

**Measuring Divorce and Separation:
Issues, and Comparability of Estimates Across Data Sources.**

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Introduction:

An extensive literature documents high levels of family instability in the U.S. and the importance of this instability for the well-being of both adults and children (Seltzer, 1994; Amato, 2000). Our efforts to better understand these relationships obviously depend on how well key transitions are measured. This is true for both the estimation of levels and for analyses of how family transitions affect, and are affected by, other events in the life course. This paper is a working draft in which we raise a number of issues relating to the accuracy of survey measures of marital disruption, and examine data on several of these.

While vital statistics are potentially the best source for measuring the incidence of marriage and divorce, their use has been extremely limited¹ Weed's estimates of life-table survival rates is a major exception (1980). In addition, the estimation of differentials from vital statistics has always been frustrated because few characteristics are recorded on divorce certificates, and even these are collected unevenly across states. Given the demise of national vital statistics reports on marriage and divorce in 1995, we are now totally dependent on survey data (Bramlett and Mosher, 2002).

We begin with a conceptual discussion of different measures of family instability and review several issues affecting accuracy. We then evaluate the quality of estimates from retrospective histories: in comparison to vital statistics, in comparison to longitudinal measures, across surveys with varying time since events, and across surveys with different substantive foci. Finally, we consider our data needs and the need for research directed specifically to improving our measures of family stability. In all but one section of this report, we focus solely on reports from women both because of the lower quality of reports from males (Pendleton, McCarthy, and Cherlin, 1983), and

¹The recording of the number of divorces by duration of marriage is confined to the low number of states in the Divorce Reporting Area, 29 in 1977, for example (Weed, 1980).

because many of the surveys we examine are limited to females.

Conceptual Issues

Divorce vs Separation as Dissolution Date. Recent estimates suggest that 43% of all first marriages will end in divorce (Schoen and Standish, 2001). While divorce is important, this number should not be seen as a revision of estimates that half of all marriages will disrupt (Cherlin 1992;Raley and Bumpass, 2003). The difference, of course, is that marriages are effectively ended when a couple permanently separates. The time between separation and divorce varies (in part because of the vagaries of the legal system) and some never divorce (Sweet and Bumpass, 1987).

The use of date of separation, rather than of divorce, affects the measurement of both the timing and levels of marital disruption, and these differences are particularly important for comparisons across population subgroups, especially between blacks and whites (McCarthy, 1978). The consequences of these measurement differences is dramatically illustrated in Figure 1 based on the National Survey of Families and Households (NSFH). If we consider only the event of divorce, we would conclude that marriages among whites are much more likely to end in the first 5 years than are those among blacks. However, just the opposite conclusion is reached when separation is used to mark disruption. These differences are large, and the pattern persists even 10 years after marriage. Hence, the use of separation rather than divorce has become the standard practice in most analyses of family instability (Sweet and Bumpass, 1987; Castro Martin and Bumpass, 1989)

Marriages vs All Unions: The title of this session reflects our longstanding focus on *marital* transitions to characterize family instability. While recognizing that many cohabiting unions are not “like marriage” (Rindfuss and Vandenh*euvel, 1990), we argue that both marital and cohabiting transitions are important for understanding family instability (Bumpass and Raley, 1995; Bumpass, Raley, and Sweet, 1995; Bumpass and Lu, 200). This is illustrated in Figure 2 which presents life-table estimates of levels of

family instability by race/ethnicity for the 1987-94 marriage cohort (Raley and Bumpass, 2003). The first two sets of columns represent only *marital* instability, for the 1980-86 and 1987-94 cohorts respectively. Looking at the more recent cohort, race differences in marital instability are evident as about a third of black marriages ended within 5 years, compared to about a quarter of white marriages. Nonetheless, race differences in *union* instability are much greater. Whereas a third of white unions ended within 5 years, over half of black unions did so. Hence black marriages are 39% more likely than white marriages to break up in 5 years, but black unions are 67% more likely to do so. When we look at trends, there is little change in the differential as measured by marriage, but a large change as measured by unions.

Including transitions into and out of cohabitation is important for research on documenting children's experience as well. Life table estimates from the 1995 National Survey of Family Growth (NSFG) show that if one counts only marital transitions, the average child experiences only 0.69 transitions by age 12, but if cohabitation is included the average child experiences 0.92 transitions. (Note that the transition from a union into marriage is not counted in the second estimate). As we would expect from the above, including cohabitation has much a bigger effect on estimates for black children than for whites. The average black child experiences fewer marital transitions than the average white child (0.55 as compared to 0.69). However, the average black child experiences many more union transitions (1.18). Put another way, more than half of the family instability black children experience occurs in transitions into and out of nonmarital unions (Raley and Wildsmith, in press).

First vs All Marriages: Finally, most of the research on trends and differentials in divorce (including our own) focuses on first marriage. This, of course, is just right when the interest is specifically in first marriages, since remarriages are selective on a number of relevant characteristics. Nonetheless, it is often the case that statistics based on first marriage are cited in discussions about marriage in general, even though about a third of

all brides have been married before and almost half of all marriages are remarriages for at least one spouse (National Center for Health Statistics, 1996). It is well-known that remarriages are less stable than first marriages. Among marriages around 1990, about 40 percent of second marriages disrupted within 10 years compared to about 32 percent of first marriages.² While it only slightly increases our estimates of disruption to include all rather than just first marriage, doing so would make a larger difference in populations with less stable marriages. Separate consideration of marriages by order is often restricted by sample size or sample design issue, but it is likely the case that we tend to focus on first marriages when addressing more general objectives either out of habit or because it is easier.

Design Issues

Retrospective vs Longitudinal Measurement

It seems self evident that longitudinal surveys provide more accurate data on transitions than retrospective histories because the time interval to be recalled is much shorter. Nonetheless, we know very little about the extent to which this is so for the various dimensions of family stability. Differences in the quality of dates collected from these two modes of measurement very likely depend on factors such as the definitiveness of the transition being measured and the time that has elapsed since the event. In addition, it is important to note that longitudinal measurements are necessarily either dated inexactly by current statuses at successive interviews, or are themselves retrospective reports, albeit for shorter time intervals. In the first instance, the range of error in the interval between events can be almost two years in the case of annual interviews³—four years if interviews are biannual. In addition, spells that began and ended between waves

²Based on life-table calculations for the 1985-94 marriage cohort in the 1995 June CPS.

³For example, a respondent who was single at one interview, married at the next, and separated at the next, may have married immediately after the first interview and separated just before last, or married just before and separated just after the middle of these three waves.

are missed altogether as was the case with cohabitation intervals in the NLSY in earlier waves (Gryn, Mott, and Burchett-Patel, 2000). It is for this reason that the PSID and NLSY now ask about the dates of transitions since the prior interview. Mott and colleagues report that male reports of fertility can be inconsistent between waves, even when asked at one or two year intervals (Mott and Gryn, 2001). It is plausible that in some instances these differences are introduced by life circumstance that affect reporting at one interview but which may be irrelevant when events are reported from a longer time perspective.

In some instances, longitudinal surveys have included a supplement in a single year that collects a retrospective history spanning prior waves. This was true for the PSID in 1985 and for NLS72 in 1986. These histories provide dates of transition that were missed when only changes in statuses between interviews were measured. Since the period in these retrospective histories covered waves for which dates were asked about events since the least interview, they provide unique opportunities for comparing retrospective and longitudinal data. Unfortunately, this is a project beyond the reach of the present paper.

Question Format and Sample Coverage

It should be clear from our discussion above that we think it is essential to ask the dates couples start and stop living together as well as dates of marriage and divorce. Aside from that, the key issue in question format with respect to marital (and union) stability has little to do with wording per se, but rather it has to do with whether marriage, separation, and divorce dates are collected as a separate history—that is, the dates of formation and dissolution of each marriage sequentially—or collected in the context of surrounding life events, that is, a life-history calendar (Freedman, et al., 1988). In a life-history calendar approach, events are recorded on a sheet laid out before the respondent so that responses about the dates of events in one domain can be used as reference points for recalling others. Suppose a respondent moved in with his girlfriend the fall after he

graduated. He might better be able to report when this cohabitation began if he placed it in the context of his month and year of graduation already recorded on the calendar.

The life-history calendar approach works very well for a young cohort such as the longitudinal study of 1961 births in Detroit (Thornton, Axinn, and Teachman, 1995), but is more difficult to apply in a sample with a broad cross-section of ages as in the NSFH, or even the NSFG. There are practical limits to how long a life-history calendar can be—in the absence of a scroll. Further, while adaptations can be made, such as sending a calendar ahead of time, life history calendars are harder to employ in telephone surveys.

In any application of this approach, however, choices have to be made about the domains used as cues for transitions in other domains. With this point in mind, it becomes clear that the format in which cohabitation histories are collected vis a vis marriage histories is a more limited version of the issues relating to life-history calendars. Given that marriage histories are more clearly defined, with marriage dates being particularly salient, the NSFH approach is to key questions about cohabitation to the marriage history. After the marriage history has been recorded, respondents are then asked about each interval before first marriage, between marriages, and after the last, as applicable. Each sequence before a marriage begins with asking whether they lived with that spouse before the marriage, and if so, when they began living together. An alternative is to ask about each cohabiting union sequentially, recording in the process whether the couple was married when they started living together, whether they ever married, and if so, the date that they married. This was the procedure followed in the the National Health and Social Life Survey (NHSLS) and in the 1986 wave of the National Longitudinal Study of the High School Class of 1972 (NLS72).

Upper Age Limits on Samples

We conclude this section by elaborating on a well-known, but oft forgotten, observation about the effects of upper age constraints. This is illustrated most clearly by the NSFG. We should make it clear that the NSFG is an invaluable source of data on

cohabitation, marriage, and divorce—and we anxiously await the release of Cycle VI. Nonetheless, a sample targeting women of reproductive age places clear limitations on estimates of disruption trends over cohorts, or even period life-tables of cumulative disruption for the years just prior to survey. The issue is simply that the earlier the marriage (or cohabitation) cohort, the younger the age at which it must have been formed. This is seen most easily with respect to a single year cohort married 20 years before survey. Among women marrying in 1975 (20 years before survey), only those who married before age 25 are represented in the 1995 survey because of the upper age limit of 45. This censoring on age at marriage obviously gets progressively more severe for each earlier cohort. Hence, trends can only be evaluated for a relatively recent past, and for marriages initiated before the youngest age represented in the earliest cohort analyzed. This limitation obviously applies as well to life-table estimates of cumulative survival, since the hazards estimated for successively longer durations are drawn from progressively younger marriages. This is undoubtedly one reason that the trend data reported by Bramlett and Mosher (2002), which we include in our comparisons shortly, was based on a file pooled across NSFG waves. It should also be noted that the problem is more severe for spells begun at older ages than first marriage, such as those following divorce, or remarriage.

Comparisons Evaluating Data Quality

We now turn to the issue of whether survey data accurately depict these experiences at the population level. We examine estimates derived from the 1985, 1990, and 1995 June Current Population Surveys (CPS), the 1987-88 National Survey of Families and Households (NSFH), the National Surveys of Family Growth (NSFG pooled), the 1979 National Longitudinal Survey of Youth (NLSY), the Survey of Income and Program Participation (SIPP), and vital statistics (in addition to the NHSLs referenced above). These surveys were collected over a wide range of dates, have different foci, and employ different questioning approaches and interview modes. For example, the main objective of

the CPS is to collect information about employment and the labor force, while the focus on the NSFG is on sex, fertility and contraception. The NSFH covers a wide range of topics centering around family life. The NSFH, NSFG and early CPS surveys were all conducted via personal interviews, but the 1995 CPS was conducted via the telephone. While most of our estimates are based on retrospective histories, those from NLSY79 are longitudinal. These differences enable us to examine how robust estimates of levels of divorce are across a variety of approaches to data collection.

Time Since Event

We are able to assess whether reporting deteriorates with increased time since an event by using June CPS data across survey years. We compare estimates for marriage cohorts based on reports at successively longer 5 year intervals since the events: i.e. we estimate the proportion divorced by a specific duration for each specific cohort as reported in the 1985, 1990, and 1995 CPS surveys. The results in Figure 3 reveal that estimated probabilities of divorce within 5 years are remarkably similar for each marriage cohort as measured across the surveys. There is no evidence that divorces which occurred even 30 years before interview are reported any less well in 1990 than in the year closest to when the divorce occurred⁴ The largest deviation of the 1990 CPS from earlier ones is that the estimate for the 1980-84 cohort is 3 percentage points lower. The one deviation that does stand out is that estimates from the 1995 CPS are about 3 percent lower than the others across all cohorts. We would likely regard such a difference as trivial, were it not for the even closer agreement among the others and the systematic nature of this difference for all cohorts. While we first thought that the shift to telephone interviewing might account for this difference, apparently two-thirds of the 1990 June CPS were completed by telephone.

The upper lines in Figure 3 represent the proportion divorced by 10 years since first

⁴Bumpass, Castro Martin, and Sweet (1991) found that 40% of those married between 1970 and 1984 had separated and reconciled at least once, but that almost all of these separations that reconciled did so within a year. Thus, to avoid overestimating levels of marital disruption for marriage cohorts just prior to the survey date, we produce an estimate for the marriage cohort shifted back one year. For example, the estimates for the 1980-84 marriage cohort using 1985 data are actually estimates of marital disruption for the 1979-83 marriage cohort. This practice is continued in the estimates using pooled data.

marriage. Here, we again see very high agreement among the estimates. Indeed, the 1995 estimates have largely caught up with the other surveys, compared to the lower levels at 5 years duration. The two major exceptions are that the estimate from the 90 CPS is 3 percentage points above that from the 85 for the 1975-79 marriage cohort and the estimate from the 1990 is about 4 percentage points above that from the 95 survey for the 1980-84 cohort. These differences at the last cohort in each survey is not a consequence of poorer reports for recent events just before survey since these are, by definition, for events 10-20 years before survey. While it may prove useful to try to solve this puzzle, the key point is that the CPS data seem to consistently provide the same estimates even when events are measured at substantially longer times after they occurred.

Survey Context

The preceding has demonstrated rather remarkable agreement across surveys using the same questions, in the same format, and in the same survey context. As noted earlier, the NSFH, NSFG, and SIPP provide varying contexts, ranging from a study dedicated to family issues, to one heavily concerned with contraception and fertility, to one focused almost solely on economic issues. When we add estimates of separation by 10 years from these surveys to those we have observed from the 1990 CPS (Figure 4), we again find rather high agreement.⁶ There are some inexplicable deviations: the NSFG estimate is about 6 percentage points below the others for the 1965-69 marriage cohort, and the SIPP estimates for the 1980-84 cohort is about 5 points below that from the 90 CPS. Nonetheless, there are no systematic difference, and we see these results as indicating a striking agreement across surveys. Indeed this agreement is the more remarkable because of the high levels of proxy reporting and imputation in the CPS.⁷

We can examine two surveys in which marriage histories were collected as part of a

⁶We select 10 years because the estimates from the NSFG are reported for this duration, and the 1990 CPS because of the systematic deviation seen in Figure 3 for the 1995 CPS.

⁷For example, McCarthy, Pendelton and Cherlin (1983) report that in the June 1980 CPS, about a quarter of the marriage histories for women were reported by someone else, and imputations were made for first marriages for about 13 percent of the marriage dates, about a quarter of the divorce dates and for over a third of the separation dates.

sequential partner history, NHSLs and NLS72, though we have to examine each of these from the perspective of the experience of birth cohorts rather than marriage cohorts. The 1986 wave of NLS72 included retrospective marriage histories embedded in partner histories. (Hence, although the study is longitudinal, the marriage history is collected through retrospective reporting.) Of those who had married by 1986, life-table estimates indicate that 25 percent had seen their marriage dissolve by six and a half years after marriage (Teachman and Paasch, 1991). From the 1990 CPS we get an estimate of 24 percent for high school graduates in this cohort. So once again, despite the difference in question design, we get amazingly comparable estimates across surveys.

The results are different when we examine the other survey that collected marital histories embedded in partner histories. Lauman and colleagues (1994) report the proportion that had divorced within 10 years of marriage for three birth cohorts, those in their 30s, 40s, or 50s at the time of the NHSLs survey. Contrary to what we saw in other comparisons, these estimates differ substantially from those we obtain for these the cohorts from the June 1990 CPS, and the difference is larger the longer the time before survey. The NHSLs estimates are higher than those from the 1990 June CPS by 33 percent for the earliest cohort, 27 percent for middle, and 12 percent for the youngest. On the face of it, this would suggest that the quality of reporting may have deteriorated as a consequence of the time over which events had to be remembered. Such a conclusion would be inappropriate from this comparison, however, because the same time had elapsed before both the CPS and NHSLs reports. We might infer that the these results come about as a consequence of the concentration on sexual topics in the NHSLs. However, this seems unlikely because only background questions, such as those on education and employment, were asked before the partnership histories.

We have only one instance in which we compare retrospective histories to longitudinal data. NLSY79 is obviously a single cohort, but we can compare the marital separation history of this birth cohort, assembled from longitudinal data, to estimates from

the 1995 CPS. We have chosen the 1995 data because we want to match at the longest durations possible and we have censored the NLSY79 observations at 1995 (which makes little difference). Once again, we are impressed with the high level of agreement across data sources. Figure 5 represents the cumulative proportion separated by successive durations since marriage. The longitudinal estimates fall only about 2 percentage points below those from the 1995 CPS at all durations. In itself, this would be a trivial discrepancy, but recall that the 1995 CPS itself consistently fell below the other sources over marriage cohorts. While not a big difference, these results may suggest some effect of even the modest attrition in the NLSY79 sample, since those who have separated and divorced are harder to locate and interview than others. In any event, these results do not support an argument that longitudinal data provide a more complete accounting of marital histories than do retrospective reports.

Estimates From Survey Marital Histories and From Vital Statistics.

In describing how we have become almost solely dependent on surveys, we began this paper by noting some of the limitations of vital statistics. Nonetheless, we must address the extent to which survey data agree or disagree with vital statistics estimates, even if this gold standard is somewhat tarnished. In this section, we compare period life-table estimates from the CPS data to published estimates from vital statistics (Weed, 1980). Previous research has concluded that survey data on marital histories underestimate levels of divorce and remarriage (Preston and McDonald, 1979; Pendleton, McCarthy and Cherlin, 1983; McCarthy, Pendleton, and Cherlin, 1989). Preston and McDonald (1979) present data suggesting that divorce is underreported by around 25 percent in the census compared to vital statistics. McCarthy, Pendleton and Cherlin found a similar discrepancy between the June 1980 Current Population Survey (CPS) and vital statistics. More recently, however, Goldstein (1999) concluded that survey data provide estimates consistent with the crude divorce rate. We further examine this issue by extending the our duration dependent estimates to comparisons with vital statistics.

Because vital statistics obviously record the legal event of divorce rather than separation, we must use date of divorce in this part of our evaluation of survey data quality. Weed's (1980) period life-table estimates for 1975 provide the best available benchmark from vital statistics for this comparison. Using pooled 1980 and 1985 CPS data, we calculated period life-table estimates for 1975 of the cumulative probability of divorce by successive marital durations, and the comparison of these to Weed's estimates from vital statistics is reported in Figure 6. The CPS estimates in this are derived from the duration specific experience during 1975 of the marriage cohorts of 1945 through 1975. Our estimates are virtually identical to Weed's over the first 5 years of marital duration. After that, a slight difference emerges with the CPS estimates about 2 percentage points lower at 10 years and above. The cumulative proportions estimated to have divorced by 30 years are about 8% percent lower based on the CPS compared to the estimates from vital statistics.

Mode Effects and Reporting Accuracy

In part because of fiscal constraints, surveys are moving increasing to computer-assisted telephone interviewing (CAPI). This raises the obvious question of whether our measures of marital instability are affected by this change in interview mode. To address this question, as well as to evaluate the accuracy of reporting, Call and Bumpass carried out a 1996 survey with a sample of divorce certificates in 4 counties in Wisconsin⁸. One member of each divorcing couple was randomly selected and assigned to be interviewed either in person, by telephone, or by a mail questionnaire. Further, these divorces were drawn from two years, 1989 and 1993, to permit evaluation of the time since the events occurred. While parameters from this sample, such as the proportion misreporting various dates, cannot be generalized to the total U.S. population, the patterns observed speak to the issue at hand.

⁸The "Life Events and Satisfaction Survey" was designed explicitly for this purpose, but marital histories were collected in the broader context of substantively relevant variables (R01 HD31035). A description of the study can be found in Coulter (2003).

The date of divorce is a legal record and can thus be evaluated with the most certainty. On the other hand, both marriage and separation dates are subject to errors on the certificates in addition to misreporting by respondents in surveys. Nonetheless, as we would expect, dates of marriage showed the greatest agreement between the survey report and the divorce certificate, and dates of separation the least. Allowing for 6 months on either side of the dates on the divorce certificate, women reported dates that agreed in about 90 percent of the cases for marriage dates, and in about 75 percent for dates of divorce (Coulter, 2003). If we widen the band to within a year on either side, it includes almost 90 percent of the cases for divorce dates and over 95 percent for marriage dates. Hence, there is a considerable amount of noise in our measurement of divorce rates, but the error does not appear to be large.

It is good news that there were no significant differences in reporting accuracy between phone and personal interviews, although the well-known lower quality of data from males is found in these data as well. At variance with our findings above, however, this study found that both separation and divorce dates were reported more accurately for events that occurred closer to the interview. The contrast between these results and the agreement across surveys for marriage cohorts suggest that this deterioration with time likely attenuates rather quickly after the first few years. And again, underreporting of marriage and divorce may be offsetting to some degree for the estimation of divorce rates.

Discussion and Recommendations

We have found surprising agreement across a number of surveys with different foci, formats, and designs. Further, the levels estimated by these surveys are closely consistent with vital statistics, though somewhat lower. On the other hand, divorce dates appear to be reported with considerable unreliability at the individual level. These results suggest that there is little bias in the reporting error, but there may be more serious implications for the relative dating of events over the life course.

Both the clarity and the saliency of dates are likely key to how these issues affect

our research, and the dates essential for our analyses of family stability vary systematically on both dimensions. Dates of marriage and of children's births are unambiguous, and they often remain salient through annual recognition. While dates of divorce have a clear legal specification, divorce decrees often occur in the stream of a process, and hence may be less clearly defined as experienced. Further, few of the divorced are likely to mark the anniversaries of their divorce.

Dates of cohabitation and separation can lie at the other end of this continuum of both definitiveness and saliency. The ability to assign an exact date to when a couple started living together while unmarried, or when they stopped living together, must have a great deal of variability. For some couples, these are single and clearly defined events and are solidly anchored in memory. For others, moving in or out of a shared household may have been repeated events, interwoven with the evolution or devolution of their relationship (Smock, 2003). When asked in a retrospective survey, such respondents may be unable to assign a clear date even if an event is recent and the question highly specific.

Our recommendations are of three kinds. The first is that we have to face up to the fact that we no longer have the data needed to address critical issues relating to family instability. This is especially true now that the June CPS no longer collects full marital histories, but it has been true in the past as well because of the absence of cohabitation data in the CPS. Our best data source at present, the NSFG, is limited by its upper age constraint as we have discussed above. An older sample is needed if we are to be able to analyze more than first marriages, and if we are to be able to follow the life course of children when as affected by their mothers' transitions after age 45. The focus of the NLSY on a younger cohort raises similar problems. While wave 1 of the NSFH provided the needed data across all ages, the longitudinal panels have not been refreshed at the younger ages. In addition, even if there were little error in measuring spells of cohabitation, our samples yield far too few cases for extensive analysis. The likely extent of measurement error makes this even more problematic.

In our attempt to understand the role of family instability in the lives of children and adults, we absolutely need cohabitation histories, samples that cover the ages when children under 18 are resident, and samples large enough to allow attention to details such as age at onset and the duration of spells. Two options seem particularly attractive here. The first would be to restore marital histories to quinquennial June CPSs, and to add cohabitation histories. This would be a major contribution. At the same time, the number of analytic variables would remain limited. The second possibility would be to add a panel of older respondents, ages 45-64, to the NSFG. It is important to remember that the mission of NSFG explicitly includes family as well as fertility. Interviews with this supplemental sample should be much shorter than the present NSFG, covering explanatory variables, birth histories, and marriage and cohabitation histories.

Second, in addition to improved data collection systems, studies are needed to test whether our findings, that we do a good job of measuring levels of instability for first marriages, can be extended to higher order marriages and cohabitation. And third, coincident with research studying whether retrospective surveys adequately capture union histories, we need research examining error at the individual level. What formats produce accurate accounts of individuals experiences?

There is obviously a relevant literature on measurement that must be underlie this effort, but that is beyond the scope of this paper. There is rather little evidence on the superiority of LHCs in collecting marital histories.. In connection with moving from a one-year to a two-year interval between waves, both the PSID (Belli, Shay, and Stafford, 2001) and the NLSY (Dugoni, Lee, and Tourangeau, 1997) report better correspondence between retrospective and panel data with life-history calendars than with conventional formats. However neither of these studies addressed marital histories. Freedman and colleagues carried out a retest with the Detroit birth cohort followup (Freedman, et al., 1988), but the results were only suggestive because of the small size of the retest.

Several potential designs come to mind. As in the Wisconsin study described above, factorial designs are needed wherein respondents are randomly assigned to alternative questioning formats. The design of alternative approaches will necessarily involve variations on life-history calendars, and it must be informed by the kind of qualitative explorations that Smock and Manning have underway (2003).

A major difficulty for this line of research is finding a standard against which to measure “accuracy.” This can be relatively straightforward by sampling from lists of marriages or divorces, but exceedingly elusive for cohabitation and separation dates for the very reasons that they are most problematic in the first place. Interviewing both partners would allow the extent of agreement to be used as an approximate standard, though there are design difficulties even with this approach. Preliminary work of this sort can be done with the data from both partners in the NSFH.

It does seem possible that alternative procedures for retrospective measurement of cohabitation and separation dates can be evaluated reasonably well in ongoing longitudinal studies. As has been done for short intervals and recent experience with the PSID and NLSY, experimental retrospective modules can be collected for comparison to the month and year of events have been collected between waves over an extensive number of years.

While it is important that we better understand the extent of erroneous reporting, and how that varies by characteristics, it is most critical that we evaluate the effect of this misreporting on our substantive analyses. It is obvious that measurement error affects the predictive ability of our models, but that is not what is at stake at the moment. Rather, there are two kinds of issues. The first concerns the extent to which the ordering of life events is misclassified, affecting our retrospective identification of states such as being in a single-parent family. The second has to do with whether the differences in intervals between events affect substantive conclusions about transition rates. It may be worth noting in this context that events misdated by even a year in retrospective surveys may yield data of comparable quality to longitudinal data without dates of between-wave

transitions.

All of this is not just toolmakers' fascination with their trade. As we noted at the outset, our concerns with the effects of marriage and divorce on the lives of children and adults absolutely must be expanded to include cohabitation—and it is the dating of the beginning and ending of spells of cohabitation that is most problematic. Ultimately, it is the critical nature of our topic for understanding family life, and for social policy affecting the lives of the nation's families, that requires improved tools and materials

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**Figure 1. Percent Disrupted Measured by Separation
and by Divorce: by Race, 1987-88 NSFH**

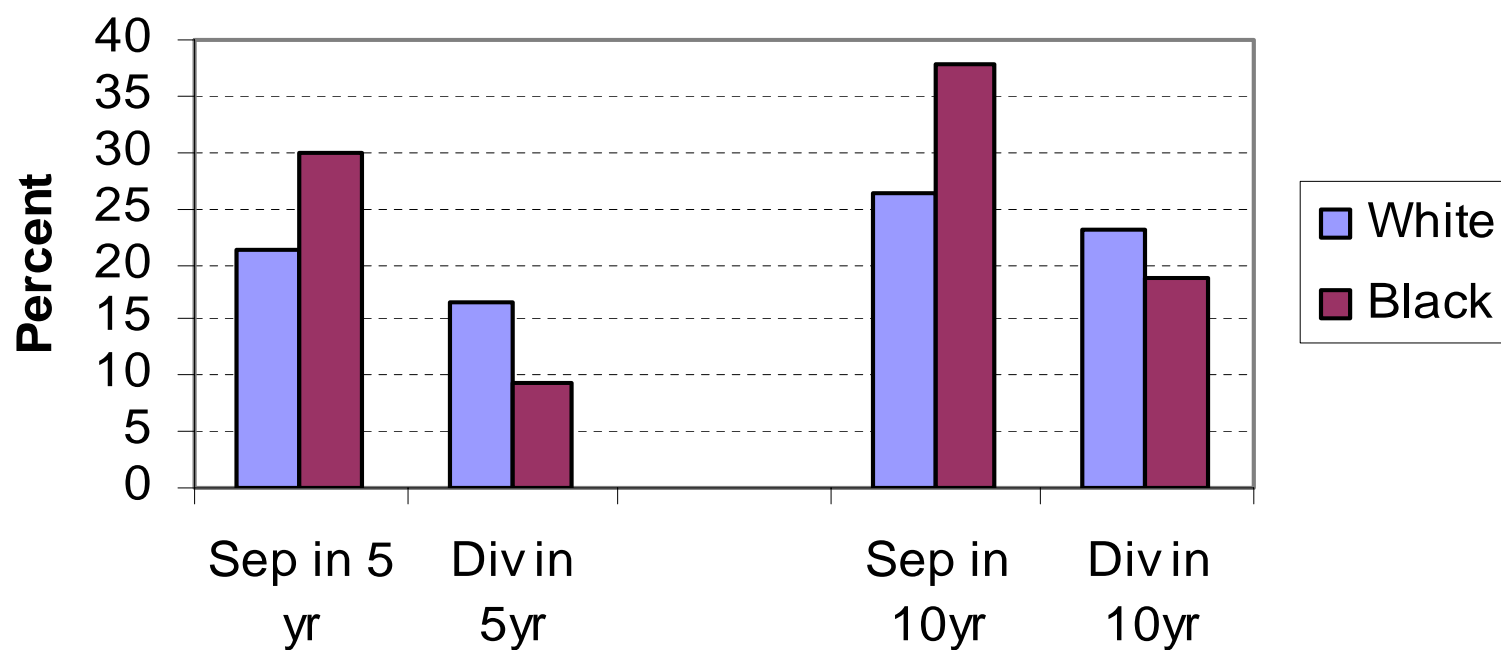


Figure 2. Percent of First Marriages/Unions Ended in 5 years by Race/Ethnicity

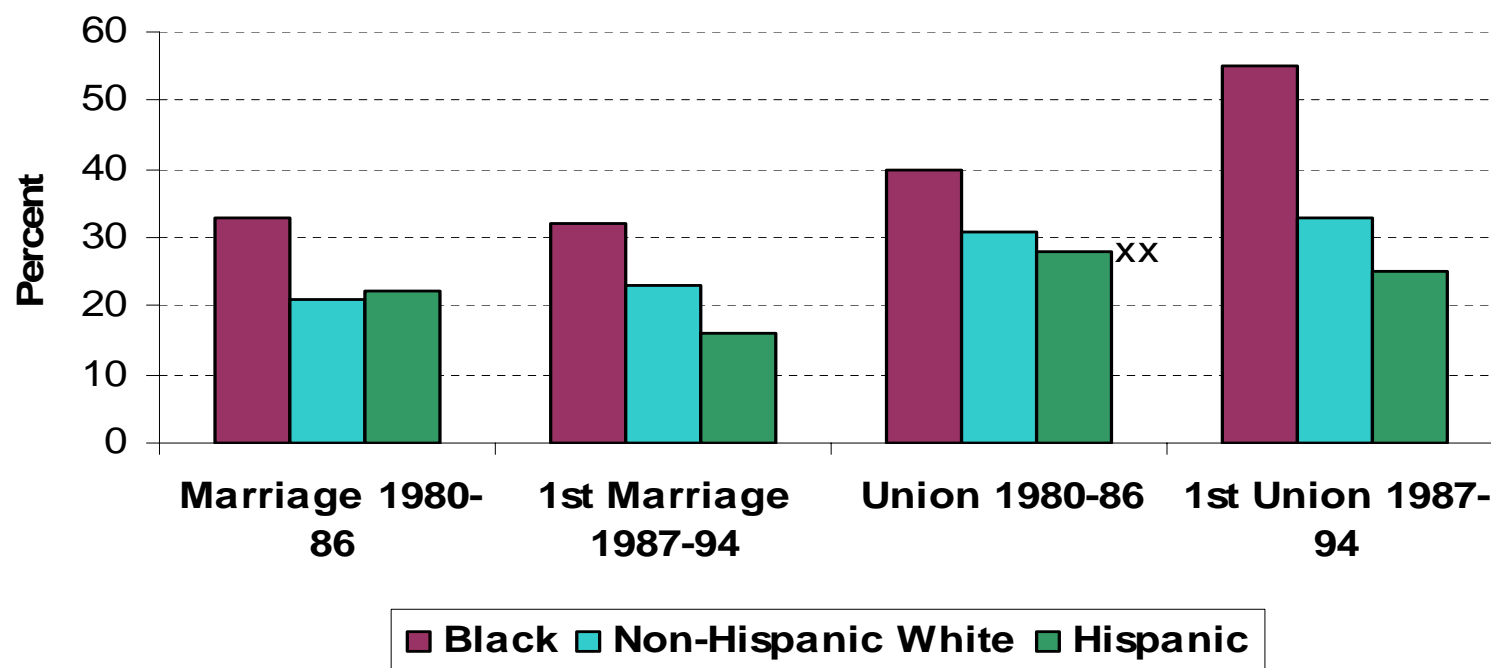


Figure 3. Percent of First Marriages Separated Within 5, and within 10, Years of Marriage, by Marriage Cohort and by Year of June CPS

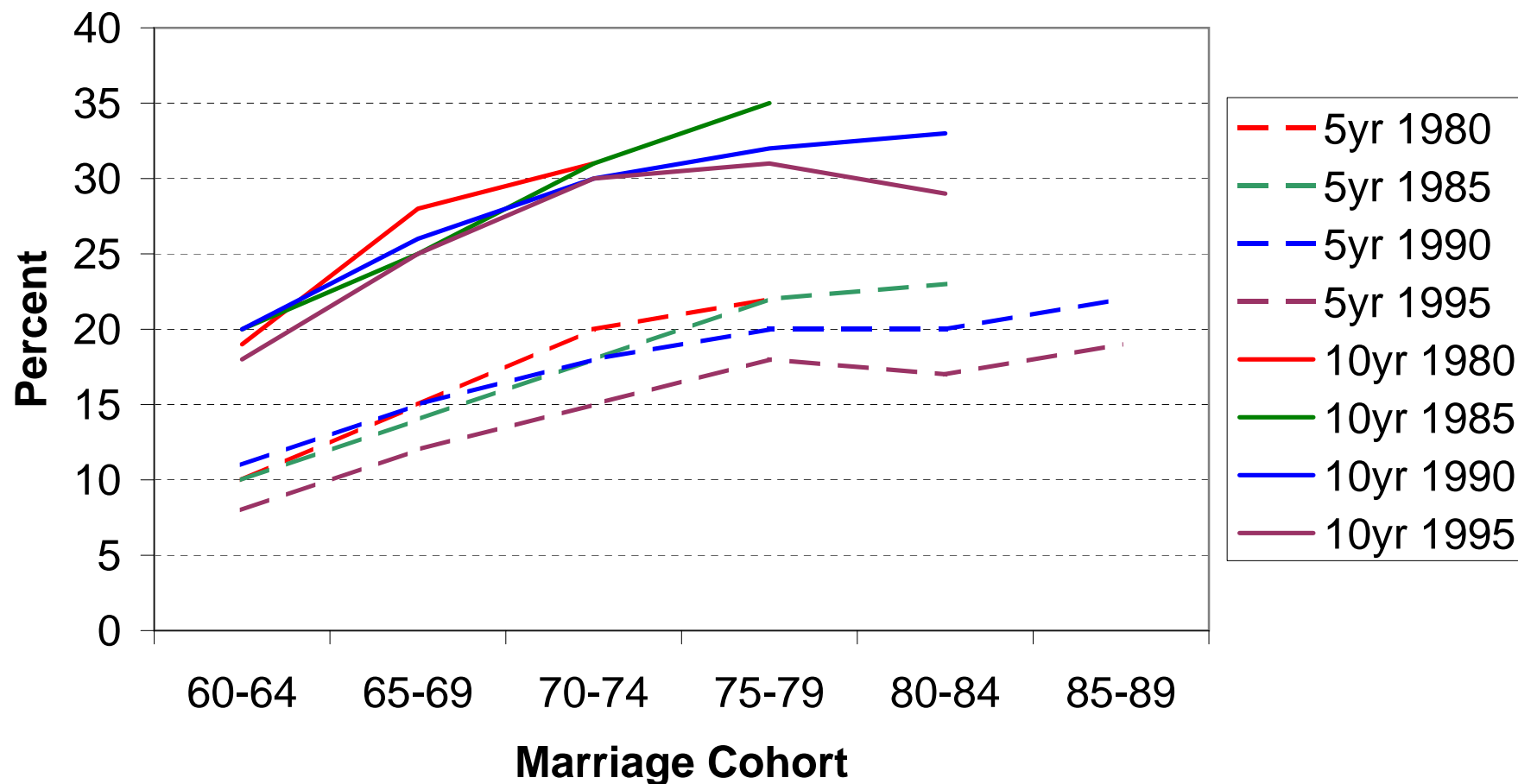


Figure 4. Percent of First Marriages Separated Within 10 Years, by Marriage Cohort and Survey

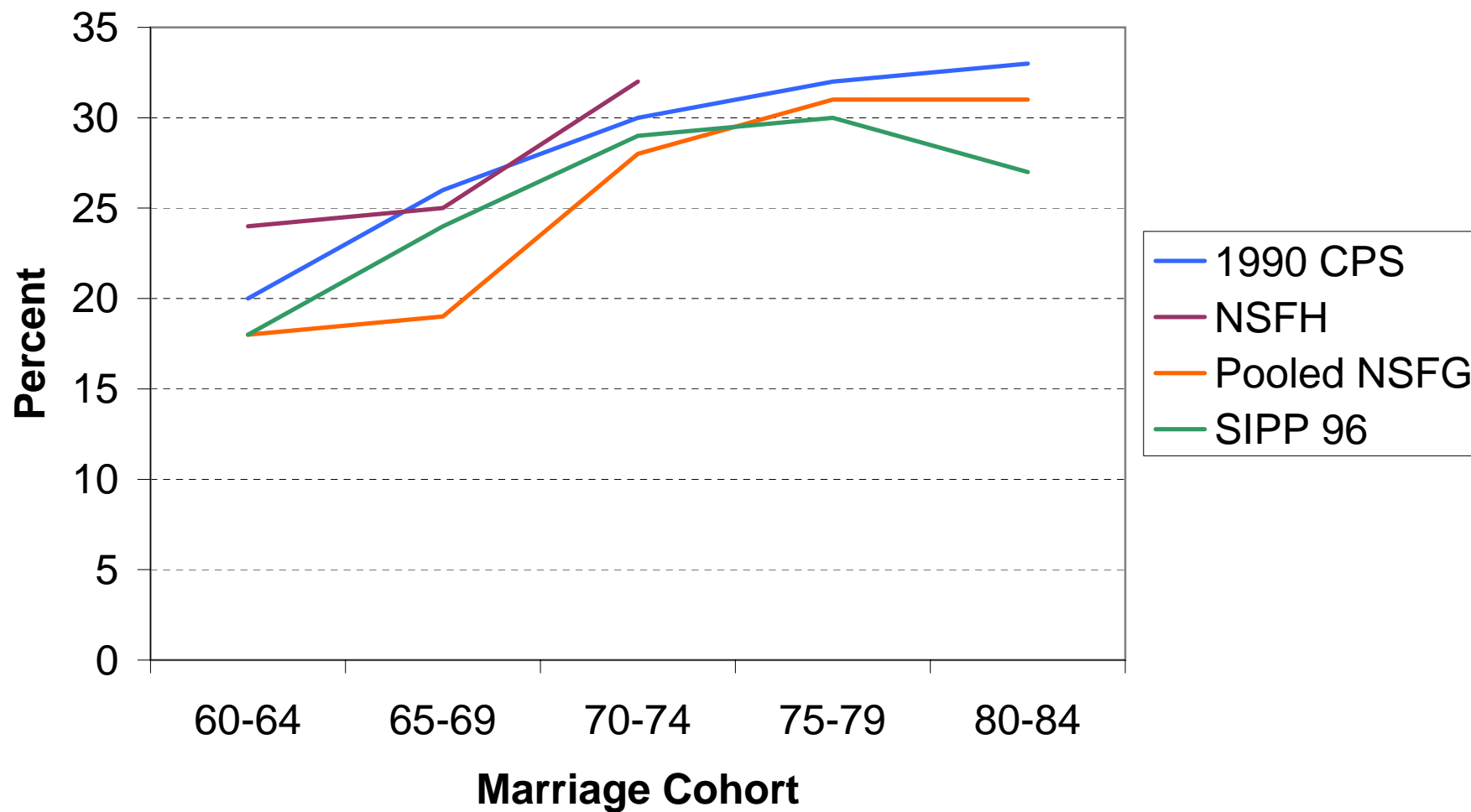
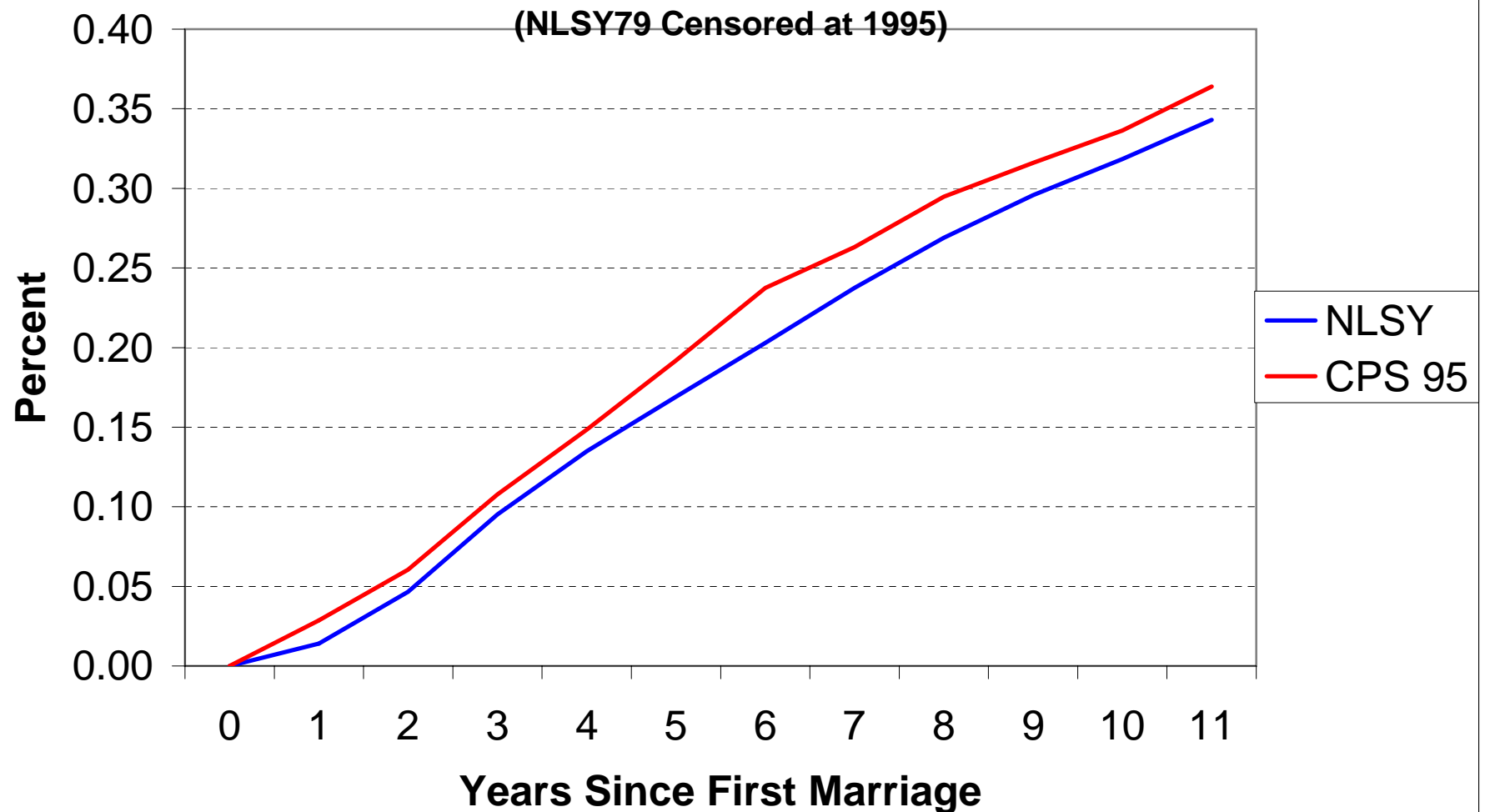


Figure 5. Cumulative Percent Separated, by Years Since First Marriage for the 1957-64 Birth cohort: NLYS79 and 1995 CPS



**Figure 6. Period Estimates for 1975 of the Cumulative
Percent Divorced by Years Since First Marriage:
Vital Statistics and 1980/85 CPS**

