

**National Jewish Population Survey 2000-01:
A Guide for the Perplexed***

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Abstract

The National Jewish Population Survey 2000-01 (NJPS) has been the focus of substantial controversy and misinterpretation. Because of both its design and implementation, there are numerous difficulties in using NJPS data to understand the contemporary American Jewish community. This “guide” analyzes key issues about NJPS, including its sampling design, response rate, assessment of Jewish identity, weighting, and structure of the instrument. Although NJPS provides valuable information for scholars and policy users, focusing on population estimates and ignoring methodological limitations makes misinterpretation likely. Estimates, to the extent they are offered, need to be discussed as ranges within confidence intervals and with caveats about their interpretation. A particular caution is that analysis of characteristics of the population must distinguish between the entire estimated Jewish population and those who are “persons of Jewish background.” The present guide outlines appropriate use of the NJPS data set, emphasizing multivariate analyses and investigations of relationships among variables. NJPS represents the most complex study of the Jewish population ever conducted, and while methodological limitations need to be taken into account, the study provides potentially important insights into the character of the American Jewish community.

The National Jewish Population Study 2000-01 (NJPS)¹ is the most complex and expensive social scientific study of American Jewry ever attempted. Even before the initial release of findings in 2002, and more intensely since the data became available in 2003, NJPS has been the focus of widespread discussion and controversy (see DellaPergola, 2003; Goldberg, 2003; Saxe and Kadushin, 2003a; Saxe and Kadushin, 2003b; Sheskin, 2003). NJPS undoubtedly provides important data on the American Jewish community, but drawing out its implications requires substantial effort and is fraught with difficulties. It will be a valuable source for analyses of interrelations between variables, but the

population estimates are problematic and continued emphasis on them ultimately will detract from the study's utility.

Scholars and policy analysts concerned with the American Jewish community need to understand the study's inherent limits and how to use the findings appropriately. Although NJPS interviewed several thousand American Jews, by itself this does not mean one can reliably estimate the number of Jews who live in the United States and how they differ from one another. Tentative answers to these questions are possible, but appropriate use of NJPS requires nuanced presentation and acknowledgement of uncertainty.

Survey Research

Socio-demographic studies of the Jewish community in the United States have a long history (see Diamond, 1977; Goldstein, 1981; Masarik, 1992; Ritterband, Kosmin, and Scheckner, 1988; Schmelz, 1969; Seligman, 1951). The U.S. Census does not include questions about religion; hence the need for independent studies.² Given the small percentage of Jews in the American population, conducting a non-governmental census of all Jews is impractical. Instead, it is necessary to conduct a survey sample. This is a daunting task, because even if all those contacted were to cooperate, one would have to interview nearly 50 non-Jewish Americans to find each Jew. Since the goal is not only to count the number of Jews, but also to learn about their characteristics (e.g., denomination, intermarriage, Jewish education), as well as the characteristics of subgroups, a large sample is required. To understand the characteristics of subgroups of Jews, such as Nazi victims, college students, or day school graduates, would require thousands of interviews of Jews, and hundreds of thousands of screening interviews.

The gold standard of survey data collection is a random sample in which every unit in a defined universe has a known probability of being included (Sudman and Bradburn, 1983). Thus, if one surveys 100 households in a community of 10,000 households, each household has a 1% chance of being selected. Knowing these probabilities is essential to being able to compute confidence intervals. It is assumed that a list of everyone in the sample universe, or its equivalent, is available. Because no such list exists, it is not possible to select a simple random sample. Instead, in area probability samples, random selection is used to identify areas (e.g., counties), then small geographic units within those areas, and finally households within which individuals are enumerated. Within each layer (stratum), the probability of its inclusion is set by design, down to finding a single person to interview within each household. If there are 100 counties, each with 10 districts, then each district has a 0.1% chance of being selected. Such procedures are very expensive, particularly if households actually are visited. The cost could be reduced

by conducting long interviews only with Jews, but most of the budget and effort would be spent mapping areas and contacting individuals to determine whether or not they are Jewish.

Beyond the particular issues of surveying a scarce population, the cost of research is rising while cooperation has declined tremendously. The General Social Survey (GSS) (Davis and Smith, 1992; Smith, 1992), the most prominent survey that uses such techniques, used to obtain a response rate greater than 90%. Current response rates are only 75%, suggesting that one would have to screen 333,333 households in order to interview 250,000.

Since area probability samples using in-person interviews are prohibitively expensive, telephone surveys have become the alternative. Although there are “coverage” problems (*e.g.*, prisoners, soldiers, and nursing-home residents may not be accessible), virtually all Americans have personal telephone access (Thornberry and Massey, 1988:38). Surveys are conducted by obtaining lists of every working telephone number and randomly dialing working numbers (random digit dialing [RDD]; Waksberg, 1978). Unfortunately, it is increasingly difficult to reach people by telephone. Even with multiple call backs, some numbers never answer and many of those reached refuse to cooperate. Not too long ago, a quality-oriented public opinion firm that was willing to stay “in the field” for a month could get a 75% or better response rate.³ The cooperation problem is not unique to telephone surveys; response rates for all survey modes have declined in the United States in recent decades (Couper, Traugott, and Lamias, 2001; Dillman, 2002; Groves, Cialdini, and Couper, 1992; Rubin and Thomas, 1966). Lower response rates have serious implications because the characteristics of those who are unavailable may be different than those who are available.

NJPS 2000-01

The purpose of NJPS was “to provide a comprehensive social and demographic portrait of the American Jewish population” and was “intended to become a valuable source of data on the Jewish community... providing crucial information to its sponsor, the United Jewish Communities (UJC), local Jewish federations, synagogues and other major participants in the Jewish community” for “... communal planning, policy making, financial resource development, Jewish education, scholarly research” (United Jewish Communities, 2003a). NJPS was conducted as an RDD telephone survey and was intended to have a sufficiently large “*n*” to facilitate analysis of specific sub-populations. Although some argue that methodological concerns about NJPS are solely an academic concern, understanding the methodology has profound implications for any policy use of the data.

NJPS Response Rates

NJPS's designers strove to develop a comprehensive portrait of American Jewry, but its usefulness is limited by its lack of success in obtaining cooperation from those it attempted to contact. More than 1.2 million phone numbers initially were selected, but interviews were conducted with only about 180,000 people. Some selected telephone numbers turned out to be business numbers or were out of service, but the key problem was that the response rate was extremely low. The overall response rate for screener questions was about 28% (United Jewish Communities, 2003c).⁴ However, since many people refused to participate in the longer interview (which asked about substantive issues), the final response rate for Jews and Persons of Jewish Background (PJBs; see below) was less than 20% (Schulman, 2003). That more than 80% of the households contacted did not participate raises a host of questions about the types of individuals who were more or less likely to be available and willing to answer.

To assess the NJPS sample, consider the contrast with its predecessor, NJPS 1990. Unlike the 2000-01 survey, NJPS 1990 screened for Jews in the framework of an omnibus survey (covering a variety of topics). The reported response rate was 47% to the omnibus survey, with a response rate of 98% to the questions on religion (Kosmin *et al.*, 1991), yielding an effective response rate of 46%. This "screening" phase identified a total of 5,146 Jewish households. Of these households, only 2,506 completed an interview in the survey phase. Treating the screened households as a separate sample, the response rate was 70% for the survey phase, leading to an overall response rate for NJPS 1990 of about 32% (46% x 70%).⁵ The omnibus survey was organized in one-week units, which allowed little time for recontacting non-responsive households or converting refusals. Also, marketing questions were asked in addition to the screening questions. It is not clear how embedding screener questions in a marketing survey affected responses regarding Jewish identity.

Although NJPS 2000-01 utilized different methods, an independent replication of the 1990 study, the American Jewish Identity Survey (AJIS) of 2001 (Mayer, Kosmin, and Keysar, 2001), is a point of comparison for response rates. AJIS was a replication of 1990 (with a few changes in questions) and surveyors screened more than 50,000 U.S. households. They found "1,668 households in which at least one person qualified as Jewish or of Jewish background" (Mayer *et al.*, 2001:47), achieving a net response rate of 16%, roughly the same as NJPS 2000-01.

Another relevant comparison is the Survey of Heritage and Religious Identification (HARI) 2001, a one-stage survey where interviews were conducted immediately upon identifying the household (Tobin and

Groeneman, 2003). HARI's reported response rate was 29%, about the same as that for the screening questions of NJPS and AJIS 2001. This suggests that the source of the problem may not be whether the screener was embedded in an omnibus survey or how it was worded but the way in which the transition from screening questions to the survey itself is handled, particularly the switch from the initial informant to a randomly selected household member. In HARI, the interviewer immediately asked to speak to the household member with the most recent birthday, while NJPS and AJIS only switched to the selected after the person who answered the telephone had given information about other household members.

High levels of non-response in NJPS are a potentially serious source of sampling bias, although how much they affect the results is difficult to assess. Most studies of the effects of non-response have been conducted using government-sponsored studies with far higher response rates (70% to 90%) than NJPS. In these studies, lower response rates tend to be associated with urban location, higher education, single-person households, no children in the household, and middle-aged household members (Groves and Couper, 1998); call screening is highest among residents of large cities and suburbs, full-time employees, homemakers, and people living in the Northeast and South; refusals are most likely from older adults, the most affluent, one-adult households, residents of large cities and surrounding suburbs (Tuckel, 2001). Some of these characteristics, particularly urbanicity, residence in the Northeast (Goldstein and Goldstein, 1996), and higher education and income (Hartman and Hartman, 1996) are characteristic of Jews. Thus, there is good reason to believe that NJPS results are underestimates of the size of the Jewish population.

Screening for Jews

In contrast with earlier national studies based on omnibus surveys, NJPS was specially commissioned and began by asking, "What is your religion, if any?" Those who said they were Jews were told that this was a survey sponsored by "all the Jewish federations in the United States." Further questions were asked about whether respondents had a Jewish mother or father, were raised Jewish, and considered themselves to be Jews (see Table 1). All Jewish households were offered the main interview, with the respondent being randomly selected from qualified household members (United Jewish Communities, 2003c), as were a proportion of households where one or more adults were identified as not currently Jewish but having some Jewish background (Persons of Jewish Background); a small proportion (c. 1.4%) of non-Jewish households were also given a long interview (the National Survey of Religion and Ethnicity) in order to generate comparison data. The content of the

interviews varied according to a household's status as Jewish, PJB or non-Jewish.

Not everyone accepted the longer interview, and further complicating the analysis, the survey contractor (RoperASW) lost about two-thirds of the screening data for these refusers ("screener incompletes") (Schulman, 2003:12-13; United Jewish Communities, 2003c). For those parts of the NJPS for which we have data on refusers, the response rate for the main interview was 61% for persons reached who were identified as Jews, 55% for PJBs and 41% for non-Jews. The loss of the screening data has serious implications, as the weighting scheme (discussed below) must include estimates for the missing data, raising additional possibilities for error. To the extent that refusers are different than those who completed the interview, they need 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 (Dillman, 1983; Groves *et al.*, 1992; Groves, Presser and Dipko, 2004; Pealer *et al.*, 2001).

Sampling and Weighting

NJPS was designed to provide information about the size of the Jewish population in the United States. But fewer than 5,000 Jews completed an interview and this sample needs to be "scaled up" to represent millions of people. To do so, weights need to be applied to each respondent. Weighting depends on a number of assumptions, and its ability to correct bias is a function of the amount of information available about the population.

Weighting NJPS incorporates two distinct techniques. First, because the survey was not a simple random sample, weights need to compensate for unequal sampling. These weights are called "design effect" weights and are built into the design of the study. For example, certain areas in New York identified as having a dense Jewish population were oversampled by a factor of 3 (the design weight is 0.33 [$1/3 = .33$]). Second, weights are used because different types of people may have responded at different rates, and the sample may not accurately represent the population. These are called "post-stratification" and are developed empirically after design weights have been applied. If a survey finds that 60% of its respondents are women, far above the actual population rate, females would be "downweighted" while males are "upweighted."

In NJPS, the United States was divided into seven sampling regions ("strata") defined by the estimated density of Jewish population. Areas

with greater Jewish population density were oversampled. In order to convert the stratified random sample (different strata were sampled at different rates) back to represent the original population, design weights have to be applied. The original geographic region design weights were changed after the initial replicate because of low response rates and the need to make sampling more efficient (United Jewish Communities, 2003c:45).

A second type of design weight in an RDD survey accounts for the fact that the more telephone numbers in a household, the greater the chance of that household being sampled. Typically, respondents are asked about how many telephone lines in their household receive voice messages. The design weight here is the reciprocal of the number of phone lines (2 phone lines = $1/2$ = a weight of 0.5).⁶ 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 are missing for about 40% of the Jewish sample. To correct for this problem, study analysts sorted completed screening interviews by subsample (Jew/PJB/non-Jew) and by household size and applied the average weights of these cases to the other screening interviews (United Jewish Communities, 2003d). Although there may be no reasonable alternative, it makes these weights potentially unreliable. If one does not include these weights or makes different assumptions about the number of telephone lines, the scaled up estimate of the Jewish population varies by as much as a million persons.

The third type of design weights apply to the longer interviews for which Jews, PJBs and non-Jews were selected at different rates (see above). The intention was to interview all persons defined as Jews with a long interview, while some PJBs were to be given a shorter interview that did not ask questions such as how frequently they attended synagogue (on the presumption that these questions would not apply). The exact proportion of PJBs selected varies by replicate.⁷ The PJBs need to be upweighted to compensate for the fact that only some were offered the full interview while others were missing by design. A replicate is a random draw from the telephone numbers that constitute the sample. There were 22 such replicates in the NJPS. The purpose of these 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.

A fourth type of design weight applies to individuals. Thus far, the focus has been on households, not individuals. Although household composition is important, the survey also includes individual opinion items. The more individuals in the household over 18, the less likely a

particular individual is to be interviewed.⁸ 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 is not a serious problem, since very few were offered a long interview. All Jews were offered a long interview, so about 73% of the refused respondents are missing. UJC chose to estimate the number of missing screeners on the basis of replicates 16 through 22 (which represent an estimated 30% of the total screening interviews), and use those estimates to arrive at estimates of the size of the population of Jews (and PJBs and non-Jews).

The problem regarding post-stratification estimates is made even more critical for NJPS because of the high percentage of non-respondents. 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? Post-stratification 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. Since there are no census data on religion, these are the only adjustments that can be made. 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 is no alternative to the assumption that non-respondents are 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 the Jewish and PJB respondents. For Jews, adjustment by age and sex are impossible because we have no outside yardstick against which to weight Jews, as the only information we have is through surveys like NJPS.

There are several obvious problems with the post-stratification weighting of NJPS. Part of the problem is caused by the imputation of a large number of missing screener cases and the lack of information about telephone lines. The number of persons in a household is, of course, important to weight individuals, but this number is dependent on whether the household was defined as Jewish or PJB (see note 13). The current weights are based on the original assignment of these categories (see below), but the figures reported for the Jewish population are based

on a somewhat different assignment, which introduces ambiguity into the weighting scheme. 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 the refusers in these latter replicates have more non-Jews in their households.

Post-stratification reweighting by Phillips *et al.* (2004) that accounts for the screener data makes clear the importance of these issues. This analysis, for example, produces different figures for intermarriage rates (the total rate of intermarriage is estimated to be 37% as compared to 31% 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 does not dramatically affect other measures; in most cases, the differences due to the weights versus non-weights tend to be less than a few percent.

What is clear is that population estimates based on UJC's weights depend on a series of questionable assumptions. The uncertainty introduced by the low response rate is particularly troublesome. More problematic, as suggested above, is the consistent evidence that Jews were less likely to respond than non-Jews (Schulman, 2003; Sheskin, 2004; Tobin and Groeneman, 2003; United Jewish Communities, 2003h). UJC's documentation on NJPS (Kotler-Berkowitz *et al.*, 2004b:31) recognizes the non-response problems, noting that "many researchers believe that the methodologies of survey research may yield undercounts of the Jewish population."

Weighting generally has costs in terms of increased variance over simple random sampling. This increased variance needs to be taken into account (see below). Also, when the data are reported in analyses using the weights, the assumptions underlying the weighting system are accepted *de facto*. If the analyst does not agree that NJPS can be scaled up to represent the population of Jews in the United States, other strategies must be used to compensate for both design effects and biases associated with which respondents were more likely to answer the survey.

One strategy is to accept the weights as the best way to compensate for the sample design and response biases, but to divide the weights by their mean in order to reduce the effective sample size to the unweighted sample size. Some statistical programs automatically adjust design weights in order to bring the survey "n" back to its original size.⁹ Doing so creates reports that are meaningful in terms of coefficients or percentages but not population figures. Such analyses should multiply the standard error by the design effect (discussed below) when evaluating for statistical significance. For programs that do not calculate design effects (e.g., SPSS without the complex samples module), a conservative design effect of 1.2 to 1.5 should be used. This is the only option when running cross-tabulations, which should include confidence intervals for

each cell. If Chi Square or other measures of independence or association are reported, they too need to be adjusted by the design effects.

Another strategy is to insert the design effects as “control variables” in analyses rather than to use survey weights. This can be done when performing multivariate analyses. Typically, the authors have used sex, household size, age, geographic region or sampling strata, education of the respondent, and often denomination. The study may be biased in terms of the proportion of people it was able to secure with these characteristics, but inserting them as controls offers some advantages. First, while the study may be biased in terms of sex (e.g., as with many surveys, there are too many women: Smith, 1979), age, education, and denomination, no post-stratification adjustments are available for these variables. Second, the analyst gains information—for example, how gender affects the outcome being studied. Third, weighting is a linear adjustment. The effect of moving from a household size of one to two is assumed to be the same as moving from two to three. But there may be important effects of moving from a one-member household to three or more, but less of an effect in moving from one to two; a classic finding is the correlation between lighting a *Hanukkah* and the presence of children in the household (Sklare and Greenblum, 1967). When variables are introduced as controls, they can be introduced as categorical (non-linear) variables as well as linear ones. Fourth, there may be interaction effects between these control variables (e.g., Orthodox men and women may differ in their responses). By using these control variables, the analyst is aware of the possibilities of interaction effects even if he or she does not necessarily introduce them. NJPS has a large sample and often can support all of these controls explicitly, especially if one is willing to assume linearity and non-interaction for most of them.

Stratified designs almost always have larger standard errors than do simple random samples. That is to say, for example, that a given percentage of the proportion of Orthodox Jews (9.6%) would lie, in 95 of 100 samples, between a higher bound of 10.7% and a lower bound of 8.5%. Design effects increase the standard errors and there are ways to calculate these, but one must use specialized programs such as Stata, WesVar or SUDDAN or special procedures for complex samples in Strata or SPSS rather than programs like MicroCase.¹⁰ (See Schulman, 2003, Appendix 5 for tentatively suggested coefficients of variation.) They range from 0.8 to as much as 37, depending on the size of the sample segment.

Counting Jews

Given that the NJPS weights are problematic with respect to generating population estimates, how can one estimate the number of Jews or the number of Jews that have a given characteristic? The short answer is

that it cannot be done with any degree of reliability or confidence. For NJPS, estimates of population size refer to the universe that was sampled, which was intended to include all non-institutionalized Jews in the United States. As described above, that universe is unknown, and there is strong evidence that Jews were underrepresented in the sample intended to estimate it. The population estimates therefore refer to a universe that we do not understand and are, consequently, impossible to interpret.

The caution against using population projections applies, in particular, to population estimates of smaller sub-groups; they have larger confidence intervals and are more susceptible to sampling bias. For example, the UJC's recent report on Jewish college students (United Jewish Communities, 2004b) presents population estimates and graphs based on cross-tabulations without confidence intervals. 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. In this survey, respondents in this age category, adjusted by the original respondent weights, were 56% female rather than the nearly even split in the population. College students among this age group were much more likely to live at home than shown in comparable data. Dormitories, for the most part, are not part of an RDD frame and college students are notoriously difficult to contact for survey research. They are unlikely to be at "home" from 6-9 PM and, increasingly, they rely on mobile phones (which are for the most part inaccessible to survey researchers). 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.

Although population estimates are problematic, proportions remain a valid way to present data because they are less sensitive to weighting adjustments. Yet proportions need to be presented with confidence intervals. This is particularly important when dealing with small subpopulations, because confidence intervals increase as sample size falls. For example, the UJC report on Nazi victims residing in the United States gives point estimates for both percentages and population figures (Kotler-Berkowitz, Blass, and Peckerman-Neuman, 2004a:3). In our view, the text should read as follows:¹¹

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.

Although the use of population estimates from NJPS cannot be endorsed, confidence intervals for populations referred to above range from 98,000 to 146,000 (rounded to the nearest thousand) victims, 67,000 to 107,000 under Nazi control, 22,000 to 49,000 “flight cases,” 12,000 to 29,000 inmates of concentration camps, and 8,000 to 23,000 people incarcerated in labor camps.

Who is a Jew?

Along with weighting issues, a key substantive problem with NJPS is deciding who is Jewish. The NJPS interview began with an open-ended question, “What is your religion, if any?” Other surveys, such as the GSS, ask about religion in closed-ended form: “What is your religious preference? Is it Protestant, Catholic, Jewish, some other religion, or no religion?” Schulman (2003) also speculates that placing the question at the very beginning of the interview may have turned some people off.¹² The usual practice is to ask sensitive questions like religion after the interviewer has gained some rapport with the respondent. The screener went further, however, also asking, “Do you have a Jewish mother or a Jewish father?”; “Were you raised Jewish?” and finally, “Do you consider yourself Jewish for any reason?” The questions were repeated for each member of the household (asking the respondent about other members of the household). Respondents to the longer interview who were randomly selected from eligible household adults were again asked these questions (unless this person had also answered the screener).¹³ These questions differ slightly from questions asked in the NJPS 1990 and these differences contribute to ambiguity over whether the Jewish population has increased, decreased or stayed about the same since 1990.¹⁴

Table 1 details the differences between the two surveys. Although the differences may seem minor, they make it difficult to compare the two surveys. In particular, the way NJPS 1990 classifies people as Jewish or not relies on the item about being born Jewish that is not present in NJPS 2000-01. Whether or not this was a good question (*c.f.* Della-Pergola, 1991 and Phillips, 1997:85-87) is moot. The result is that direct comparisons between NJPS 1990 and 2000-01 are problematic. “Core Jews” as reported by NJPS 1990 consisted of Jews by religion plus Jews of no religion. In contrast, NJPS 2000-01 relied on 19 different Sample Allocation Codes (SAC). Each SAC was assigned to one of three

groups: Jews, PJBs, and non-Jews (NSRE, as they received the National Survey of Religion and Ethnicity). Because PJBs received a truncated interview, if an analyst decides to change some of them to a Jewish classification, he or she should be aware that these interviews will have some questions missing—for example, keeping kosher or belonging to Hillel while at college—which are present in the full Jewish interview.

Table 1
Comparing NJPS 1990 and 2000-01 Screener Questions

	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?*** Was your father born Jewish?***
Self-identification	-	Do you consider yourself Jewish for any reason?

* 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?”

The original sample allocation codes used to determine who would be given which interview were sufficiently different from the 1990 NJPS classification that a new classification scheme was developed after completion of the survey (see North American Jewish Data Bank, 2003). The match to NJPS 1990 is inexact, however, because the basic questions differ. Jews *plus* Jewish-connected equal NJPS 1990 core Jews; the Jewish-connected category has *no parallel* in NJPS 1990. If an analyst wants to include the Jewish-connected persons along with the Jews, then it must be noted that more than 300 of these respondents were not asked key Jewish behavior questions.¹⁵ Since individual weights depend in part on how many persons are in the household and were calculated separately for Jewish, PJB and non-Jewish households, changing the number of persons who are counted as Jews in Jewish households and who should have been interviewed may affect the respondent and selected child weights (these weights remain based on

the original classification scheme). Since these weights have not been changed to reflect the new classification, the size of possible effects is unknown. Household weights are *not* affected by the reclassification.

Table 2
The Effects of the UJC Reclassification of Respondents

Post-Survey Classification	Initial Classification			Total post Survey Classification
	Jews	People of Jewish Background	Non-Jews	
Jews	4,147	0	0	4,147
Jewish-connected	73	303	0	376
Non-Jews of Jewish background	264	361	0	625
Non-Jews	0	0	4,027	4,027
Total initial classification	4,484	664	4,027	9,175

The number of Jews found by NJPS is obviously dependent on the definition of who is Jewish. Contemporary surveys of American Jewry use a sociological definition, based on individuals' descriptions of themselves, rather than a *halachic* definition. One advantage of NJPS is that intrepid analysts can use the data to construct their own definition. In addition, the data on multiple religions of all household members gives an extraordinary opportunity to study the effect of different mixes of religions within the same household. Both of these advantages have yet to be explored to any great degree.

Instrument

In addition to the complex sampling design and screening protocol, the NJPS survey instrument is also complex. It includes elaborate "skip" patterns that confused even the programmers who developed the Computer Aided Telephone Interview (CATI) system used to prompt interviewers and record the data. Given the ambitious range of topics the survey was intended to assess, the intent was to avoid asking people questions that did not apply to them. Thus, for example, the question about *b'nei mitzvah* depends at a minimum on answers to whether a person had a Jewish mother, a Jewish father, was being raised Jewish, year of conversion to Judaism (if any), age, and gender. The problem was that this resulted on occasion in questions being omitted that should have been asked (see United Jewish Communities, 2003c, 2003f).

Missing by design. Apart from CATI programming errors, the skip patterns at times were designed in ways that may cause problems for analysts. For example, the questions about conversion from Judaism (Q021, Q022, Q022A) were only asked of PJBs who identified with another religion. But the survey considered as Jews, rather than PJBs, people of Jewish parentage who said they are currently exclusively Christian (or any other non-Jewish religion) provided they consider themselves to be Jews; thus, they were not asked questions about conversion from Judaism. Users must carefully examine the survey instrument to check for skip patterns about any variable of interest.

Questions not asked of PJBs. Users of NJPS will need to consider carefully how to deal with questions not asked of PJBs. The approach used by UJC, presenting analyses of questions not asked of PJBs without a “no information” category may be misleading. Unless one reads the caveats very carefully, it is easy to assume the proportions given apply to the entire population.¹⁶ The impact of the questions not asked of PJBs can be seen when we examine observance of *kashrut* at home, reported as practiced by 21% of “Jewish” people. When zeroes are imputed for people not asked the question, the proportion observing *kashrut* drops to 17%. Since *kashrut* is extremely unlikely to be practiced by those who are only “Jewish connected,” it is a reasonable solution. Treating the proportion who light Sabbath candles usually or always in the same way lowers the rate from 28% to 23%.¹⁷

Where it is unreasonable to impute zeroes for PJBs, a category accounting for these missing-by-design values is an important check on incorrect interpretation. The effect of doing so can be seen for current denomination, which was not asked of many “Jewishly connected” respondents.

The question on current denomination systematically excludes people with marginal Jewish identities. It therefore makes the study of denominational switching difficult, if not impossible. UJC’s analyses of Jewish denominations fail to identify the unknown or missing category, thereby giving overly optimistic pictures of market share and denominational retention (United Jewish Communities, 2004a, 2004c, 2004d). They also directly compare Jews in a given denomination (drawn from the 4.3 million population) with the entire Jewish community (5.2 million population), inviting misunderstanding.

Labeling missing data. A related issue is that there are no markers for missing data, other than when an individual refused or didn’t know the answer to a question. That is, the usual categories of Does Not Apply (the question was not asked by design) and No Answer (the question was asked but no answer was obtained) are not present in the dataset. A respondent who was skipped on purpose appears as a blank cell, just like a person who was never asked a question due to CATI

Figure 1
Percentage Identifying with Current Denomination Without Unknowns (“Jewish” but not “Jewish Connected” Respondents)

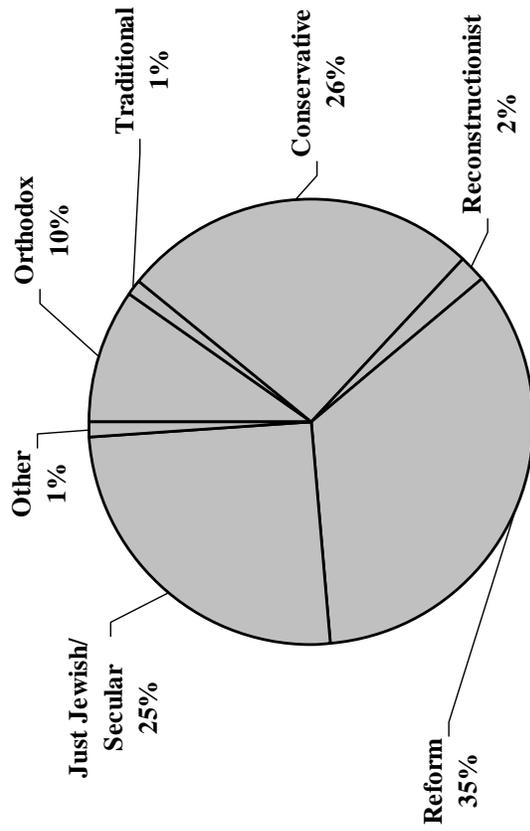
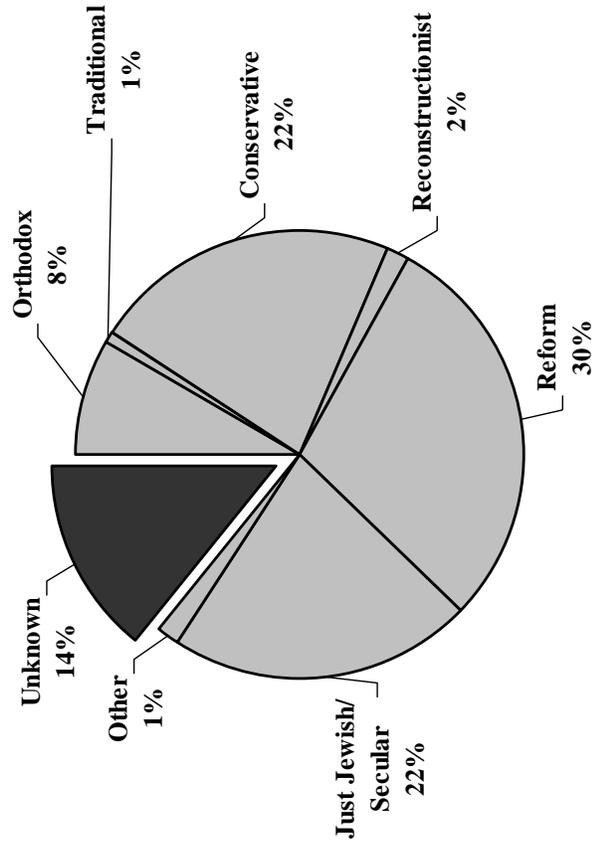


Figure 2
Current Denomination With Unknowns (“Jewish” and “Jewish Connected” Respondents)



errors. Nor is there any differentiation made for people skipped for different reasons, while researchers may wish to deal differently depending on why a case was skipped.

Complex coding. In an effort to give survey analysts flexibility, the codes for questions are often very detailed, resulting in multiple variables derived from one question. Since, for example, persons could receive more than one kind of Jewish education, there is a provision for the number of schools of different types a person might have attended, and an elaborate code for the first, second, third, etc., school mentioned. To discover how many years a respondent attended a particular type of Jewish school requires a significant amount of programming.

These features of the survey will require that analysts read and reread the documentation on the survey released by the UJC, including the *Study Documentation* (United Jewish Communities 2003c), the *Datafile User Guide* (United Jewish Communities, 2003f) and the independent methodological report developed to assess the study (Schulman, 2003). It is not possible to utilize data correctly without a review of the detailed documentation. In addition, analysts will need to check the accuracy of recodes, particularly for missing cases.

GUIDELINES FOR USE

Despite methodological problems, data from NJPS 2000-01 have the potential to enhance our understanding of the American Jewish community. Some of the ways that NJPS can be used already have been suggested. Below, these suggestions are summarized and elaborated in terms of their implications for both scholarly and policy analysis.

Estimates of Population Size

NJPS does not generate reliable estimates of the size of the Jewish community or subpopulations within it; population estimates produced from NJPS data, including those in the study report and other UJC publications, are likely incorrect. There is strong evidence that Jews are less likely to participate in surveys, in part due to the demographic characteristics of the Jewish community (wealth, education, residence in the Northeast). If Jews are less likely to respond than Americans of other religions, the size of the Jewish community will be underestimated. The estimates are unreliable because the characteristics of the universe to be sampled are unknown.

Using Proportions

Given that population estimates are problematic, one alternative is to use proportions. The proportion of the sample with a particular characteristic or engaging in a particular behavior is less likely to be biased than population estimates because it is relative to the size of the group

being analyzed. If the Jews who did not agree to be interviewed are similar to those who did, it does not matter if the estimate of the size of the Jewish community is 5.3 million or 5.7 million because the proportion of synagogue members (or other items of interest) would remain the same.

Even so, proportions are not without their dangers. An estimate by itself tells us little, and the smaller the sample size, the larger the confidence interval. As an example, estimates of the proportion of women among various Jewish denominations are shown below with confidence intervals estimated at the 0.95 level (see Table 3).¹⁸ The proportion of females varies considerably from a high of 58% among Conservative Jews to a low of 47% among Orthodox Jews. The difference between Conservative and Orthodox Jews is significant at the 0.95 level, but not at the more conservative 0.99 level. Note, in addition, that other similar comparisons (*e.g.*, Reconstructionists vs. Orthodox) will have even wider confidence intervals, ranging from about 35% to about 62%.

Table 3
Percentage of Women in the NJPS Among the Different Denominations, with 95% Confidence Intervals

Current Denomination	N	Lower Bound	Estimated Percentage Female	Upper Bound
Orthodox	416	40.8%	46.5%	52.3%
Conservative	1,080	54.0%	57.7%	61.3%
Reconstructionist	84	36.0%	48.6%	61.5%
Reform	1,413	50.0%	53.1%	56.4%
Just Jew	883	46.9%	51.0%	55.2%
Other Jew	83	34.4%	48.2%	62.2%
No denomination	228	48.0%	54.8%	61.5%

Even if a survey is unbiased, it is preferable to make comparisons *between* groups rather than present figures that represent the entire population. This is because surveys are sensitive to even slight changes in question wording that can affect the absolute size of a reported proportion. Rather than say that between 26% and 30% of identified Jews usually or always light candles on Friday night,¹⁹ one might report that between 80% and 89% of Orthodox do this as compared with between 33% to 41% of Conservatives and 13% to 18% of Reform Jews. This highlights the significant variation between movements rather than the absolute proportion, which could be affected by question wording.

Subpopulations

If estimates of the Jewish community as a whole are biased and unreliable, and if confidence in the accuracy of estimates decreases with the number of respondents, subpopulation estimates based on a fraction of the sample definitely should be avoided. But these are not the only reasons, as data on subpopulations in NJPS may have other biases. For example, as we suggested, college students living outside the parental home appear to have been particularly difficult to find, resulting in a population estimate for this age group that is too low.

Choosing the Right Denominator

NJPS asked many questions only of people initially classified as current Jews. To maximize comparability with NJPS 1990, UJC counts some people who did not receive the full set of questions as Jews. The 4.3 million “Jews” *plus* the additional 900,000 “Jewish connected” people estimated by NJPS are as close as possible, given the different questions asked, to the “core Jews” in NJPS 1990. The result of the reclassification is that many “Jewish connected” respondents were not asked questions like current synagogue membership or observance of *kashrut*. Analyses that present data from the 5.2 million population then switch to the 4.3 million population (asterisked in UJC reports) for other questions are misleading. Although the asterisks are technically correct, they lead to misinterpretation.²⁰ Responsible presentation should keep the unknown as a category in analysis or impute zeroes for PJBs where such an assumption is reasonable (such as observing *kashrut*). Mixing estimates from the “Jewish” and “Jewish connected” samples in the same presentation is problematic, and few readers of such analyses may understand the caveats.

Weights

In theory, the weights provided on the NJPS dataset correct for sample biases. Unfortunately, many variables were not included in the weights. Confidence intervals, while strongly recommended, assume that the sample is unbiased. Analysts should avoid using simple frequency distributions and even cross-tabulations because the proportions may be affected by biases not corrected for by the weighting process. Multivariate analyses are preferable, as they address how variables interact and are more resistant to bias. Any estimates should be treated with caution and regarded, at best, as a very general indicator. One should attribute *substantive* differences between groups only if the differences are large (e.g., at least 10 percentage points for populations of 1,000) even if a more narrow range is statistically significant.

Comparing NJPS to Other Studies of Jewish Communities

The greater the differences in method (design, sampling, questions), the more difficult it is to compare the results of two studies. NJPS represented a major departure from other studies of American Jewish communities, both in how it sampled individuals and the kinds of questions it used to identify Jews. The apparent consistency of response rates across three recent studies of the American Jewish community (NJPS, AJIS and HARI) suggest that despite the differences in method, the overall level of success was about the same and may have reached similar groups of Jews. We do not know for certain, however, and any comparison across studies is fraught with difficulties.

As noted above, question wording may have an impact on responses and great caution is needed when different language is used. It is difficult to know if Jewish topics are particularly sensitive to wording, as studies look at different communities at different times using different methodologies. NJPS changed the wording of many questions from NJPS 1990. Users should check the exact question wording used (and categories of answers) between surveys in order to judge what comparisons, if any, can be made.

The definition of Jewishness used in surveys of Jewish communities also has implications for making comparisons. UJC's current definition of Jewishness is a close but not exact approximation of NJPS 1990. Other factors (*e.g.*, questions not asked of PJBs, the extremely low response rate), however, make comparison between the two studies extremely difficult. The AJIS definition of Jewishness, being identical to NJPS 1990, reasonably can be compared to NJPS 2000-01 when using UJC's revised definitions of Jewishness; when the AJIS dataset is made public, closer comparison will be possible. HARI is particularly problematic since it uses a unique definition of Jewishness; it is impossible to compare data from NJPS to the HARI report. Local studies use a variety of definitions that must be evaluated on a case-by-case basis.

An additional issue for all comparisons is that NJPS effectively has two standards of Jewishness, one for the short and another for the long questionnaire. Items not asked of PJBs are, in effect, impossible to compare to NJPS 1990, AJIS or any other study that does not use self-identification as a part of its classificatory scheme. In studies that *do* ask a self-identification question, it may be possible to construct an equivalent definition of Jewishness to NJPS. Yet, so many factors affect the extent to which it is possible to compare NJPS to other studies of Jewish communities that it is impossible to offer a general rule. Users should remain cautious in drawing (or accepting) comparisons between NJPS and other studies.

What NJPS 2000-01 Can Tell Us

Our focus has been on the limits of NJPS; in particular, that it does not permit reliable estimates of population sizes or yield accurate point estimates for subpopulations and that comparisons with others studies are problematic. *These problems, however, do not render NJPS useless; on the contrary, the survey provides valuable data for analyzing interrelations between variables.* Although much of the commentary about NJPS has focused on the population numbers, whether the Jewish population is growing or shrinking is less important than knowing which factors are associated with changes. To the extent that population figures are important, they are probably most useful at the local level where they can be used more directly for planning and resource allocation. But because there are too few cases from most communities for meaningful projections, the study never could tell us much about the size or characteristics of these areas.²¹

NJPS's principal use is to help local and national leaders understand which factors are associated with which outcomes. It was these analyses, not population estimates, that generated the most interesting findings of NJPS 1990: What variables are associated with increased probability of intermarriage? (Being raised by intermarried parents, lower levels of Jewish education, dating patterns, among others) (Phillips, 1997). How does moving affect involvement in Jewish life? (Negatively) (Goldstein and Goldstein, 1996). How do the lives of Jewish women and men differ? (Hartman and Hartman, 1996). Looking forward, NJPS will be used to better understand why intermarriage occurs and what its consequences will be, why some Jews cease to identify with the Jewish community and why some non-Jews choose to be Jewish, how the emerging category of "half Jews" think and behave, as well as more nuanced analyses of different types of Jewishness together with their prevalence and covariates, learning more about the factors affecting Jewish fertility, and many other topics. These issues are not only of interest to scholars; sophisticated analysis can illuminate the processes of Jewish life in ways that head counts and cross-tabulations do not. Non-academics should find in these data ideas that they can use to strengthen the community. Just as a decade ago, population estimates will be among the least important products of NJPS.

It is hoped that we are now in a position to move beyond arguing over the methodological merits and demerits of NJPS. We now know most of what NJPS can and cannot be used for. Let us move beyond debate about NJPS and use the available data to help us understand the nature of Jewish identity and engagement.

NOTES

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¹ Throughout the article, NJPS will refer to NJPS 2000-01. Earlier versions of NJPS will be referred to by their year (*e.g.*, NJPS 1990).

² Although the U.S. Census has never included a question on religion, other Census Bureau studies have addressed religious matters (Diamond, 1977). Between 1850 and 1936, the bureau conducted the Census of Religious Bodies, which included religious groups' estimates of the number of their members (see Stark, 1992). There were plans to include a question on religion in the 1960 Census, and a question on religion was included in the Current Population Survey of March 1957, but the proposal failed due to opposition on constitutional grounds, mainly coming from the Jewish community (Good, 1959; Ritterband *et al.*, 1988).

³ Response rates to market research always have been lower. People are more reluctant to participate in surveys that ask about shopping or specific kinds of products. These surveys used to get about 25% true response rates. The point is that there are different standards and expectations in the polling industry for opinion research, as compared with market research.

⁴ The date of the interview is not the same as the date of the replicate, so persons concerned about the effect of current events or interviewer "drift" should use the time stamp of the interview, not that of the replicate. The survey is called NJPS 2000-01 for good reason: only one-third of the interviews were completed in 2000.

⁵ These calculations use Waksberg's (1996:347) disposition codes. If one assumes that the survey phase is an extension of the omnibus sample rather than a separate sample taken from a high probability stratum, the response rate is 22.3% ($48.7\% \times (2,506 / 5,146)$).

⁶ No matter how carefully the question is phrased, there are some respondents who misinterpret it to mean the number of extensions they have for the same number. In this study, up to 13 numbers were reported in the screening interviews; up to six or more in the longer interviews (the number was truncated at six 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.

⁷ The proportion of PJBs selected for these interviews is not entirely clear from the documentation. It appears that in the first replicate (round

of interviews), 50 percent of PJBs were selected, and in replicates 2 through 15, 33 percent were selected. In replicates 16 through 22, all were selected.

⁸ 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 (PJBs), only PJBs could be selected. Non-Jewish adults were only eligible to be respondents in a household where there were no Jewish or PJB adults. Respondents are weighted for the number of eligible adults in the household. In other words, in a household with two Jews, a PJB 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).

⁹ The `pweight` command in Stata (StataCorp 2004), for instance, adjusts population weights to base inferential statistics on the unweighted number of cases.

¹⁰ Versions 13.0 and above of SPSS have an optional complex samples module.

¹¹ These analyses were performed using SVY commands in Stata (2004) using Westat's (2004) `RESPWT` variable and a merge of the NJPS and NSRE datasets (United Jewish Communities 2003e), transferred to Stata/SE format using `dfPower DBMS/Copy v8.0.0` (DataFlux Corporation 2002). An alternative strategy would be to estimate in `WesVar` (Westat, 2002) using Westat's (2004) jackknife weights, which we found to produce identical results to Stata.

¹² This may not be the case, as AJIS 2001 and HARI 2001 had similar screener response rates despite asking the questions about religion later on in the interview, as we discuss regarding response rates.

¹³ The questions about Jewish parentage are exceptions to this, not being asked of people who were identified in the screener as having no Jewish parent.

¹⁴ Tobin and Groeneman's (2003) estimate of 6.02 million Jews is even more difficult to evaluate. The authors use a different definition of who is a Jew, different questions to ascertain the information needed to classify people as Jews or not, and a different context in which the questions appear. With so many simultaneous changes, there is no common standard with which to compare the study to either NJPS 1990 or 2000-01 or AJIS 2001. When the dataset becomes available, it may be possible to find enough common ground between the definitions of Jewishness used in these studies to develop equivalent projections of the U.S. Jewish population in order to test the authors' claim that their methodology reduces Jewish refusals and false negatives. Its estimate is based on a

sample of 259 Jews, thus having much wider confidence intervals than either NJPS 2000-01 (4,523 Jews and Jewish-connected people) or AJIS 2001 (1,668 Jews).

¹⁵ The value 1 (“Jewish”) on variables J1 through KJ_CH (the “J code” variables for each household member, plus spouse, unmarried partner and randomly selected child) in the NJPS dataset includes *only* people who would have been classified as Jews in NJPS 1990 *and* for whom a full interview is available.

¹⁶ This is not a theoretical concern. A recent *Jewish Week* article (Nussbaum Cohen, 2004) directly compares the distribution of current denominational identification between NJPS 1990 and 2000-01, despite the fact that many people who would have been asked what denomination they identify with in 1990 were not asked in 2000-01.

¹⁷ Depending on the question, imputing zeroes for PJBs may be a reasonable procedure for multivariate analyses. Such assumptions need to be clearly noted, however.

¹⁸ The confidence intervals are based on the existing weighting scheme, since there is no other. They therefore are only approximate. Further, since the weights are based on a projection of the estimates for the replicates 16 through 22 onto replicates 1 through 15, the degrees of freedom properly should be much less than the full sample. It is likely, therefore, that the confidence intervals as calculated are too small.

¹⁹ *Halachah* might suggest asking not about lighting candles but reciting the blessing over the candles. This might lead to a different proportion of those reporting candle lighting.

²⁰ A recent *Jerusalem Post* Op-Ed (Weil, 2004) claims that 69 percent of unaffiliated Jews light Hanukkah candles and 39 percent fast on Yom Kippur (both figures taken from Table 10 of Kotler-Berkowitz *et al.*, 2004b:11). This simply is wrong. An estimated 69 percent of “Jews” and “Jewish connected” people light Hanukkah candles, while 39 percent of “Jews” *but not* “Jewish connected” people fast on Yom Kippur – these proportions are based on different denominators and cannot be compared.

²¹ Local data is available in the form of studies of individual Jewish communities, many of which can be found at the North American Jewish Data Bank website (<http://jewishdatabank.org>). See Sheskin (2001) for the results of community studies on selected topics.

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