2 and *rddadj*2 correct for differential probability of response by community characteristics derived from census data, such adjustments are by nature approximate and cannot account for specifically religious and ethnic factors. The extensive screener was designed with this in mind in order to permit analysis of difficult to reach cases, such as those that required multiple contacts and/or initially refused to participate in the survey. Although it is impossible to know with absolute certainty that persistent nonrespondents resembled difficult respondents, it is reasonable to suspect that considerable similarities exist. Accordingly, probability of refusal was modeled using refusal conversions as persistent refusals in a logistic regression (see Table 5:7, below). See, however, my earlier discussion of Lin and Schaeffer's (1995) rejection of this type of reasoning.

Table 5:7. Odds Ratios of Logistic Regression on Selected Variables

| | Odds ratio |
|---|---|
| Number of organizations | 0.548*** |
| | (-5.23) |
| Number of organizations$^2$ | 1.05*** |
| | (4.41) |
| Orthodox or Conservative | 1.88** |
| | (2.57) |
| Household level of education | 1.20** |
| | (2.60) |
| Number of household members | 0.414* |
| | (-2.17) |
| Household members$^2$ | 1.17** |
| | (2.57) |
| *F* | 7.06 |
| D.F. | 4,594 |

*Note:* Figures in parentheses are t-values.

A logistic regression is used to model variables which have two discrete outcomes, in this case initial compliance or initial refusal. As with the multinomial logit analyses previously, coefficients are in odds ratios. More highly educated households were more likely to refuse to participate, as were households that belong to many organizations (there was a J-curve), Orthodox and Conservative households (no significant difference existed between the coefficients), and households with more members (though again, there was a J-curve).

The estimated probability of refusal was calculated as:

$$\Pr(y=1 \mid \mathbf{x}) = \frac{1}{1+\exp(-\mathbf{x}_i\boldsymbol{\beta})} \tag{5.32}$$

Cases were weighted by the inverse of the predicted probability of refusal, assigning the greatest weights to the cases estimated to be the most likely to refuse.

$$hhadj2_i = \frac{1}{\Pr(y=1 \mid \mathbf{x})} \tag{5.33}$$

Household weights adjusted for estimated probability of refusal were calculated as follows:

$$hhwt3_i = hhwt2_i hhadj2_i \tag{5.34}$$

In order to ensure that the estimated population total was correct, the weighted population estimate was adjusted to the match the estimated number of households (*h*) in the study area (see Appendix I for further details):

$$hhadj3_i = \frac{\hat{N}_{h,2005}}{\sum hhwt3_i} \tag{5.35}$$

The final weight for screener households for the *s*th sampling stratum was:

$$\begin{aligned} &hhwt4_i = hhwt4_i\, hhadj4_i, \text{ where } s \leq 3 \\ &hhwt4_i = 0, \text{ where } s = 4 \end{aligned} \tag{5.36}$$

MAIN WEIGHTS

As with any survey that randomly selects a respondent after a series of screening questions, the Boston Jewish Population Study suffered from drop-out—cases where the selected respondent was unable or unwilling to be interviewed (or, after substitution of the initial informant for the respondent was introduced, the informant refused to be interviewed further). As Kadushin et al. (2005) showed with respect to the National Jewish Population Survey 2000-01, drop-out tends to be greater among specific subpopulations, particularly the intermarried. Accordingly, it was necessary to adjust the household weights for variables in the main instrument to account for differential probability of response.

To account for eligible households that dropped out between the screener and main interview, main instrument weights had to be developed that would still sum to the total number of eligible households. Extensive modeling found that number of organizational ties, current denomination, Jewish incidence of area of residence, and number of adults in the household were significantly related to probability of drop-out. Informant intermarriage, a hypothesized cause for screener to main dropout, was not found to be significant once these variables were controlled for.

Rim weights were calculated for these variables following the procedure outlined regarding adjustment to census estimates on pp. 324-328 above, with the weighting targets equal to the marginal distributions for organizational ties, denomination, incidence, and number of adults in the household for all eligible screened households (those with one or more adults who had a Jewish parent, were raised Jewish, or were currently Jewish). The adjusted weight ($hhwt5_i$) was equal to the $t$th weight of the raking procedure. As was the case with the screener household weights, an additional adjustment was necessary to ensure the sum of the household weights was equal to the estimated number of eligible households (*jewhh*).

$$hhadj5_i = \frac{\sum_{i \in jewhh}^{i} hhwt4_i}{\sum_{i}^{n} hhwt5_i} \tag{5.37}$$

Thus:

$$hhwt6_i = hhwt5_i hhadj5_i \tag{5.38}$$

WEIGHT TRIMMING

After applying both design and post-stratification weights, it became apparent that the high variation in probability of selection inherent in the study design had caused certain cases to have extremely large weights. In the case of the final eligible household weight, the maximum/minimum ratio was 4,600:1. These large weights were essential to obtain correct population figures and correct for overall bias, but made the data highly sensitive to individual observations, and contributed to high variance and unacceptably large confidence intervals for certain variables. It became obvious that some sort of "trimming" of the larger weights was necessary to reduce variance and decrease the importance of individual cases. The most basic method of weight trimming, and the first one attempted, was to simply place an overall ceiling on the weights, preventing any weight from exceeding a given value. The initial attempt to apply this method to the weights involved setting a ceiling such that the overall maximum/minimum ratio would not exceed 10:1. However, while this method did reduce variance, it was found to introduce large amounts of bias into the sample by disproportionally downweighting those with less Jewish affiliation. Because virtually all the large weights were a result of the low probability of selection in the random digit dialing frame, placing a ceiling on the weights biased the sample towards the list frame, which was substantially more "Jewish." It became apparent that any

sort of weight trimming algorithm would have to be a compromise between decreased variance and decreased bias.

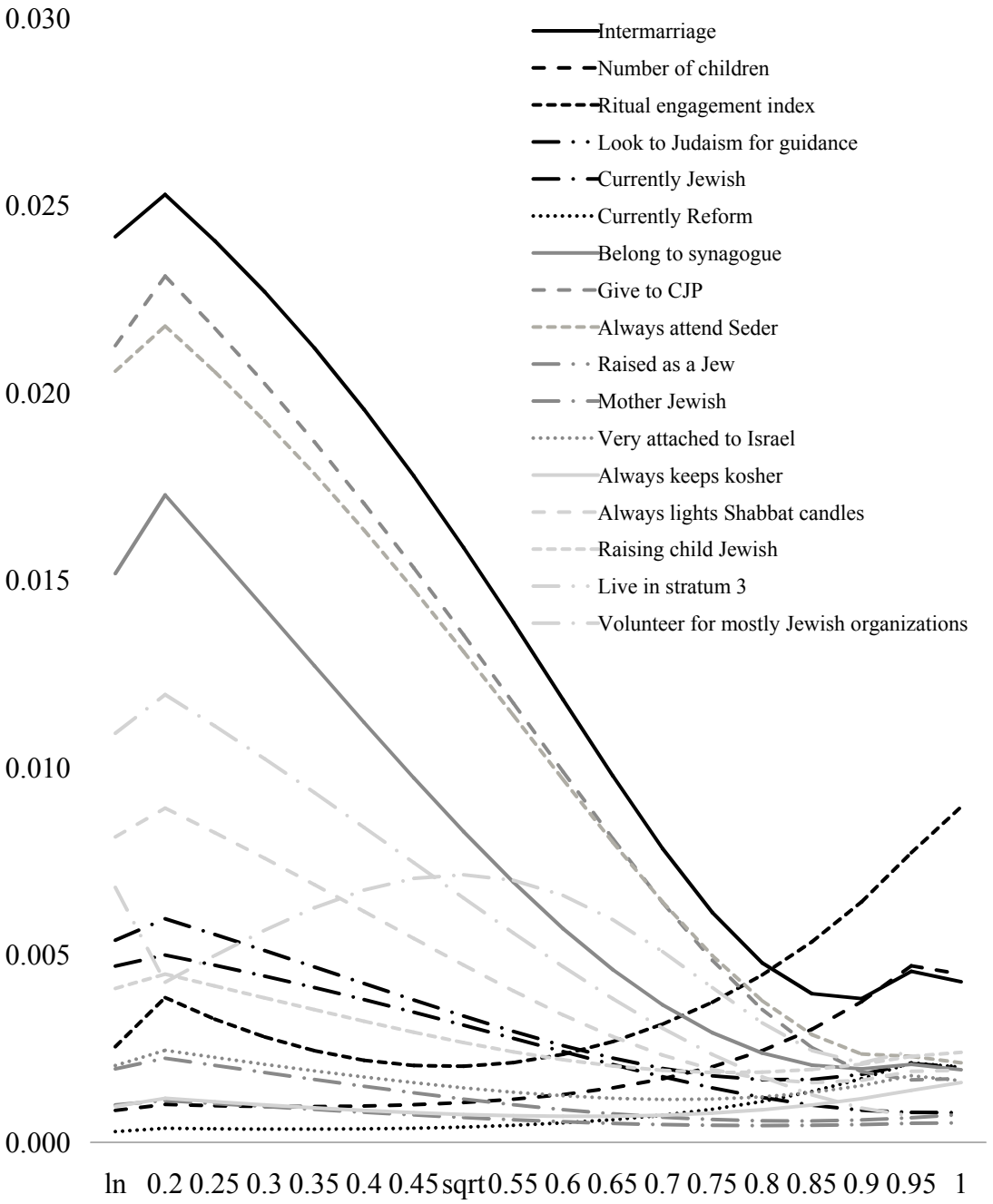
It was therefore necessary to construct an algorithm that would decrease the absolute size of the largest weights, while still maintaining the distinctiveness between two weights of different sizes. This could be accomplished by applying a logarithmic or fractional-power transformation to either all of some of the weights. Such a transformation would dramatically lower the very highest weights, but have a minimal effect on the lower ones. In addition, the ordinal relationship between two weights of different sizes would still be maintained, even though both might be dramatically diminished in magnitude. However, there was concern that the final choice of an algorithm would still be arbitrary. Christopher Winship therefore suggested an empirical procedure in which the weights would be transformed by a series of powers ranging from a natural logarithm exponent to increasingly large fractions for a number of different dependent variables. For each variable the mean standard error (*MSE*) for that variable can be calculated across each type of weighting transformation using the formula below:

$$MSE_{wx} = (SE_{wx})^2 + (\bar{x}_{ov} - \bar{x}_w)^2 \tag{5.39}$$

That is, the mean standard error for a given variable ($x$) and weight ($w$) is the squared standard error (*SE*) for that variable and weight, plus the squared difference between the estimated mean of the variable ($\bar{x}$) using the untrimmed weights and that of the given weight. The resulting measure will thus reflect some estimate of the trade-off between high variance and high bias. In order to identify the transformation that would

minimize the mean standard error across variables, the mean standard error of key variables was plotted by strength of transformation (Figure 5:3).

Figure 5:3. Mean Standard Errors by Weight Transformation



The inflection point for most variables appeared to be .85. Accordingly that value seemed to be the appropriate trimming coefficient.

The final household weight was therefore calculated as:

$$hhwt7_i = hhwt6_i^{.85} \tag{5.40}$$

## INDIVIDUAL WEIGHTS

Weights computed for households, however, were not appropriate for questions that are asked of a randomly selected household member (e.g., attitudinal questions asked of the respondent), so additional weights were computed with respect to the screener informant, the respondent to the main survey, and the household member who is the subject of the focal child section. Each of these weights was derived in a similar fashion, equaling the household weight multiplied by the number persons eligible for selection.

In addition to attitudinal items asked of a single household member, individual weights can be used to generate approximately accurate estimates of characteristics of household members from whose ranks an individual was selected.

*Respondent Weight*

The respondent weight was calculated as the number of adults eligible for selection as the respondent (*numelrs*), namely those born Jewish, raised Jewish, or currently Jewish, multiplied by the household weight:

$$rswt1_i = hhwt7_i\, numelgrs_i \tag{5.41}$$

In contrast to the informant weight, the respondent weight was adjusted for systematic differences between the distribution of respondents and the distribution of eligible household members on age, gender, denomination, and highest degree via rim weighting. This was necessary because a nonrandom procedure was instituted in cases where the initial informant refused to transfer to the selected respondent. Weighting targets were set as:

$$p_x = \frac{\sum_{ij}^{nJ} x_{ij}\, hhwt6_i}{nJ} \tag{5.42}$$

where $x$ is the variable of interest and there are $n$ ($i$ = 1,2,…,$n$) households containing $J$ ($j$ = 1,2,…,$J$) adults eligible for selection as the respondent. (See pp. 324-328 for a detailed description of a rim weighting procedure.) In other words, the weighted number of eligible adults (who are either current Jews, were raised Jewish or had a Jewish parent) with the $x$th characteristic of interest divided by the total number of eligible adults in

survey households. The adjusted respondent weight was equivalent to the *t*th weight of the raking procedure. Thus:

$$\begin{aligned} rswt2_i &= \sum_{i=1}^{n} W_i^{(t,3)} \text{ where target variables non-missing} \\ rswt2_i &= rswt1_i \text{ where target variable missing} \end{aligned} \tag{5.43}$$

*Focal Child Weight*

In order to reduce the burden on respondents in households with multiple children, detailed questions on education and ethno-religious upbringing were only asked of a randomly selected child. The selection procedure was a random draw from all children in the household, regardless of whether they were being raised as Jews. Focal child weights were calculated as the product of household weights and number of children in the household (*nkids*):

$$fchwt1_i = hhwt6_i nkids_i \tag{5.44}$$

USING WEIGHTS IN SURVEYS OF AMERICAN JEWS

The weighting procedure described in this chapter is elaborate by any virtually any standard, especially those of Jewish community studies. Is it really necessary? Unfortunately, the answer is affirmative. Weighting Jewish population studies is particularly difficult given the lack of external data. This leaves two options: assuming that the households

that did not respond are the same as those that were interviewed or extrapolating from hard to reach cases. As the nonresponse analyses have shown, significant differences did exist between respondents and nonrespondents. While it is unlikely that the poststratification weights were perfect, they almost certainly reduced the overall level of bias.

## ENDNOTES

[1] Variables used were as follows [bracketed figures designate Census summary file (SF1 or SF3) and table number]: percentage white alone (non-Hispanic) [SF1 P004005], percentage black alone (non-Hispanic) [SF1 P004006], percentage Hispanic (any race) [SF1 P004002], percentage other (non-Hispanic) [net of SF1 P4], median age [SF1 P13], average household size [SF1 P17], linguistic isolation [SF3 P20], educational attainment divided into less than high school, high school, college graduate, and postgraduate [SF3 P37], employment status [SF3 P43], median household income [SF3 P53], percentage below poverty level [SF3 P87], median number of rooms [SF3 H24], mean number of bedrooms [SF3 H41], and mean vehicles available [SF3 H48].

[2] The number of organizational ties are arranged in five categories of respectively 1, 2, 3-4, 5-9, and 10+ ties. In order to keep cell sizes above 20, categories four to five were collapsed in the low density stratum and three to five in the very low density stratum.

[3] List replicates 1 to 75 and random digit dialing replicates 1 to 100. Stratification is determined by the stratum to which the case's telephone exchange code was assigned, which differs in 6.4 percent of cases from the stratum determined by the street address.

[4] All individuals under the age of 25 years were classified as having less than high school education due to the limitations of the available Census data.

# Chapter 6

## Evaluating and Improving Surveys of American Jews

The number of souls has been a key indicator of communal vitality since the Biblical era. Regardless of its historicity, Num. 1:1-3 shows a keen interest in enumerating the Israelite fighting force and provides a detailed and workable methodology for doing so. Similar interests have moved American Jews across the years to number themselves and, later, understand their characteristics. Answering the simple question—How many American Jews are there?—has, however, proved to be inordinately complicated. The U.S. Census, the basic source for information on the American population, has never asked about religious identity and excludes Jewishness as an option from recent questions about ancestry. Counts of the Jewish population, then, had to be organized by American Jews themselves. Lacking a list of all American Jews or the wherewithal to mount a census of all Americans, Jews interested in knowing their strength in numbers had to fall back on the means available to them at the time. Methods for studying populations, however, have been transformed again and again across the previous century as technology and methodology have improved and as changes in the social and technological environment have made previous methods obsolete. For subpopulations like American Jews, the pressure of

social change has been twofold. Not only is the adequacy of a given method affected by its degree of fit with the general social and technological environment, but it must also be appropriate for the subpopulation. The dual pressure of the general environment and the specifics of the Jewish community have made the history of American Jewish population studies one of near permanent—though ever varying—problems and continual dissatisfaction. It is this history that I have explored.

## INSTITUTIONAL CENSUSES

The first attempt at counting something like the American Jewish population was, ironically, conducted by the U.S. Census Bureau. While the Census of Population and Households has never asked about religion, in 1850 the assistant marshals charged with collecting information from households were saddled with additional task of collecting information on a variety of other topics from other sources. Foremost among these were farms and business establishments (which went on to become the Census of Agriculture and the Economic Census respectively), but also schools and libraries. In addition, the assistant marshals were to collect various data on churches and other places of worship (later to be known as the Census of Religious Bodies). Consistent with the other supplementary topics, the collection of data on religion was a census of establishments—corporate entities rather than individuals. In keeping with this focus, the questions focused on the value of church property (and churches it was, as the expectations and definitions were grounded on Protestant expectations) and the number of pews. It was not until 1890 that the Census of Religious Bodies included a question on membership. These

statistics had numerous problems. Some synagogues known to exist at the time were excluded, likely because they did not match assistant marshals' expectations of the public and permanent character included in the definition of a church. Counts of pews carried additional problems. Some Jews, even then, did not deign to enter a synagogue. It is not known whether women's sections were included and children, of course, likely ran about as ever, unwilling to sit still and probably not provided for in any case. The annual ritual of overcrowding on the High Holidays was not unknown, with the overflow presumably being left standing or on temporary seats. In other cases, it was quite possible that some synagogues were built at a time when a Jewish community was larger, or with perhaps greater optimism than was warranted, and the pews were never entirely filled. Later censuses asked about the number of "communicants," but here too lay problems. Only the head of the family was counted in synagogues. Orthodox synagogues had few full members but many who bought seats, the less expensive alternative, and it is not clear whether the latter group were counted. The overall number of synagogues in 1890, when the number of communicants was first tabulated, was suspiciously close to that of previous decades, and likely excluded the *landsmanschaften* (relatively informal immigrant societies of migrants from a particular town or region, often providing for worship as well as mutual assistance and social gatherings) of the new generation of migrants. The definition was revised again in 1916, with "communicant" abandoned in favor of "member," as defined by the reporting congregation. Some women were included, but likely were disproportionately reported by non-Orthodox congregations and the status of children is not known. The Jewish enumerators in the final two censuses of religious bodies of 1926 and

1936 forewent congregations altogether, to the displeasure of the Census Bureau, estimating with questionable accuracy the size of the entire Jewish population.

The first Jewish attempt at enumeration mirrored these efforts. The 1880 *Statistics of Jews of the United* States, sponsored by the Board of Delegates of American Israelites and the Union of American Hebrew Congregations, followed the practices of the Census Bureau, albeit with more diligence, including estimates of the membership of various Jewish organizations besides congregations. The 1880 institution census shared the lack of clarity regarding what constituted membership of its model and suffered from the refusal of certain congregations (most notably in Baltimore) to report on their members.

## GUESSTIMATES

From 1899 to 2004 the *American Jewish Year Book* published detailed counts of American Jews based on the estimates of local reporters. Similar counts were reported in the *Jewish Encyclopedia* of 1906 and the 1926 and 1936 censuses of religious bodies, each of which were prepared by the same enumerator as the *Year Book* of the time. Such counts are only as accurate as the reporters' estimates. These, unfortunately, were far from unimpeachable. Different reporters from the same community at times provided widely differing estimates. Changes in reporters led to sudden increases or decreases in counts for a specific locality for year to year.

While the *Year Book* never provided (and probably never knew) the basis of each estimate, it is likely that the overwhelming majority were more or less informed guesstimates. Even in later years, most places reporting estimates had not undertaken a popula-

tion study. The population studies themselves, as I will discuss subsequently, varied dramatically in quality. Estimates derived from population studies were sometimes modified in subsequent years according to the beliefs of the reporter regarding the population's growth or decline.

Careful readers during the *Year Book* estimates' last decades would notice that the population counts prepared by the Council of Jewish Federations and its successor, United Jewish Communities, virtually always differed from the estimates of the United States in the section on the world Jewish population prepared by Israeli demographers. The Council of Jewish Federations counts were inevitably higher than the demographic estimates, as well as those of national surveys of the Jewish population, which tended to be the lowest of all.

## EARLY COMMUNITY STUDIES

The early studies of American Jewish communities were conducted in the 1900s and 1910s and mirrored the approaches used in socially focused "surveys" of the same era. These surveys of the landscape drew on a variety of sources of data: observation, interviews with key informants (usually public officials), and interpretation of statistical information on the public records. Police lists of households, for instance, were scrutinized for Jewish names, while statistical data on areas known to have concentrations of Jews from state and federal censuses was used to understand the characteristics of the Jewish population. Notably absent from either Jewish or general surveys of the social landscape was information obtained from a systematic census or survey of inhabitants of the com-

munity. This was a result of methodology and cost. Sampling was in its infancy and had not yet been applied to large populations. Without a way to use a fraction of the population to represent the whole, the only alternative was to conduct a census, which was an expensive and time consuming undertaking.

## PROXY MEASURES

The lack of a method other than a census to enumerate the Jewish population led to the most inventive, though not particularly effective, ways of counting American Jews. While their details differ somewhat, proxy methods share a set of common assumptions. Each used some characteristic that was associated with Jewish identity for which data was readily available and multiplied it by a suitable coefficient to estimate the size of the Jewish population. Proxies used included data on the number of migrants from various countries, U.S. Census estimates of the number of native Yiddish speakers, U.S. Census estimates of the number of people with Russian ancestry, the number of children absent from public schools on Yom Kippur, people with distinctive Jewish names in telephone directories and other population lists, and numbers of Jewish funerals. The common assumptions that bound these methods together were also their undoing. The adequacy of each method depended on the accuracy of the coefficient used. In each case, a considerable number of Jews did not have the chosen characteristic and, in all but the case of native Yiddish speakers, some proportion of non-Jews also shared the characteristic. To make matters worse, in some cases an additional level of surmise was required. Estimating the number of children absent on Yom Kippur, for instance, only gave the percentage

of Jews among the school age population (assuming this could be calculated accurately). Determining the size of the entire Jewish population required an additional coefficient to inflate the school age population to the total. In every case, the paradox was that the only way to accurately compute the required coefficient or coefficients was to have nonproxy data on the Jewish population, which would have rendered the entire exercise moot. Beyond this, proxy measures were not suitable for making comparisons between areas, as the relationship between the proxy and the Jewish population would have differed. The proportion of non-Jews with distinctive Jewish names, for example, was greater in areas with high levels of non-Jewish German migration. Given their obvious flaws, it is disconcerting that proxy measures appear to have been favored most in larger communities and were used long after better methods became available. The 1968 estimate of the Jewish population of New York City, for instance, was based on the Yom Kippur absence method.

## LIST-BASED CENSUSES

Surveys gradually became more common throughout the early decades of the twentieth century, coming into their own in the 1930s when George Gallup accurately predicted not only the results of the 1936 presidential election, but the final woefully inaccurate tally of the well-known *Literary Digest* poll as well. Following this trend, the first attested surveys of American Jews date from this time. Initially, though, the Jewish studies differed from Gallup's approach by attempting to conduct a census of the entire Jewish community. These efforts succeeded or failed by how successful they were at developing a list of

all Jewish households. The best implementations of this approach developed a master list by combining the membership and/or contributor lists of scores of Jewish organizations (including synagogues) with households with distinctive Jewish names from the telephone directory, a house to house canvass of areas with the highest concentration of Jews, and finally a question asking respondents from surveyed households to list any other Jewish families they knew. While probably still missing some Jewish households, such methods were appropriate to their time. Jews were still heavily concentrated in certain urban neighborhoods and generally tied into the network of Jewish organizations through exclusion from much of general society, and drawn into a dense network of Jewish ties by both preference and external prejudice.

## UNSYSTEMATIC SURVEYS

The end of the Second World marked a critical transition in both survey research and the development of the Jewish community. Various government departments drew heavily on survey research during the war, training a generation of survey researchers as well as whetting the federal appetite for such data. While providing contracts for the early academic and commercial research organizations, the federal government also mandated the use of scientific sampling methods, hastening the adoption of improved methods. In the Jewish community, while acculturation and increasing residential dispersion were trends before the war, they hastened markedly at its end. Prejudice dropped rapidly, while the G.I. Bill enabled Jews to attend college in greater numbers and directly and indirectly provided the means for Jews to move into new mixed suburban areas. Greater acculturа-

tion and residential dispersion meant that the old ways of surveying Jewish communities rapidly diminished in effectiveness as Jews were less likely to belong to Jewish organizations, to reside in areas of dense Jewish population, and be part of a heavily intertwined web of Jewish friendships.

The first steps toward Jewish surveys actually occurred immediately prior to the Second World War, with several 1938 surveys based on samples of households drawn, at least in part, by selecting given blocks from cities. These efforts were, however, compromised by the apparent absence of weights to control for differential probabilities of selection as well as nonrandom selection procedures. These efforts were actually quite advanced for their time, as methods for drawing geographically based samples were refined during and after the Second World War. These efforts, however, were not the norm, as most surveys in the decades following the war were only of households found on master lists of the Jewish community, and would consequently have missed Jews living in areas with little Jewish population, as well as those who were not affiliated.

## UNIVERSAL SAMPLES

The first Jewish population survey to use a randomly selected geographic sample took place in 1956. Few were implemented thereafter due to the high level of expense involved. In order to estimate the Jewish population it is necessary to determine what proportion of the overall population is Jewish. This in turn necessitates conducting interviews until a Jewish sample of a given size is reached. The overall number of interviews conducted is in proportion to the incidence of Jewish households among overall

households. Were we to collect a sample of 1,000 Jewish households in an area that is 10 percent Jewish, the overall sample size would be 10,000. Were the area to be five percent Jewish, 20,000 interviews would be required. At two percent Jewish, a sample of 50,000 households would be needed. Geographic samples require in-person interviews, which are very expensive. When combined with the multiplier effect of having to find Jewish households, the costs rapidly become extreme. To help manage costs, all area-probability samples of Jewish communities have used dual-frame designs that draw part of their sample from a master list of Jewish households and part via an area-probability sample. The master list frame effectively increases the incidence of Jewish households, minimizing the number of interviews with households of unknown status, albeit at the price of greater confidence intervals around estimates.

Even using dual-frame designs, area-probability samples were expensive. By the later 1970s, as costs grew in tandem with increased concerns regarding interviewer safety, these designs became infeasible for surveying Jewish communities, especially as the Jewish population grew more dispersed and consequently more expensive to survey. These problems were not confined, however, to Jewish surveys—survey researchers in general felt the pinch of higher costs. Fortuitously, the 1970s also saw the maturation of the telephone system, with coverage exceeding 90 percent of the population, enabling survey researchers to use telephone surveys based on random digit dialing methods. Telephone surveys were much less expensive as they made more efficient use of staff time and minimized expenses for travel and accommodation.

The effect of these changes on Jewish surveys was dramatic, with a massive increase in the number of studies using universal samples that covered the entire popula-

tion. Costs were so far reduced that it was possible to undertake studies that were deliberately economically inefficient, spending disproportionate resources in low density areas in order to better understand Jews in these places. This low cost environment did not last for long. From the mid-1980s, pure random digit dialed studies were replaced by dual-frame studies that reduce costs by taking advantage of lists of Jewish organizations. Oversampling of low density areas gradually ceased, as studies sampled at an equal rate across their area and finally concentrated efforts in the areas with the most Jews.

An unfortunate accompaniment to the new random digit dialing studies was the proliferation of substandard methods. The signal study of the random digit dialing era, the National Jewish Population Survey of 1990, used a commercial omnibus survey to screen for Jewish households, leading to a low response rate for its time. The effect of the low response rate is, however, unclear. Far more egregious steps were taken in other surveys. A line of studies begun by Gary Tobin and continued by Ira Sheskin used distinctive Jewish names samples in combination with random digit dialing samples. There is no fundamental problem with using households with "Jewish" names in a dual-frame design in the place of a list frame. The errors lay in the implementation. For a dual-frame design to be valid each frame (random digit dialing and distinctive Jewish names in this case) must be separate, with elements found on one and only one frame. Tobin and Sheskin did not do so, effectively counting distinctive Jewish names households twice. To adjust for the bias introduced by this step, these researchers adopted a naïve form of weighting that successively made the distinctive Jewish names sample look like the random digit dialing sample on a number of variables. The most serious problem is that this procedure does not make up for the fact that two samples are used rather than one, and

consequently it is impossible to calculate the variance of statistics, and inferential statistics cannot be used. Without a means of understanding the confidence intervals that surround survey results, survey data is of little use. Sheskin's studies are further saddled with nonstandard response rate calculations that make it difficult to gauge survey quality.

## CONTEMPORARY CHALLENGES

The 2000s have seen the emergence of challenges as serious for telephone survey research as the 1970s posed for in-person interviews. Response rates have fallen precipitously, even after increased efforts have been made to reach households in the form of incentives, greater number of callbacks, and increased efforts at refusal conversion. More troubling still is the challenge posed by the adoption of cell phones, which are excluded from standard random digit dialing frames as a result of restrictions imposed by the Federal Communications Commission. In combination with other new technologies like caller identification, large sections of the American population are not found in random digit dialing samples. Young adults and people with low socioeconomic status are particularly at risk of undercoverage. Among households that can be reached, refusal rates have also increased. No comprehensive solution has yet been found for these difficulties.

These problems are, as ever, magnified for American Jews. The effect of increased cost per call is exacerbated by the need to contact many non-Jewish households to find each Jewish household. Understanding who a survey did not reach is complicated by the lack of a reliable source of data on American Jews beyond surveys.

The response to these challenges for Jewish surveys has been less than overwhelming. Oversamples of high density areas have increased in an attempt to minimize costs, but the problems of nonresponse and noncoverage have not been addressed. The best known of the contemporary Jewish population studies—the National Jewish Population Survey (NJPS) of 2000-01—is emblematic of these problems. Despite the known difficulties of the survey research environment, the study made practically no efforts to increase response rates. Standard strategies like prenotification letters and preincentives were not used and little effort was made to understand the effect of the survey introduction and screening questions in advance. NJPS 2000-01 also suffered from poor management, with a badly structured contract with the survey research contractor and serious errors by the contractor combining to balloon the survey's cost to an enormous $6 million. Technical decisions made by the study's authors also interacted to ill effect. The decision to ask a limited set of questions asked of households that were not classified as Jewish backfired as a result of the decision to use a definition of Jewish identity that differed from that used in NJPS 1990 and made little sense altogether. The result was a survey that was limited in key respects. Certain variables could not be analyzed due to errors by the survey contractor or by the decision not to ask them of people who were not initially classified as Jews. Serious biases exist in the survey's sample of young adults, which is heavily tilted toward Orthodox Jews, and for households with both Jews and non-Jews, which were disproportionately likely to drop out. The study achieved an extremely low response rate, which casts further suspicion on its findings.

While many of the difficulties experienced by NJPS 2000-01 were unique, the history of Jewish population studies suggests that their presence is part of a longer trend. Baldly put, Jewish population studies have often been conducted with little or any regard to their accuracy. The need to have some data all too often trumped considerations of whether that information was accurate. In its early days, the *American Jewish Year Book* had little choice but to rely on the estimates of local reporters, but as knowledge of research methods grew and evidence of the problems with these guesstimates mounted, the estimates remained there in rank upon serried rank, column after column, in full misleading precision: 150 Jews in Fayetteville, 2,650 in Tulsa, 49,500 in San Francisco, 227,300 in the Boston Metropolitan region, 314,500 in Manhattan, and so on (all estimates from 2001).

Estimates by proxy methods were known to have insurmountable difficulties when they were computed, but were the method of choice of the largest communities. They lasted longest in the largest Jewish community of all, with an estimate of the Jewish population of New York in 1968 based upon the Yom Kippur absence method.

Small to medium sized communities prior to the Second World War adopted the welcome approach of actually surveying their members, rather than inferring their number and characteristics. As these studies were not based on universal samples, their adequacy was determined by the level of coverage. While the first such studies had a carefully constructed master-list developed from many sources, later ones progressively omitted elements of the master-list. Such studies remained in use long after social conditions in the Jewish community ceased to be appropriate, and persist to the present day.

The advent of random digit dialing dramatically increased the number of studies based on universal samples. However, quality varied greatly. Tobin and Sheskin have each been extremely prolific, but very few of their studies have used satisfactory methods. The most prominent representatives of the genre, NJPS 1990 and NJPS 2000-01, each had serious questions. Other studies that used appropriate sampling methods at times stumbled with respect to other aspects of the process, like understanding nonresponse and noncoverage, which can lead to major misestimates.

Clarity about the methods and shortcomings of surveys has often been neglected in surveys of Jewish populations. The case of NJPS 2000-01 is instructive. While today the survey is extraordinarily well documented, this was the result of considerable external pressure rather than a basic operating principle. Prominent local studies have faced similar questions, sometimes resulting in clear documentation, others in the resignation of members of the technical advisory panel. The lack of clarity about the shortcomings of surveys is deeply unfortunate given their place in determining communal policy. While uncertainty may be anathema to planners who want solid facts to base their decisions on, it is an inescapable result of the probabilistic process of survey research. Failure to understand uncertainties and biases can lead to misinformed policy decisions.

## IMPROVING SURVEYS OF AMERICAN JEWS

It was with these problems in mind that the Boston Jewish Community Study was developed. The study tested new approaches to sampling, directly compared the very different national and local approaches to screening for Jewish households, used prenotification

and incentives in an attempt to improve the response rate and reduce bias, and included a detailed analysis of bias.

*Sampling*

The most important aspect of a survey is its sample, as it affects the cost of the survey, the accuracy of its estimates, and is a significant source of potential bias. These factors apply all the more to surveys of Jewish communities, which are particularly sensitive to cost. It is surprising, then, that Jewish surveys have not taken advantage of advances in sampling methods for rare populations. In the case of many studies conducted by Sheskin and Tobin, as well as some others, the samples used are actually biased. In other cases, such as those by Ukeles and Associates, the samples are unbiased but the weighting scheme is needlessly inefficient, not following standard methods for dual-frame designs. National studies, by virtue of their pure random digit dialing sampling schemes, have foregone an important opportunity to reduce costs by using a dual-frame sample based on lists or distinctive Jewish names. Common approaches to minimizing cost and variance of studies of rare populations include multiplicity and network samples, as well as formal attempts to optimize the allocation of a sample across various strata and/or frames based on prior knowledge of the distribution of the rare population.

The Boston Jewish Community Study tested two such approaches. One was to return to an approach used in the first surveys of American Jews by assembling a master-list of known Jewish households from 85 lists of various Jewish organizations. The other was to implement a multiplicity sample that asked respondents for details regarding first order relatives, and selected one that lived in the study area for further interviewing. In

both cases, the rationale was to minimize reliance on expensive random digit dialing by identifying as many Jewish households as possible using other methods.

The multiplicity sample experienced considerable problems, as few people asked to provide contact information for a parent, sibling, or adult child in fact did so, a necessary step for interviewing that person. Further, the questions required to determine the number of eligible family members, which were necessary to determine the probabilities of selection, were particularly long-winded. The response rate among people whose contact information was provided was tantalizingly high, but even complete cooperation would have been too little to ensure a valid sample given the limited cooperation received when asking for contact information.

The expanded list frame had very different results. It roughly doubled the level of coverage of the list frame compared to the conventional approach of using a Federation list alone. Even this was still far short of a majority, accounting for roughly a third of Jewish households. Greater success would, however, have had unintended consequences. While greater levels of coverage by a list frame are desirable, allowing researchers to reduce their reliance on random digit dialing, it is possible to have too much of a good thing. As the level of coverage by the list frame increases, Jewish households in the random digit dialing frame become increasingly scarce, and thus more expensive to find. As useful as it would be, there is no optimal level of coverage for all surveys. It depends on the relative costs of interviewing Jewish households on the list and random digit dialing frames and the overall size and distribution of Jews in the population at large. An additional wrinkle is that every list added increases the cost of developing the list frame.

While it is not possible to find an optimum approach for all situations, it is possible to determine the ideal balance between list and universal frames in a specific situation. To examine this, I used methodological data from the Boston study to simulate a variety of situations ranging from a random digit dialing only study to a list frame composed of a Federation list only to the full 85 lists actually used. In this situation, the best approach appeared to be to use the 10 or so largest organizational lists. Too many more, and the costs of collecting additional lists outweighed the added efficiency they brought to the sample. In a community where the lists covered a significantly greater proportion of the Jewish population, the paradoxical results of success would be felt and the list frame would effectively price the random digit dialing frame out of the market.

It is also possible to optimize the allocation of cases between sampling frames and strata, achieving the lowest level of variance for a given statistic at a specified cost. Doing so requires the researcher to make educated guesses about the distribution of the Jewish population across strata and frames, as well as differences in the cost of interviewing. I compared the results of cost-variance optimization to the *a priori* survey design that was actually used in the Boston study. The result was a large reduction in variance for the estimate of Jewish incidence. Similar reductions in variance were seen for estimates of the intermarriage rate, which the design was not optimized for. As there is little external data available on Jewish populations, one challenge is to obtain data on the distribution of the Jewish population in advance of the survey. The extended list frame was in fact a quite accurate predictor of Jewish incidence and it is possible that counts of households with distinctive Jewish names could also be adapted for this purpose. As with any procedure based on educated guesses, it is possible that inaccurate assessments may cause

additional problems. However, survey designs are already based on researchers' educated guesses about the distribution of the Jewish population. Cost-variance optimization simply systematizes the process. Moreover, the level of reduction in variance observed once the design was optimized suggests that the procedure will be quite robust.

*Introduction and Screening Questions*

One major division between national and local studies of Jewish populations lies in the nature of the introduction and screening questions used. Where national studies of Jewish populations have chosen to initially hide the Jewish sponsorship and purpose of the survey, community studies are typically upfront about both issues. A similar split applies to the screening questions. As the introduction to national studies cannot ask directly about Jewish identity without giving the game away, they ask first about religion and only later, if at all, about Jewish identity in general. By contrast, local studies go straight to the point. There are reasonable theoretical arguments to be made for either approach. Because of differences in other aspects of the design of local and national studies, it has not been possible to directly compare local and national response rates. The Boston study tested variations of each alternative in the survey's pilot phase while holding all else constant, allowing such a comparison for the first time. The local variation was more successful, measured by the overall response rate of each. The magnitude of the effect means that it is unlikely that it was a product of the Jewish respondents alone. However, as the proportion of nonrespondents that were Jews and non-Jews was not known and because the true proportions of non-Jews and Jews in the population were also unknown, it is impossible to tell which, if either, approach leads to greater levels of bias.

*Prenotification and Incentives*

Most surveys of American Jews do not notify selected households in advance about the survey and do not provide incentives in advance of the survey or on completion. Due to the overwhelming evidence in the general survey literature the Boston study did not test a no incentive or no prenotification condition. There is little reason to believe that Jewish surveys should differ from other kinds of surveys in this respect. More surprising was the equivalence of a $2 preincentive and an $18 postincentive both from the perspective of response rates and overall cost. The general literature indicates that a small preincentive should be more effective than a relatively large offer of payment for completing a survey. The cost structures of these alternatives will, however, depend on the relative size of the Jewish population. Where there are many Jewish households, a small preincentive will be more cost-effective, while in low density areas a larger conditional payment will be more efficient.

*Understanding Bias*

Researchers are increasingly coming to realize that bias endemic to surveys. While this awareness drives efforts to reduce bias through procedures like those outlined above, such efforts will never be entirely successful. The operative question is how to understand and correct for bias and effectively communicate the shortcomings of a survey. Jewish surveys have historically done a poor job of each. While there are no external data to weight Jewish surveys to, it is possible to gain some insight into bias through ancillary data. Where respondent and nonrespondent households are located can be determined with a reasonable degree of accuracy using reverse address matching or the prefix of the

telephone number. This information can then be matched with census data on the characteristics of the area. While the specific household may differ from the norm in the area, in aggregate the characteristics of areas should provide some guide to those of respondents and nonrespondents. Respondent households with characteristics associated with lower probabilities of response accordingly received greater weights, adjusting for these observed biases. Another means of understanding response bias is to use the characteristics of the hardest to reach cases to model the characteristics of those that proved impossible to reach. In the case of the Boston survey, households that initially refused but later participated were used to estimate the characteristics of persistent refusers, and corrective weights were assigned on the basis of the analysis.

Sampling bias requires a different approach. In the case of the Boston Jewish Community Study, there was a strong presumption that the sample of young adults would be biased based on the experience of NJPS 2000-01 and the general literature on the disproportionate absence of young adults from random digit dialing frames due to their use of cell phones. While there was no direct yardstick to compare estimates of this population to, detailed analysis showed the same telltale bulge of Orthodox young adults observed in NJPS that was out of line with younger and older cohorts. Accordingly, analyses of this population are curtailed in the study report and strongly worded cautions are given.

# CONCLUSION

American Jewry has a long history of using numbers to understand its vitality, originally in the form of population counts but later expanding to encompass the characteristics of the community. This has given rise to a long but uneven series of attempts to count American Jews. While laudable, all too often more attention has been paid to the need to have a number than the means used to arrive at that estimate. Questions of accuracy and the level of uncertainty surrounding an estimate are brushed aside as arcane scholarly matters. If numbers are essential for policy, as organizations sponsoring survey research suggest, this cannot stand. No responsible organization would want to plan activities using an incorrectly calculated budget. Where there are areas of uncertainty in a budget, such as the revenues to be received through gifts and the performance of investments over the coming fiscal year, appropriate steps would be taken to account for unknowns to make sure spending did not exceed sustainable levels. Survey data present an analogous scenario. In the first place, accuracy is critical—procedures that lead to systematic biases cannot be tolerated. Second, uncertainty must be minimized. Third, biases and uncertainty must be recognized, understood, and incorporated into decision making. Recognizing the weaknesses of survey research is, with no small degree of irony, essential to strengthening surveys of American Jews, knowledge of the American Jewish community, and ultimately American Jewry itself.

## Appendix A

## Census Estimates of Number of Jewish Congregations by County

| State | County | 1850 | 1860 | 1870 | 1890 |
|---|---|---|---|---|---|
| Connecticut | Hartford | 2 | 1 | | 2 |
| | New Haven | | | | 4 |
| | New London | | | | 2 |
| Massachusetts | Berkshire | | | | 1 |
| | Suffolk | 1 | 2 | | 7 |
| | Worcester | | | | 1 |
| Rhode Island | Newport | 1 | 1 | | 1 |
| | Providence | | 1 | | 4 |
| Vermont | Rutland | | | | 1 |
| New Jersey | Atlantic | | | | 1 |
| | Cumberland | | | | 2 |
| | Essex | | 1 | | 8 |
| | Hudson | | | | 3 |
| | Mercer | | | | 1 |
| | Middlesex | | | | 1 |
| | Monmouth | | | | 1 |
| | Morris | | | | 1 |
| | Passaic | | | | 3 |
| | Salem | | | | 1 |
| | Union | | | | 2 |
| New York | Albany | 1 | 2 | | 1 |
| | Chemung | | | | 2 |
| | Clinton | | | | 1 |
| | Dutchess | | | | 1 |
| | Erie | | 1 | | 4 |
| | Kings | | 1 | | 8 |
| | Livingston | 2 | | | |
| | Monroe | | 1 | | 3 |
| | Montgomery | | | | 1 |
| | New York | 5 | 13 | | 135 |
| | Niagara | | | | 1 |

| State | County | 1850 | 1860 | 1870 | 1890 |
|---|---|---|---|---|---|
| | Oneida | | | | 2 |
| | Onondaga | 1 | 2 | | 6 |
| | Orange | | | | 1 |
| | Oswego | | | | 1 |
| | Queens | | | | 1 |
| | Rensselaer | | | | 2 |
| | Richmond | | | | 1 |
| | Schenectady | | | | 1 |
| | St Lawrence | | | | 1 |
| | Ulster | | | | 3 |
| | Westchester | | | | 3 |
| Pennsylvania | Allegheny | 1 | 1 | | 3 |
| | Berks | | | | 1 |
| | Blair | | | | 1 |
| | Crawford | | | | 2 |
| | Dauphin | | | | 1 |
| | Erie | | | | 1 |
| | Lackawanna | | | | 4 |
| | Lancaster | 1 | | | 2 |
| | Luzerne | 1 | 1 | | 4 |
| | Lycoming | | | | 1 |
| | McKean | | | | 1 |
| | Montour | | | | 1 |
| | Northampton | 1 | 1 | | |
| | Philadelphia | 4 | 6 | | 9 |
| | Schuylkill | | 2 | | 1 |
| | Wayne | | 1 | | 2 |
| | York | | | | 1 |
| Illinois | Adams | | 1 | | 1 |
| | Cook | | 2 | | 17 |
| | Mclean | | | | 1 |
| | Montgomery | | | | 1 |
| | Peoria | | | | 2 |
| | Rock Island | | | | 1 |
| | Sangamon | | | | 1 |
| Indiana | Allen | | 1 | | 2 |
| | Elkhart | | | | 1 |
| | Knox | | | | 2 |
| | La Porte | | | | 1 |
| | Marion | | | | 6 |
| | Miami | | | | 1 |
| | Noble | | | | 1 |
| | Posey | | | | 1 |
| | St Joseph | | | | 1 |
| | Tippecanoe | | 1 | | 1 |

| State | County | 1850 | 1860 | 1870 | 1890 |
|---|---|---|---|---|---|
| | Vanderburgh | | | | 3 |
| | Vigo | | | | 1 |
| | Wabash | | | | 1 |
| Michigan | Bay | | | | 3 |
| | Grand Traverse | | | | 1 |
| | Kalamazoo | | | | 1 |
| | Kent | | | | 1 |
| | Wayne | | | | 4 |
| Ohio | Belmont | | | | 1 |
| | Butler | | | | 1 |
| | Clark | | | | 2 |
| | Cuyahoga | 1 | 2 | | 11 |
| | Franklin | | | | 2 |
| | Hamilton | 2 | 6 | | 6 |
| | Lucas | | | | 2 |
| | Mahoning | | | | 1 |
| | Miami | | | | 1 |
| | Montgomery | | | | 2 |
| | Muskingum | | | | 3 |
| | Scioto | | | | 1 |
| | Summit | | | | 1 |
| Wisconsin | Dane | | | | 1 |
| | La Crosse | | | | 1 |
| | Milwaukee | | 1 | | 5 |
| | Outagamie | | | | 1 |
| Iowa | Lee | | | | 1 |
| | Muscatine | | | | 1 |
| | Polk | | | | 3 |
| | Scott | | | | 1 |
| Kansas | Atchison | | | | 1 |
| | Leavenworth | | | | 2 |
| | Saline | | | | 1 |
| | Sedgwick | | | | 1 |
| | Shawnee | | | | 1 |
| Minnesota | Hennepin | | | | 2 |
| | Ramsey | | | | 3 |
| Missouri | Buchanan | | | | 2 |
| | Cole | | | | 1 |
| | Jackson | | | | 3 |
| | Livingston | | | | 2 |
| | St Louis | 1 | 2 | | 9 |
| Nebraska | Douglas | | | | 5 |
| | Lancaster | | | | 1 |
| North Dakota | Ramsey | | | | 1 |
| Virginia | Albemarle | | | | 1 |

| State | County | 1850 | 1860 | 1870 | 1890 |
|---|---|---|---|---|---|
| | Arlington/Alexandria | | | | 2 |
| | Campbell | | | | 1 |
| | Dinwiddie | | | | 1 |
| | Henrico | 1 | 2 | | 4 |
| | Norfolk | | | | 1 |
| | Ohio | | 1 | | |
| | Rockingham | | | | 1 |
| Alabama | Barbour | | | | 1 |
| | Dallas | | | | 1 |
| | Jefferson | | | | 2 |
| | Madison | | | | 1 |
| | Marengo | | | | 1 |
| | Mobile | | 1 | | 1 |
| | Montgomery | | 1 | | 1 |
| Arkansas | Garland | | | | 1 |
| | Jefferson | | | | 1 |
| | Ouachita | | | | 1 |
| | Phillips | | | | 1 |
| | Pulaski | | | | 1 |
| Florida | Duval | | | | 1 |
| Georgia | Bibb | | | | 1 |
| | Chatham | | 1 | | 3 |
| | Floyd | | | | 1 |
| | Fulton | | | | 1 |
| | Glynn | | | | 1 |
| | Muscogee | | | | 1 |
| | Richmond | | | | 1 |
| Louisiana | Caddo | | | 1 | 1 |
| | East Baton Rouge | | | | 1 |
| | Iberville | | | | 1 |
| | Orleans | 1 | 5 | 4 | 9 |
| | St Landry | | | | 1 |
| Mississippi | Adams | | | | 1 |
| | Claiborne | | | | 1 |
| | Lauderdale | | | | 1 |
| | Madison | | | | 1 |
| | Warren | | | | 1 |
| | Washington | | | | 1 |
| North Carolina | Iredell | | | | 1 |
| | New Hanover | | | | 1 |
| | Wake | | | | 1 |
| | Wayne | | | | 1 |
| South Carolina | Charleston | 2 | 2 | 2 | 3 |
| | Richland | 1 | 1 | 1 | |
| Texas | Bexar | | | | 1 |

| State | County | 1850 | 1860 | 1870 | 1890 |
|---|---|---|---|---|---|
| | Cooke | | | | 1 |
| | Dallas | | | | 1 |
| | Galveston | | | | 1 |
| | Harris | | | | 1 |
| | Harrison | | | | 1 |
| | Johnson | | | | 1 |
| | McLennan | | | | 1 |
| | Smith | | | | 1 |
| | Travis | | | | 1 |
| | Washington | | | | 1 |
| Kentucky | Daviess | | | | 1 |
| | Henderson | | | | 1 |
| | Jefferson | 1 | | | 4 |
| | McCracken | | | | 1 |
| Maryland | Allegany | | | | 1 |
| | Baltimore | | 3 | | 11 |
| Tennessee | Davidson | | | | 2 |
| | Hamilton | | | | 2 |
| | Haywood | | | | 2 |
| | Knox | | | | 1 |
| | Shelby | | | | 2 |
| West Virginia | Cabell | | | | 1 |
| | Kanawha | | | | 1 |
| | Ohio | | | | 1 |
| Colorado | Arapahoe | | | | 4 |
| | Las Animas | | | | 1 |
| | Pueblo | | | 1 | |
| Montana | Lewis and Clark | | | | 1 |
| New Mexico | San Miguel | | | | 1 |
| Utah | Salt Lake | | | | 1 |
| California | Alameda | | | | 2 |
| | Los Angeles | | | | 1 |
| | Sacramento | | 1 | 1 | 1 |
| | San Bernardino | | | 1 | |
| | San Diego | | | | 1 |
| | San Francisco | | 3 | 4 | 6 |
| | San Joaquin | | 1 | | 2 |
| | Santa Clara | | | 1 | 1 |
| | Sonoma | | | | 1 |
| Oregon | Multnomah | | | | 3 |
| Washington | Spokane | | | | 1 |
| District of Columbia | | | | 2 | 2 |

## Appendix B

## Compendium of Studies of United States Jewish Communities

| Community | Year | Methods | Reports |
|---|---|---|---|
| Maryland, MD | 1903 | Deaths<br>Key informants | Barnett (1903) |
| Worcester, MA | 1915 | Lists?<br>Key informants<br>Official data | Talamo (1915) |
| Boston, MA | 1921 | Official data<br>Key informant | Rosen (1921) |
| Bath Beach, NY | 1922 | Unknown | National Jewish Welfare Board (1922a) |
| Jersey City, NJ | 1922 | Unknown | National Jewish Welfare Board (1922e) |
| Newark, NJ | 1922 | Unknown | National Jewish Welfare Board (1922g) |
| Rochester, NY | 1922 | Unknown | National Jewish Welfare Board (1922f) |
| St. Louis, MO | 1922 | Unknown | National Jewish Welfare Board (1922d) |
| Syracuse, NY | 1922 | Unknown | National Jewish Welfare Board (1922c) |
| Detroit, MI | 1923 | Unknown | Bureau of Jewish Social Research (1923) |
| Cleveland, OH | 1924 | Unknown | Jewish Community Federation (1924) |
| New York, NY | 1928 | Deaths | Bureau of Jewish Social Research (1928a; 1928b; 1929) |
| Omaha, NE | 1929 | Unknown | Lapidus (1929) |
| Chicago, IL | 1930 | Deaths | Hauser (1938)<br>Jaffe (1943) |
| New London and Norwich, CT | 1930 | Lists | Wessel (1943) |
| Passaic, NJ | 1930 | Unknown | Levitan (1930)<br>Robison (1943b) |
| Trenton, NJ | 1930 | Unknown | Rosen (1929) |
| Chicago, IL | 1931 | Unknown | Goldberg (1934) |

| Community | Year | Methods | Reports |
|---|---|---|---|
| Detroit, MI | 1935 | Unknown | Meyer (1943) |
| Minneapolis, MN | 1936 | Lists<br>DJN?<br>House-to-house in dense areas<br>Snowball | Council of Jewish Federations and Welfare Funds (1936) |
| Trenton, NJ | 1937 | Lists<br>DJNs<br>Family members | Bureau of Jewish Social Research (1937)<br>Robison (1943c) |
| Passaic, NJ | 1937 | List<br>Limited snowball<br>Some house-to-house | Robison (1943b) |
| Buffalo, NY | 1938 | Primitive area-based sample | Engelman (1943) |
| Pittsburgh, PA | 1938 | Lists<br>Jewish children in public schools<br>Blocks from high density areas | Taylor (1941; 1943) |
| Rochester, NY | 1938 | Unknown | Phillips (1939) |
| San Francisco, CA | 1938 | Lists<br>Jewish hotel guests<br>Snowball | San Francisco Community Chest (1938)<br>Moment (1943) |
| Erie, NY | 1940 | Unknown | Cited in Seligman (1950) |
| New Orleans, LA | 1941 | Unknown | Feibelman (1941) |
| Los Angeles, CA | 1942 | Unknown | Kohs (1942) |
| Trenton, NJ | 1942 | Unknown | Bureau of Jewish Social Research and National Jewish Welfare Board (1937)<br>National Jewish Welfare Board (1937) |
| Worcester, MA | 1942 | Unknown | Mopsik (1942) |
| Lynn, MA | 1943 | Lists<br>Snowball | Cited in Kohs (1944) |
| Milwaukee, WI | 1943 | Unknown | National Jewish Welfare Board (1943) |
| Portland, ME | 1943 | Lists<br>Snowball | Cited in Kohs (1944) |
| Stamford, CT | 1943 | Lists<br>Snowball | Cited in Kohs (1944) |
| Chicago, IL | 1944? | Unknown | National Jewish Welfare Board (1947) |
| Duluth, MN | 1944 | Unknown | National Jewish Welfare Board, Jewish Welfare Federation, and Jewish Educational Center Association (1944) |
| Grand Rapids, MI | 1944 | Unknown | National Jewish Welfare Board (1945) |
| Toledo, OH | 1944 | Unknown | Cited in Seligman (1950) |
| Trenton, NJ (?) | 1944? | Unknown | Rudavksy (1944) |
| Jacksonville, FL | 1945 | Unknown | (Boxerman 1945; 1947) |

| Community | Year | Methods | Reports |
|---|---|---|---|
| Scranton, PA | 1945 | Unknown | Cited in Seligman (1950) |
| New York City, NY | 1946 | Unknown | National Jewish Welfare Board (1946) |
| Albany, NY | 1947 | Unknown | Council of Jewish Federations and Welfare Funds et al. (1947) |
| Atlanta, GA | 1947 | Unknown | Cited in Seligman (1950) |
| Chicago, IL | 1947 | Unknown | Jewish Community Centers of Chicago (1947) |
| Portland, OR | 1947 | Unknown | Cited in Seligman (1950) |
| Camden, NY | 1948 | Unknown | Council of Jewish Federations and Welfare Funds (1953a; 1954) |
| Charleston, SC | 1948 | Unknown | Cited in Seligman (1950) |
| Essex County | 1948 | Unknown | Robison (1948)<br>Jewish Community Council of Essex County (1948) |
| Indianapolis, IN | 1948 | Unknown | National Jewish Welfare Board (1948) |
| Newark, NJ | 1948 | Unknown | Cited in Seligman (1950) |
| Tucson, AZ | 1948 | Unknown | Cited in Seligman (1950) |
| Utica, NY | 1948 | Unknown | Jewish Community Council (1948) |
| Elmira, NY | 1949 | Unknown | Cited in Seligman (1950) |
| Gary, IN | 1949 | Unknown | Cited in Seligman (1950) |
| Miami, FL | 1949 | Unknown | Cited in Seligman (1950) |
| Nashville, TN | 1949 | Unknown | Jewish Community Council (1949) |
| Passaic, NJ | 1949 | Unknown | Cited in Seligman (1953) |
| Rockville Center, NY | 1949 | Unknown | Cited in (Seligman 1951a) (1951a) |
| Sacramento, CA | 1949 | Unknown | Boxerman (1949) |
| Salt Lake City, UT | 1949 | Unknown | Cited in Seligman (1951a) |
| Trenton, NJ | 1949 | Unknown | Robison (1949) |
| Los Angeles, CA | 1950 | List | Los Angeles Jewish Community Council (1951)<br>Massarik (1953) |
| Port Chester, NY | 1950 | Unknown | Council of Jewish Federations and Welfare Funds (1950) |
| Savannah, GA | 1950 | Unknown | Cited in Seligman (1953) |
| Toledo, OH | 1950 | Unknown | Jewish Community Council (1950) |
| Sioux City, IA | 1951 | Unknown | Cited in Seligman (1953) |
| New Orleans, LA | 1953 | Unknown | Council of Jewish Federations and Welfare Funds (1953b) |
| Pittsburgh, PA | 1953 | "Sampling" | Seligman (1949) |

| Community | Year | Methods | Reports |
|---|---|---|---|
| Chicago, IL | 1954 | Unknown | Rosenthal (1952)?<br>Hauser (1954)<br>Beckenstein (1959)? |
| Fall River, MA | 1954? | Unknown | Cited in Seligman and Chenkin (1954) |
| Ft. Wayne, IN | 1954? | Unknown | Cited in Seligman and Chenkin (1954) |
| Hudson, NY | 1954? | Unknown | Cited in Seligman and Chenkin (1954) |
| Jacksonville, FL | 1954? | Unknown | Jewish Community Council (1954) |
| Lancaster, PA | 1954? | Unknown | Cited in Seligman and Chenkin (1954) |
| Louisville, KY | 1954? | Unknown | Cited in Seligman and Chenkin (1954) |
| Tulsa, OK | 1954? | Unknown | Cited in Seligman and Chenkin (1954) |
| Canton, OH | 1955 | List | Jewish Community Federation of Canton (1956) |
| Lynn, MA | 1955 | Unknown | Jewish Community Federation of Greater Lynn (1956) |
| Des Moines, IA | 1956 | Unknown | The Jewish Population of Des Moines (Samuel Baum for Jewish Welfare Federation) |
| Detroit, MI | 1956 | Unknown | Mayer (1959a) |
| Washington, DC | 1956 | Area-probability List | Bigman (1957) |
| Seattle, WA | 1957 | Unknown | National Jewish Welfare Board (1957) |
| Worcester, MA | 1957 | List | Cohen (1961)<br>Worcester Jewish Federation (1961) |
| Minneapolis, MN | 1958 | Unknown | Cited in Chenkin (1963) |
| New Orleans, LA | 1958 | Unknown | Reissman (1958) |
| Chicago, IL | 1959 | Unknown | Beckenstein (1959) |
| Detroit, MI | 1959 | Unknown | Mayer (1959b) |
| Los Angeles, CA | 1959 | Unknown | Massarik (1959a) |
| Memphis, TN | 1959 | Unknown | The Southville (Memphis, Tenn.) Survey of Jewish Attitudes (Manheim S. Shapiro for American Jewish Committee) |
| San Francisco, CA | 1959 | Unknown | Massarik (1959b) |
| Dade County, FL | 1961? | Unknown | Shapiro (1961) |
| New York, NY | 1961 | Yom Kippur | Federation of Jewish Philanthropies of New York |

| Community | Year | Methods | Reports |
|---|---|---|---|
| Rochester, NY | 1961 | Unknown | Jewish Community Council of Rochester (1961) |
| South Bend-Mishawaka, IN | 1961 | Unknown | Jewish Community Council of St. Joseph County (1961) |
| Trenton, NJ | 1961 | Unknown | Sterne (1962) |
| Baltimore, MD | 1962 | Unknown | American Jewish Committee and Baltimore Jewish Council (1964) |
| Bergen Co., NJ | 1962 | Unknown | Cited in Chenkin (1963) |
| Kansas City, KS | 1962? | Unknown | Shapiro (1962) |
| Long Beach/ Lakewood/Los Alamitos, CA | 1962 | Unknown | Massarik (1963) |
| Northern Delaware, DE | 1962 | Unknown | Jewish Community Center (1963) |
| Rochester, NY | 1962 | Unknown | Cited in Chenkin (1963) |
| Baltimore, MD | 1963? | Unknown | Shapiro (1963) |
| Cook County, IL | 1963 | Unknown | Kaplan (1966) |
| Detroit, MI | 1963 | Unknown | Mayer (1964) |
| Pittsburgh, PA | 1963 | Unknown | United Jewish Federation of Pittsburgh (1963) |
| Camden, NJ | 1964 | Unknown | Westoff (1965) |
| Providence, RI | 1964 | List | Goldstein (1964) |
| Boston, MA | 1965 | Area-probability<br>Lists | Axelrod et al. (1967) |
| Detroit, MI | 1965 | Unknown | Mayer (1966) |
| Milwaukee, WI | 1965 | Unknown | Mayer (1967) |
| Philadelphia, PA | 1966 | Unknown | Federation of Jewish Agencies of Greater Philadelphia (1966) |
| Flint, MI | 1967 | List<br>DJN | Mayer (1969b) |
| Springfield, MA | 1967 | Unknown | Goldstein (1968) |
| Baltimore, MD | 1968 | Unknown | Associated Jewish Charities of Baltimore (1968) |
| Fargo, ND | 1968 | Unknown | Lazar (1968) |
| Los Angeles, CA | 1968 | Unknown | Massarik (1969) |
| New York, NY | 1968 | Yom Kippur<br>DJN | Unknown |
| Columbus, OH | 1969 | Unknown | Mayer (1969a) |
| Charleston, WV | 1970 | Unknown | B'nai Jacob Synagogue (1974) |
| North Jersey, NJ | 1970 | Unknown | Verbit (1971) |
| Oakland, CA | 1970 | Unknown | Unknown |

| Community | Year | Methods | Reports |
|---|---|---|---|
| Toledo, OH | 1970 | Unknown | Jewish Population Survey 1/70 (Harry R. Rosen in Toledo Jewish News, April 1970, p. 17) |
| Cincinnati, OH | 1971 | List | Jewish Data Bank holdings |
| Hollywood, CA | 1971 | List | Jewish Data Bank holdings |
| Minneapolis, MN | 1971 | Unknown | Lazarus and Erickson (1973) |
| St. Paul, MN | 1971 | List | Jewish Data Bank holdings |
| New Orleans, LA | 1973 | Unknown | Jewish Welfare Federation of New Orleans (1973) |
| Dallas, TX | 1974 | List | Maynard (1974) |
| Akron, OH | 1975 | Unknown | Wagner, Deitchman, and Kapusinski (1975) |
| Baltimore, MD | 1975 | Unknown | Unknown |
| Boston, MA | 1975 | List<br>Area-probability | Fowler (1976) |
| Omaha, NE | 1975 | Unknown | University of Nebraska at Omaha and Omaha Jewish Federation (1976) |
| Tidewater, VA | 1975 | Unknown | Unknown |
| Trenton, NJ | 1975 | Unknown | Jewish Data Bank Holdings |
| Houston, TX | 1976 | List | Schulman, Gottlieb, and Sheinberg (1976) |
| Salt Lake City, UT | 1976 | List<br>DJN<br>Snowball | United Jewish Council (1977) |
| Valparaiso, IN | 1976 | Unknown | Indiana Jewish Historical Society (1976) |
| Vineland, NJ | 1976 | Unknown | Vineland Jewish Community Council (1977) |
| Kansas City, MO | 1977 | Unknown | Jewish Federation of Greater Kansas City (1977) |
| Pittsburgh, PA | 1977 | Unknown | Unknown |
| Shreveport, LA | 1978 | | Zionts (1979) |
| Los Angeles, CA | 1979 | RDD | Huberman, Riley, and Rieger (1983)<br>Phillips (1986a) |
| San Diego, CA | 1979 | Unknown | D.B. Rosen & Associates (1980) |
| San Francisco, CA | 1979 | Unknown | Unknown |
| Seattle, WA | 1979 | Unknown | McCann and Friedman (1979) |
| Cincinnati, OH | 1980 | Unknown | Unknown |
| Raritan Valley, NJ | 1980 | Unknown | Unknown |
| Rochester, NY | 1980 | Unknown | Jewish Community Federation of Rochester (1981a; 1981b) |

| Community | Year | Methods | Reports |
|---|---|---|---|
| Tampa, FL | 1980 | List<br>DJN<br>Snowball | Wheeler (1980) |
| Chicago, IL | 1981 | RDD | Jewish Federation of Metropolitan Chicago (1982) |
| Cleveland, OH | 1981 | List<br>DJN | Jewish Community Federation (1982) |
| Denver, CO | 1981 | RDD | Phillips and Judd (1981) |
| Minneapolis, MN | 1981 | List | Geer, Griffel, and Lazarus (1981) |
| New York, NY | 1981 | DJN<br>RDD | Federation of Jewish Philanthropies of New York (1984) |
| North Jersey, NJ | 1981 | Unknown | Unknown |
| St. Louis, MO | 1981 | Unknown | Tobin (1982) |
| St. Paul, MN | 1981 | Unknown | Jewish Data Bank holdings |
| Ft. Wayne | 1982 | Unknown | Unknown |
| Hartford, CT | 1982 | RDD | Abrahamson (1982) |
| Miami, FL | 1982 | DJN<br>List | Sheskin (1983) |
| Nashville, TN | 1982 | Unknown | Hendrix (1982) |
| Oklahoma City, OK | 1982 | Unknown | Unknown |
| Portland, OR | 1982 | Unknown | Unknown |
| Sarasota/Manatee, FL | 1982 | Unknown | Jewish Data Bank holdings |
| Tidewater, VA | 1982 | RDD | Ephross and Weiss (1982) |
| Toledo, OH | 1982 | List<br>DJN<br>Snowball | Jewish Data Bank holdings |
| Atlanta, GA | 1983 | RDD | Cohen and Taylor (1985) |
| Manchester, NH | 1983 | List<br>DJN | Jewish Data Bank holdings |
| Philadelphia, PA | 1983 | RDD<br>Area-probability | Yancey and Goldstein (1984) |
| Phoenix, AZ | 1983 | RDD<br>List | Jewish Federation of Greater Phoenix (1984) |
| Richmond, VA | 1983 | Expanded list<br>DJN | Jewish Community Federation of Richmond (1984) |
| Washington, DC | 1983 | List<br>RDD | Waksberg, Greenblatt, and Tobin (1984) |
| Milwaukee, WI | 1984 | Reverse directory | Phillips and Weinberg (1984) |
| New Orleans, LA | 1984 | Unknown | Unknown |
| Pittsburgh, PA | 1984 | RDD<br>List<br>Other organization lists | Schorr (1984a)<br>United Jewish Federation of Greater Pittsburgh (1986) |
| South County, FL | 1984 | RDD | Sheskin (1984) |
| Atlantic County, NJ | 1985 | RDD | Tobin (1986a) |

| Community | Year | Methods | Reports |
|---|---|---|---|
| Baltimore, MD | 1985 | RDD<br>DJN | Tobin (1986b) |
| Boston, MA | 1985 | RDD<br>List | Israel (1987) |
| Chicago, IL | 1985 | Unknown | Jewish Federation of Metropolitan Chicago (1985) |
| Kansas City, MO | 1985 | List<br>DJN | Tobin, Levy, and Asher (1986) |
| MetroWest, NJ | 1985 | RDD<br>DJN | Meier, Greenfield, and Tobin (1988) |
| Staten Island, NY | 1985 | Unknown | Federation of Jewish Philanthropies of New York (1985) |
| Houston, TX | 1986 | RDD | Smith and Phillips (1986) |
| Palm Springs, CA | 1986 | List<br>DJN<br>Modified RDD by taking the last digit of a telephone directory listed Jewish surname "seed" phone number and altering it by adding or subtracting digits | R & E Research Associates (1986) |
| Rochester, NY | 1986 | List<br>DJN | Tobin and Fishman (1987a) |
| San Francisco, CA | 1986 | List<br>RDD<br>DJN | Tobin and Sassler (1988b) |
| Worcester, MA | 1986 | RDD<br>Lists | Tobin and Fishman (1987b) |
| Cleveland, OH | 1987 | RDD | Schorr (1988) |
| Harrisburg, PA | 1987 | Unknown | Unknown |
| New Haven, CT | 1987 | DJN | Cohen (1987) |
| Oklahoma City, OK | 1987 | Unknown | Jewish Federation of Greater Oklahoma City (1987) |
| Rhode Island, RI | 1987 | RDD<br>List | Goldscheider and Goldstein (1988) |
| West Palm Beach, FL | 1987 | RDD<br>DJN | Sheskin (1987) |
| Dallas, TX | 1988 | RDD<br>DJN<br>List | Tobin and Fishman (1988) |
| Ft. Bend | 1988 | Unknown | Unknown |
| New Orleans, LA | 1988 | DJN<br>List | Tobin and Sassler (1988a) |
| Tidewater, VA | 1988 | RDD<br>List | Ruchelman (1989) |
| Chicago, IL | 1990 | RDD<br>List | Groeneman and Cue (1991) |
| Columbus, OH | 1990 | List<br>RDD | Mott and Mott (1990) |

| Community | Year | Methods | Reports |
|---|---|---|---|
| Detroit, MI | 1990 | List<br>RDD<br>those who contributed over $1,000 to the Allied Jewish Campaign | Ukeles and Cohen (1989) |
| Memphis, TN | 1990 | Unknown | Memphis Jewish Federation and Memphis Jewish Community Center (1990) |
| San Antonio, TX | 1990 | List<br>DJN | Proust (1990) |
| Seattle, WA | 1990 | List<br>RDD | Einwohner and Burstein (1992) |
| South Broward County, FL | 1990 | DJN<br>RDD | Tobin and Sheskin (1990) |
| Chicago, IL | 1991 | Unknown | Groeneman and Cue (1991) |
| New York, NY | 1991 | RDD | Horowitz (1993) |
| Omaha, NE | 1991 | Unknown | University of Nebraska at Omaha and Omaha Jewish Federation (1991) |
| Southern New Jersey, NJ | 1991 | List<br>DJN<br>RDD | Ukeles Associates (1992) |
| Sarasota/Manatee, FL | 1992 | RDD<br>DJN | Sheskin (1992) |
| St. Paul, MN | 1992 | List<br>DJN | Tobin (1993) |
| Minneapolis, MN | 1993 | List<br>DJN<br>List of "substantial donors" | Project Research Inc., Tobin, and Berger (1994) |
| Orlando, FL | 1993 | RDD<br>DJN | Sheskin (1993) |
| Harrisburg, PA | 1994 | RDD<br>DJN | Sheskin (1994d) |
| Miami, FL | 1994 | RDD<br>DJN | Sheskin (1994a) |
| Pinellas County, FL | 1994 | RDD<br>DJN | Sheskin (1994b) |
| Richmond, VA | 1994 | RDD<br>DJN | Sheskin (1994c) |
| Toledo, OH | 1994 | List | Odesky (1994) |
| Alaska, AK | 1995 | Unknown | Reisman and Reisman (1995) |
| Boston, MA | 1995 | RDD<br>List | Israel (1997) |
| Delaware, DE | 1995 | RDD<br>DJN | Ukeles et al. (1995) |
| Erie County, NY | 1995 | RDD<br>List | Goldhaber Research Associates (1995) |
| Las Vegas, NV | 1995 | RDD<br>List<br>DJN | Tobin (1995a) |

| Community | Year | Methods | Reports |
|---|---|---|---|
| South Palm Beach, FL | 1995 | RDD | Sheskin (1995) |
| St. Louis, MO | 1995 | RDD<br>List<br>DJN | Tobin (1995b) |
| Atlanta, GA | 1996 | RDD<br>DJN | Atlanta Jewish Federation (1996) |
| Los Angeles, CA | 1996 | RDD<br>List | Herman (1998) |
| Milwaukee, WI | 1996 | RDD<br>DJN<br>List | Sheskin (1996) |
| Philadelphia, PA | 1996 | List<br>DJN<br>RDD | Ukeles Associates (1997) |
| Broward County, FL | 1997 | RDD | Sheskin (1997b) |
| Charleston, SC | 1997 | Unknown | Charleston Jewish Federation (1998) |
| Charlotte, NC | 1997 | RDD<br>DJN | Sheskin (1997c) |
| Cleveland, OH | 1997 | RDD<br>Lists<br>DJN | Raff (1996) |
| Dayton, OH | 1997 | | Unknown |
| Denver, CO | 1997 | RDD<br>List<br>RDD | Ukeles and Miller (1998) |
| Monmouth County, NJ | 1997 | DJN<br>RDD<br>Distinctive Sephardic names | Sheskin (1997a) |
| MetroWest, NJ | 1998 | Unknown | Unknown |
| Palm Springs, CA | 1998 | RDD<br>List | Ukeles, Miller, and Sheskin (1998) |
| Akron, OH | 1999 | List<br>DJN<br>Advertising in local papers | Wise (1999) |
| Baltimore, MD | 1999 | RDD<br>List | Ukeles and Miller (2001b) |
| Howard County, MD | 1999 | RDD<br>List | Ukeles and Miller (2001a) |
| Martin-St. Lucie, FL | 1999 | Unknown | Jewish Data Bank holdings |
| Rochester, NY | 1999 | RDD<br>DJN | Sheskin (2000b) |
| West Palm Beach, FL | 1999 | RDD | Sheskin (1999a) |
| York County, PA | 1999 | RDD<br>DJN | Sheskin (1999b) |
| Hartford, CT | 2000 | RDD<br>DJN | Sheskin (2000a) |
| Milwaukee, WI | 2000 | List | Sheskin (2000c) |

| Community | Year | Methods | Reports |
|---|---|---|---|
| Seattle, WA | 2000 | List<br>RDD | Herman and Phillips (2001c) |
| Bergen County, NJ | 2001 | RDD | Sheskin (2001a) |
| Columbus, OH | 2001 | RDD<br>List | Horner, Stewart, and Stec (2001) |
| Houston, TX | 2001 | Expanded list | Herman and Phillips (2001a) |
| New York, NY | 2001 | RDD<br>List | Ukeles and Miller (2004b) |
| Sarasota/Manatee, FL | 2001 | RDD<br>DJN | Sheskin (2001c) |
| Tidewater, VA | 2001 | RDD<br>DJN | Sheskin (2002a) |
| Westport/Weston/ Wilton/Norwalk, CT | 2001 | RDD<br>DJN | Sheskin (2001b) |
| Jacksonville, FL | 2002 | RDD<br>DJN<br>List | Sheskin (2002b) |
| Nashville, TN | 2002 | List | Yacoubian Research (2002) |
| Phoenix, AZ | 2002 | RDD<br>List | Ukeles and Miller (2002) |
| Pittsburgh, PA | 2002 | RDD<br>List | Ukeles and Miller (2003a) |
| Rhode Island, RI | 2002 | RDD<br>DJN | Sheskin (2003a) |
| Tucson, AZ | 2002 | RDD<br>DJN | Sheskin (2002c) |
| Youngstown, OH | 2002 | List | Hart (2003) |
| San Diego, CA | 2003 | RDD<br>List | United Jewish Federation of San Diego County (2004a; 2004b) |
| Washington, DC | 2003 | RDD<br>DJN | Sheskin (2003b) |
| Atlanta, GA | 2004 | RDD<br>List | Ukeles and Miller (2004c) |
| Atlantic County, NJ | 2004 | RDD<br>DJN | Sheskin (2004a) |
| Cleveland, OH | 2004 | List<br>Commercial Jewish names | Immerman and Jacobs (2004) |
| Miami, FL | 2004 | RDD | Sheskin (2004b) |
| Minneapolis-St. Paul, MN | 2004 | RDD<br>DJN<br>Distinctive Russian Jewish first names | Sheskin (2004c) |
| San Francisco, CA | 2004 | List<br>RDD<br>List of Russian-speaking house-holds | Phillips (2005) |

| Community | Year | Methods | Reports |
|---|---|---|---|
| Boston, MA | 2005 | RDD<br>List<br>Multiplicity | Saxe et al. (2006) |
| South Palm Beach, FL | 2005 | RDD | Sheskin (2005a) |
| West Palm Beach, FL | 2005 | RDD | Sheskin (2006) |

## Appendix C

## Lists of Distinctive Jewish Names

| Kohs[a] (American Jewish Historical Society 1946) | Sheskin (2003a) | Sheskin (1997d) Sephardic names |
|---|---|---|
| | Aaronson | |
| Abraham | Abraham | |
| Abrahams | | |
| Abrahamson | | |
| Abramovitz | Abramovitz | |
| Abrams | Abrams | |
| Abramson | | |
| Adelman | Adelman | |
| | | Ades |
| | | Adjmi |
| Agranoff | | |
| | | Anteby |
| Aronson | | |
| | | Ashear |
| | | Ashkenazi |
| Bercovitz | Bercovitz | |
| Berkowitz | Berkowitz | |
| Berman | Berman | |
| Bernstein | Bernstein | |
| | | Betesh |
| | | Beyda |
| | | Beydah |
| Birnbaum | Birnbaum | |
| Blumberg | Blumberg | |
| Blumenthal | Blumenthal | |
| Bornstein | Bornstein | |
| | | Braha |
| Brodsky | Brodsky | |
| Brody | Brody | |
| Cahn | Cahn | |
| Caplan | Caplan | |

| Kohs[a] (American Jewish Historical Society 1946) | Sheskin (2003a) | Sheskin (1997d) Sephardic names |
|---|---|---|
| Cohen | Cohen | |
| Cohn | Cohn | |
| | | Dweck |
| Eisenberg | Eisenberg | |
| Eisner | Eisner | |
| Epstein | Epstein | |
| Feinberg | Feinberg | |
| Feingold | Feingold | |
| Feinstein | Feinstein | |
| Feldman | Feldman | |
| Finkelstein | Finkelstein | |
| Freedman | Freedman | |
| Friedman | Friedman | |
| | | Gammal |
| | | Gemal |
| Ginsberg | Ginsberg | |
| Ginsburg | Ginsburg | |
| Gold | Gold | |
| | Goldberg | |
| Goldburg | | |
| Goldfarb | | |
| Goldman | Goldman | |
| Goldstein | Goldstein | |
| | Goodman | |
| Gottlieb | Gottlieb | |
| Greenbaum | Greenbaum | |
| Greenberg | Greenberg | |
| Greenwald | Greenwald | |
| | Gross | |
| Grossman | Grossman | |
| Halperin | Halperin | |
| Halpern | Halpern | |
| Halprin | Halprin | |
| Horowitz | Horowitz | |
| Horwitz | Horwitz | |
| Hurwitz | Hurwitz | |
| Hyman | Hyman | |
| Isenberg | Isenberg | |
| | Jacobs | |
| | Jaffe | |
| Kahn | Kahn | |
| Kaplan | Kaplan | |
| Katz | Katz | |
| Katzman | | |

| Kohs[a] (American Jewish Historical Society 1946) | Sheskin (2003a) | Sheskin (1997d) Sephardic names |
|---|---|---|
| Kohn | Kohn | |
| Lefkowitz | Lefkowitz | |
| Lerner | Lerner | |
| Levi | Levi | |
| Levin | Levin | |
| Levine | Levine | |
| Levinson | Levinson | |
| Levitt | Levitt | |
| | Levy | |
| | Lieberman | |
| Margolin | Margolin | |
| Margolis | Margolis | |
| Markowitz | Markowitz | |
| | | Mizrahi |
| Moskowitz | Moskowitz | |
| | | Musry |
| Nathan | Nathan | |
| Nathanson | Nathanson | |
| Perlman | Perlman | |
| Pincus | Pincus | |
| Rabinowitz | Rabinowitz | |
| Rappaport | Rappaport | |
| Rosen | Rosen | |
| Rosenbaum | | |
| Rosenberg | Rosenberg | |
| Rosenblatt | | |
| Rosenbloom | | |
| Rosenblum | | |
| Rosenstein | | |
| Rosenthal | Rosenthal | |
| Rothman | Rothman | |
| Rothschild | Rothschild | |
| Rothstein | Rothstein | |
| Ruben | | |
| Rubenstein | Rubenstein | |
| Rubin | Rubin | |
| | | Safdieh |
| | | Saka |
| Samuels | Samuels | |
| Schulman | Schulman | |
| | Schwartz | |
| Segal | Segal | |
| | | Serouya |
| | | Seruya |

| Kohs[a] (American Jewish Historical Society 1946) | Sheskin (2003a) | Sheskin (1997d) Sephardic names |
|---|---|---|
| | | Shamah |
| Shapiro | Shapiro | |
| Shulman | Shulman | |
| Siegel | Siegel | |
| Silverman | Silverman | |
| Silverstein | Silverstein | |
| | Stern | |
| Straus | | |
| Strauss | Strauss | |
| Sugarman | Sugarman | |
| | | Sutton |
| | | Tawil |
| Weinberg | Weinberg | |
| Weiner | Weiner | |
| Weinstein | Weinstein | |
| Weintraub | Weintraub | |
| | Weiss | |
| Wexler | Wexler | |
| Zeitlin | Zeitlin | |
| Zuckerman | Zuckerman | |

*Note:*

[a] I would like to thank Jonathan Sarna for making this reference available to me.

## Appendix D

## Most Commonly Occurring Surnames in the Boston Jewish Community Study

| Surname | n | Surname | n | Surname | n | Surname | n |
|---|---|---|---|---|---|---|---|
| Cohen | 19 | Feldman | 5 | Morse | 4 | Hoffman | 3 |
| Levine | 19 | Goodman | 5 | Newman | 4 | Isenberg | 3 |
| Kaplan | 18 | Grossman | 5 | Oppenheim | 4 | Jacobson | 3 |
| Goldberg | 12 | Kahn | 5 | Rosenberg | 4 | Jones | 3 |
| Miller | 12 | Kaufman | 5 | Segal | 4 | Lebowitz | 3 |
| Goldstein | 11 | Lieberman | 5 | Siegel | 4 | Lerner | 3 |
| Shapiro | 11 | Mayer | 5 | Weinberg | 4 | Meltzer | 3 |
| Fox | 10 | Rosenthal | 5 | Weinstein | 4 | Plotkin | 3 |
| Goldman | 10 | Solomon | 5 | Wolf | 4 | Rich | 3 |
| Katz | 10 | Stein | 5 | Abramson | 3 | Richmond | 3 |
| Friedman | 9 | Stone | 5 | Alexander | 3 | Robbins | 3 |
| Schwartz | 9 | Weiss | 5 | Axelrod | 3 | Sandler | 3 |
| Adler | 8 | Alpert | 4 | Bauman | 3 | Schneider | 3 |
| Brown | 8 | Aronson | 4 | Becker | 3 | Schultz | 3 |
| Fisher | 8 | Berkowitz | 4 | Berkman | 3 | Shore | 3 |
| Gordon | 8 | Bloom | 4 | Bloch | 3 | Shuster | 3 |
| Levy | 8 | Finer | 4 | Block | 3 | Silverman | 3 |
| Greenberg | 7 | Freedman | 4 | Cole | 3 | Simon | 3 |
| Levin | 7 | Glick | 4 | Ellis | 3 | Simons | 3 |
| Rubin | 7 | Gold | 4 | Evans | 3 | Slater | 3 |
| Sherman | 7 | Goldsmith | 4 | Finkelstein | 3 | Steinberg | 3 |
| Berman | 6 | Green | 4 | Geller | 3 | Stern | 3 |
| Singer | 6 | Harris | 4 | Gilbert | 3 | Sussman | 3 |
| Smith | 6 | Jacobs | 4 | Goldblatt | 3 | Trachtenberg | 3 |
| Swartz | 6 | Klein | 4 | Gottlieb | 3 | Winer | 3 |
| Weiner | 6 | Lesser | 4 | Gurevich | 3 | Wolff | 3 |
| Bernstein | 5 | Levenson | 4 | Hecht | 3 | Young | 3 |
| Davis | 5 | Lewis | 4 | Hill | 3 | | |
| Epstein | 5 | Mandell | 4 | Hirsch | 3 | | |

*Note:* Minimum three occurrences in Jewish households.

| Surname | N | Surname | N | Surname | N | Surname | N |
|---|---|---|---|---|---|---|---|
| Cohen | 817 | Stone | 133 | Gross | 81 | Levenson | 56 |
| Levine | 382 | Weinstein | 132 | Silver | 81 | Winer | 56 |
| Miller | 371 | White | 130 | Robinson | 80 | Heller | 55 |
| Goldberg | 344 | Lewis | 129 | Freeman | 79 | Horowitz | 55 |
| Kaplan | 329 | Stein | 128 | Wolf | 79 | Karp | 55 |
| Katz | 328 | Rosenthal | 127 | Feinberg | 78 | Wilson | 55 |
| Shapiro | 283 | Solomon | 119 | Davidson | 73 | Block | 54 |
| Goldman | 266 | Stern | 119 | Johnson | 73 | Robbins | 54 |
| Goldstein | 265 | Simon | 114 | Sullivan | 73 | Taylor | 54 |
| Gordon | 253 | Hoffman | 112 | Berkowitz | 72 | Wasserman | 54 |
| Schwartz | 253 | Cooper | 108 | Becker | 71 | Silverstein | 53 |
| Smith | 238 | Gold | 108 | Allen | 70 | Brooks | 52 |
| Brown | 226 | Siegel | 108 | Geller | 69 | Forman | 52 |
| Rubin | 223 | Harris | 107 | Greene | 69 | Gilman | 52 |
| Friedman | 204 | Steinberg | 105 | Lerner | 69 | Mann | 52 |
| Levy | 204 | Berger | 104 | Diamond | 68 | Martin | 52 |
| Feldman | 202 | Fine | 104 | Lee | 68 | Williams | 52 |
| Levin | 201 | Marcus | 104 | Parker | 68 | Abramson | 50 |
| Rosenberg | 185 | Abrams | 103 | King | 67 | | |
| Green | 184 | Kramer | 102 | Rose | 67 | | |
| Weiner | 183 | Snyder | 96 | Sandler | 67 | | |
| Freedman | 181 | Fishman | 95 | Hurwitz | 66 | | |
| Greenberg | 181 | Kahn | 95 | Altman | 65 | | |
| Rosen | 177 | Fisher | 94 | Shuman | 64 | | |
| Goodman | 171 | Segal | 93 | Bornstein | 63 | | |
| Silverman | 165 | Swartz | 92 | Murphy | 63 | | |
| Berman | 162 | Jacobson | 91 | Roberts | 63 | | |
| Epstein | 156 | Singer | 91 | Herman | 62 | | |
| Bernstein | 152 | Fox | 90 | Hirsch | 62 | | |
| Grossman | 146 | Baker | 88 | Alpert | 61 | | |
| Jacobs | 146 | Weinberg | 87 | Golden | 60 | | |
| Klein | 146 | Adler | 86 | Marks | 60 | | |
| Davis | 144 | Frank | 85 | Young | 60 | | |
| Kaufman | 144 | Newman | 85 | Brenner | 59 | | |
| Ross | 141 | Aronson | 84 | Lieberman | 59 | | |
| Sherman | 141 | Schneider | 84 | Gilbert | 58 | | |
| Weiss | 135 | Shulman | 82 | Morris | 58 | | |
| Bloom | 133 | Gould | 81 | Finn | 57 | | |

*Notes:* Most commonly occurring surnames from list frame. Minimum 50 occurrences.

## Appendix E

## NJPS 2000-01 Weighting Adjustment for Household Type

The National Jewish Population Survey (NJPS) 2000-01 weighting scheme only takes account of households that were eligible for the main instrument but declined to be interviewed on the following variables: type of household (Jewish, persons of Jewish background, non-Jewish), number of adults in household, and stratum. Within each cell of this typology, *adjwgt* is calculated in the following way:

$$adjwgt_i = \frac{\left(n_{complete} + n_{incomplete}\right)}{n_{complete}}$$

This upweights each cell of the typology to numerically account for the number of incompletes. However, this procedure assumes that completes and incompletes are identical on all other characteristics. In general, due to the limited number of questions asked in the screener, we don't know about other characteristics we could weight on. An exception, however, is the ethno-religious status of household members, as all questions used to classify individuals as Jewish, Jewish-connected or non-Jewish were asked of up to six adults living in the household. Were the assumptions behind *adjwgt* to be correct, household composition within the Jewish and persons of Jewish background subsamples would be identical between complete and incomplete datafiles. This is not the case. Instead,

households that refused to participate in the longer interview were far more likely to be ethno-religiously mixed (Table E:1). Assuming that ethno-religiously mixed households will show lower levels of Jewish behavior, failing to take these differences into account produces overestimates of Jewish behavior and underestimates of intermarriage.

Appendix Table E:1. Comparing Complete and Incomplete Household Types

| | *Jewish* | | *Persons of Jewish Background* | | *Non-Jewish* | |
|---|---|---|---|---|---|---|
| *Household type* | *Complete* | *Incomplete* | *Complete* | *Incomplete* | *Complete* | *Incomplete* |
| Jew only | 70.8% | 52.3% | | | | |
| Jew + Jewish-connected | 0.7% | 2.0% | | | | |
| Jew + Jewish-connected + non-Jew | 0.4% | 0.9% | | | | |
| Jew + non-Jew | 16.5% | 32.0% | | | | |
| Jewish-connected only | 0.9% | 0.3% | 18.9% | 7.2% | | 0.1% |
| Jewish-connected + non-Jew | 1.2% | 0.9% | 30.6% | 27.3% | | |
| Non-Jew only | 9.5% | 11.5% | 50.5% | 65.5% | 100% | 99.9% |
| Total | 100% | 100% | 100% | 100% | 100% | 100% |

The likely reason for this pattern is that respondents are more likely to agree to participate in studies that interest them and appeal is itself conditioned by the salience of the topic. Indeed, studies of denominational participation when approached using the NJPS protocol have found that Orthodox Jews are the most likely to agree to participate once contacted (Tobin and Groeneman 2003; United Jewish Communities 2003k). People who intermarry (probably accounting for most of the ethno-religiously mixed house-holds) tend to have a less intense Jewish upbringing and to exhibit lower levels of Jewish behavior. Another factor that may be at work is a fear of judgmentalism on the part of the interviewer among Jews with lower levels of Jewish behavior.

## REWEIGHTING PROCEDURES

The only information we can use for weighting to account for the differences between screener incompletes and incompletes is household composition, namely the number of Jewish, Jewish-connected and non-Jewish people in the household as defined by the J variables developed when United Jewish Communities changed the classification of Jewishness (United Jewish Communities 2003j). Weighting on the initial respondent would be less effective, as the initial respondent was whoever answered the phone (a clear source of selection bias). Randomly selecting a respondent *post facto* from adults in the household would also be possible, but is also problematic. Both alternatives are less efficient as they fail to use all the available information and would result in misclassification (i.e. a household with three Jews and one Jewish-connected adult might be classified as Jewish-connected). To account for all adults in the household in a single variable, I created the following household typology:

- Jew only
- Jew + Jewish-connected
- Jew + Jewish-connected + non-Jew
- Jew + non-Jew
- Jewish-connected only
- Jewish-connected + non-Jew
- Non-Jew only

To implement this classification scheme, I modified the J code syntax to work with the screener complete, screener incomplete and NJPS datasets. Having arrived at an analogous measure of the Jewish identity of each household member, I then constructed indicator variables for the presence of Jews, Jewish-connected people and non-Jews. These indicator variables allowed me to create a household composition variable using the above classification scheme.

Because screener incompletes are only available for replicates 16-22, I used comparable data from the screener complete file. In both files I ran household composition by subsample (Jew, persons of Jewish background, non-Jewish) to account for the different subsampling rates. The results are shown in Tables E:2 and E:3.

Note that in some cases illogical results will appear, such as a number of exclusively non-Jewish households in the Jewish subsample. This is a result of the reclassification of the original sampling scheme into the J variables. As the weights are based on the reclassified Jewishness variables they will differ in some cases when a particular group was reclassified. For example, the 253 Christian and Muslim cases in Sample Allocation Code 8 (Jewish parent(s), consider self Jewish, currently identify with a non-Jewish religion) were sampled as Jews but were subsequently reclassified as non-Jews.

Appendix Table E:2. Household Composition (Screener Completes)

| | *Subsample* | | | |
|---|---|---|---|---|
| *Household composition* | *Jewish* | *PJB* | *NSRE* | *Total* |
| Jew only | 980 | | | 980 |
| | (70.8%) | | | (2.0%) |
| Jew + Jewish-connected | 9 | | | 9 |
| | (0.7%) | | | (0.0%) |
| Jew + Jewish-connected + non-Jew | 6 | | | 6 |
| | (0.4%) | | | (0.0%) |
| Jew + non-Jew | 229 | | | 229 |
| | (16.5%) | | | (0.5%) |
| Jewish-connected only | 13 | 63 | | 76 |
| | (0.9%) | (8.9%) | | (0.2%) |
| Jewish-connected + non-Jew | 16 | 102 | | 118 |
| | (1.2%) | (0.6%) | | (0.2%) |
| Non-Jew only | 131 | 168 | 46,322 | 46,621 |
| | (9.5%) | (0.5%) | (100%) | (97.0%) |
| *Total* | 1,384 | 333 | 46,322 | 48,039 |
| | (100%) | (00%) | (100%) | (100%) |

*Source:* United Jewish Communities (2003e).

*Note:* Replicates 16-22 only

Appendix Table E:3. Household Composition (Screener Incompletes)

| | *Subsample* | | | |
|---|---|---|---|---|
| *Household composition* | *Jewish* | *PJB* | *NSRE* | *Total* |
| Jew only | 464 | | | 464 |
| | (52.3%) | | | (22.4%) |
| Jew + Jewish-connected | 18 | | | 18 |
| | (2.0%) | | | (0.9%) |
| Jew + Jewish-connected + non-Jew | 8 | | | 8 |
| | (0.9%) | | | (0.4%) |
| Jew + non-Jew | 284 | | | 284 |
| | (32.0%) | | | (13.7%) |
| Jewish-connected only | 3 | 19 | | 23 |
| | (0.3%) | (7.2%) | | (1.1%) |
| Jewish-connected + non-Jew | 8 | 72 | 1 | 80 |
| | (0.9%) | (27.3%) | (0.1%) | (3.9%) |
| Non-Jew only | 102 | 173 | 922 | 1197 |
| | (11.5%) | (65.5%) | (99.9%) | (57.7%) |
| *Total* | 887 | 264 | 923 | 2,074 |
| | (100%) | (100%) | (100%) | (00%) |

*Source:* United Jewish Communities (2003f)

*Note*: Replicates 16-22 only

The next step was to combine the relevant parts of these tables to determine what the sample would have looked like (in terms of household composition within subsamples) had all screener incompletes actually been screener completes (Table E:4).

Appendix Table E:4. Household Composition (All Screeners)

| | *Subsample* | |
|---|---|---|
| *Household composition* | *Jewish* | *PJB* |
| Jew only | 1,444 | |
| | (63.6%) | |
| Jew + Jewish-connected | 27 | |
| | (1.2%) | |
| Jew + Jewish-connected + non-Jew | 14 | |
| | (0.6%) | |
| Jew + non-Jew | 513 | |
| | (22.6%) | |
| Jewish-connected only | 16 | 82 |
| | (0.7%) | (13.7%) |
| Jewish-connected + non-Jew | 24 | 174 |
| | (1.1%) | (29.1%) |
| Non-Jew only | 233 | 341 |
| | (10.3%) | (57.1%) |
| *Total* | 2,271 | 597 |
| | (100%) | (100%) |

*Source:* United Jewish Communities (2003e; 2003f).

*Note:* Replicates 16-22 only.

I then calculated rim weights that would adjust the proportional size of the given cell in the screener complete table to the proportional size of the same cell in the combined screener table:

$$\textit{Cell rim weight} = \text{Cell\%}_{\text{COMPLETE}} / \text{Cell\%}_{\text{INCOMPLETE}}$$

Or to give an actual example, the calculation for the rim weight of exclusively Jewish households in the Jewish subsample was as follows:

$$\textit{Exclusively Jewish household rim weight} = .7080925 / .6358432 = 0.897966$$

The rim weights are shown in Table E:5.

Appendix Table E:5. Rim Weights for Household Composition

| | *Subsample* | |
|---|---|---|
| *Household composition* | *Jewish* | *PJB* |
| Jew only | 0.897966 | |
| Jew + Jewish-connected | 1.828269 | |
| Jew + Jewish-connected + non-Jew | 1.421987 | |
| Jew + non-Jew | 1.365214 | |
| Jewish-connected only | 0.750059 | 0.726011 |
| Jewish-connected + non-Jew | 0.914135 | 0.951522 |
| Non-Jew only | 1.083936 | 1.132179 |

A variable containing these weights (*incwgt*) was then calculated for each case in the NJPS dataset (determined by household composition based on initial sample allocation codes). For each case, respondent and, where appropriate, selected child weights were then multiplied by *incwgt* to produce three new weights (*newhhwt*, *newrswt*, and *newchwt*).

These weights tend to slightly marginally decrease estimates of the Jewish population based on all household members. Using all household members weighted by *wt2hhcp*, we generate a Jewish population of 5,086,418 (plus 100,000 institutionalized and unenumerated persons). The same procedure using *newhhwt* gives an estimate of 5,033,103 Jews.

EFFECTS OF THE NEW WEIGHTS

In general, the new weights did not have a large effect on most results. One analysis I did was to replicate Table 8 (Jewish connections by region) in the NJPS report using *wt3resp* and *newrswt*. As can be seen (Tables E:6, E:7, and E:8), the new weights generally resulted in a uniform, but slight, decrease in estimates of Jewish behavior.

Appendix Table E:6. Jewish Connections by Region with Unadjusted Weights

| | *Region* | | | |
|---|---|---|---|---|
| *Variable* | *Northeast* | *Midwest* | *South* | *West* |
| Half or more close friends are Jewish | 61% | 45% | 51% | 42% |
| Hold/attend Passover seder | 76% | 67% | 66% | 55% |
| Light Chanukah candles | 79% | 70% | 68% | 66% |
| Fast on Yom Kippur* | 65% | 56% | 59% | 50% |
| Light Shabbat candles* | 33% | 25% | 26% | 22% |
| Keep kosher at home* | 28% | 16% | 15% | 14% |
| Attend Jewish religious service monthly or more* | 30% | 29% | 27% | 23% |
| Belong to synagogue* | 50% | 54% | 44% | 37% |
| Belong to JCC* | 23% | 21% | 22% | 15% |
| Belong to other Jewish organization* | 29% | 34% | 32% | 21% |
| Volunteer under Jewish auspices* | 25% | 28% | 25% | 23% |
| Participate in adult Jewish education* | 25% | 30% | 22% | 22% |
| Visited Israel | 40% | 30% | 35% | 30% |
| Contribute to Federation campaign* | 31% | 39% | 36% | 22% |
| Contribute to Jewish cause (not federation) | 46% | 42% | 40% | 35% |

*Source:* United Jewish Communities (2003d)

*Note:* * 4.3 million population

Appendix Table E:7. Jewish Connections by Region with Adjusted Weights

| | *Region* | | | |
|---|---|---|---|---|
| *Variable* | *Northeast* | *Midwest* | *South* | *West* |
| Half or more close friends are Jewish | 59% | 45% | 49% | 41% |
| Hold/attend Passover seder | 75% | 68% | 66% | 56% |
| Light Chanukah candles | 79% | 71% | 69% | 67% |
| Fast on Yom Kippur* | 64% | 55% | 57% | 49% |
| Light Shabbat candles* | 31% | 23% | 24% | 21% |
| Keep kosher at home* | 26% | 15% | 14% | 14% |
| Attend Jewish religious service monthly or more* | 29% | 28% | 26% | 21% |
| Belong to synagogue* | 48% | 51% | 43% | 35% |
| Belong to JCC* | 22% | 20% | 21% | 14% |
| Belong to other Jewish organization* | 28% | 31% | 30% | 20% |
| Volunteer under Jewish auspices* | 24% | 27% | 25% | 21% |
| Participate in adult Jewish education* | 24% | 28% | 21% | 21% |
| Visited Israel | 39% | 30% | 34% | 29% |
| Contribute to Federation campaign* | 29% | 36% | 34% | 21% |
| Contribute to Jewish cause (not federation) | 48% | 48% | 45% | 41% |

*Source:* United Jewish Communities (2003d)

*Note:* * 4.3 million population

Appendix Table E:8. Difference between Adjusted and Unadjusted Weights

| | *Region* | | | |
|---|---|---|---|---|
| *Variable* | *Northeast* | *Midwest* | *South* | *West* |
| Half or more close friends are Jewish | -2 | 0 | -2 | -1 |
| Hold/attend Passover seder | -1 | 0 | 0 | 1 |
| Light Chanukah candles | 0 | 1 | 1 | 1 |
| Fast on Yom Kippur* | -1 | -1 | -2 | -1 |
| Light Shabbat candles* | -2 | -1 | -1 | -1 |
| Keep kosher at home* | -2 | -1 | 0 | 0 |
| Attend Jewish religious service monthly or more* | -1 | -1 | -1 | -2 |
| Belong to synagogue* | -2 | -3 | -1 | -2 |
| Belong to JCC* | -1 | -2 | -1 | -1 |
| Belong to other Jewish organization* | -1 | -3 | -2 | -1 |
| Volunteer under Jewish auspices* | -1 | -1 | -1 | -1 |
| Participate in adult Jewish education* | -1 | -2 | -1 | -1 |
| Visited Israel | -1 | 0 | -1 | -1 |
| Contribute to Federation campaign* | -2 | -3 | -2 | -1 |
| Contribute to Jewish cause (not federation) | 2 | 6 | 4 | 6 |

*Source:* United Jewish Communities (2003d).

*Note:* * 4.3 million population.

There was, however, a significant effect on intermarriage, with the new respondent weight significantly increasing estimates of intermarriage rates (Table E:9).

Appendix Table E:9. Effect of Adjustment Weight on Intermarriage Rates

| *Year of marriage* | *Unadjusted* | *Adjusted* |
|---|---|---|
| Pre-1970 | 12.6% | 15.5% |
| 1970-79 | 28.1% | 34.6% |
| 1980-84 | 37.9% | 44.7% |
| 1985-90 | 43.2% | 50.4% |
| 1991-95 | 43.2% | 51.1% |
| Post-1995 | 47.1% | 52.5% |
| *Total* | 30.9% | 37.0% |

## Appendix F

## Letters Requesting Permission to Use Lists

*Combined Jewish Philanthropies Letter of Assurance*

May 24, 2004

To: …
From: Mark Niedergang, Consultant for Community Organizations,
Combined Jewish Philanthropies (CJP)

**RE: Use of …'s lists for 2005 Boston Jewish Community Study**

Dear …;

Following our telephone conversation this morning, I'm writing with a formal request for the use of the … lists for the 2005 Greater Boston Jewish Community Study, to be conducted by Combined Jewish Philanthropies (CJP). We would like to receive ….

**I want to assure you that your list will be used only for this research project, and will not be used for marketing, fundraising, events, publications, or any other purpose.** All the lists we borrow from Jewish organizations will be sent to and stored at the Cohen Center for Modern Jewish Studies at Brandeis University. Only Cohen Center employees will have access to the lists. Confidentiality and privacy will be rigorously protected. Once analysis of the survey and the final report have been completed, all organizational membership lists will be deleted. If it would be helpful, I can also send memoranda that promise this from Martin Abramowitz of Combined Jewish Philanthropies and Len Saxe, Director of the Cohen Center.

Why do we want to use these … lists for this one-time research study? Our last demographic study of the Boston-area Jewish community (1995) sampled respondents by random digit dialing and also from the CJP list (which now has about 57,000 households, more than half of the Jewish households in 1995 in Greater Boston). This time we want to cast as wide a net as possible and reach beyond those who are on CJP's list. We are interested in the … lists because we believe that many of the people on them may not be on the CJP list or on other lists that we will also borrow. ***We want to hear the voices of Jews who may not be connected to CJP.*** The Jewish community is changing and is increasingly diverse; this requires new research methods and better data to accurately portray the community. Thus we believe that use of your lists will help increase the accuracy of the Boston 2005 Community Study.

Lists from Jewish organizations such as the … will be used for two purposes:

**First, for comparative purposes.** We want to compare the membership of each list to that of every other list in order to decide which lists to sample from and to help us better understand how Greater Boston Jews affiliate with Jewish organizations. This part of the study will not involve contact with any people on your list.

**Second, we would use your list for a single telephone survey to be conducted by a professional university-based survey research firm that CJP will hire**. All the lists we borrow will be merged into a database at the Cohen Center at Brandeis. A small random sample will be drawn from this database, and in the spring of 2005, those households will receive a letter and a call asking them to participate in a half-hour telephone interview.

CJP is working closely on the 2005 Boston Community Study with the Cohen Center for Modern Jewish Studies at Brandeis. As a result, the research process will be supervised by Brandeis University's Committee for the Protection of Human Subjects to ensure that the highest ethical standards are upheld.

The 2005 Community Study will be the fifth (since 1965) demographic study done by CJP, the first Jewish federation in the country to periodically survey its Jewish community. These research studies have consistently been useful to CJP, Jewish social service agencies, congregations, the Jewish denominations, the national research community, and other Jewish organizations, and they have served as models of methodological excellence.

Thank you for considering this request. Your assistance would help us to draw a more accurate portrait of the Boston-area Jewish community and enable CJP, and other local Jewish organizations, to better understand, plan for, and provide service to the Jewish community.

If you have any questions, I will be happy to answer them, just email or call me.

Sincerely yours,

Mark Niedergang
Consultant for Community Organizations,
Combined Jewish Philanthropies

Telephone: (XXX) XXX-XXXX day or evening
email: xxxx@xxxx.xxx

*Cohen Center for Modern Jewish Studies Letter of Assurance*

**2005 BOSTON JEWISH COMMUNITY STUDY**

October 18, 2004

With support from Combined Jewish Philanthropies (CJP), the Cohen Center for Modern Jewish Studies at Brandeis University is designing the 2005 Boston Jewish Community Study. In order focus the study's resources on interviews with members of the Jewish community rather than on efforts to locate Jews, the Cohen Center is developing an expanded database of households associated with Jewish organizations and institutions which will supplement the random sample, for which an increasing amount of money is spent "screening" all households.

We are committed to ensuring the confidentiality of every person found on a list and of each participating organization. The lists will be kept at Brandeis University and will be used only for the 2005 study. *No information about list members or participating organizations will be released to outside parties. In addition, the lists will not be used for fundraising or any other purposes by Brandeis University or any other institution.* Once analysis of the survey has been completed and the final report is presented, all organizational membership lists will be deleted.

Your participation is critical! To ensure accurate representation of the entire Jewish community, we need lists from as many organizations as possible. Each list brings us closer to a more complete picture of the entire Boston Jewish community.

The study will draw a sample of households from the database. Again, no personally identifiable information about research subjects from your list or participating organizations will be shared or made public. Reports using the 2005 Boston Jewish Community Study will not contain any information about individual respondents; organizations will not be mentioned by name unless we receive your explicit permission. Likewise, all information that could identify a person or the organizations they belong to will be removed from the data files.

Sincerely,

[signature]

Leonard Saxe, Ph.D.
Professor of Social Policy

*List of Frequently Asked Questions*

Thank you for participating in the 2005 Boston Jewish Community Study. You will be contacted by an employee of the Cohen Center for Modern Jewish Studies at Brandeis University regarding the technical side of making your list available. This answers some questions about what we're doing, what we need, and what we would like.

**Where will the data be stored? Who will have access to it? What guarantee do I have it won't be used for fundraising?**
Lists will be stored at the Cohen Center for Modern Jewish Studies and will only be accessed by Cohen Center employees. A version of the file containing vital contact information may be sent to a survey research contactor for interviewing. Combined Jewish Philanthropies will not have access to your list at any point. The Cohen Center and CJP will not use information we receive for fundraising, advertising events, distributing publication, or any other purpose other than research.

**What format do I need to provide?**
Ideally, we would like to receive the list in Access or Excel format. If you can't export files in these formats, don't worry! We have a lot of experience in working with different formats.

**What information do you absolutely need from our database?**

- Names (first and last)
- Address
- Phone number

**Why do you need this?**
The name, address and telephone number let us identify the same person in different lists. This is important because we want a single record for every household; otherwise households found on more than one list will be overcounted, giving misleading information about the proportion of the Jewish community found on lists.

**What other information would you like?**

- Alternative contact information: email address, other addresses and phone numbers.
- Any other demographic information: e.g., gender, age, marital status.
- **No financial information, please**! We don't need this and do not want to have access to it.
- ID number

**Why?**
Alternative contact information is very important. If someone's home phone number has changed, we can't contact them unless we have other contact info. We don't want to lose people who have moved. Other information may be useful to determine which types of Jews are more likely to respond to surveys. The ID number makes it easier to update information in our database if, for example, someone moves after we receive your list.

**Contact**
Benjamin Phillips
(email) bphillips@brandeis.edu (phone) 781-736-3855 (fax) 781-736-3929
(address) Cohen Center for Modern Jewish Studies, MS014 Brandeis University, 415 South Street, Waltham, MA 02454-9110 (web) cmjs.org

## Appendix G

## Development of the List Frame

Where permission was given and an organization's list was made available to the Cohen Center for Modern Jewish Studies, it was added to the database of lists. Each list received was reformatted to match database fields. Columns were renamed to match field names in the database. Where necessary, names were broken into separate fields for the first, middle, and last names of the first and second persons mentioned, addresses were separated into components (first and subsequent lines of the street or post office box address, city, state, zip) and designators (e.g., street, avenue) were standardized, and telephone numbers were reformatted where necessary. Attempts were made to remove cases lacking both phone numbers and street addresses, where the address and telephone number listed belonged to a business, and out of state cases.

Initial efforts at deduplication began in June 2004 as part of the planning study. David Tobey, Cohen Center data manager, developed a Web-based tool which enabled users to identify potentially related cases via customizable search criteria and, if sufficient evidence existed, designate one case as the "parent" and the other case or cases as its "children." Following a month of manual deduplicating, a number of automated scripts were developed that were carefully vetted to eliminate false positives. The scripts (named for Red Sox players) are shown in Table G:1, below.

Appendix Table G:1. Initial Deduplication Algorithms

| Name | Search Algorithm |
|---|---|
| Johnny | *first*,*last*,*hadd*1,*hadd*2 |
| Manny | *hadd*1,*hadd*2,*hphone* |
| Papi | *last*,*hadd*1,*hadd*2 |
| Trot | *last*,*first*,*hadd*1 |
| Foulke | *last*,*first*,*hphone* |
| Schilling | *last*,*hadd*1,*hphone* |

Where:

*first* = First name

*last* = Last name

*hadd*1 = First line of street address

*hadd*2 = Second line of street address

*hphone* = Telephone number

In addition, a complex set of criteria were used to ensure cases were not wrongly matched where a field was blank, to assign the parent case, and add missing fields to the parent. Once all lists were added to the database, the scripts were run until no more duplicates were found. At this point, Cohen Center for Modern Jewish Studies staff most familiar with the system used the manual interface to search for resolvable duplicates. These problems were often created by misspellings on the original lists that involved assumptions that could not be written into a computer program. We might, for instance, have a Jon Rosenburg of 123 Walnut Street in Newton (ph: 617-123-4567) on one list and John Rosenberg of 123 Wanlut Street in Newton (ph: 617-123-4567). However, a large group of likely duplicates remained. For example, we could have a Juliet Kaplan-

Amram on one list at an apartment at 1200 Commonwealth Ave. and on another at an apartment at 1300 Beacon St. Given the unusual combination of names, it would be strange if they were different people (unlike the many individuals with common first and last names). Even so, no match could be made as it was impossible to determine which one, if any, was the current address. It was also to be expected that similar situations would obtain where individuals had less unusual names.

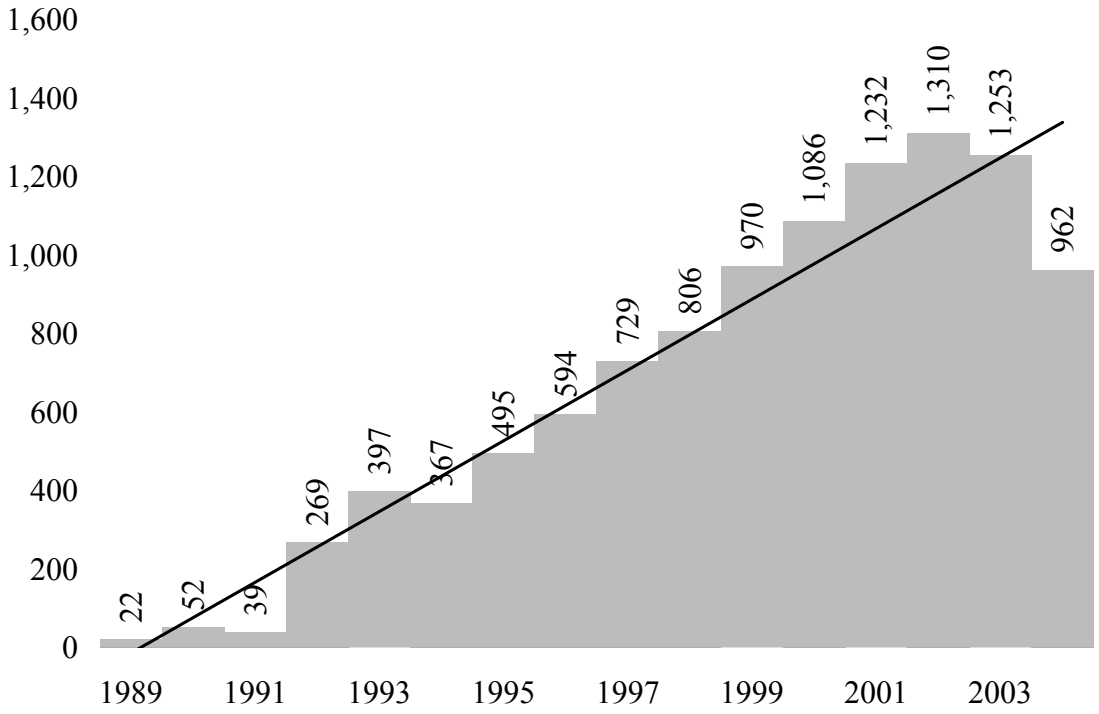
To match cases with incorrect information, it was necessary to verify the contact information for households in the list database, add missing addresses and telephone numbers, update information where households had moved, and scrub cases where the primary individual associated with the household had died. Accordingly, the Cohen Center contracted with AccuData, an information vendor, to convert records into standardized format, update with U.S. Postal Service change of address records, verify that addresses were deliverable, update with Postal Service Maximum Change of Address records (covering two moves and 60 months), identify deceased cases, and append latitude and longitude for subsequent analyses. The list database sent to AccuData on February 16, 2005, contained the 143,972 "parent" cases, each assumed to represent a single household, out of the 327,548 cases contained in the list database. It was returned to the Cohen Center on March 3, 2005.

As was the case with the survey instrument, the procedures involved were significantly more complicated than initially estimated; the vendor had to write a custom program to correctly append the new fields. The work performed by AccuData was extremely thorough; the file received contained 250 variables documenting each stage of

the process together with extensive documentation, enabling Cohen Center staff to make fine distinctions between cases.

Once the updated file of "parent" cases was received from AccuData it was carefully studied and tested before being used to update the database. We examined peripheral indicators of data quality regarding cases identified as deceased or with bad addresses in an attempt to determine whether the cases appeared in patterns in line with *a priori* expectations. An analysis of number of deceased cases by year matches the expected pattern, with the greatest number of deceased cases reported for the years immediately prior to 2004, and subsequently declining in a highly predictable fashion as—we posit—deceased members were identified and removed by organizations (see Figure G:1).

Appendix Figure G:1. Number of Deceased Cases by Year of Death



When run by lists, the distribution was less predictable, being a function of both the proportion of elderly members and how well maintained the list is. Lists with high proportions of deceased cases appear to come from lists that might have been gathered in the past or had older populations. The lists with fewest deceased cases generally focused on young adults or the newly married. There was also considerable variation among lists in the proportion of bad addresses. Organizations with young adult populations (which are presumably highly mobile) tend to have the highest proportion of bad addresses, while synagogues dominate the list of organizations with the fewest bad addresses.

Once updated information on the 143,972 parent cases was received from Accu-Data, the same automatic scripts were run again. After the scripts had run, 129,741 parent cases remained (90.1 percent). Once the data were received from the vendor, many parent cases were identified as problematic. Table G:2, below, displays the number and proportion of cases with bad information in various nonexclusive categories. Addresses were considered problematic where there was substantial evidence that a case no longer lived at a given address and no forwarding address was available, where an address was listed as undeliverable by the U.S. Postal Service, where an address was listed as a business address, or was outside the study area. The criteria for an undeliverable address are based in part on advice from AccuData's mail house. Telephone numbers were listed as problematic only when no number existed in the original record of the parent case or its child cases, and AccuData did not find a listed telephone number associated with the address. All cases where substantial evidence exists for a deceased household member were treated as problematic.

Appendix Table G:2. Problematic Cases after Initial Deduplication

| Category | N | Percentage of whole | Notes |
|---|---|---|---|
| Address | | | |
| Moved, no address | 7,950 | 5.5% | USPS Change of Address and Maximum Change of Address data |
| Undeliverable address | 11,097 | 7.7% | USPS Delivery Point Validation data |
| Business address | 8,043 | 5.6% | USPS Change of Address, Maximum Change of Address and Delivery Sequence File data |
| Out of area | 32,225 | 22.4% | |
| Telephone number | 21,436 | 14.9% | Missing, non-listed telephone numbers |
| Deceased | 9,279 | 6.4% | AccuData report |

Source: Parent cases of first round of deduplication

Cases with problematic information in one category also tended to be represented in other categories. Table G:3, below, shows the proportion of cases that are problematic in one category that were also problematic in another category.

Appendix Table G:3. Overlap between Problem Categories

| | *move* | *badaddr* | *business* | *deceased* | *outarea* | *nophone* |
|---|---|---|---|---|---|---|
| *move* | 100% | 1% | 2% | 9% | 5% | 5% |
| *badaddr* | 2% | 100% | 0% | 1% | 13% | 14% |
| *business* | 2% | 0% | 100% | 3% | 6% | 15% |
| *deceased* | 11% | 1% | 4% | 100% | 8% | 16% |
| *outarea* | 20% | 39% | 24% | 26% | 100% | 20% |
| *nophone* | 12% | 27% | 40% | 38% | 18% | 100% |

*Source:* Parent cases of first round of deduplication

*Notes:* Percentage of row found in column

The number of problematic cases was calculated on the following basis:

*business*=1 | *deceased*=1 | *outstate*=1 | *outarea*=1 | (*phonstat*=1 & (*bad_addr*=1 | *move*=1))

Cases lacking a phone number, therefore, were only flagged for exclusion if an address had been found to be associated with a business, moved without a forwarding address, or was undeliverable, while cases with bad addresses or which had moved without a forwarding address were retained unless there was no phone number whatsoever. A bad address was defined as one that was listed as undeliverable by the U.S. Postal Service. Once cases that met this criterion were removed, 97,673 remained eligible for further analysis.

Table G:4, below, displays the incidence of cases with bad information following the second round of the deduplicating scripts.

Appendix Table G:4. Cases with Bad Information

| Category | N | % | Notes |
|---|---|---|---|
| *Address* | | | |
| Moved, no address | 7,348 | 5.7% | USPS Change of Address and Maximum Change of Address data |
| Undeliverable address | 10,560 | 8.1% | USPS Delivery Point Validation data |
| Business address | 7,028 | 5.4% | USPS Change of Address, Maximum Change of Address and Delivery Sequence File data |
| Out of area | 29,702 | 22.9% | |
| *Telephone number* | 20,446 | 15.8% | Missing and non-listed telephone numbers |
| *Deceased* | 8,729 | 6.7% | AccuData report |

*Source:* Parent cases of second round of deduplication

*Notes:* Percentages of total

Table G:5, below, shows the overlap between types of bad information among parent cases from the second pass of the deduplication scripts.

Appendix Table G:5. Overlap between Problematic Categories

| | *move* | *badaddr* | *business* | *deceased* | *outarea* | *nophone* |
|---|---|---|---|---|---|---|
| *move* | 100% | 1% | 2% | 10% | 5% | 5% |
| *badaddr* | 2% | 100% | 0% | 1% | 14% | 15% |
| *business* | 2% | 0% | 100% | 3% | 6% | 15% |
| *deceased* | 12% | 1% | 4% | 100% | 8% | 16% |
| *outarea* | 20% | 39% | 25% | 26% | 100% | 27% |
| *nophone* | 13% | 31% | 37% | 37% | 19% | 100% |

Source: Parent cases of second round of deduplication

Notes: Percentage of row cases found in column

Once bad cases had been removed from calculations, the list size decreased to 85,939. Nevertheless, considerable numbers of duplicate cases remained. After intensive manual exploration of the database, three additional algorithms were developed to locate duplicate cases (see Table G:6). As the three new algorithms were significantly more powerful than the original six, in order for cases matching the algorithm to be identified as duplicates, an extensive set of criteria had to be met based on information from Accu-Data that was not previously available (see Table G:7). Similarly, additional layers of logic determine which case is designated the "parent" (see Table G:8). Before implementation, further manual testing was performed to check for false positives and negatives.

Appendix Table G:6. Final Deduplication Algorithms

| Name | Search Algorithm |
|---|---|
| Johnny | *first*,*last*,*hadd*1,*hadd*2 |
| Manny | *hadd*1,*hadd*2,*hphone* |
| Papi | *last*,*hadd*1,*hadd*2 |
| Trot | *last*,*first*,*hadd*1 |
| Foulke | *last*,*first*,*hphone* |
| Schilling | *last*,*hadd*1,*hphone* |
| Hanley | *hadd*1,*hphone* |
| Tek | *hadd*1,*hcity* |
| Pedro | *hphone* |

Appendix Table G:7. Algorithm Match Expressions

| *Algorithm* | *Expression* |
|---|---|
| Hanley | if (((*hadd*1$_A$=*hadd*1$_B$) & (*hphone*$_A$=*hphone*$_B$)) & (*hadd1*≠" & *hphone*≠") & (*deceased*$_A$≠1 & *deceased*$_B$≠1) & (*business*$_A$≠1 & *business*$_B$≠1)). |
| Tek | if (((*hadd1*$_A$=*hadd1*$_B$) & (*hcity*$_A$=*hcity*$_B$)) & (*hadd*1≠" & *hphone*≠") & (*deceased*$_A$≠1 & *deceased*$_B$≠1) & (*miss_apt*$_A$≠1 & *miss_apt*$_B$≠1)). |
| Pedro | if ((*hphone*$_A$=*hphone*$_B$) & *hphone*≠" & (*deceased*$_A$≠1 & *deceased*$_B$≠1) & (*business*$_A$≠1 & *business*$_B$≠1)). |

Appendix Table G:8. Algorithm Replacement Expressions

| Algorithm | Expressions |
|---|---|
| Hanley | Case with most information |
| Tek | if ($phonstat_A > phonstat_B$) *parent*=A |
| | if ($phonstat_A < phonstat_B$) *parent*=B |
| | if (($phonstat_A = phonstat_B$): Case with most information |
| Pedro | if ($phonstat_A > phonstat_B$) *parent*=A |
| | if ($phonstat_A < phonstat_B$) *parent*=B |
| | if (($phonstat_A = phonstat_B$) & ($bad_addr_A \neq 1$ & $bad_addr_B = 1$)) *parent*=A |
| | if (($phonstat_A = phonstat_B$) & ($bad_addr_A = 1$ & $bad_addr_B \neq 1$)) *parent*=B |
| | if (($phonstat_A = phonstat_B$) & ($bad_addr_A = bad_addr_B$) & ($mis_apt_A \neq 1$ & $mis_apt_B = 1$)) *parent*=A. |
| | if (($phonstat_A = phonstat_B$) & ($bad_addr_A = bad_addr_B$) & ($mis_apt_A = 1$ & $mis_apt_B \neq 1$)) *parent*=B. |
| | if (($phonstat_A = phonstat_B$) & ($bad_addr_A = bad_addr_B$) & ($mis_apt_A = mis_apt_B$)): Case with most information |

Table G:9, below, displays the incidence of cases with bad information following the third round of the deduplicating scripts.

Appendix Table G:9. Overlap between Problem Categories

| *Category* | *N* | *%* | *Notes* |
|---|---|---|---|
| *Address* | | | |
| Moved, no address | 6,926 | 5.7% | USPS Change of Address and Maximum Change of Address data |
| Undeliverable address | 9,467 | 7.8% | USPS Delivery Point Validation data |
| Business address | 6,836 | 5.6% | USPS Change of Address, Maximum Change of Address and Delivery Sequence File data |
| Out of area | 28,535 | 23.5% | |
| *Telephone number* | 18,831 | 15.5% | Missing and non-listed telephone numbers |
| *Deceased* | 7,985 | 6.6% | AccuData report |

*Source:* Parent cases of third round of deduplication

Table G:10, below, shows the overlap between types of bad information among parent cases from the second pass of the deduplication scripts.

Appendix Table G:10. Final Overlap Between Problematic Cases

| | *move* | *badaddr* | *business* | *deceased* | *outarea* | *nophone* |
|---|---|---|---|---|---|---|
| *move* | 100% | 1% | 2% | 10% | 5% | 4% |
| *badaddr* | 2% | 100% | 0% | 1% | 13% | 15% |
| *business* | 2% | 0% | 100% | 3% | 6% | 16% |
| *deceased* | 11% | 1% | 4% | 100% | 8% | 15% |
| *outarea* | 21% | 40% | 25% | 27% | 100% | 28% |
| *nophone* | 12% | 30% | 44% | 35% | 19% | 100% |

*Source:* Parent cases of third round of deduplication

*Note:* Percentage of column cases found in row

Once bad cases were removed from calculations, the list size decreased to 76,093. Manual exploration found no further cases that could be positively identified as duplicates. It is highly probable, though, that some of the remaining parent cases were either bad (e.g., moved from listed address more than 60 months or more than two moves ago) or duplicates (e.g., multiple cases lacking an apartment number in the same multi-family dwelling unit).

## Appendix H

## Towns and Cities by Sampling Strata

| Community | Listed Households | Occupied Housing Units (U.S. Census) | Synagogues (MA Syn. Council) | Percentage Jewish Households | Households per Synagogue |
|---|---|---|---|---|---|
| *High Density Stratum (20%-100%)* | | | | | |
| Sharon | 2,906 | 5,934 | 8 | 49.0% | 363 |
| Newton | 14,212 | 31,201 | 13 | 45.5% | 1093 |
| Brookline | 8,002 | 25,594 | 7 | 31.3% | 1143 |
| Needham | 2,851 | 10,162 | 4 | 28.1% | 713 |
| Weston | 894 | 3,718 | 0 | 24.0% | 9999 |
| Wayland | 1,095 | 4,625 | 2 | 23.7% | 548 |
| *Total* | 27,054 | 81,234 | 34 | 33.3% | 796 |
| | | | | | |
| *Medium Density Stratum (10%-19%)* | | | | | |
| Lexington | 2,134 | 11,110 | 3 | 19.2% | 711 |
| Sudbury | 1,022 | 5,504 | 2 | 18.6% | 511 |
| Stoughton | 1,612 | 10,254 | 2 | 15.7% | 806 |
| Wellesley | 1,312 | 8,594 | 0 | 15.3% | 9999 |
| Roslindale | 603 | 4,570 | 0 | 13.2% | 9999 |
| Canton | 1,036 | 7,952 | 1 | 13.0% | 1036 |
| Framingham | 3,308 | 26,153 | 3 | 12.6% | 1103 |
| Dover | 230 | 1,849 | 0 | 12.4% | 9999 |
| Sherborn | 166 | 1,423 | 0 | 11.7% | 9999 |
| Natick | 1,466 | 13,080 | 2 | 11.0% | 733 |
| Allston-Brighton | 3,391 | 30,505 | 7 | 11.1% | 484 |
| Ashland | 602 | 5,720 | 1 | 10.5% | 602 |
| Cambridge | 4,311 | 42,615 | 3 | 10.1% | 1437 |
| Lincoln | 280 | 2,790 | 0 | 10.0% | 9999 |
| *Total* | 21,473 | 172,119 | 24 | 12.5% | 895 |
| | | | | | |
| *Low Density Stratum (5%-9% or other evidence*)* | | | | | |
| Belmont | 966 | 9,732 | 1 | 9.9% | 966 |
| Randolph | 1,026 | 11,313 | 3 | 9.1% | 342 |
| Holliston | 425 | 4,795 | 1 | 8.9% | 425 |
| Boston | 4,889 | 56,889 | 5 | 8.6% | 978 |
| Southborough | 253 | 2,952 | 0 | 8.6% | 9999 |
| Concord | 505 | 5,948 | 1 | 8.5% | 505 |
| Jamaica Plain | 1,277 | 15,768 | 0 | 8.1% | 9999 |

| Community | Listed Households | Occupied Housing Units (U.S. Census) | Synagogues (MA Syn. Council) | Percentage Jewish Households | Households per Synagogue |
|---|---|---|---|---|---|
| Easton | 564 | 7,489 | 1 | 7.5% | 564 |
| Acton | 552 | 7,495 | 1 | 7.4% | 552 |
| Bedford | 322 | 4,621 | 0 | 7.0% | 9999 |
| Carlisle | 111 | 1,618 | 0 | 6.9% | 9999 |
| Westwood | 343 | 5,122 | 1 | 6.7% | 343 |
| Arlington | 1,248 | 19,011 | 0 | 6.6% | 9999 |
| Avon | 109 | 1,705 | 0 | 6.4% | 9999 |
| Milton | 557 | 8,982 | 2 | 6.2% | 279 |
| Watertown | 862 | 14,629 | 0 | 5.9% | 9999 |
| West Roxbury | 665 | 12,083 | 1 | 5.5% | 665 |
| Medfield | 217 | 4,002 | 0 | 5.4% | 9999 |
| Hopkinton | 220 | 4,444 | 0 | 5.0% | 9999 |
| Somerville [1 3] | 1,529 | 31,555 | 2 | 4.8% | 765 |
| Winchester | 354 | 7,715 | 0 | 4.6% | 9999 |
| Waltham [1 3] | 1,028 | 23,207 | 1 | 4.4% | 1028 |
| Hull [3] | 189 | 4,522 | 2 | 4.2% | 95 |
| Medway [2] | 171 | 4,182 | 0 | 4.1% | 9999 |
| Boxboro [3] | 75 | 1,853 | 0 | 4.0% | 9999 |
| Stow | 83 | 2,082 | 0 | 4.0% | 9999 |
| Hingham [3] | 273 | 7,189 | 1 | 3.8% | 273 |
| Foxboro [2] | 221 | 6,141 | 0 | 3.6% | 9999 |
| Dedham | 297 | 8,654 | 0 | 3.4% | 9999 |
| Winthrop | 263 | 7,843 | 0 | 3.4% | 9999 |
| Millis [3] | 100 | 3,004 | 1 | 3.3% | 100 |
| Franklin [3] | 323 | 10,152 | 1 | 3.2% | 323 |
| Norwood [2 3] | 327 | 11,623 | 1 | 2.8% | 327 |
| Burlington [3] | 223 | 8,289 | 1 | 2.7% | 223 |
| Walpole | 213 | 8,060 | 0 | 2.6% | 9999 |
| Medford [1 3] | 581 | 22,067 | 1 | 2.6% | 581 |
| Malden [1 3] | 586 | 23,009 | 4 | 2.5% | 147 |
| Revere [3] | 485 | 19,463 | 3 | 2.5% | 162 |
| Melrose [3] | 237 | 10,982 | 1 | 2.2% | 237 |
| Norfolk | 62 | 2,818 | 0 | 2.2% | 9999 |
| Brockton [1 3] | 692 | 33,675 | 1 | 2.1% | 692 |
| Marlborough [3] | 284 | 14,501 | 1 | 2.0% | 284 |
| Maynard | 87 | 4,292 | 0 | 2.0% | 9999 |
| Holbrook [3] | 76 | 4,076 | 1 | 1.9% | 76 |
| Duxbury [3] | 87 | 4,946 | 1 | 1.8% | 87 |
| Wrentham | 59 | 3,402 | 0 | 1.7% | 9999 |
| Milford [2 3] | 157 | 10,420 | 2 | 1.5% | 79 |
| Quincy [3] | 561 | 38,883 | 2 | 1.4% | 281 |
| Wakefield [3] | 157 | 9,747 | 1 | 1.6% | 157 |
| Chelsea [3] | 183 | 11,888 | 3 | 1.5% | 61 |
| Bellingham | 54 | 5,557 | 0 | 1.0% | 9999 |
| *Total* | 25,128 | 564,395 | 48 | 4.5% | 524 |

| Community | Listed Households | Occupied Housing Units (U.S. Census) | Synagogues (MA Syn. Council) | Percentage Jewish Households | Households per Synagogue |
|---|---|---|---|---|---|
| *Excluded* | | | | | |
| Cohasset | 74 | 2,673 | 0 | 2.8% | 9999 |
| Norwell | 72 | 3,250 | 0 | 2.2% | 9999 |
| Hyde Park | 210 | 11,520 | 0 | 1.8% | 9999 |
| Braintree | 229 | 12,652 | 0 | 1.8% | 9999 |
| Charlestown | 209 | 11,946 | 0 | 1.7% | 9999 |
| Stoneham | 158 | 9,050 | 0 | 1.7% | 9999 |
| Reading | 145 | 8,688 | 0 | 1.7% | 9999 |
| Scituate | 107 | 6,694 | 0 | 1.6% | 9999 |
| North Reading | 64 | 4,795 | 0 | 1.3% | 9999 |
| Bridgewater | 89 | 7,526 | 0 | 1.2% | 9999 |
| Dorchester | 370 | 31,292 | 0 | 1.2% | 9999 |
| Hanover | 50 | 4,349 | 0 | 1.1% | 9999 |
| Woburn | 142 | 14,997 | 0 | 0.9% | 9999 |
| Marshfield | 78 | 8,905 | 0 | 0.9% | 9999 |
| Wilmington | 58 | 7,027 | 0 | 0.8% | 9999 |
| Everett | 110 | 15,435 | 0 | 0.7% | 9999 |
| Weymouth | 152 | 22,028 | 0 | 0.7% | 9999 |
| E Bridgewater | 29 | 4,344 | 0 | 0.7% | 9999 |
| Abington | 34 | 5,263 | 0 | 0.6% | 9999 |
| Roxbury | 122 | 20,473 | 0 | 0.6% | 9999 |
| Whitman | 29 | 4,999 | 0 | 0.6% | 9999 |
| W Bridgewater | 14 | 2,444 | 0 | 0.6% | 9999 |
| Kingston | 20 | 4,248 | 0 | 0.5% | 9999 |
| South Boston | 66 | 14,038 | 0 | 0.5% | 9999 |
| Mattapan | 54 | 12,478 | 0 | 0.4% | 9999 |
| Hanson | 13 | 3,123 | 0 | 0.4% | 9999 |
| Rockland | 27 | 6,539 | 0 | 0.4% | 9999 |
| Pembroke | 22 | 5,750 | 0 | 0.4% | 9999 |
| Halifax | 8 | 2,758 | 0 | 0.3% | 9999 |
| East Boston | 30 | 14,326 | 0 | 0.2% | 9999 |
| *Total* | 4,281 | 348,714 | 0 | 1.2% | 9999 |

* Other evidence includes anecdotal evidence regarding Jewish settlement, adjacency to higher density areas and large absolute numbers of list households:

[1] Absolute size
[2] Adjacency
[3] Presence of synagogue

## Appendix I

### Estimation of Stratum Population Size

The size of the RDD universe (including list overlap) was estimated to be equal to the sum of projected of the number of households within each sampled city and town in 2005. As household-level projections for Massachusetts cities and towns were not available for 2005, I derived estimates of the number of households in cities and towns (*c*) in the study area:

$$\hat{N}_{hs,2005} = \sum_{c \in s} \left( \frac{\hat{N}_{hc,2005}}{\hat{r}_{hc,2005}} \right) \tag{I.1}$$

where $\hat{N}_{c,2005}$ is an estimate derived from linear regressions of 1980, 1990 and 2000 U.S. Census population estimates and 2010 middle series cohort-component projections from the Massachusetts Institute of Social and Economic Research ([MISER] 2003) with quadratic terms:

$$\hat{N}_{hc,2005} = E\left(N_{hc,2005} \mid N_{hc} \cdot N_{kc}^2\right) \tag{I.2}$$

and where $\hat{r}_{2005}$ is the estimated ratio of individuals to households:

$$\hat{r}_{h,2005} = E\left(r_{h,2005} \mid r_{hc} \cdot {r_{hc}}^2\right) \tag{I.3}$$

derived from the regression of ratios of individuals to households using 1980, 1990 and 2000 U.S. Census estimates of $N$ and $N_h$ in towns and cities in the study area, and the 2010 MISER (2003) population projection,[1] and a 2010 projection of the number of households in towns and cities in the study area derived from the 1980, 1990, and 2000 Census estimates of the ratio of individuals to households within towns and cities:

$$\hat{N}_{hc,2010} = E\left(N_{hc,2005} \mid N_{hc} \cdot N_{hc}^2\right) \tag{I.4}$$

Estimates of the number of households per stratum are shown in Table I:1, below.

Appendix Table I:1. Number of Households per Stratum by Year

| | Year | | |
|---|---|---|---|
| Stratum | 2000* | 2005† | 2010‡ |
| 1 | 81,684 | 82,585 | 83,877 |
| 2 | 180,385 | 186,128 | 192,342 |
| 3 | 571,355 | 591,422 | 612,405 |
| 4 | 279,014 | 289,194 | 300,978 |
| Total | 1,112,438 | 1,149,329 | 1,189,602 |

*Notes:*

* Census estimate
† Author's estimate
‡ MISER (2003)

ENDNOTES

[1] As MISER (2003) estimates were at a town/city level only, no population projections were available for Boston neighborhoods. Boston neighborhood populations were estimated using 1980, 1990 and 2000 U.S. Census data, and subsequently adjusted to match the MISER estimates for the City of Boston.

## Appendix J

## Multiplicity Screener Questions

*Non-resident children*

Q38 Next, how many of *your* children of <u>any</u> age*,* if any, do not currently live with you? (new)

INTERVIEWER NOTE: Do not include deceased children.

NUMBER HERE
98 DK
99 REF

1. Ask Q39 for each child who doesn't currently live with RS.
    a. i.e. IF Q38>0 & Q38<98 CONTINUE AT Q39.
2. IF Q38=0 or 98 or 99, SKIP TO instructions before Q133.

Q39 How old is your [oldest etc.] child who does not live with you? (new)

| | | |
|---|---|---|
| AGE: _____ | | SKIP TO INSTRUCTIONS BEFORE Q144 |
| 98 | DK | ASK N28 |
| 99 | RF | ASK N28 |

N28 Is your [oldest etc.] child 18 years of age or older? (new)

1 Yes
2 No
8 DK
9 RF

1. IF any kids in Q39>17 OR N28=1, SKIP TO Q44.

2. IF all kids in Q39<17 OR N28=2/8/9, SKIP TO instructions before Q133.

ASK FOR NONRESIDENT CHILDREN > 17 YEARS

Q44 [IF NONRESIDENT CHILDREN=1: Does your child who does / IF NONRESIDENT CHILDREN>1: Do any of your children who do] not live with you live inside of route I-495? (new)

| | | |
|---|---|---|
| 1 | Yes | SKIP TO N29 |
| 2 | No | SKIP TO N30 |
| 8 | DK | SKIP TO N30 |
| 9 | RF | SKIP TO N30 |

If q38 = 1 (only 1 child who doesn't live with RS) and q44 = 1 (lives inside route I-495), THEN AUTOPUNCH n29(1)= 1 and skip to instructions before q133.

N29 Which of your children live inside I-495? (new)

Record roster

| ALSO HAS TO BE OLDER THAN 18 Q39>17 OR N28=1 | LIVE IN GB AREA |
|---|---|
| | N29=1 |
| Oldest non-res child | |
| 2ND oldest non-res child | |
| 3RD oldest non-res child | |
| 4TH oldest non-res child | |
| 5TH oldest non-res child | |
| 6TH oldest non-res child | |
| 7TH oldest non-res child | |
| 8TH oldest non-res child | |
| 9TH oldest non-res child | |
| 10TH oldest non-res child | |

IF NO (DOES NOT LIVE INSIDE I-495) N29 FOR ANY CONTINUE (will also ask N30 for any q44=2, DK or RF):

N30 In what town or city does your [relationship] live?

| | | |
|---|---|---|
| 126 | Other (specify) | CONTINUE |
| 998 | DK | CONTINUE |
| 999 | RF | CONTINUE |

*Parents*

IF SELRESP=1 & IF HH_ROST INCLUDES MOTHER
OR IF SELRESP=2 & IF S23 INCLUDES MOTHER.
THEN

1. AUTOPUNCH Q57=1 & Q58=S5B &
2. ([IF S5B1>5 & S5B1<8: N32=1] OR [IF S5B1=8:N32=2] OR [IF S5B1=9:N32=8] OR [IF S5B1=10:N32=9]) AND
3. AUTOPUNCH Q59=1 & Q59A=996

That is:

If RS's mother already lives in HH

1. Autopunch Q57=1 (mother alive) &
2. Autopunch 7Q58=S5B (screener age for mother)&
3. If S5B1>5 & S5B1<8 (mother is 18-64 years old), then AUTOPUNCH N32=1 OR
4. If SB1=9 (DK mother's age), then AUTOPUNCH N32=8 OR
5. If S5B1=10 (Ref mother's age), then AUTOPUNCH N32=9

AND

6. Autopunch Q59=1 (lives inside I-495) & Q59A=996 (lives with respondent).

I would like to ask you a few questions about your parents.

Q57 Is your mother alive?

If needed: "Whoever you consider to be your mother." If still unclear: "The person who was most like a mother to you"

| | | |
|---|---|---|
| 1 | Yes | CONTINUE |
| 2 | No | SKIP TO Q60 |
| 7 | NA | SKIP TO Q60 |
| 8 | DK | SKIP TO Q60 |
| 9 | RF | SKIP TO Q60 |

Q58 How old is your mother?

| | | | |
|---|---|---|---|
| ENTER AGE | | _____ | SKIP TO Q59 |
| 97 | 97+ | | SKIP TO Q59 |
| 98 | DK | | CONTINUE |
| 99 | REF | | CONTINUE |

Q59 Does your mother live within route I-495? (new)

| | | |
|---|---|---|
| 1 | Yes | SKIP TO Q60 |
| 2 | No | CONTINUE |
| 8 | DK | CONTINUE |
| 9 | REF | SKIP TO Q60 |

Q59A In what city or town does she live? (new)

| | | |
|---|---|---|
| 126 | Other (specify) | CONTINUE |
| 996 | VOL Lives with respondent | CONTINUE |
| 998 | DK | CONTINUE |
| 999 | RF | CONTINUE |

IF SELRESP=1 & IF HH_ROST INCLUDES FATHER
OR IF SELRESP=2 & IF S23 INCLUDES FATHER,
THEN

1. AUTOPUNCH Q60=1 & Q61=S5B &
2. ([IF S5B1>5 & S5B1<8: N33=1] OR [IF S5B1=8:N33=2] OR [IF S5B1=9:N33=8] OR [IF S5B1=10:N33=9]) AND
3. AUTOPUNCH Q62=1 & Q62A=996.

That is:

If RS's father already lives in HH
1. Autopunch Q60=1 (father alive) &
2. Autopunch Q61=S5B (screener age for father)&
3. If S5B1>5 & S5B1<8 (father is 18-64 years old), then AUTOPUNCH N33=1 OR
4. If SB1=9 (DK father's age), then AUTOPUNCH N33=8 OR
5. If S5B1=10 (Ref father's age), then AUTOPUNCH N33=9.

AND
6. Autopunch Q62=1 (lives inside I-495) & Q62A=996 (lives with respondent).

Q60 Is your father alive?

If needed: "Whoever you consider to be your father." If still unclear: "The person who was most like a father to you"

| | | |
|---|---|---|
| 1 | Yes | CONTINUE |
| 2 | No | SKIP TO Q53 |
| 7 | NA | SKIP TO Q53 |
| 8 | DK | SKIP TO Q53 |
| 9 | RF | SKIP TO Q53 |

Q61 How old is your father?

| | | | |
|---|---|---|---|
| ENTER AGE | | _____ | SKIP TO Q62 |
| 97 | 97+ | | SKIP TO Q62 |
| 98 | DK | | CONTINUE |
| 99 | REF | | CONTINUE |

Q62 Does he live inside route I-495? (new)

| | | |
|---|---|---|
| 1 | Yes | SKIP TO Q53 |
| 2 | No | CONTINUE |
| 8 | DK | CONTINUE |
| 9 | REF | SKIP TO Q53 |

Q62A Where does he live? (new)

| | |
|---|---|
| 126 | Other (specify) CONTINUE |
| 996 | VOL Lives with respondent CONTINUE |
| 998 | DK CONTINUE |
| 999 | RF CONTINUE |

*Siblings*

Next, I would like to ask some questions about any brothers and sisters you might have who don't live in this household.

Q53 How many living brothers and sisters do you have who do NOT live in this household? (new)

| | | | |
|---|---|---|---|
| NUMBER | | _____ | IF Q53=0 SKIP TO Q154<br>IF Q53>0 & Q54<98 CONTINUE |
| 98 | DK | | SKIP TO Q154 |
| 99 | REF | | SKIP TO Q154 |

Q56 [IF Q53=1: Does that sibling / IF Q53>1: Do any of your siblings] live inside of Route I-495? (new)

| | | |
|---|---|---|
| 1 | Yes | IF Q53=1, AUTOPUNCH N34 FOR THAT SIBLING AND SKIP TO Q154<br>IF Q53>1 CONTINUE |
| 2 | No | SKIP TO N35 |
| 8 | DK | SKIP TO Q154 |
| 9 | RF | SKIP TO N35 |

N34 Which of your brothers or sisters live inside I-495?

[IF 53>1 OLDEST, 2nd OLDEST, 3rd, etc. sibling]
[IF 53=1] [brother/sister]

IF N34=1 (yes live inside—that sibling from roster mentioned) SKIP TO q154.
IF N34 not mentioned or REF, ask N35.

N35 ONLY ASK N35 IF sibling does not live inside I-495 or REF.

That is: If in N34, sibling does NOT live inside I-495, or answer was REF, then ask N35. If sibling does live inside in N34 or REF, then skip N35.

In which city or town does [READ IN WHICH SIBLING] live?

INTERVIEWER INSTRUCTION: N35: THIS QUESTION IS ABOUT SIBLINGS WHO DO NOT LIVE INSIDE I-495.

USE LIST FROM Q135.

Non-resident Sibling ROSTER

LIVE IN GB AREA

| N34=1 |
|---|
| Oldest sibling |
| 2ND oldest sibling |
| 3RD oldest sibling |
| 4TH oldest sibling |
| 5TH oldest sibling |
| 6TH oldest sibling |
| 7TH oldest sibling |
| 8TH oldest sibling |
| 9TH oldest sibling |
| 10TH oldest sibling |

## Appendix K

## Contact Letters

*CJP Prenotification Letter with $2 Preincentive*

[CJP LETTERHEAD]

[Date]

[LAST NAME, HOUSEHOLD]
[STREET ADDRESS]
[CITY], [STATE], [ZIP]

Dear Sir/Madam:

I am writing from Combined Jewish Philanthropies of Greater Boston to invite you to participate in a very important research study about religion and public life of people of all faiths and backgrounds in the Boston area. Let me emphasize that this is a research study and is NOT an effort to sell you anything or raise funds in any way. I think you will find it an interesting study. Participation in this study involves completion of a telephone interview with us.

A few days from now, you will receive a phone call from SRBI National Public Policy Research Center (www.srbi.com) asking for your opinions on a variety of important topics. Your household has been randomly selected to participate in this study. In order to give an accurate picture of the attitudes of people in the Boston area, it is important that people like you take part in the survey. A low response rate will make the results less precise. Therefore, the success of this project depends upon your willingness to give us a few minutes of your time. If we happen to call at an inconvenient time, we will be happy to set an appointment to call back at a more convenient time for you. All information you provide will be kept strictly confidential. Your participation is voluntary and you may withdraw your consent at any time.

If you have questions about this research project, please call Joseph Blechman at the SRBI National Public Policy Research Center toll-free at 1-800-772-9287.

Thank you in advance for your help. We look forward to speaking to you!

Sincerely,

[signature]

Barry Shrage
President,
Combined Jewish Philanthropies of Greater Boston

PS. As a way of saying thanks for reading this letter, we have enclosed $2. This is yours to keep regardless of whether or not you decide to participate in the study.

*SRBI Prenotification Letter with $2 Preincentive*

[SRBI LETTERHEAD]

[Date]

[LAST NAME, HOUSEHOLD]
[STREET ADDRESS]
[CITY], [STATE], [ZIP]

Dear Sir/Madam:

I am writing from SRBI National Public Policy Research Center (www.srbi.com) to invite you to participate in a very important research study about religion and public life of people of all faiths and backgrounds in the Boston area. Let me emphasize that this is a research study and is NOT an effort to sell you anything or raise funds in any way. I think you will find it an interesting study. Participation in this study involves completion of a telephone interview with us.

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If you have questions about this research project, please call Joseph Blechman at the SRBI National Public Policy Research Center toll-free at 1-800-772-9287.

Thank you in advance for your help. We look forward to speaking to you!

Sincerely,

[signature]

Mark A. Schulman, Ph.D.
President,
SRBI National Public Policy Center

PS. As a way of saying thanks for reading this letter, we have enclosed $2. This is yours to keep regardless of whether or not you decide to participate in the study.

*CJP Prenotification Letter with $18 Postincentive*

[CJP LETTERHEAD]

[Date]

[LAST NAME, HOUSEHOLD]
[STREET ADDRESS]
[CITY], [STATE], [ZIP]

Dear Sir/Madam:

I am writing from Combined Jewish Philanthropies of Greater Boston to invite you to participate in a very important research study about religion and public life of people of all faiths and backgrounds in the Boston area. Let me emphasize that this is a research study and is NOT an effort to sell you anything or raise funds in any way. I think you will find it an interesting study. Participation in this study involves completion of a telephone interview with us. If you are chosen for a longer interview, we will send you or a charity chosen by you $18 upon completion of the interview.

A few days from now, you will receive a phone call from SRBI National Public Policy Research Center (www.srbi.com) asking for your opinions on a variety of important topics. Your household has been randomly selected to participate in this study. In order to give an accurate picture of the attitudes of people in the Boston area, it is important that people like you take part in the survey. A low response rate will make the results less precise. Therefore, the success of this project depends upon your willingness to give us a few minutes of your time. If we happen to call at an inconvenient time, we will be happy to set an appointment to call back at a more convenient time for you. All information you provide is protected by law and will be kept strictly confidential. Your participation is voluntary and you may withdraw your consent at any time.

If you have questions about this research project, please call Joseph Blechman at the SRBI National Public Policy Research Center toll-free at 1-800-772-9287.

Thank you in advance for your help. We look forward to speaking to you!

Sincerely,

[signature]

Barry Shrage
President,
Combined Jewish Philanthropies of Greater Boston

*SRBI Prenotification Letter with $18 Postincentive*

[SRBI LETTERHEAD]

[Date]

[LAST NAME, HOUSEHOLD]
[STREET ADDRESS]
[CITY], [STATE], [ZIP]

Dear Sir/Madam:

I am writing from SRBI National Public Policy Research Center (www.srbi.com) to invite you to participate in a very important research study about religion and public life of people of all faiths and backgrounds in the Boston area. Let me emphasize that this is a research study and is NOT an effort to sell you anything or raise funds in any way. I think you will find it an interesting study. Participation in this study involves completion of a telephone interview with us. If you are chosen for a longer interview, we will send you or a charity chosen by you $18 upon completion of the interview.

A few days from now, you will receive a phone call from us asking for your opinions on a variety of important topics. Your household has been randomly selected to participate in this study. In order to give an accurate picture of the attitudes of people in the Boston area, it is important that people like you take part in the survey. A low response rate will make the results less precise. Therefore, the success of this project depends upon your willingness to give us a few minutes of your time. If we happen to call at an inconvenient time, we will be happy to set an appointment to call back at a more convenient time for you. All information you provide is protected by law and will be kept strictly confidential. Your participation is voluntary and you may withdraw your consent at any time.

If you have questions about this research project, please call Joseph Blechman at the SRBI National Public Policy Research Center toll-free at 1-800-772-9287.

Thank you in advance for your help. We look forward to speaking to you!

Sincerely,

[signature]

Mark A. Schulman, Ph.D.
President,
SRBI National Public Policy Research Center

*Prenotification for No Telephone Households with $2 Preincentive*

[CJP LETTERHEAD]

[Date]

[LAST NAME, HOUSEHOLD]
[STREET ADDRESS]
[CITY], [STATE], [ZIP]

Dear Sir/Madam:

I am writing from Combined Jewish Philanthropies of Greater Boston to invite you to participate in a very important research study about religion and public life of people of all faiths and backgrounds in the Boston area. Let me emphasize that this is a research study and is NOT an effort to sell you anything or raise funds in any way. I think you will find it an interesting study. Participation in this study involves completion of a telephone interview with us.

Your household has been randomly selected to participate in this study. In order to give an accurate picture of the attitudes of people in the Boston area, it is important that people like you take part in the survey. A low response rate will make the results less precise. Therefore, the success of this project depends upon your willingness to give us a few minutes of your time. If we happen to call at an inconvenient time, we will be happy to set an appointment to call back at a more convenient time for you. All information you provide will be kept strictly confidential. Your participation is voluntary and you may withdraw your consent at any time.

Unfortunately, we do not have your phone number to contact you. In order to participate in this very important study, please call Joseph Blechman at the SRBI National Public Policy Research Center (www.srbi.com) toll-free at 1-800-772-9287 to schedule a time for a phone interview.

Thank you in advance for your help. We look forward to speaking to you!

Sincerely,

[signature]

Barry Shrage
President,
Combined Jewish Philanthropies of Greater Boston

PS. As a way of saying thanks for reading this letter, we have enclosed $2. This is yours to keep regardless of whether or not you decide to participate in the study.

*Prenotification Letter for No Telephone Households with $18 Postincentive*

[CJP LETTERHEAD]

[Date]

[LAST NAME, HOUSEHOLD]
[STREET ADDRESS]
[CITY], [STATE], [ZIP]

Dear Sir/Madam:

I am writing from Combined Jewish Philanthropies of Greater Boston to invite you to participate in a very important research study about religion and public life of people of all faiths and backgrounds in the Boston area. Let me emphasize that this is a research study and is NOT an effort to sell you anything or raise funds in any way. I think you will find it an interesting study. Participation in this study involves completion of a telephone interview with us. If you are chosen for a longer interview, we will send you or a charity chosen by you $18 upon completion of the interview.

Your household has been randomly selected to participate in this study. In order to give an accurate picture of the attitudes of people in the Boston area, it is important that people like you take part in the survey. A low response rate will make the results less precise. Therefore, the success of this project depends upon your willingness to give us a few minutes of your time. If we happen to call at an inconvenient time, we will be happy to set an appointment to call back at a more convenient time for you. All information you provide is protected by law and will be kept strictly confidential. Your participation is voluntary and you may withdraw your consent at any time.

Unfortunately, we do not have your phone number to contact you. In order to participate in this very important study, please call Joseph Blechman at the SRBI National Public Policy Research Center (www.srbi.com) toll-free at 1-800-772-9287 to schedule a time for a phone interview.

Thank you in advance for your help. We look forward to speaking to you!

Sincerely,

[signature]

Barry Shrage
President,
Combined Jewish Philanthropies of Greater Boston

## Appendix L

## Refusal Conversion Letter

[CJP LETTERHEAD]

[Date]

[LAST NAME, HOUSEHOLD]
[STREET ADDRESS]
[CITY], [STATE], [ZIP]

Dear Sir/Madam:

Several weeks ago you received a call regarding a study about religion and public life for people in the Boston area. It is my understanding that the interviewer from the SRBI National Public Policy Research Center (www.srbi.com) was unable to complete an interview with you. I understand that your time is extremely valuable. Because your answers matter so much, *we will pay $18 to you or a charity you designate to complete a five minute interview*, regardless of whether you are selected for a longer series of questions.

I am writing because an interview with you is essential. It's only through hearing from everyone we contact that we can be sure our findings accurately represent the beliefs and opinions of people in eastern Massachusetts.

While Combined Jewish Philanthropies of Greater Boston is undertaking a broader research effort that focuses on the characteristics of Jews in eastern Massachusetts, this survey is intended for all people, whether Jewish or not. The responses from people who have already been interviewed are contributing an up-to-date picture of the beliefs of residents of the Boston area and their attitudes to key questions about the relationship between governments and religious organizations. As the U.S. Census does not ask about religion, we rely entirely on the cooperation of people like you to learn about these important and topical issues. We think that many people will be interested in our findings, which will be made publicly available.

Let me emphasize that this is a research study and is NOT an effort to sell you anything or raise funds in any way. Protecting the confidentiality of people's answers is very important to us and your contact information will remain confidential and will be deleted at

the conclusion of the study. If for any reason you decide you no longer want to participate during the interview, you may withdraw your consent at any time.

A few days from now, you will receive a phone call from SRBI National Public Policy Research Center (www.srbi.com). If we happen to call at an inconvenient time, we will be happy to set an appointment to call back at a more convenient time for you.

If you have questions about this research project or would like to schedule an interview, please call Joseph Blechman at the SRBI National Public Policy Research Center toll-free at 1-800-772-9287.

Sincerely,

[signature]

Barry Shrage
President,
Combined Jewish Philanthropies of Greater Boston

## Appendix M

## Final Dispositions and Outcome Rates

The calculation of outcome rates is a disconcertingly subjective process requiring considerable judgment on the part of the computer, who must determine which categories to assign temporary dispositions (e.g., exceeded maximum number of callbacks). A disposition is the outcome of particular phone call for a particular household. Does this mean the number contacted was a household and should be classified as a noncontact, or should it be considered of unknown eligibility? Certain dispositions (e.g., deceased respondent or temporarily on vacation) are unclear. As the Boston Jewish Community Study is a household survey, the death of one member of the household does not make the household ineligible for the survey, unless the person conveying the information was not a member of the household and the deceased was the only household member. The same logic applies, *mutatis mutandis*, for being temporarily on vacation and similar instances. It is even more difficult when one is not in direct contact with the interviewers and supervisors. I have attempted to follow both the letter and the spirit of the American Association for Public Opinion Research (AAPOR) (2006) *Standard Definitions* for random digit dialing surveys in assigning the temporary dispositions of our survey research contractor, Schul-

man, Ronca, & Bucuvalas Inc. (SRBI) and in calculating outcome rates, which I will presently outline. At times, I assign temporary dispositions differently to SRBI, resulting in slightly lower outcome rates than in the study report, which is emblematic of the subjectivity I reflected on earlier. My one significant divergence from the AAPOR procedures is that I use only the most recent temporary disposition of the call (though SRBI codes at times take account of the previous history of calls) rather than examining the entire call history (up to 42 calls) for reasons of convenience.

Under the AAPOR standards, final dispositions are assigned to eight categories which form the basis of all outcome rate calculations:

*I* Complete interview (this includes $I_1$, $I_{2a}$, $I_{2b}$, and $I_3$),

*P* Partial interview,

*R* Refusal,

*NC* Noncontact,

*O* Other,

*UH* Unknown if household,

*UO* Unknown, other, and

*NE* Not eligible (I add this last diagraph for reasons of convenience).

The tables that will be subsequently presented do not include *P*, as break-offs were treated as refusals. Accordingly, I do not present outcome rates that include *P* in the denominator, as they are identical to those excluding *P*.

An additional element of calculations is *e*: the estimated proportion of cases of unknown eligibility (*UH* and *UO*) that are eligible. I follow the Council of American Survey Research Organizations method for calculating *e* (Frankel 1983), a relatively conservative estimator, for reasons of convenience as alternative methods are considerably more complicated (Brick, Montaquila, and Scheuren 2002; Smith 2003). It is calculated as:

$$e = \frac{I + R + NC + O}{I + R + NC + O + NE}$$

Contrary to usual usage, tables include two columns for *I*, one containing completed main interviews with Jewish households ($I_1$), the other completed eligible screener interviews ($I_2$; see chapter 4, n. 18, outcome rates regarding screener households).

The formulae for screener outcomes are as follows:

RR3 $$\frac{I}{I + R + NC + O + e(UH + UO)}$$

COOP1 $$\frac{I}{I + R + O}$$

COOP3 $$\frac{I}{I + R}$$

REF2 $$\frac{R}{I + R + NC + O + e(UH + UO)}$$

CON2 $$\frac{I + R + O}{I + R + NC + O + e(UH + UO)}$$

Calculating response rates for surveys that screen for eligible subpopulations is more complicated. Following Ezzati-Rice et al. (2000), I calculate the response rate to the main interview (RR3) as:

$$\frac{I_1}{I_1 + I_{2a} + I_{2b} + r(R + NC + O) + re(UH + UO)},$$

where the estimated rate of eligibility ($r$) is calculated as:

$$\frac{I_1 + I_{2a} + I_{2b}}{I_1 + I_{2a} + I_{2b} + I_3}.$$

Completed interviews with Jewish households are designated $I_1$, while unresolved callbacks and refusals of households that screened Jewish are coded as $I_{2a}$ and $I_{2b}$ respectively, and non-Jewish households that were screened are $I_3$. Other outcome rates for the main interview follow the same logic:

COOP1
$$\frac{I_1}{I_1 + I_{2a} + I_{2b} + r(R + O)}$$

COOP3
$$\frac{I_1}{I_1 + I_{2a} + I_{2b} + r(R)}$$

REF2
$$\frac{I_{2a} + I_{2b} + r(R)}{I_1 + I_{2a} + I_{2b} + r(R + NC + O) + re(UH + UO)}$$

CON2

$$\frac{I_1 + I_{2a} + I_{2b} + r(R+O)}{I_1 + I_{2a} + I_{2b} + r(R+NC+O) + re(UH+UO)}$$

## FINAL DISPOSITIONS AND OUTCOME RATES

| | Frame | | | |
|---|---|---|---|---|
| | RDD | List | Multiplicity | Total |
| *Final Dispositions* | | | | |
| Main interview ($I_1$) | 401 | 1,365 | 33 | 1,799 |
| Screener only | 2,283 | 743 | 1 | 3,027 |
| Qualified callback ($I_{2a}$) | 42 | 76 | 0 | 118 |
| Qualified refusal ($I_{2b}$) | 58 | 83 | 1 | 142 |
| Screened nonqualified household ($I_3$) | 2,183 | 584 | 0 | 2,767 |
| Refusal (R) | 1,940 | 1,032 | 10 | 2,982 |
| Non-contact (NC) | 1,740 | 500 | 5 | 2,245 |
| Other (O) | 1,042 | 498 | 0 | 1,540 |
| Physically or mentally incompetent | 232 | 138 | 0 | 370 |
| Language | 567 | 215 | 0 | 782 |
| Miscellaneous Other | 243 | 145 | 0 | 388 |
| Unknown if household/occupied (UH) | 5,443 | 1,284 | 1 | 6,728 |
| Always busy | 2,048 | 6 | 1 | 2,055 |
| No answer | 1,794 | 334 | 0 | 2,128 |
| Answering machine | 1,476 | 923 | 0 | 2,399 |
| Call-blocking | 32 | 11 | 0 | 43 |
| Technical phone problems | 93 | 10 | 0 | 103 |
| Unknown, other (UO) | 50 | 9 | 0 | 59 |
| Not eligible | 17,849 | 1,276 | 8 | 19,133 |
| Out of sample | 85 | 119 | 5 | 209 |
| Fax/data line | 1,754 | 63 | 0 | 1,817 |
| Not working/disconnected number | 12,682 | 726 | 2 | 13,410 |
| Cell phone | 49 | 17 | 0 | 66 |
| Nonresidence | 3,300 | 357 | 1 | 3,658 |
| No eligible respondent | 28 | 11 | 0 | 39 |
| Total calls | 30,748 | 6,707 | 58 | 37,513 |
| *Outcome Rates* | | | | |
| Screener RR3 | .298 | .411 | .682 | .341 |
| Screener COOP1 | .474 | .579 | .773 | .516 |
| Screener COOP3 | .580 | .671 | .773 | .618 |
| Screener REF2 | .215 | .201 | .201 | .211 |
| Screener CON2 | .628 | .710 | .882 | .660 |
| Main RR3 | .238 | .368 | .662 | .298 |
| Main COOP1 | .379 | .519 | .750 | .451 |
| Main COOP2 | .465 | .601 | .750 | .540 |
| Main REF2 | .275 | .244 | .221 | .254 |
| Main CON2 | .628 | .710 | .882 | .660 |

## Appendix N

## Estimation of Survey Costs

The cost of a survey is typically specified as:

$$C = C_o + \sum_{1}^{H} C_h n_h \tag{N.1}$$

That is, the overall cost of a survey ($C$) is the sum of fixed and variable costs. The fixed costs ($C_o$) include tasks like developing the survey instrument and sampling scheme which are independent of the number of interviews. Variable costs ($C_h$) differ for each sampling stratum ($h$). Thus the cost of the $h$th stratum is the product of the per-case costs and number of cases ($C_h n_h$). (Costs can, of course, vary by primary, secondary, etc. sampling unit when multistage sampling is used, as well as by frame and treatment.) The variable costs across all strata are simply the sum of the costs of all $H$ strata ($\sum_1^H C_h n_h$).

For the sake of convenience in decomposing costs for the Boston Jewish Population Survey, I will largely focus on the costs of the survey research contractor. With the exception of developing the list frame, the costs of the Cohen Center were entirely fixed; these expenses would have been incurred regardless of the number of cases. They therefore do not affect cost-variance optimization, as I allocate a budget exclusive of all fixed

Cohen Center costs. This also matches the financial arrangements of the survey, where the survey operations budget was tracked more closely than in-house operations.

## SURVEY COSTS

I received a spreadsheet from Schulman, Ronca, & Bucuvalas Inc. containing the total billed costs by frame (random digit dialing and list) for the following fields: interviewing, sample management, computer assisted telephone interviewing programming, data processing, project management, prenotification letter mailing, preincentives, post-incentives including processing, noncontact letter mailing, and refusal conversion letter mailing. Of these, computer assisted telephone interviewing programming and data processing were treated as fixed costs unrelated to sample size and characteristics. As the calculation of nonfixed costs was not included in the spreadsheet, it was necessary to model per unit costs. Modeling was guided by the need to equalize costs between the random digit dialing and list frames. That is, while the characteristics of the frames differed, the cost per unit should be identical. For example, while the random digit dialing frame required many more calls per eligible household interviewed than the list frame, the cost per telephone call should be approximately equal. The key, then, was to determine which unit predicted roughly equal per unit costs for the list and random digit dialing frames. Per unit costs can then be used to model the total costs of different sampling frames. The units are listed in Table N:1.

Appendix Table N:1. Difference between Modeled Costs for List and RDD Frames

| Element | Unit | RDD cost per unit | List cost per unit |
|---|---|---|---|
| Interviewers | Live contact | $10.02 | $11.21 |
| Sample management | Calls per household | $0.04 | $0.04 |
| Project management | Screened household | $7.65 | $7.95 |
| Prenotification letter mailing | Sampling unit | $1.51 | $1.51 |
| Preincentive | Sampling unit | $2.00 | $2.00 |
| Postincentive | Completed interview | $19.51 | $19.51 |
| Refusal conversion letter mailing | Refusal conversion households | $1.51 | $1.51 |

It proved impossible to determine a satisfactory model for noncontact mailing costs, a trivial expense, which are omitted from subsequent analyses. The likely effect on the accuracy of cost estimates is minimal as noncontact letter mailing amounted to less than three percent of nonfixed costs.

*Jewish Incidence in Very Low Density Areas*

In order to estimate population density in the very low density area that was not part of the residual random digit dialing sample, it seems reasonable that the observed relationship between the estimated proportion of Jewish households derived from the ratio of list households to 2000 census occupied households to estimated 2005 currently Jewish households, provided there is broad agreement between the estimates. The relationship between list and survey estimates can be seen below.

Appendix Table N:2. Estimated Incidence of Jewish Households

| Stratum | List-based estimate | Survey-based estimate | List to survey ratio |
|---|---|---|---|
| 1 | .330 | .425 | .784 |
| 2 | .125 | .187 | .668 |
| 3 | .045 | .061 | .730 |
| 4 | .012 | **.016** | **.723** |

*Note:* Bolded figures indicate estimated values.

As can be seen, there is a relatively high level of agreement between the list and survey estimates, allowing an approximate estimate of the size and incidence of the Jewish population in stratum 4 as 1.6 percent, based on the mean list-survey ratio of .723.

*Experimental Treatments*

Cases with a given frame and stratum were randomly assigned an experimental treatment. Not all experimental treatments were created equal. While no experimental treatment tested in the main survey proved especially effective (or ineffective), per unit costs differed significantly. As the purpose of this chapter is to investigate the cost-effectiveness of various strategies, I will project costs and results using the most effective approach available.

Appendix Table N:3. Jewish Household Interview Cost by Stratum and Treatment

| Treatment | Stratum | | | |
|---|---|---|---|---|
| | 1 | 2 | 3 | 4 |
| RDD/Unlisted/$0 | $219.25 | $353.35 | $779.94 | **$3,905.84** |
| RDD/Unlisted/$18 post | $208.99 | $288.76 | $539.03 | **$3,295.30** |
| List/$2 pre | $286.98 | $673.47 | $973.62 | **$4,087.85** |
| List/$18 post | $272.66 | $793.97 | $913.12 | **$3,714.58** |
| RDD/Listed/$2 pre/CJP | $50.12 | $55.26 | $60.40 | $86.67 |
| RDD/Listed/$18 post/CJP | $65.16 | $70.43 | $75.85 | $80.03 |
| RDD/Listed $2 pre/SRBI | $187.86 | $497.43 | $876.29 | **$2,693.34** |
| RDD/Listed $18 post/SRBI | $167.18 | $1,460.04 | $361.97 | **$2,893.72** |

*Note:* Bolded items indicate projected costs.

Table N:3, above, displays the total estimated cost per completed interview with a qualified household (containing an adult who is currently Jewish, was raised Jewish, or has a Jewish parent) by treatment. The bolded cells are estimates for random digit dialing Stratum 4 using the Jewish incidence estimated above and the average rates of types of final disposition to sampled units within a given treatment.[1]

For random digit dialing telephone numbers for which no address was available, an $18 incentive proved more effective than no incentive per completed interview with a Jewish household. The same held true for both the $18 postincentive for random digit dialing households with a known address and for list households. A $2 preincentive, however, proved more cost-efficient for list samples.

LIST FRAME DEVELOPMENT COSTS

Determining fixed and variable costs for list interviews in a multiple list sample is particularly complicated due to the "hidden" costs associated with acquiring, preparing, and deduplicating the lists. The linear cost model for a dual-frame survey is typically given as:

$$C = c_o + c_R n_R + c_L n_L \tag{N.2}$$

where $c_o$, $c_R$, and $c_L$ are respectively the fixed costs of the survey, the variable (i.e. per unit) costs of the random digit dialing frame (any universal sampling frame could be substituted) and the variable costs of the list frame.[2] When more than one list is used, however, a more appropriate cost model would take account of the costs attendant on developing a list frame, which are neither as fixed as expenses like computer assisted telephone interviewing programming nor as variable as interview costs, analogous to the costs of multilevel samples. An additional component for the cost of developing the list ($c_\gamma$) is added.

$$C = c_o + c_R n_R + c_L n_L + c_\gamma, \tag{N.3}$$

Broadly speaking, the cost of developing a list frame can be divided into two components: acquiring lists and processing lists. I model $c_\gamma$ as:

$$c_\gamma = c_\alpha + \sum_j^J c_j + c_\chi j + c_\delta n \tag{N.4}$$

where:

$c_\alpha$ is the fixed cost of developing the list frame;

$c_j$ is the total cost of collecting the *j*th list;

$c_l$ is the staff cost per list for processing; and,

$c_\delta$ is the cost of cleaning and deduplicating *n* sampling units.

In other words, the cost of developing the list frame includes a fixed component for startup expenses, an estimate of the cost of collecting lists that takes into account the level of difficulty (i.e. time and money) of acquiring the lists, the salary of staff involved in cleaning and deduplicating lists as a function of the number of lists collected, and an adjustment for the cost of commercial services used for address matching and standardization. I will explain these components below.

*List Acquisition Costs*

Acquiring lists from Jewish organizations was not a simple process. No comprehensive list of Jewish organizations active in the Boston area, for instance, existed. In order to

determine which organizations should be approached as a matter of priority, an organizational directory had to be cobbled together from various sources by Mark Niedergang, a consultant to Combined Jewish Philanthropies and community activist. Although I lack objective measures for the directory's comprehensiveness, the final result (which I cannot claim any credit for) was impressive, including ones as obscure as Jewish Business Executives (a pseudonym), a defunct corporate networking association, the Ajax Outdoor Club (also pseudonymous), a recreational sporting group with few members, and a loose association of Unitarian-Universalists from Jewish backgrounds (which was not sampled).

Once organizations to approach had been selected (see p. 232), it was necessary to elicit their cooperation. Again, Mark Niedergang, the Combined Jewish Philanthropies consultant, took the lead role. The process of attempting to gain cooperation—for both organizations that ultimately opted to cooperate and those that did not—was very labor intensive. Organizations had understandable concerns regarding the uses to which their membership lists would be put to, the confidentiality of their records and responses which members of their lists might give, and, in many cases, assurances that their participation in the project would not be publicly revealed. In many cases, the investigator would need to make inquiries to determine the appropriate person to approach, make contact (which often proved to be very difficult), possibly be referred to another person, respond to sometimes extensive requests for further information, and potentially follow up with additional individuals, such as board members. Accounting for the consultant's time on a per list basis is complicated by the fact that some lists proved more difficult to acquire than others. Mr. Niedergang estimated the level of effort between the earliest and latest

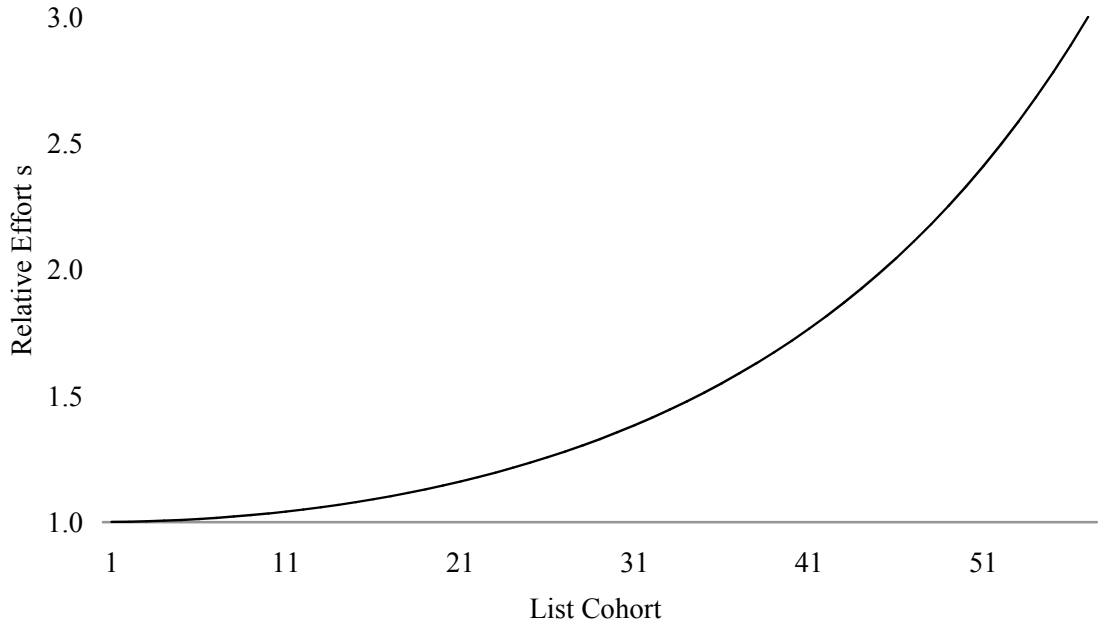
lists was 3:1 with an approximately exponential increase in efforts. Accordingly, the effort ( $\vartheta$ ) required to collect the *j*th list (*j* = 1,2,…,J) was modeled as:[3]

$$\vartheta_j = \left(3^{1/T^2}\right)^{t_i^2} \tag{N.5}$$

where *t* (*t* = 1,2,…,*T*) is the period in which the *j*th list was received.

The estimated distribution of effort is shown in Figure N:1, below.

Appendix Figure N:1. Relative List Collection Effort Expended by List Cohort



The variable cost for *j*th list ( $c_j$ ) was therefore modeled as:

$$c_j = \frac{C_J \vartheta_j}{\sum_j^J \vartheta} \tag{N.6}$$

where:

$C_J$ = the total cost of list acquisition ($15,000).

The resulting estimates are, undoubtedly, far from perfect, being based on recollection of the relative effort required (a difficult concept) and also based on my operationalization of list costs via cohorts, where relative position was in reality a function of the order in which organizations were approached as well as the time required. It also does not take account of the time spent on uncooperative organizations Granting these problems, the solution offered is likely more realistic than the alternative approach of assuming each list required constant effort.

Consent from senior lay or professional leaders, however, was merely a first step. Obtaining the list almost always required liaising with administrative staff or volunteers who were sometimes difficult to contact (especially in smaller organizations). Although list administrators were typically helpful and responsive, they often were uncertain of how to export a file containing membership information. This was particularly true for smaller organizations and those using database software such as Microsoft Access rather than simple (but inflexible) software such as Microsoft Excel. It is worth noting that such

uncertainty about relatively simple database operations may betoken more general problems of maintaining and utilizing membership information. It has been suggested that surveys using list frames should encourage and support donor organizations to update their records and use standard database software in advance of sampling.[4]

*List Processing Costs*

Once received, lists were far from immediately useful. The sheer number of issues precludes comprehensive treatment, so I shall only highlight the most frequent problems. Lists arrived in a number of formats ranging from paper printouts to Excel files to database files. Except for those lists requiring hand entry, conversion to Excel format was quite straightforward. Far more time-consuming were the fields themselves. A name, for our purposes, was stored in the following fields (the character "|" separates fields, "_SP" indicates a second mentioned name, typically that of the spouse/partner): PREFIX | FIRST | MIDDLE | LAST | PREFIX_SP | FIRST_SP | MIDDLE_SP | LAST_SP. Records, however, arrived in formats as diverse as "PREFIX. FIRST MIDDLE LAST, SUFFIX" (i.e. all one field) or "FIRST and FIRST_SP MIDDLE LAST," and many other permutations. Addresses and telephone numbers presented similar problems. Needless to say, transforming fields took considerable time and ingenuity. Address standardization also proved difficult, given different conventions for streets (e.g., "street" and "st."), apartment numbers "1234 Walnut Street | Apt. 3" and "3/1234 Walnut St." The most difficult lists would use several different formats within the same file. Interestingly, the amount of time associated with transforming a given list was seemingly unrelated to size, due to both the generally better format of larger lists and our extensive use of search and

replace algorithms. This fact, as I will return to later, allows relatively accurate cost estimates per list absent detailed accounting for time spent on each list (which does not exist).

So far, all list-related expenses have been variable, with each list having a discrete marginal cost. There were a number of fixed costs ($c_\alpha$), however. The lists were deduplicated using a web-based tool developed by the Cohen Center's data manager, who estimates development took approximately three weeks ($2,400). Had only one list been used, such development would have been unnecessary. I estimate my involvement took approximately three months ($10,000), while only two weeks ($1,575) would have been required had the list frame used only the CJP list. The cost of staff time spent on list cleaning and deduplicating ($c_\chi$) was estimated as the cost of summer research assistants ($2,765) plus 80% of one year for a full-time research assistant ($24,400). Had only the CJP list been used, summer research assistants would not have been required and the full-time research assistant's hours could have been reduced to a month ($2,000). I estimate I spent about a third of a year on list-related matters ($13,650). In addition, approximately $7,000 was spent on the external data processing outlined above (see p. 234).

ENDNOTES

[1] For example, the number of bad telephone numbers for Stratum 4 within Treatment 1 was estimated as the sum of bad telephone numbers in Strata 1-3 within Treatment 1 (4,762+2,531+1,169) divided by the sum of telephone numbers sampled in Strata 1-3 within Treatment 1 (6,002+3,212+1,522), for a proportion of .768 bad numbers per sampled telephone number. This procedure assumes there is no relationship between sampling stratum and ratios of outcomes to

sampling units (i.e. units in Stratum 3 are no more likely to refuse than units in Stratum 1). An alternative would have been to regress the disposition ratios on stratum, treating outcomes in stratum 4 as an extension of trends in ratios across the other three strata. Two factors mitigated against this approach. First, stratification was not directly conditioned on socioeconomic status, a likely cause of variation in disposition ratios. While the strata were associated with socioeconomic status due to the role of the socioeconomic profile of the Jewish community, the relationship was certainly not perfect, fitting poorly with the linear regression assumption that $x$ is a ratio variable where each unit increases is of like size. Second, the relatively few cases in lower density strata significantly increase the variability of rates, having the potential to influence estimates for Strata 4 in undesirable ways. A weighted least-squares approach would have mitigated the latter concern, though at the cost of a major increase in computational difficulty, as spreadsheet software supports ordinary least squares but not weighted least squares based projections.

[2] I follow Hartley's (1962) use of the capitalized subscripts $R$ and $L$ to represent the random digit dialing and list sampling frames ($A$ and $B$ were used originally) and the lowercase subscripts $r$, $l$ and $lr$ to represent the domains containing, respectively, the population only belonging to the random digit dialing frame, the list frame, and both random digit dialing and list frames (the "overlap domain"). Hence, $N_R = N_r + N_{lr}$ and so on. In this case, list cases are treated as only appearing on the overlap domain. Thus, $N_R = N_r + N_{lr} = N_r + N_L$.

[3] Graham Wright helped me find the appropriate transformation.

[4] I am indebted to Tanya Price, a doctoral student in education at Boston College, formerly of the Heller School for Social Policy and Research and the Cohen Center for Modern Jewish Studies at Brandeis University, for these suggestions. She drew my attention to problems of list quality and format well in advance of the survey, which was extremely helpful.

## Appendix O

## Cost-Variance Optimization

OPTIMUM ALLOCATION BETWEEN SAMPLING FRAMES

Following Choudhry (1989), the optimum values $\omega$ and $\alpha$ with each stratum are determined by iteratively calculating $\omega$ and $\alpha$ beginning by setting $\omega_o$ to one. The value of $\omega$ is calculated as:

$$\omega = \left( \frac{\eta(1-\alpha)^2 K_4}{\lambda\delta\left(K_1 + \alpha^2 K_2 - 2\alpha K_3\right)} \right)^{1/2} \tag{O.1}$$

and

$$\alpha = \frac{\delta K_3 + K_4/\omega}{\delta K_2 + K_4/\omega} \tag{O.2}$$

where

$\eta = c_R / c_L$ ;

$K_1 = R_r(1-\lambda)(1-R_r+\lambda R_r)$;

$K_2 = \lambda R_L (1 - \lambda R_L)$;

$K_3 = R_r R_L \lambda (1 - \lambda)$;

$K_4 = \lambda R_L (1 - R_L)$;

$\lambda = N_L / N_R$ ;

$\delta = deff_R / deff_L$ ;

$R_r = \sum_{i \in r}^{N_r} x_i \Big/ N_r$ ; and,

$R_L = \sum_{i \in L}^{N_r} x_i \Big/ N_L$ , where $x$ is a binary variable set to 0 and 1.

The value of $\omega_o$ is set to one and $\alpha_0 = \alpha(w_o)$. The process is then iterated, obtaining successively $\omega_t = \omega(\alpha_{t-1})$ and $\alpha = \alpha(\omega_t)$. The process is halted when $|\omega_t - \omega_{t-1}| < \varepsilon$ and $|\alpha_t - \alpha_{t-1}| < \varepsilon$, where $\varepsilon$ is a predefined level of tolerance in the error (here $10^{-4}$ following Choudhry). No more than four iterations were required for each stratum. The value $\omega$ is interpreted as relative sampling rate in the list frame ($\omega = W_R / W_L$). See Choudhry (1989) for the derivation of these values.

OPTIMUM ALLOCATION BETWEEN STRATA

The sampling fractions for each stratum are calculated following Groves (1989). The sampling variance of the overall mean is minimized for the *h*th strata when:

$$f_h = \frac{n_h}{N_h} = \frac{KS_h}{\sqrt{c_h}} \tag{O.3}$$

where

$$S_h = \left( \frac{\sum_{i=1}^{n_h} w_{hi}\left(x_{hi} - \bar{x}_h\right)^2}{\left(n'_h - 1\right)\sum_{i=1}^{n_h} w_{hi} \Big/ n'_h} \right)^{1/2}$$

where

$n'$ is the number of nonzero weights in the *h*th stratum;

$w_{hi} = W_h$ (the inverse sampling rate of the appropriate frame in the *h*th stratum);

*K* is a constant to produce the desired sample size; and,

$c_h$ is the variable cost for the *h*th stratum.

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