

American Jewish Demography: Inconsistencies that Challenge

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When I first submitted a title for this paper, I suggested “American Jewry in the Mid-1980s – the Known and the Unknown.” I intended to update my 1980 review of national and community studies (Goldstein, 1981), with particular attention to changes since then in composition, patterns of distribution, and growth or decline resulting from the combined effects of fertility, mortality, migration, intermarriage, and assimilation. As the proposed title suggested, anticipating that our knowledge would still not be complete, the pessimist (or should I say realist?) in me argued that attention also be given to identification of gaps in our knowledge.

At least three considerations argued for modifying my focus, and my title:

(a) For a short paper, such as this must be, the goal was far too ambitious.

(b) My preliminary review of available material suggested that the underlying and general patterns observed in 1980 seem to have continued during the succeeding half decade. In the interval, serious questions have been raised about the validity of earlier projections pointing to population decline due to below-replacement fertility and high intermarriage rates (e.g., Cohen and Goldscheider, 1984); as yet however, no firm basis seems to exist for reversing the previous findings about the general levels of these demographic processes in recent years.

(c) Most important, very few published studies give adequate information on their study design, methods of data collection, specific coverage of the samples, and definitions of basic concepts; most do not provide comparable categories in published tabulations; and they suffer from an absence of adequate control variables in the analyses. All these limitations argued against attempting the proposed review. The deficiencies precluded fair comparisons of the published results reported by different studies. This dilemma might be partly resolved by special tabulations from data tapes, but this is still difficult to achieve, given the absence of central depositories and resources. A full solution requires coping with more basic problems of design, concepts, data handling, and analysis. Such a situation complicates any effort to generalize about the American Jewish community as a whole despite the growing number of individual community studies; nor can we, except in rare instances, make comparisons between individual communities or evaluations of changes over time for the limited number of places that have had more than one study.

These considerations led me to abandon the attempt to provide an updated overview of changes since 1980. Instead I believe this paper can best serve the purpose of raising the quality and usefulness of future research by stressing, at this international congress, as I did at a national symposium (Goldstein, 1984), what I see as some

of the major problems and needs of Jewish demographic studies in the United States. In particular by calling attention, on an illustrative basis, to some of the inconsistencies that characterize recent research and sometimes the conclusions of the same researcher at different times, I hope to stimulate the efforts initiated under sponsorship of the National Technical Advisory Committee on Jewish Population Studies of the Council of Jewish Federations to achieve higher quality and greater standardization in community studies, and to enhance their value for gaining insights into the local and national scenes.

Let me stress that, in citing particular authors and studies as illustrative of such inconsistencies, my purpose is solely to point to the challenges the field faces in resolving legitimate concerns that have been raised about the accuracy of earlier conclusions. Moreover, this paper does not address issues dealing with the quality of Jewish life or the future strength of Jewish identification in the United States. I am here solely concerned with questions related to data compilation and their analysis, although the findings of such research obviously have implications for assessing the future quality of American Jewish life. Indeed, it is because this is so that we need to concern ourselves first and foremost with the quality of the methods and the data on which such assessments are based.

The Research Problem

After a slowdown in the early 1970s following the National Jewish Population Survey in 1970–71, an impressive number of local Jewish population studies have been undertaken. Since 1980, at least 33 Jewish communities, including the largest ones, have conducted surveys, so that about 73% of the total American Jewish population has been surveyed within the last 5 years. Equally encouraging, a growing number of the 1980 surveys represent the second or even the third round of studies for their communities, attesting to the value of the first rounds and pointing to the increasing prevalence of regular collection of population data at the local level. Judged, therefore, by the number of studies, by the percentage of total Jewish population covered, and by the growing number of repeat surveys, the demographic study of America's Jews has advanced remarkably.

Especially ironic, despite the proliferation of surveys and some advances in their level of sophistication, their quality and usefulness for research purposes still varies considerably, and problems of comparability and generalizability persist. Much remains to be achieved. Recognizing these problems, a Workshop in Jewish Population Studies was sponsored by Brandeis University's Center for Modern Jewish Studies in 1982 to exchange "insights and experiences in the design, conduct and utilization of Jewish population studies" (Cohen, Woocher and Phillips, 1984, p. xii). This book based on the papers presented at the Workshop makes a major contribution toward trying to advance the quality of Jewish population studies in the United States.

In March 1984, the Council of Jewish Federations (the sponsor of the NJPS and a major force in providing stimulation and guidance for local population studies) sponsored a Colloquium for planners and research scholars designed to build on the strong foundation laid by the Brandeis Workshop. The Colloquium was charged with identifying means of enhancing cooperation between planners and researchers and

developing mechanisms to insure appropriate research designs for testing theory and for providing a sound, scientific body of data for community planning. As a result of the Colloquium deliberations, the Council created a National Technical Advisory Committee on Population Studies to further the technical levels of study methodology, to evaluate alternative data sets, to foster the development of data banks and cooperative research, and to review the feasibility of developing a national profile of American Jewry. This work has begun, but the magnitude of the tasks and the limited resources available to date mean that progress is likely to be slow. What are some of the research problems we face?

Let me begin by stressing first that, despite the concerns I have already expressed about the quality of the data, and despite differences in interpretation of the accuracy of the statistical parameters and their meaning for future growth, the literature on the demography of American Jews (Goldstein, 1981; Goldscheider, 1982; Cohen, 1983a) clearly documents that in this last quarter of the 20th century the Jews of America are characterized by low fertility, decreasing household size, high levels of intermarriage, considerable population movement and redistribution, a substantial aging of the population, and high levels of education and occupational achievement concurrent with rising proportions in the professions and lower levels of self-employment in business. Striking as these underlying similarities among communities in many basic patterns are, considerable intercommunity variations still exist in the specific levels of demographic processes and in demographic and socioeconomic composition.

The existence of these variations suggests that a community's size, the duration of Jewish settlement, and its regional location may all have important effects on its population structure and dynamics. This, in itself, is a key reason why we continue to need individual community studies, even while concurrently longing for reliable national data. Indeed, desire for national data reflects the fact that all too often evaluations of the viability and the vitality of the American Jewish community as a whole, based on the demographic features and indicators of Jewish identity for individual communities, have led to very differing conclusions about future prospects. In part, these differences undoubtedly reflect real variations among the particular communities; in part, however, they may also result from variations in the quality of data and the sophistication of the analysis. They may even reflect differences in the proclivities and perspectives of individual researchers that affect their optimism or pessimism in interpreting the future of the Jewish community. Isolating those effects that emanate from the research design and the data from those reflecting real differences is one of the major challenges we face. Eliminating the problems associated with the former rates the highest priority.

Identifying the Universe and Choosing a Sample

Basic to any attempt to assess the Jewish population, local or national, regardless of data source, is the question of who is to be regarded as a Jew and who is to be included in the sample. Is identification and/or inclusion to be based on the Jewish identity of the parents or even of grandparents at the time of the birth of the respondent? Is it to be based on the religion in which the respondent was raised or in which

his or her children are being raised? Is it to be the Jewish identity of the head of the household, regardless of the respondent's identity? Will it follow halachic standards, or is it to be based on self-identification or selected behavioral tests?

As we know, a definition based on household criteria can make a vast difference in the number enumerated as Jews: in NJPS the differential amounted to 430,000 persons (Massarik and Chenkin, 1973). The inclusion of non-Jewish spouses or children of a Jewish parent could considerably distort selected characteristics of the population being studied. Yet, given the situation in the United States, can we afford from a research perspective to completely exclude all household members who do not either identify themselves as Jews or qualify halachically as Jews at the time of the study. Many of these play an important role in influencing the intensity of Jewish identification of other members of the household (Mayer, 1978; 1983). Considerable efforts, then, must still be invested in clarifying whom we wish to regard as a Jew conceptually and in deciding about which non-Jewish household/family members we need to collect information. We need to develop a standardized classification scheme with information collected on individuals in all the categories, so that for some purposes certain persons may be included in the analysis but for other purposes they are excluded. In this way, a wider definition of Jewishness for data collection in surveys does not preclude adopting narrower definitions for tabulation and analysis. Use of a standard set of criteria and availability of data on all categories would also enhance comparability across communities and allow aggregation of results from various studies.

The problem becomes more complex, of course, in the context of questions related to sample design, since crucial decisions are made at the sampling stage that affect who will eventually be included in or excluded from the study. Here again, a host of alternative methods have been developed. The literature is replete with examples of reliance upon federation lists, distinctive Jewish names, language spoken, residential clustering, and even friendship networks as mechanisms for identifying and/or screening Jewish households and Jewish individuals (Himmelfarb, Loar and Mott, 1983; Kobrin, 1983; Lazerwitz, 1983; see also Cohen, Woocher and Phillips, 1984). Serious questions remain about the representativeness of samples that result from reliance upon one or another such mechanisms for identifying Jewish respondents. A particular method that seems to work well in one community may be far less appropriate in another because of differences in the generational and ethnic composition of the Jewish population, the levels of intermarriage, and the rates of affiliation. All too often, one or another of these approaches has been used almost indiscriminately in choosing our samples and at least partially determining who is Jewish, despite the obvious biases such approaches may have.

Fortunately, we have in recent years begun to evaluate such mechanisms more carefully, and especially to assess their biases (Himmelfarb, Loar and Mott, 1983; Lazerwitz, 1983). Too frequently, however, after making the evaluation, the resulting data have been used with little acknowledgement of their limitations; consumers of the material, including planners, community lay leaders and rabbis, as well as the press, all too often make banner headlines of findings which should have been seriously qualified because of the nature of the samples chosen and the procedures used in identifying Jews and in ascertaining eligibility for inclusion in the study.

In his review of ten Jewish community studies, eight of them undertaken in the 1980s, Bruce Phillips (1984) compared the sampling strategies followed. All of the

study directors had access to the latest methodological innovations, and all studies were intended to achieve the best possible cross-section of the Jewish population while taking account of the different local situations, including availability of information and the costs inherent in different sampling methods.¹ The review covered use of federation membership lists, lists based on individuals with Distinctive Jewish Names (DJN) compiled from various sources, and Random Digit Dialing (RDD), as well as combinations of these methods; it confirms that sampling remains one of the most problematic areas of Jewish communal research.

The Use of DJNs

Indicative of the problems we face in sampling are the different conclusions reached on the basis of assessments of the use of DJNs for developing a representative sample of the Jewish population. Let me illustrate. Fred Massarik (1983), a strong advocate of the use of DJNs, has argued forcefully that multiple research strategies are needed, and has singled out the DJN method as deserving particular attention. His argument was based on assessments of the accuracy yielded by different combinations of DJNs when compared with all households in the NJPS. (I believe that this is not a completely fair test, since part of the NJPS sample was selected according to DJNs.) Massarik (1983, p. 119) concluded that “it is not advisable to rely on 35 or 106 DJN figures (names) as such, especially if we seek substantially accurate representations of key variables, such as religious self-identification, household composition, and occupation.” Rather, he advocated both a more elaborate ‘New Index’, based on DJNs that seems to yield more satisfactory estimates, and further experimentation to obtain “reasonably satisfactory Jewish population data – if not necessarily ideal data sets – on a cost-effective basis.”

In a later assessment of DJNs, drawing on data from the NJPS, Himmelfarb, Loar and Mott (1983, p. 256) investigated the differences between DJN persons and other Jews in the NJPS sample on a total of eight measures of background characteristics, nine single-variable measures of Jewish identification, and eight Jewish identification scales. They concluded that:

The most important point of our findings, however, is not the direction of the differences which exists between those with DJNs and other Jews, but the fact that the differences are very slight and considerably less than those which are obtained when samples are from Jewish organizational membership lists. Therefore, we believe that a random sample of persons with DJNs is likely to produce a fairly representative sample of American Jews.

This conclusion however is qualified by the authors’ recommendation that consideration should also be given to effects of (a) geographical and community differences on DJN Jews and other Jews; (b) the effects of mixed marriages on the ability to identify Jewish women and converts with non-Jewish names; and (c) the inclusion of Sephardic and new Russian immigrants. I might add that the inclusion of Israeli migrants may also affect the effectiveness of DJNs.

In contrast to the positive evaluation by Himmelfarb, Loar and Mott, two other researchers have been more cautious. Lazerwitz (1983), who also used NJPS data but

from a different perspective, found that the DJN and non-DJN groups differed on such variables as number of generations in the United States, education, denominational preference, synagogue membership, and extent of voluntary community activity. Nonetheless, Lazerwitz observed that, on the whole, when geographic subdivisions were compared and included control for community size, "DJNs do form a Jewish population percentage that varies within a reasonably delimited range." Is the range he observes – from a low of 11% in medium-sized Western communities to 23% in small Jewish communities of the Midwest and West – in fact so delimited? A narrower range holds only for comparisons among larger places, 14–19%. Overall, Lazerwitz (1983, pp. 6–7) concludes that "the current tendency to conceive of a probability sample of DJN American Jews as being an adequate probability sample of all Jews must be avoided." Area probability samples of Jewish populations who are not known to federations are likely to include DJNs more traditionally inclined than the non-DJN Jews. Exclusive use of DJNs, therefore, gives biased results and, he argues, should be restricted to use in designing samples (including, for example, screening for areas of Jewish density); it constitutes a less than optimal device for estimating local Jewish populations.

Bruce Phillips (1983) was able to assess the results that would have been obtained by use of a DJN list compared to those from use of a true probability sample of the Jewish population of Denver obtained by reliance upon Random Digit Dialing (RDD). Phillips compared the DJNs included in the total sample (a) with those in the total sample who appeared on the federation, synagogue, and other Jewish organization lists, and (b) with those in the total RDD sample. He found that the DJN list was superior to a list sample, but did not provide a representative sample of the community. Among the substantial differentials noted, DJNs over-sampled organization and synagogue members, underenumerated every marital category but the married, and significantly missed intermarried couples as well as recent migrants to Denver and recent movers to new residences within Denver. Of the variables assessed, only age and income were fairly accurately represented. On the basis of this assessment, Phillips concludes that Lazerwitz's cautions about the representativeness of DJN samples on a national scale are even more pertinent to Denver and other new Jewish communities in the Southwest. The growing challenges to be faced nationwide in identifying Jewish households on the basis of DJNs is illustrated by a recent announcement of the synagogue wedding of Erica Bolski to John W. Douglas III. How will future surveys, using DJNs, identify Mr. and Mrs. John W. Douglas III as Jews?

These varied assessments must raise serious questions about the use of DJNs to sample American Jewry. More particularly they point to strong inconsistencies between available evidence and ongoing practices, such as the conclusions reached by Steven Cohen that "it is safe to assume that DJNs are neither more nor less Jewishly committed than non-DJN Jews" (Cohen, 1983, p. 2); that "most researchers who have examined DJN samples now believe that they reasonably reflect the distribution of American Jews' socio-demographic characteristics as well as the diversity in Jewish affiliation and ritual-observance patterns." (Cohen, 1985, pp. 5–6).

In 1981–82, 1983, and 1984, sponsored by the American Jewish Committee (AJC), national samples of American Jews – on which Cohen (1983b; 1983c; 1985) based his claim – were chosen on the basis of Distinctive Jewish Names listed in telephone directories. These DJNs were based on an earlier inventory of some 37,000 family names

frequently possessed by Jews, compiled from lists obtained by AB Data Corporation of America. The lists include such diverse sources as affiliated and active Jewish donors and organization members; Jewish teachers, lawyers, doctors, and dentists; subscribers to various Jewish publications; and Israeli stamp collectors. The names so identified are purported to account for about 40% of all American Jews (Cohen, 1985). However, given the very selected sources of these names, the concerns raised by earlier assessments of the DJN approach must lead to serious doubts about whether a national sample based on such lists of strongly identified and affiliated Jews constitutes a reliable source for a representative sample of the entire American-Jewish population and especially of those at or near the margins of the community.

Cohen himself "opposes the use of DJNs for demographic research that requires sensitive measure," such as those used in fertility analysis (private communication to author, October 1985). He does, however, believe that DJN-based samples are acceptable in research that attempts to delineate broad differences or changes in attitudes or characteristics, such as in the political arena. I differ with this position and believe that it is dangerous to employ such different standards in undertaking sample surveys whose findings are to be used for planning purposes, especially when the method is known to involve the risk of yielding biased results. Such a procedure is particularly questionable when the findings are given wide circulation and are likely to be used by planners and journalists as if they had been based on far more rigorous methods.

Unless the purpose of a study requires a focus on the more identified, affiliated segments of the community, evaluations of use of DJNs suggest that exclusive or even heavy reliance on samples based on DJNs is not justified and may give misleading results. A community's demographic structure and processes may well be distorted and are likely to be biased against intermarrieds and migrants, and to overrepresent married persons at the expense of the single, the divorced, and the widowed. These are biases that could give a distorted profile of American Jewry and of the dynamics of the demographic changes it is undergoing.

Random Digit Dialing

RDD to select samples has become increasingly popular as an alternative way to insure a fully representative sample, but the value and costs of this sampling method have not been adequately assessed; they may prove to be prohibitive (Phillips and Weinberg, 1984) in the very areas where RDD serves its purpose best, namely, those parts of a community where Jewish density is lowest and where Jewish households are less likely to be identified on the basis of DJNs or federation lists. Yet these are the areas where the high ratios of non-Jews to Jews, sometimes requiring as many as 100 or 200 calls to locate one Jewish household, make RDD an inefficient, costly procedure.

Interestingly, for the New York City study (Ritterband and Cohen, 1984), exclusive reliance on DJNs to select the sample was initially rejected. DJNs did, however, enter the final sampling process. On the grounds of efficiency, RDD was restricted to those telephone exchanges which encompassed approximately 90% of all the DJNs; they were located in one-quarter of all exchanges. When data collection was undertaken,

however, the modified RDD approach was augmented by DJN mailback and telephone sampling in order to reduce costs.

Ritterband and Cohen maintain that their evaluation of the RDD and DJN samples suggested a close resemblance in characteristics. Differences occurred mainly in the subsamples of Israelis, Russian immigrants, and the intermarried. Such differences could be adjusted on the basis of the RDD distributions.

Adjustments were also made for non-coverage of non-DJNs and for inclusion of non-Jews with DJNs in order to obtain population estimates. A constant adjustment factor for each situation (3.23 and 0.92, respectively) was used in all sampling areas of Greater New York to estimate the total number of Jewish households. Use of such a constant for all areas, in my view, is questionable. Moreover, how adjustment could be made for specific sets of characteristics is not clear. Use of DJNs obviously introduces problems, the resolutions of which are not always satisfactory. But, as noted, RDD is not without its own problems.

Until further innovations allow us to overcome the existing limitations in our sampling procedures and to take full advantage of the strengths of other methods or combinations of different methods, study designs will continue to rely on methods similar to those employed in recent surveys. While doing so, we must be honest with ourselves and with consumers of our data, and insist that they, in turn, be honest with themselves and with the public generally about how the particular sampling procedure used affects what the data mean and what they do not mean, and how the different procedures employed, coupled with the variations in practices regarding inclusion or exclusion of non-Jews, affect comparability across time and communities.

Selecting a Standard

While the representativeness of a sample can be questioned on the basis of methods used for choosing respondents, it may also be open to criticism because of reductions in the samples due to ineligibility and to high rates of non-response. For example, of the 1,600–2,500 persons receiving questionnaires in the initial samples selected for the national AJC surveys discussed earlier, only 1,200–1,800 were eventually considered eligible; this reduction reflects the effects of inaccessibility for a large number of potential respondents, and the elimination of a small number of confirmed non-Jews. Of this balance, only about half responded to the questionnaire. The representativeness of this considerably reduced sample was then assessed by comparison with the results obtained from recent studies of the Boston and New York Jewish communities in which more sophisticated and costly sampling techniques had been used. Such comparisons are affected not only by the different basis of selecting the initial samples, but, as Cohen fully recognizes, by the low response rate in the national surveys and the resulting selectivity bias. Detailed comparisons of the national and local samples are not in place here, but several general concerns must be expressed to illustrate the problems that I see facing surveys of American Jewry.

In addition to questioning heavy reliance on DJNs as a basis for sampling, one must also question the use of one or two major metropolitan areas as the basis for testing the representativeness of national samples, as has been done in the national surveys discussed above that were sponsored by the American Jewish Committee. What does

a comparison with New York or Boston prove? The New York study is obviously an attractive standard, because of its relatively large sample size and because the area contains almost one-third of U.S. Jewry. Yet, close to 70% of America's Jews live outside the New York area, and many in communities quite different from New York City. Indeed, one could perhaps build a very strong case that New York is an atypical Jewish community. For example, Cohen's comparisons of selected variables in the national data set with those from the Boston and New York data indicate a 12-year difference in median age; considerable variation in marital status; and differences in denominational composition. I suspect that the differences would be even more striking if New York's characteristics were compared with those of other communities (Tobin and Lipsman, 1984). Nevertheless, Cohen (1985, p. 90) concluded that the "respondents in the (1981-82) National Survey hardly differ from those in the New York and Boston studies"; and despite instances of under- or overrepresentation in the 1983 and 1984 surveys of Jews with certain characteristics, he concluded that the overall biases were thought to be small and that the 1984 sample fairly accurately represented the political attitudes of American Jews (Cohen, 1985, p. 51). Is such a conclusion justified given both what we know about use of DJNs, and the fairness of using only New York and Boston as the standard for judging representativeness?

What do such comparisons prove? If communities other than Boston or New York had been chosen as the standard, the similarity might have been greater on some variables and less on others, as any attempt to compare the results of the National Survey with the different communities in the Tobin and Lipsman (1984) compendium would indicate. What is the typical U.S. community? In many respects, New York is the least typical. How does one insure representativeness of medium and small size communities in such tests? I fully appreciate the difficulties the AJC researchers faced in undertaking a national sample survey and testing the results. My point in questioning reliance upon DJNs for sample selection and in challenging the procedures used to test the results is to stress how far Jewish population studies have yet to go in developing both the ideal sampling frame and an acceptable standard for evaluating the representativeness of the survey population. I also want to stress the dangers in concluding that findings based on such studies necessarily represent all American Jews. They may well misrepresent the very individuals in whom we are most interested, those who are only marginally identified with the Jewish community.

Comparability and Standardization

Recognition of these real and potential problems argues strongly for much more careful attention than heretofore to standardizing the procedures used in identifying our universe, designing our samples and questionnaires, tabulating our data, and analyzing and reporting the results. Our goal should be to ensure maximum comparability among community studies while still meeting the unique needs of individual communities.

As I already noted, for the publication emerging from the Brandeis Workshop, Gary Tobin and Julie Lipsman (1984; see also Tobin and Chenkin, 1985) undertook a most ambitious effort to assemble and, where possible, to compute comparable data

collected since 1979 from population surveys in 13 American cities. Their task was not a simple one; their efforts and frustrations in interpreting some of the differences among communities make clear the problems of comparability that arise because different communities use different sampling techniques and interviewing methods, define the same variables in different ways, ask the same basic questions in varied forms, and tabulate and report the findings in different formats or not at all. A few examples will illustrate the problem.

Why does Cleveland's 1981 survey show an average household size of 2.8 and Minneapolis 2.6, compared to only 2.3 for St. Paul and 2.2 for Denver in the same year? Why are Cleveland and Minneapolis characterized by the same high level that characterized the 2.8 average of the NJPS ten years earlier, while Los Angeles, St. Paul and San Diego households average almost half a person less in 1981? Have the latter declined, was their situation already different from the national average, or are the changes due to differences in coverage and/or definition? Do some surveys include non-Jewish household members in the average size while others exclude non-Jews? Did some surveys include college students living away from home, often in other locations, while others, like the census, did not count such absent students as members of their parental households? Did some count Jewish college students living off-campus in their own units, thereby inflating the percent of small household units, while others missed college students altogether? Did some include institutional populations, such as students and residents of homes for the aged, as household members or perhaps even count them as individual households, while others overlooked them entirely? Why were 30% of the households in Denver and 33% in Los Angeles one-person units, whereas Cleveland had only 19% and Chicago 21% in the same period? Is it a function of differences in the general community, in age composition, in living style, or is it a definition/measurement problem? We rarely are given enough information either about the study design or through the interpretation of the results to allow us to gain insights on the reasons for such differences.

Do we know why in Denver, Seattle, and Rochester 20% of the Jewish population was under age 20, while in San Diego, Los Angeles and Nashville it was about 30%? Footnotes in a few studies suggest that problems of categorization may account for some of the difference. For others, we can only speculate whether differentials in enumeration procedure or differentials in migration, fertility, or mortality account for the variations. Unfortunately, the general absence of direct data on fertility, and inadequate information on migration, with proper controls for age composition, preclude gaining further insights on the roles of these factors.

Similar comparative assessment can be made for marital status. Not surprisingly, 23% of Miami's relatively aged Jewish population is widowed, but it is not at all clear why in St. Louis 17% are widowed since only 22% of the population is aged 60 and over, while in Minneapolis, with about the same percentage aged 60 and over, only 7% are widowed. Comparable questions could be raised about the percentage single, which ranges from lows of 7% in Miami and 9% in St. Louis to a medium level of 15% in New York and highs of 22% of Minneapolis and 23% in Chicago and Denver. Chicago's may be explained, in part, by inclusion of all persons 18 years old and over in the statistics on marital status, but what accounts for the highs in Denver and Minneapolis? Are there differences because some refer to all adults (Chicago), some to household heads only (New York), and others to respondents only (St. Louis), or do

the patterns vary because the communities actually differ in the marital status of their populations?

Other socioeconomic indicators show similar high degrees of variation. The percentage with college education varies from a low of 57% in St. Paul to a high of 78% in Chicago; the percentage of professionals ranges between a low of 23% in Minneapolis and a high of 45% in Rochester; and managers/proprietors vary between Los Angeles' low of 16% and Minneapolis' high of 42%. Even blue collar workers show a ratio of almost 3:1 between Los Angeles' high of 11% and Nashville's low of 4%. Furthermore, despite the low percentage of professionals/managers (49%) in St. Louis in 1982, the city was reported to have 40% of its households with incomes of \$40,000 and over, compared to only 25% with such incomes in Rochester in 1980, where 64% of the work force were professionals/managers. The mysteries of intercommunal variation thus persist.

This comparative evaluation is, of course, oversimplified, and the stress on differences is perhaps exaggerated. What I am trying to suggest is that all too often, despite our generalizations about such characteristics as family status, socioeconomic status, and age structure (many of which may be generally correct), individual community variations seem to be substantial. In the absence of standardized definitions, coverage and tabulations, however, the inconsistencies may be spurious and lead to erroneous conclusions about the nature of the specific community, about differences between communities, and about change over time. Even more serious, using data based on one or two communities to generalize about the total American community or to test national data derived from alternative sources, as was done for the AJC studies, can be dangerous.

The foregoing discussion very clearly indicates that standardization of procedures in all phases of research as a basis for greater comparability and as a means for achieving economy in research efforts is the key to enhancing the quality and the usefulness of Jewish population studies in the future. Common to all studies are a large number of variables that we all recognize as crucial to understanding the community and to planning for its future. Where we differ is in how we identify and sample the population to be studied, and how we define these variables and handle them in the data gathering, tabulation, measurement, and analysis stages. To recognize the need for standardization and for comparability does not deny the importance of tailoring studies to the needs of individual communities to reflect their particular size, age, composition, or special problems. With proper attention to defining concepts, choosing samples, asking questions, measuring variables, and tabulating data, inconsistencies due to varying procedures and definitions can be eliminated, and higher levels of comparability can be achieved at the same time that information is obtained to meet the needs of individual communities. Both the larger American and the local community can then be better served, and both the resulting studies and the community planning based on them will be greatly enriched.

Interpretation of Data: The Fertility Debate

Even more frustrating than the unexplained inconsistencies in the results obtained from different community studies are the inconsistencies associated with results

obtained in a single community and even from the same data set when different authors interpret the data, and especially when the same author does so at different times. The current debate on whether our numbers are or are not in danger of seriously shrinking and whether fertility is at replacement or non-replacement levels is a case in point (cf. Schmelz and DellaPergola, 1983, and Goldscheider and Goldscheider, 1989). As before, let me illustrate the problem of inconsistency by citing some of the literature.

In doing so, I refer largely to papers recently authored by Steven Cohen and Calvin Goldscheider. Both scholars have come to assess the demographic situation of American Jews quite differently both from their own earlier evaluations and from those offered recently by other researchers working in the field. The inconsistencies among these varied assessments lend added weight to the need to overcome the limitations inherent in our current research designs and data sets and to achieve greater standardization and comparability in our research efforts. Only by doing so can we hope to assure a full and correct evaluation of the dynamics of demographic change in the Jewish community and the implications of these changes for the future.

In his comprehensive, insightful analysis of *American Modernity and Jewish Identity*, which relies heavily on data from the 1965 and 1975 Boston Jewish Community Surveys, Steven Cohen (1983a, p. 118) concluded that

...on the basis of past experience, it does seem safe to say that the completed Jewish birthrate for today's Jewish parents may remain well below the number needed for replacement...Barring a significant rise in national fertility, Jewish birthrates – if they follow historic patterns – should continue to reside in the region of NPG (Negative Population Growth).

Following his review of a number of factors to explain this situation, Cohen concluded further,

Undoubtedly, these several aspects of Jewish distinctiveness aside, so long as middle-class, urbanized Americans experience low birthrates, so will comparable Jews. Jewish birth patterns will generally follow those of the larger society as they have in the past. If anything, advancing assimilation may well bring Jewish fertility behavior into even closer alignment with that of their non-Jewish contemporaries. (Cohen, 1983a, p. 120)

The data cited by Cohen seem to firmly support this conclusion. Boston's ever-married Jewish women aged 25–34 averaged only 1.5 children in 1965; in 1975, the 25–34 year group had an average of only 0.7 children. The national data cited for Jews, although somewhat higher at 2.1 and 1.2 in the 1960s and 1970s respectively, pointed in the same direction. Yet, about two years after writing this, in a September 1984 interview in *Moment*, Cohen and his co-interviewee Calvin Goldscheider argued to the contrary, claiming that that data of the 1960s and 1970s were a fluke of the particular cultural moment in American history and that the issue was one of timing – reflecting late marriage rather than reduction in total fertility. Cohen suggests that “even if we assume that the women of the late ‘60s and early ‘70s were indeed less family oriented, less prone to have large families – which is very doubtful if you look at the data (a reference to New York data which appeared to show an average of 2.1 children for married women 35–44) – there is no reason to assume that the same predilections are

carrying over into the early and mid-eighties" (Cohen and Goldscheider, 1984, p. 41).

Goldscheider, in the same interview, questions the validity of any conclusions of reduced fertility, suggesting that the question revolves more about the proportion of Jewish women marrying and the timing of their fertility in relation to age at marriage. He acknowledges, however, that we do not really know whether marriage patterns have changed, and whether, as a result, overall fertility levels have changed or whether they are below replacement, if one takes into account the non-married. As Goldscheider himself stated so well in an earlier interview with William Novak:

What has changed? As I said, most Jewish couples tend to have two children; relatively few have none, or only one child, or more than two. But what is changing is the proportion of people who are getting married – it has declined significantly in the past 10 years. If everybody gets married, and has about two children, then we have replacement, and possibly even a little growth; certainly we have stability. But if suddenly 20% of the Jewish population doesn't get married – and that's a hypothetical figure – then you can see how that changes things. In the past, universal marriage has been a mainstay of Jewish life; suddenly, we can't take it for granted. (quoted in Novak, 1981, p. 51)

Clearly then, the situation is very complex. All agree that in the United States, Jews have averaged lower fertility than non-Jews for a century or more. It is also true that at a time when most Jewish women married, Jewish fertility hovered at about two children per woman and, therefore, met natural replacement levels. Few would dispute Goldscheider's (1985, p. 12) strong belief that "projections about the drastic numerical decline of the American Jewish population in the next generation is demographic nonsense." I have criticized this overly pessimistic view, too (Goldstein, 1981). However, in the absence of a sharp reversal in trends, some decline still seems likely to me; its probable extent remains open to question until inconsistencies in evidence, in interpretation, and in ways of measurement are resolved. The argument (Goldscheider, 1985, p. 12) that "neither the educational attainment nor the career orientation of younger American Jewish women poses a threat to the demographic continuity of the American Jewish population" also remains to be tested definitively to ascertain its generality. If Cohen's earlier claim that Jewish fertility will likely follow or align with that of the larger society proves correct, there is added reason to question the validity of Cohen's and Goldscheider's more recent optimism about the level of Jewish fertility and its adequacy for replacement.

In view of past patterns of lower Jewish fertility, explanations need to be provided about why the future situation should be characterized by averages equal to or higher than that of the general population – a condition that would have to exist if the revised Goldscheider-Cohen view holds and if U.S. Census Bureau projections prove correct. The Bureau of the Census (Das Gupta, 1985) has estimated that the average completed number of children born by the end of childbearing age to white women, who still had no children at age 20 and who reproduced at 1980 rates during their reproductive years, would be only 1.48, and for women childless at age 24 (probably closer to the current Jewish age at marriage), the average would decline still lower to only 1.08, about 50% below replacement. If, as data suggest, a high percentage of Jewish women marry late, and if they should follow such fertility patterns, these projections must raise serious doubts about the persistence of replacement level fertility. Goldscheider

is correct in stressing that a key factor is marriage, but his arguments are often misread by wishful thinkers when they overlook that an average of 2.0 children on the part of the married is not sufficient to replace both themselves and the unmarried who have no children, especially if the latter group constitutes a substantial number; an average below 2.0 compounds the problem.

The complex situation is further illustrated by data from a Bureau of the Census (1983) fertility report on births completed and birth expectations. It found that for wives of all races who had married at ages 22–24 and who were already aged 25–29 in 1983, average expected completed fertility was 2.07; for those aged 30–34, the average was 2.08. Even if Jews follow these patterns exactly, the fertility of married women would be just at replacement level. The major question remains: how many will not marry and how much higher would Jewish marital fertility have to be to compensate for the non-married? Furthermore, will Jewish women in fact follow these marital fertility patterns?

This Census survey also showed that among white women aged 18–34 with four or more years of college, the average completed fertility, if expectations are realized, will be 1.93 children, with 14% being childless. For those with five or more years of college, the average will be 1.72 with 19% childless.² Among women aged 18–34 surveyed by the Census who were not married at the time, the lifetime births expected averaged only 1.83 per woman, almost half a child less than the average for currently married women of similar age; for those 25–29 year-old unmarried women, the average was only 1.56. The expectations of the unmarried, therefore, provide no basis for believing that the averages will rise above replacement for all women if these single women eventually bear children.³ To the extent that Jewish women are characterized by both late marriage and high levels of education, is there good reason to believe that Jews will in the future deviate substantially from these general patterns and in ways different from the past?

In considering data on expectations, one should particularly keep in mind the assessment of childbearing intentions offered by Nathan Keyfitz's (1982) evaluation of the various efforts made since 1940 to rely on surveys to elicit childbearing plans. While recognizing that the Bureau of the Census relies on such procedures, Keyfitz (1982, p. 741) concludes that, "after 40 years of effort, this instrument (survey) has come to seem uncertain, even controversial." Moreover, he stresses that extension of the questioning to women who are not yet married is not likely to add much information; "a girl of 15 can hardly give a meaningful answer to the question of how many children she intends to have" (Keyfitz, 1982, p. 741). Clearly, data on expectations for the total population and for Jews need to be used with considerable caution, especially when they are dependent also on expectations with respect to marriage behavior.

Although these data are obviously of very limited use in assessing current and future Jewish fertility because they refer to all whites, and not just Jews, and in a few instances to whites and blacks combined, and because they include inadequate controls for a number of factors, I believe they serve a purpose. To the extent that Jewish fertility in the United States has rarely if ever exceeded non-Jewish fertility and more often has been substantially lower, the data for total whites provide an upper limit – one which Jewish fertility would resemble if it came up to the national average in future years (or if the national average came down to Jewish levels). As such, it would seem more reasonable to hypothesize that Jewish fertility will not likely exceed

replacement levels, and more likely will be below them. This interpretation, like Cohen's and Goldscheider's views, remains speculative. If we are to accept that Jewish fertility behavior will rise above the national levels, then the evidence and reasons for this very basic change in pattern have to be presented much more clearly and convincingly than has been the case to date.

Above all, these illustrations are intended to point out the inconsistencies that characterize both our data and especially our interpretation of them (cf. Schmelz and DellaPergola, 1983). Beyond this, they are intended to argue strongly for the need to include appropriate questions on marriage and fertility in Jewish population surveys and for appropriate assessment of the resulting data.⁴ One outstanding defect of Jewish population studies, in the past and currently, is the limited and often misleading analysis, due to faulty measures of comparisons, of fertility. It is ironic that the one demographic variable that may be of greatest interest to those concerned with the future of the Jewish community tends to be the most neglected of all. The current debate, I believe, is a healthy one if it serves to stimulate more careful research. It is dangerous only if the findings on either side are accepted uncritically as the basis for what may be false alarms or unjustified complacency. The inconsistencies challenge all of us to greater efforts at resolution.

Conclusions

Given the limitations of both methods and available data, I would argue strongly that the research done to date provides no firm basis for resolving the debate about the current or future number of Jews. The existing data sets raise and leave unanswered a number of key questions about our success in identifying and measuring the total Jewish population and in assessing the interrelations between population change and the current and future character and strength of Judaism and Jews in the United States. What these studies do suggest, despite their varied and at times serious limitations, is that the Jewish population has undergone dramatic changes and will continue to do so; that these changes have significant implications for the degree and character of Jewish identity and Jewish practices; that the extent of Jewish integration into the larger American scene as well as the persistence of Jewish exceptionalism in certain areas will jointly determine future Jewish demographic behavior and patterns; and that while future Jewish patterns will therefore probably even more closely resemble those of the American population as a whole with respect to many variables, they will not necessarily do so for all characteristics.

Both the changes being experienced in common by Jews and by the national population generally and those unique to the Jews make it most important to continually monitor the demographics of American Jewry. We must be able to assess their implications for individual localities and for the Jewish community as a whole, and to do so whenever possible within the comparative context of the changes occurring in the larger community. Researchers therefore continue to be challenged by the necessity of helping to insure that the methods used and the resulting data will allow the best possible assessment of the present situation of the community and where it is heading.

In-depth evaluation of studies already completed and of alternative data sources should rate the highest priority if we are to make our future studies more accurate and

more useful. Fortunately, we have begun to see some such critical assessments, as, for example in the use of DJNs for sampling (Phillips, 1983; Lazerwitz, 1983; Massarik, 1983). These mark only a small beginning; they must be followed by concentrated efforts to assess alternative approaches to sampling, data collection, measurement, data analysis, and population estimates, and to resolve the differences in conclusions about the levels of fertility, intermarriage, conversion, assimilation, and overall growth. We must strive to reach agreement on standardized definitions and procedures and on a set of up-to-date guidelines on how best to proceed, and on how to insure results that allow comparative analysis over space and assessment of change over time, as well as cumulation of comparable local studies to obtain regional and eventually even national data sets.

One basic goal for such efforts is development of a good current standard against which to judge the representativeness or even the reasonableness of the results obtained. The NJPS has provided some clues, but it is far too outdated to constitute an acceptable standard. Use of individual communities such as New York, Boston, or Los Angeles is questionable. In the absence of such a single standard, we must give much more thought to appropriate alternatives. This includes consideration of how one or more synthetic standards might be developed from existing, available information, and whether aggregation of weighted data from individual community studies would better serve the purpose.

Under ideal conditions, a new, well-designed national study would provide such a standard. In its absence, achievement of a high degree of standardization and comparability among community studies might well provide a viable alternative, especially if a large number of communities and a high percentage of the total population continue to be covered by local sample surveys. It is, however, highly unlikely that we can obtain national results by aggregating indiscriminately the results of individual community studies. But with proper planning and centralized supervision, it may be possible to select a range of communities which are judged by experts to be representative of the country as a whole. By then using these particular communities as a kind of national sample, and aggregating their individual survey results, approximation of a national sample survey may be achievable. Moreover, by properly staggering the times at which communities of different size and type undertake their surveys, a more continuous assessment of the changing Jewish-American scene might also be possible.

Such a program would clearly have to incorporate extensive evaluation of the characteristics of the communities to be used and requires a high degree of standardization. Indeed, it might be possible and desirable to identify across the country a series of local areas which together might qualify as being representative of the national American-Jewish scene. Samples in these areas could then be monitored on a fairly regular basis to continually assess changes in the structure, composition, distribution, attitudes, and behavior developing a type of national Jewish population laboratory (cf., the Current Population Survey, Bureau of the Census; and the Gallup Poll). Such a development would allow identification of important changes soon after they occur, so that the community as a whole could then decide if action were needed or if larger or more intensive studies should be initiated to assess the full scope and implications of the changes detected.

In sum, we must continuously enhance and substantially modify our thinking about the design, conduct, and utilization of Jewish population studies (cf. Cohen,

Woocher and Phillips, 1984). Reevaluation becomes especially necessary as we gain new experience from ongoing studies and from analysis of data sets collected earlier; as our colleagues in the social sciences develop more sophisticated methods of analysis; and as we broaden our reliance upon varied sources of information. Such constant reassessment is especially critical because of constraints on undertaking new studies. It becomes crucial, therefore, not only to develop guidelines and manuals on how to undertake community population studies, how to ensure greater comparability over time and space, and how to interpret and utilize the results: we must also identify, evaluate, and prescribe methods for exploiting every possible data source that allows us to make relatively reliable assessments of changes in the size, composition, and distribution of the Jewish population, as well as in its components of change.

Only through such full and careful exploration of data will answers be obtained to the questions raised by the debates recently making the headlines about growth vs. decline, replacement vs. non-replacement fertility, and the negative vs. positive effects of intermarriage and assimilation. Only in this way will we enhance the likelihood of resolving the inconsistencies that have come to characterize our findings and assessments, and in so doing, provide the community with a firmer basis on which to plan its future.

Notes

1. For a good discussion of the considerations entering into the choice of methods for choosing samples, see Ritterband and Cohen (1984) who report on their experience with the New York population study.
2. The effect of education is similarly indicated by the combined data from recently analyzed 1973 and 1976 National Surveys of Family Growth (Mosher and Hendershot, 1984). They showed the total births expected by Jewish married women aged 15–44 to be only 2.16 for those with some college education, that is, just at replacement level, and lower than that of less educated Jews and of college educated Protestants and Catholics.
3. In the same set of sessions at which this paper was presented, one presented by Calvin and Frances Goldscheider (1989) assessed the birth expectations of a cohort of young men and women, the High School Class of 1972. Using longitudinal data from interviews with this group in 1973 and 1979, the evidence lends support to the argument that American Jews “will achieve fertility levels in the next several decades averaging close to two children per family, sufficient for population replacement.” In 1973, the 197 Jewish women expected an average of 2.34 children. By 1979, the 167 who were followed-up expected 1.96. These averages assume that those who reported expecting to have 4 or more children will average 5.0. For the total white population (excluding Jews) the corresponding averages were 2.40 and 2.06. While these data in themselves suggest that this particular Jewish cohort will, if their expectations are fully realized, average close to 2 children per woman, several questions must be raised about the likelihood of such an outcome. Given that between 1973 and 1979, the expected average declined by 0.4 children, or 16%, how can we assume there will be no further reductions? Between 1973 and 1979, 23% of the ever-married and 31% of the never-married had already reduced their expectations, whereas only 13 and 18% of these respective groups had increased the number of children expected. How many will, in fact, not marry at all? How many will face fertility problems? How many will experience divorce and how will this affect fertility? Beyond this are even more basic questions related to the nature of the sample itself. Religious identification was based on the question “What reli-

gion were you brought up in?" It does not tell us the religion at the time of the survey, nor anything about religious identification after marriage. How this identification is or will be affected by marriage and how many respondents are or will remain Jewishly identified remains to be determined. Assessment of the results of another survey of high school seniors in 1980 showed that only 5% of the Jewish males and 2% of the Jewish females expect not to marry, the lowest percentages of any religious group, but a much larger proportion planned to marry late. The authors themselves recognize the limitations of their data. They state, "as in the case of birth expectations, we do not know the predictive power of these marriage expectations" (Goldscheider and Goldscheider, 1989). Taking all of these concerns into account, these data can only be suggestive of future patterns. In themselves they certainly do not point to above-replacement levels of fertility and, given the concerns expressed, one could easily build a case that they point to fertility levels below replacement.

4. Among the questions that would be appropriate for inclusion in surveys are the following: 1) What is your current marital status? 2) Have you been married more than once? 3) What was the date of your first marriage? 4) How was the first marriage terminated? 5) How many babies has the woman ever had, not counting stillbirths? 6) How many children in all do you expect to have?

References

- Cohen, S.M. (1983a). *American Modernity and Jewish Identity*. Tavistock Publications, New York.
- Cohen, S.M. (1983b). "The 1981-82 National Survey of American Jews". *American Jewish Year Book*, Vol. 83. pp. 89-110.
- Cohen, S.M. (1983c). *Attitudes of American Jews Toward Israel and Israelis: The 1983 National Survey of American Jews and Jewish Communal Leaders*. American Jewish Committee, New York.
- Cohen, S.M. (1985). *The 1984 National Survey of American Jews: Political and Social Outlooks*. American Jewish Committee, New York.
- Cohen, S.M. and Goldscheider, C. (1984). "Jews, More or Less". *Moment*, September. pp. 41-48.
- Cohen, S.M., Woocher, J.S. and Phillips, B.A. (1984). *Perspectives in Jewish Population Research*. Westview Press, Boulder, Colo.
- Das Gupta, P. (1985). *Future Fertility of Women by Present Age and Parity*, Bureau of the Census Current Population Reports, Series P-23, No. 142. Government Printing Office, Washington, D.C.
- Goldscheider, C. (1982). "Toward the Fifth Generation: The Challenges to Assimilate in the American Jewish Community". Bernard Horwich Jewish Community Center 25th Anniversary Commemorative Lecture, Chicago.
- Goldscheider, C. and Goldscheider, F. (1989). "Family Size Expectations of Young American Jewish Adults". This volume.
- Goldstein, S. (1981). "Jews in the United States: Perspectives from Demography", *American Jewish Year Book*, Vol. 81. pp. 3-59.
- Goldstein, S. (1984). "Jewish Demography: The Research Challenge", in: Winter, J. and Levin, L.I. (eds.), *Colloquium on Jewish Population Studies*, Council of Jewish Federations, New York.

- Himmelfarb, H.S., Loar, R.M. and Mott, S.H. (1983). "Sampling by Ethnic Surnames: The Case of American Jews". *Public Opinion Quarterly*, Vol. 47. pp. 247–260.
- Keyfitz, N. (1982). "Can Knowledge Improve Forecasts?" *Population and Development Review*, Vol. 8, December. pp. 729–751.
- Kobrin, F.E. (1983). "National Data on American Jewry, 1970–71: A Comparative Evaluation of the Census Yiddish Mother Tongue Subpopulation and the National Jewish Population Survey", in: Schmelz, U.O., Glikson, P. and DellaPergola, S. (eds.), *Papers in Jewish Demography, 1981*. Institute of Contemporary Jewry, The Hebrew University, Jerusalem. pp. 105–127.
- Lazerwitz, B. (1983). "Some Comments on the Use of Distinctive Jewish Names in Surveys". Unpublished manuscript.
- Massarik, F. (1983). "A Changing Era in U.S. Jewish Population Research: Multiple Research Strategies – Indexes and Heuristics", in: Schmelz, U.O., Glikson, P. and DellaPergola, S. (eds.), *Papers in Jewish Demography, 1981*, Institute of Contemporary Jewry, The Hebrew University, Jerusalem. pp. 105–127.
- Massarik, F. and Chenkin, A. (1973). "United States National Jewish Population Study: A First Report, *American Jewish Year Book*, Vol. 74. pp. 264–306.
- Mayer, E. (1978). *Patterns of Inter-marriage Among American Jews*. American Jewish Committee, New York.
- Mayer, E. (1983). *Children of Inter-marriage. A Study in Patterns of Identification and Family Life*. American Jewish Committee, New York.
- Mosher, W.D. and Hendershot, G.E. (1984). "Religious Affiliation and the Fertility of Married Couples". *Journal of Marriage and the Family*, Vol. 46, August. pp. 671–677.
- Novak, W. (1981). "American Jewry is Losing the Number's Game, Right? Wrong!". *Baltimore Jewish Times*, December. pp. 50–52.
- Phillips, B.A. (1983). "DJN and List Samples in the Southwest: Addendum to Lazerwitz". *Contemporary Jewry*.
- Phillips, B.A. (1984). "Sampling Strategies in Jewish Community Studies", in: Cohen, S.M., Woocher, J.S. and Phillips, B.A.O. (eds.), *Perspectives in Jewish Population Research*. Westview Press, Boulder, Colo. pp. 67–79.
- Phillips, B.A. and Weinberg, E. (1984). "Data Collection Procedures in Random Digit Dialing Screening Studies: Interviewers and Respondents", in: Cohen, S.M., Woocher, J.S. and Phillips, B.A. (eds.), *Perspectives in Jewish Population Research*, Westview Press, Boulder, Colo. pp. 97–106.
- Ritterband, P. and Cohen, S.M. (1984). "Sample Design and Population Estimation: The Experience of the New York Jewish Population Study", in: Cohen, S.M., Woocher, J.S. and Phillips, B.A. (eds.), *Perspectives in Jewish Population Research*, Westview Press, Boulder, Colo. pp. 81–95.
- Schmelz, U.O. and DellaPergola, S. (1983). "The Demographic Consequences of U.S. Jewish Population Trends", *American Jewish Year Book*, Vol. 83. pp. 141–187.
- Tobin, G. and Chenkin, A. (1985). "Recent Jewish Community Population Studies: A Roundup", *American Jewish Year Book*, Vol. 85. pp. 154–178.

Tobin, G. and Lipsman, J. (1984). "A Compendium of Jewish Population Studies", in: Cohen, S.M., Woocher, J.S. and Phillips, B.A. (eds.), *Perspectives in Jewish Population Research*. Westview Press, Boulder, Colo. pp. 137-166.

U.S. Bureau of the Census (1983). *Fertility of American Women*. Current Population Reports. Series P-20, No. 395, Government Printing Office, Washington, D.C.