American Jewish Marriages: Erosion or Transformation?

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Over the last half-century, major changes have occurred in the marriage patterns and family structure of the American Jewish population. Given the centrality of family life for the transmission of Jewish culture and for the continuity of the American Jewish community, these changes are particularly important both to document and interpret. The major outline of these changes has already been suggested in the literature and has been documented in a series of scholarly publications based on extensive research. Some have focused at the local level, arguing that family changes are part of the broader question of ethnic continuity. Ethnic studies are most clearly carried out at the community level as a national focus neutralizes local variation and misses some important ways that Jews relate to their Jewishness. Others have addressed the issue of family change in the context of the broader revolutions in women and family roles, and they have used national American data sources when a sufficient data base exists. Regardless of the specific focus, the issues associated with family change have been identified and the general contours have been investigated in previous studies.

In the essay by DellaPergola and Schmelz, "Demographic Transformations of American Jewry: Marriage and Mixed Marriage in the 1980s," this research has been carried forward. We are all indebted to them for the careful and systematic documentation of these family patterns among North American Jews in the recent period, the evaluation of the limitations of existing data sources and their continual struggle to squeeze comparative detail from published materials so as to better understand family changes among American Jews. Although the patterns that they document have been discussed and analyzed in the scholarly literature, having new data with some details on particular communities adds confidence that the patterns others have observed are generalizable and extend into the most recent period.

Yet, beyond their data organization and documentation, they present an argument, interpretations and an overall theme. There are disagreements about their conceptualization of the issues, their assumptions and their use of evidence. At times, these are simply questions of style and language, of no major consequence for the "fact-oriented review" that DellaPergola and Schmelz undertake. More often, there are issues associated with futures and unknowns. How do we know

about the future and on what do we base our guesses, estimates and projections? No one has a firm handle on the future. But our conceptualizations and interpretations of current trends take on particular significance when they become the basis for evaluating alternative future patterns of family life. They are of critical importance to the policy suggestions and program priorities that emerge from social science research. On these interpretative and theoretical issues, I think that there are significant limitations to the arguments of DellaPergola and Schmelz.

The most basic issue of disagreement surrounds their conceptualization of the processes underlying family changes as "erosion." According to DellaPergola and Schmelz, the fundamental question is whether "erosive processes are currently at work in the demography of the Jewish family" or whether such "demographic erosion either does not exist or is only temporary and insignificant in the long run." I disagree. The core issue is whether we treat the family changes that are occurring, and around which there is little disagreement, as erosion or transformation. They have argued for the former (despite the title of their article), whereas I and some of my colleagues have argued for the latter.

Does it make any difference how we treat family changes? Most assuredly! The issue is not simply semantic but goes to the very core of our understanding of the sociology and demography of American Jews. By demographic erosion they mean "a serious population problem for American Jewry" derived from a variety of demographic changes and "a negative balance between identificational inflows to, and outflows from, the Jewish community." The consequences of these trends is "a negative balance between Jewish births and Jewish deaths and between accessions and secessions—leading to Jewish population decline." On the other hand, transformation means that radical structural and cultural changes are occurring, but the consequences for the Jewish community in terms of continuity and change remain unclear and require systematic study. This is not only because we have limited data (of course, we have less precise data than we need) but mainly because, with transformation, the past becomes more problematic as a guide to the future. The transformation of family patterns means that there are new unprecedented patterns emerging in the modern period. How these unprecedented family patterns are related to Jewish continuity remains to be studied, not inferred from the patterns of the past.

A simple example will illustrate the difference between "erosion" and "transformation." Increases in the divorce rate are viewed from the point of view of erosion as another indicator of the breakdown of the family and as part of the decline of traditional sources of Jewish family values. In the past divorce rates were low and family centrality characterized Jews everywhere. Increases in the rate of divorce are therefore part of the serious population problems of American Jews. Once placed in the erosion context, divorce rates can be presented (as DellaPergola and Schmelz do in their Table 3) without attention to whether remarriage occurs following divorce, to the timing of divorce in the life course, or to whether being "currently divorced" has an impact on demographic behavior or Jewishness. These types of relationships are more difficult to analyze, but the data to do so are available from the sources cited by DellaPergola and Schmelz. Yet, within their erosion framework, they feel no need to go beyond the presentation of simple

changes in the divorce rate to indicate that there is a problem here, along with other erosion tendencies. Erosion necessarily follows, then, by inference from the rates of increase.

When the authors do go beyond data on the increase in divorce, they cite with unqualified support a published report on the current Jewishness of the "ever-divorced" (not the currently divorced). Here, unlike for their demographic data, they attempt no evaluation of the data they present, although there are serious methodological problems in connecting current ritual observance with divorce patterns that may have occurred in the distant past. What is cause and what is effect? And they do not cite studies of Boston, New York and national U.S. data that show high rates of remarriage among the divorced and that there are few significant differences in the Jewishness of the ever-divorced and the currently married.

In contrast, the transformation argument views increasing divorce rates in the context of both the costs and the benefits. Divorce implies greater independence of men and women in their choices about marriage partners and radical changes in the roles of men and women over the last several decades. Examining remarriage rates of American Jewish men and women reveals that divorce does not lead simply to a decline in family life as a large proportion of the divorced remarry relatively soon after divorce. Thus, the trend toward increasing divorce implies that adults are not rejecting marriage per se but rejecting a particular spouse. Most important, the ways changes in divorce affect how Jews and their children are linked to the Jewish community and whether an increase in divorce rates implies a breakdown in Jewish communal affiliations and ethnic-religious identification are issues that need to be researched, not conclusions that can be assumed. Hence, those who argue for transformation will not automatically treat the rise in divorce as necessarily affecting the quantitative or qualitative basis of Jewish continuity.

In addition to divorce the two key family themes that DellaPergola and Schmelz treat within the context of erosion are changes in the entry to marriage and in intermarriages. It is on their analysis and interpretation of these two family issues that I want to focus. First, how do they treat changes in the extent and timing of marriage?

The data presented by DellaPergola and Schmelz in their Table 1 show that the proportion of Jewish women aged 35 to 44 who were never married is between 3 and 6.6 percent in recent surveys of U.S. Jewish communities (and in Canada the proportion in 1981 is 6 percent, significantly less than in 1941 and 1951). These are neither high nor alarming proportions. Moreover, young Jewish adults in the United States and in Canada have a higher proportion of singles than non-Jews. (In their comparisons, no controls for socioeconomic status are included. Detailed analytic studies show that these higher rates among Jews largely reflect the higher educational levels of young Jewish women.)

An increasing proportion of Jews aged 25 to 34 are single, with wide variation by community. Here the key question is how many of these unmarried Jews will remain single and how many will simply marry at a later age than previous cohorts. They suggest that the correct interpretation of these data is a decline in the propensity to marry rather than a postponement of marriage. However, their calculations to support such an argument are flawed. The measure they use, period proportion

ever-marrying (PEM) presented in their Table 2, is based on the assumption that age-specific marriage rates observed during the 1970s will persist in the future. Such an assumption in times when the marriage regime is changing is seriously distorting, as their own data show when their PEM for 1951–61 shows that more than 100 percent of the women in Canada have ever married! Indeed, their own conclusions suggesting that "simple extrapolation" from the past "can no longer be accepted" applies to their own calculations. Predicting future marriage trends using current data is therefore risky. The only evidence we have about the future (cited in their note 78) is based on the expected marriage patterns of young Jewish adults in the United States. It shows a high level of expected eventual marriage, even when postponed. Thus there is as yet no reliable evidence that Jewish marriage postponed is marriage foregone.

However, even if it turns out that delayed marriage results in higher proportions of non-marriage, will that characterize all future cohorts? We do not know, but we cannot assume that change is always in one direction only. The Canadian data they present show that marriage rates have fluctuated widely in the past and that current levels of non-marriage are no higher than those of the 1930s and 1940s. These cohort fluctuations further suggest that even if the marriage rates of the 25–34 age cohort continue to be low, later cohorts may adjust in new ways to the sex-role revolution. The patterns of the 1930s and 1940s did not continue, nor did those of the 1950s and 1960s. So, there is a risk in extending indefinitely into the future the patterns of the 1970s and 1980s.

Issues of mixed marriage are more complex both because the data are more problematic and the interpretations are more difficult. Looking at the proportion of Jews with a non-converted, non-Jewish-born spouse among recent marriage cohorts or younger age groups, they document both variation and increase in mixed marriages. The demographic implications of these patterns focus on the spouses and the number and Jewishness of children in these marriages. Let us examine each of these in turn.

With regard to spouses DellaPergola and Schmelz confirm the shift toward fewer formal conversions to Judaism. But the Jewish identification of persons is not limited to formal conversions and the gap between conversions and identification increases with secularization. Further, both conversions and Jewish identification vary over the life cycle. Studies have shown an increase in Jewish identification with marriage and childbearing, particularly when children reach school age. Thus, the National Jewish Population Study (NJPS) survey of 1971 showed that of those married in 1965–71 (i.e., five or fewer years), 23 percent of all non-Jewish-born spouses had converted to Judaism by the time of the study. But we know nothing about their Jewish identification and nothing about subsequent (post-1971) conversions to Judaism or changes in their Jewish identification. Even so, on the basis of the data presented, there is currently about a 20 percent gain of Jewish adults through conversion relative to total in-marriages. Because we do not know the conversion rate of Jews to non-Jewish religions, we cannot judge the net demographic effects.

And what about their children? DellaPergola and Schmelz use NJPS data to compare the fertility pattern of mixed and non-mixed Jewish marriages. But the

NJPS data are seriously outdated and hence much less relevant to their discussion of *current* patterns as the meaning of intermarriage has been changing. The fertility measures used are completed fertility to women aged 45 and over and children ever-born for younger women. The last cohort of women aged 45 and over in the NJPS study are women born in the late 1920s and early 1930s. Is this an appropriate base to examine the future fertility of young persons marrying and intermarrying in the 1970s and 1980s?

To look at the Jewishness of young children of the cohort marrying in 1965–71 (most below school age) and studied in 1971 compounds the distortion because that covers only the youngest children of this cohort born to those who married at an early age. The authors' conclusions based on the last NJPS marriage cohort that there was a net fertility loss of 15 percent as a result of intermarriage is clearly an overestimate of loss: we do not know how many of these children will become Jewish nor do we know the completed fertility of these couples. If there are demographic issues about the fertility patterns of intermarried couples, data from the Canadian census and from the studies cited by DellaPergola and Schmelz are available for analysis.

Considering the evidence they present, and contrary to the conclusions they reach, most of the major studies they cite show that about 50 percent of the children in mixed marriages are being raised as Jews, pointing to essential demographic stability when the conversions of spouses and the higher rate of Jewish identification among the converted is included. Examining only the religious activities and ritual observances of the children of the intermarried, as they do, to the exclusion of other expressions of Jewishness is inadequate because so much of Jewish identity among Jews in the United States is based on family and communal and associational networks. Their reference to studies that included information on the mixed marriage of parents as well as of the current generation again raises serious methodological questions: mixed marriages of a generation or two ago are unlike mixed marriages in contemporary America.

DellaPergola and Schmelz provide an important corrective to the literature when they insist that future studies examine both Jewish and non-Jewish activities (e.g., Passover and Christmas celebrations, Jewish and Christian education) within the same mixed marriage household as well as investigate the eventual Jewishness of the children at later points in the life cycle. It is also important to interpret intermarriage (as they do) as a process that is not automatically a step toward total assimilation and loss to the Jewish community. It is a process that is probably associated with the weakening of Jewish identity; but whether it results in total assimilation in the longer run needs to be studied and represents a challenge for research.

New family forms are emerging and the traditional nuclear Jewish family is declining. Their conclusion implies that the past patterns can no longer be a guide to the future. If higher education was associated with higher Jewish divorce rates in the past, that association may not necessarily characterize current patterns when divorce rates are higher and re-marriages more common. If high levels of education and female labor participation in the past were associated with lower marriage propensities, those may no longer be appropriate bases for extrapolating to the future. Particular marriage and family patterns of non-Jews may no longer be applicable to

American Jews when the social and economic characteristics of American Jews have become distinctive. That is the core meaning of transformation and why I reject the fundamental assumptions of those who argue about continuous erosion based on past patterns.

In addition to the problems of their conceptualization, there are several other limitations to the analysis presented by DellaPergola and Schmelz. These include questions about the comparisons they make and the types of empirical evidence they use.

Multiple comparisons between Jewish family patterns in America and Jewish populations elsewhere (and with non-Jews of similar status in countries where Jews live) are necessary to go beyond description and documentation toward analysis and interpretation. DellaPergola and Schmelz primarily emphasize comparisons among Jewish populations, and they leave unspecified which comparisons are addressed to which analytical questions. When they compare Jews with non-Jews in America and with Jews in other countries, their contrasts are confusing. For example, they write that "from a comparative perspective, marriage patterns of Jews in the United States and Canada generally featured significantly lower rates of singlehood, accompanied by lower ages at marriage, than among Jews in Europe." But in their discussion, they switch and conclude that "the basic trends of family patterns in the United States . . . are quite similar to those observed in, say, France." This latter observation leads them to assert that the "similar structural position and cultural vulnerability of Jewish population minorities everywhere" is an "alternative analytical approach" to those who argue that the American Jewish community has distinctive social and demographic features. What they mean by "cultural vulnerability" is a mystery; and clearly the size of the American Jewish population, its high educational and occupational achievements, the pluralism of its Judaism and the broader sociopolitical context of American society are distinctive.

No one has ever argued seriously for only one type of comparison. Some have argued (I among them) for the inadequacy of studying the demographic processes of Jews anywhere without making systematic comparisons with non-Jews. That is a fundamental methodological strategy when particular analytical rather than descriptive themes are addressed. To analyze the relative impact of Jewishness on demography and to examine general trends, issues and relationships a focus on other white U.S. population sub-groups is the most directly appropriate comparison. Superficial similarities with Jewish demographic trends in other countries may be important descriptively—and interesting in the context of Diaspora Jewry—but they are not appropriate for analyzing relationships between social and demographic processes.

When DellaPergola and Schmelz compare Jews and non-Jews—for example, in the proportion of ever-married among Jews and non-Jews in Canada (Table 2 and discussions throughout comparing Jews with the total U.S. population)—no attention is addressed to the major socioeconomic and geographic differences between Jews and non-Jews. Because these characteristics are related to the family processes under discussion, it is unclear whether they are showing the effects of socioeconomic and urban concentration or particular Jewish characteristics. If these comparisons serve analytical rather than descriptive purposes, comparing Jews to the total U.S. or Canadian population is unacceptable. Appropriate data with con-

trols for socioeconomic and residential concentrations are available in the data sets the authors use.

Throughout their discussion of demographic trends, some of their explanations refer to demographic imbalances, the size of the Jewish population and marriage markets. These demographic factors have important effects on marriage rates and intermarriage patterns, and these should be studied. Nevertheless, there is no evidence presented for their claims (a) that the "relatively small size and segmented structure of the pool of potential Jewish marriage candidates" tended to lessen marriages among the Jews in the past; (b) that "later marriages have contributed to the overall decline in fertility that has occurred in America since the 1960s"; or (c) that changes in the rate of male and female Jewish intermarriages reflect imbalances in the number of potential grooms and brides or in cohort size. More important to the demographic changes that are discussed in their essay is the enormous impact of the revolution in women's social roles in the United States, and this is not reviewed.

Their discussion of overall identification balance is clearly inadequate (as they admit) because hardly any statistical evidence on secession is available. Nevertheless, why do they cite NORC data when these reveal nothing about the issue they want to address; those data focus on self-declared not "ex-" Jews? There are endless research studies that are unacceptable, methodologically problematic and that contain data that they "do not recommend giving too much credence to." In order to focus on the scholarly issues and clarify areas where further scientific research is necessary, a goal I share, more attention needs to be paid to the available reliable research. In this regard, their exclusive focus on published data from census and from Jewish community surveys limits their ability both to make appropriate comparisons and to pursue issues of analytical importance.

What do all these critical points add up to? In part, I would argue that the data DellaPergola and Schmelz present, however we might argue about possible interpretations, cannot test in any decisive way *hypotheses* about demographic erosion in American Jewry. Although everyone who has studied the demography and sociology of American Jews shares their conclusion that there have been assimilation processes characterizing American Jews, their further assertion that these assimilation processes result in the demographic erosion and "consistent attrition" of American Jewry cannot be justified on the basis of the research they have presented. Assimilation, in the sense of changes and adaptation to the society where Jews live, has occurred in America as it occurred in other societies where Jews are living and have lived in the past. But what has not occurred and what their data cannot test or confirm is the "assimilation hypothesis" that argues for the linkage between assimilation processes and loss of community, that is, the erosion of Jewish life.

I have argued for a reexamination of the assumptions underlying the interpretations they have presented and the need for a reassessment of the strengths and weaknesses, however defined, of the American Jewish community. When these analytical issues of transformation are studied directly and systematically, we shall be in a better position to consider the future of the American Jewish community and suggest policy alternatives to address that future. Building on preconceptions of demographic erosion and cultural assimilation, I argue, distorts the demographic and sociological understanding of the American Jewish community.

In their evaluation of Jewish marriage patterns in America, they suggest new "Jewish policy decisions aimed at strengthening Jewish identification and at ensuring a meaningful Jewish life in America." It is most gratifying that in this, at least, we agree. Their suggestions for new family-based policies and away from a limited emphasis on demographic considerations that too many have argued for in the past, I hope implies a serious rethinking of the evidence and a move from a policy centered on demographic quantity to issues of improving the quality of Jewish life in America.