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## Are Divorce Studies Trustworthy? The Effects of Survey Nonresponse and Response Errors

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### Abstract

Researchers rely on relationship data to measure the multifaceted nature of families. This article speaks to relationship data quality by examining the ramifications of different types of error on divorce estimates, models predicting divorce behavior, and models employing divorce as a predictor. Comparing matched survey and divorce certificate information from the 1995 Life Events and Satisfaction Study ( $N = 1,811$ ) showed that nonresponse error is responsible for the majority of the error in divorce data. Misreporting the divorce event was rare, and more than two thirds of respondents provided a divorce date within 6 months of the actual date. Nevertheless, divorce date error attenuated effects of time since divorce on outcomes. Gender, child custody, marital history, and education were associated with divorce error.

### Keywords

*divorce; marriage and close relationships; measurement; panel studies; relationship processes; survey research*

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A large proportion of research on families relies on respondents reporting their relationship history. This information may come from questions about current marital status or whether the respondent has ever been in particular relationship types, or from the completion of full relationship histories. As families have increased in complexity over the past several decades, research on the number and timing of relationship entries and exits has increased substantially (Milardo, 2000), and some suggest that models are still not complex enough, as a result of poor measurement of timing, type, and number of relationships (Hofferth & Casper, 2007). To date, research on the quality of relationship information is limited (Bumpass & Raley, 2007; Knab & McLanahan, 2007; Pollard & Harris, 2007). Understanding the extent that people misreport relationship events and dates is important because data quality determines the likelihood of models being misspecified or temporally incongruent and results biased. This article addresses relationship data quality by examining survey-gathered divorce information.

During the past several decades, social scientists have studied the causes and consequences of divorce considerably, often using survey-collected divorce data (Amato, 2000; Coleman, Ganong, & Fine, 2000). Because of their versatility, survey-collected divorce data are particularly important to scholars, and their significance has only increased with Vital Statistics no longer compiling divorce information (Bramlett & Mosher, 2002). Survey-collected data may be used to estimate the number of divorces, population divorce rates, or hazard rates of divorce, and by adding independent variables to hazard estimates, scholars

can produce models of the predictors of divorce and divorce timing. Analogously, divorce experience and timing are often used as predictors of many dependent variables in studies of remarriage, mental health, economic outcomes, and child well-being—fields that have seen tremendous research attention over the past two decades. In fact, in the 1990s alone, more than 850 articles were published on stepfamilies, which by definition require some measure of divorce experience, although not all used survey-collected data (Coleman et al., 2000).

Previous research also suggests survey-gathered divorce data are sometimes very inaccurate. Depending on the study, scholars have found that the survey estimates of divorce are between 8% and 25% less than the Vital Statistics figures (Bumpass, Castro-Martin, & Sweet, 1991; McCarthy, Pendleton, & Cherlin, 1989; Preston & McDonald, 1979; O'Connell, 2007). Thus, assuming that the Vital Statistics tallies are correct, survey data on divorce are decidedly unreliable and imprecise. Scholars have hypothesized the sources of the discrepancies between the Vital Statistics data and survey data, with some suggesting deliberate misreporting of information by divorced respondents, and others suggesting higher refusal rates for divorced individuals. None of the research, however, has systematically explored sources or explanations for the data problems. And because some sources of error have substantially worse effects on divorce studies, understanding how the different errors contribute to the total amount of error will better enable the field to adjust for this data problem.

One well-documented finding in the literature is that divorce information from men tends to be less accurate than divorce information from women (Bumpass et al., 1991; McCarthy et al., 1989; Preston & McDonald, 1979; U.S. Bureau of the Census, 1975). This knowledge has compelled researchers to distrust male reports of divorce information. Yet possible reporting differences by important variables such as income and education have not been examined and may have substantial ramifications for the study of divorce.

The purpose of this article is to investigate the sources, implications, and correlates of error in survey-gathered divorce data. To carry out my analysis, I capitalize on unique data to compare divorce histories reported in survey interviews to the divorce certificate information of the respondents and evaluate discrepancies between the two sources of data. This work makes two meaningful additions to the literature. First, it identifies and measures the different sources of divorce error and their consequences on models using survey-gathered divorce information. Second, it investigates the extent to which the particular sources of error vary by different groups (e.g., gender, education, income). Doing so fills an exigent gap in the literature about how divorce error is associated with several known correlates of divorce behavior. These two additions to the knowledge will enable researchers to better account for error in studies of divorce. In addition, knowledge of the amount of error on divorce dates may be indicative of error of other union transition dates, such as cohabitation, marriage, and separation.

This article consists of three sections. The first section presents the data used in this study, which facilitates a more intuitive discussion of the sources and consequences of the multiple types of error. The second section examines the correlates of the different types of error. Finally, the last section discusses implications and limitations of this research.

## Method

This study uses data from the Life Events and Satisfaction Study, a study of divorced individuals conducted by researchers at the University of Wisconsin – Madison in 1995. To generate the sampling frame, the staff of the Wisconsin State Registrar of Vital Statistics extracted a list of all divorce decrees issued in 1989 and 1993 in Columbia, Dane, Rock, and

Sauk counties. From the sampling frame, 1,074 divorces for each year were randomly selected ( $\approx 45\%$  sample). Researchers recorded all the information from the 2,148 divorce certificates and then randomly selected one individual from each divorced couple to participate in the survey component of the study. The selected individuals were randomly assigned to one of three modes: mail questionnaire, computer-aided telephone interview (CATI), and computer-aided personal interview (CAPI).

For the people assigned to the mail questionnaire, researchers first sent a personalized advanced letter three days prior to questionnaire mailing. The first mailing included a personalized cover letter and envelope, the questionnaire, a stamped self-addressed envelope, and a promised \$10 honorarium. Researchers then sent a follow-up postcard 3 days after the first mailing. The second mailing, 21 days after the first mailing, contained the same package as the first mailing with the addition of a modified cover letter. Twenty-one days after the second mailing, the third mailing used a new personalized letter, a preprinted envelope (with teaser copy notifying the respondent that a check, for \$1, was inside) in addition to a package similar to the first mailing. Three weeks after the final mailing, telephone interviewers telephoned all nonrespondents and refusals at least 6 times at different times of day. Researchers also used an additional conversion incentive (\$5) during the call.

For the people assigned to CATI and CAPI, researchers sent a personalized advanced letter 3 days prior to start of interviews. Interviewers attempted to contact respondents at least 6 times, at different times of the day. Interviewers promised a \$10 honorarium for responding. Participants assigned to the personal interview mode who lived outside the state were moved into the telephone interview group. Three weeks after a respondent refused to answer the survey, a trained refusal conversion interviewer offered an additional conversion incentive (\$5). Fourteen days after the telephone refusal and nonresponse phase finished, researchers sent a special mailing (similar to the final mailing of the mail questionnaire group) to nonrespondents and refusals.

Because the divorces occurred from 2 to 6 years prior to the intended survey collection date, researchers used several tracking methods to locate the addresses for 1,836 individuals (85% of the sample). Because this study is a follow-up of administrative records, the 312 study participants who were never located are lost to attrition and thus not within the scope of this study. An important assumption—that is supported by assessments not shown—is that the people lost to tracking problems are not significantly different from the analysis sample. In addition, 25 people were removed from the study because they were affiliated with the University of Wisconsin, too ill to complete the survey, or deceased, thus leaving 1,811 people in the analytic sample. To avoid contamination, special care was taken throughout the data collection process to avoid divulging that the study focused on divorce.

The divorce date on each certificate was recorded by the county clerk. I expect the error of this date—to the month and year—to be trivial or nonexistent. And although no data source is perfect, the legal ramifications of this date make it a reasonable choice as the comparison standard. Respondent-reported divorce information was taken from questions asked during the marital history section of the survey (the third section of the survey) and was identical to the marital history section of the National Survey of Families and Households. Respondents were asked screening questions about if (and how many times) they had ever been married, followed by questions about the month and year that the couple married and started living together (if prior to the marriage date). The survey then asked whether respondents were still married to that person, how it ended, the date they stopped living together, and finally the date of divorce (if applicable). This information was asked for all the marriages indicated by the respondent. Question wording and order was identical across modes.

I combine the certificate, survey, and interviewer data to classify respondents by the type of survey error they commit. As Figure 1 indicates, gathering survey data is a stepwise process. Therefore, I estimate the probability of not completing one step, conditional on completing the previous step. That is, I estimate six probabilities: (a) the probability of not being contacted; (b) the probability of not responding to the survey, conditional on being contacted; (c) the probability of not responding to the marital history questions, conditional on participating in the survey; (d) the probability of not reporting whether the divorce occurred, conditional on responding to the marital history questions; (e) the probability of not reporting the correct divorce date to the month, conditional on reporting the divorce occurred; and (f) the probability of reporting the divorce date too close to the current date (i.e., telescoping forward), conditional on misreporting the divorce date. I used these probabilities to examine the major sources of error for divorce reporting described herein.

## Results

Correct inference of population characteristics from sample characteristics requires that two conditions be met: the responses given in the survey must accurately represent the characteristics and experiences of the respondent, and the respondents in the survey must have similar characteristics to the population (Groves et al., 2004). Any violation of these two conditions produces error in a survey statistic. In the case of the first condition, response error occurs when a respondent provides information that incorrectly represents the actual respondent attribute. A violation of the second condition produces nonresponse error, which refers to the several possible ways information from selected individuals is not included in the survey data. When either of these two errors is nonrandom, inferential statements are unreliable.

There are several sources of both nonresponse and response error that I describe here and that Figure 1 illustrates. By understanding the major sources of error for divorce, researchers can more properly manage error in divorce studies. In addition, each source of error may have unique predictors and different influences on estimated statistics and models of divorce. The following paragraphs outline the different locations of error nested within nonresponse and response error.

### Nonresponse Error

Nonresponse error is a missing data problem that typically changes the count of a particular characteristic compared to what the count would be if there were no missing data (Groves & Couper, 1998; Little & Rubin, 2002). I differentiate between three types of nonresponse error. First, sampled individuals who are not contacted to participate in the study generate noncontact error. Second, unit nonresponse error arises when contacted individuals do not respond to the survey. Third, respondents who fail to answer questions produce item nonresponse error for measures generated from those questions. Negative consequences of nonresponse error occur when the number of nonresponders is large compared to the sample and when the nonresponder and responder groups differ substantially on important characteristics (Lin & Schaeffer, 1995). As a result of data limitations, I do not analyze coverage error, a fourth source of nonresponse error that occurs when the sampling frame does not accurately portray the population, but I address it in the discussion section.

Of the 1,811 potential respondents from the analytic sample, the 229 people who were never contacted are coded as having noncontact error. Thus, the probability of contributing noncontact error is 0.13. There is a similar probability for unit nonresponse error of 0.14, because I categorize as unit nonresponse error, the 224 people who were contacted but refused to participate in the survey. The 22 survey respondents who failed to answer the marital history questions are coded as having item nonresponse error. The low item

nonresponse error (probability of .016) shows that most people, conditional on responding, will answer the marital history questions.

Although the noncontact and unit nonresponse errors are nontrivially large, of particular importance is how these sources of nonresponse error are different for the divorced and comparison group populations (typically the continuously nondivorced). If nonresponse error is not similar between divorced and nondivorced populations, the relationship between the divorced and nondivorced counts is distorted or biased. Nevertheless, if the nondivorced and divorced populations have similar amounts of nonresponse error, there would be no bias despite possibly high levels of error. Unfortunately, exact estimates of the differences in nonresponse error by marital history are extremely rare, but some limited evidence suggests that divorced people have higher nonresponse error than their nondivorced counterparts (Groves & Couper, 1998; O'Connell, Gooding, & Ericson, 2007; Tolonen et al., 2006). Assuming the literature on divorced and nondivorced nonresponse error rates can be applied here, a divorce rate calculated using these survey data (and a count of the married population) would be underestimated by about 10 – 11%. Even so, using weighting procedures to account for the nonresponse of divorced people would reduce this bias (Groves et al., 2004; Lin & Schaeffer, 1995). Also, if divorce experience is used as a predictor or even an outcome, the only additional difficulty to a reduction in power would be if there were key differences between the responding and nonresponding divorcees.

### Response Error

Respondents produce response error when their stated attribute does not match the attribute that the researcher intends to measure. I divide response error of divorce reporting into two distinct types: not reporting a divorce occurred and, if the divorce is reported, misreporting the divorce date. The first type occurs when a divorced person fails to report a divorce or a nondivorced person mistakenly reports a divorce (the latter of which is virtually nonexistent) (McCarthy et al., 1989). The second type occurs when a divorced respondent acknowledges the divorce but provides an incorrect divorce date. These two types of response errors have unique consequences on the study of divorce and are examined separately here.

**Divorce event**—Error caused by failing to report a divorce is actually more sinister than nonresponse error, because in addition to reducing the number of divorces by one (as with nonresponse error), the respondent has also increased another response category (e.g., married or single) to a higher count than actually existed. When predicting divorce behavior, if some divorced respondents report not being divorced, then instead of comparing divorced and not divorced respondents, the researcher is in fact comparing divorced respondents to a mix of divorced and nondivorced respondents. Therefore, if this type of error is large, many of the studies of divorce, and particularly studies exclusively following divorced samples, may be significantly crippled.

If the estimate of response error in the divorce event (see Figure 1) is indicative of other studies, it appears that the possible deleterious effects of misreporting the divorce are low. Of those who responded to the marital history, 48 respondents failed to acknowledge the divorce, for an error probability of 0.036. Approximately one quarter ( $n = 13$ ) of that group appear to have reconciled with their previous spouse and do not report that the divorce ever took place. Another quarter ( $n = 14$ ) reported being divorced but failed to report the divorce we know of (i.e., they had been divorced more than once). The remaining 21 misreporters evenly reported being never married, currently married, or widowed. Although the misreport is low in number, the ramifications of the misreport of divorce may be consequential if there are key characteristic differences between divorce reporters and misreporters.

**Divorce date**—Misreporting the divorce date also generates two errors. First, respondents not only reduce the number of divorces in one period but also necessarily add an extra divorce that did not occur during another time period. Depending on the amount and direction of the divorce date misreport, divorce estimates for a particular time period may be under- or overestimated. On the microlevel, misreporting the divorce date leads to measurement error in timing measures (e.g., time since divorce)—which may attenuate effect sizes (Groves et al., 2004). Second, date misreporting may also affect causal research by disturbing the proper temporal order of events (Singer & Willett, 2003). Thus, if divorce date information is used as either an outcome (e.g., divorce timing) or a predictor of another outcome (e.g., financial or emotional adjustments to divorce), and the divorce date is misreported, the temporal ordering of the model may be incorrect. Of course, if the amount of error is relatively small, with either a small number of respondents providing incorrect dates or only a few months of error, the causal ordering would most likely still be correct, on average.

If a respondent reported that the divorce occurred, I subtracted the divorce date (in months) reported in the survey from the divorce date on the certificate. Thus, the 647 people with no difference in the two dates have no error (to the month) resulting from misreporting the divorce date (error probability = 0.498). The remaining 641 respondents who provided inaccurate dates are coded as providing misreporting error. Of the 1,187 respondents who provided dates, approximately 90% of the dates were within 1 year of the correct date, and 68% were within 6 months of the correct divorce date. Of the respondents who misreported their divorce date, 66% reported their divorce date further in the past (i.e., telescoping backward) than the certificate divorce date.

The fact that 90% of the divorce dates are within a year of the certificate divorce date has important implications. First, divorce date misreporting likely has little influence on estimates of divorce rate. Second, as long as outcomes of interest are over a year after the divorce (or are similarly misreported) there should be no major timing problems in causal analyses. Nevertheless, because about 66% of the respondents backward telescoped their divorce date, any variables that are timed closely to the divorce date would reverse the temporal ordering when predicting divorce, assuming that the other variables' timings are correct.

Even if the misreporting of the divorce date does not negatively affect temporal ordering, it does produce measurement error that may attenuate effects. As an illustration, I regressed 13 well-being measures (e.g., subjective well-being measures in several areas: overall, mental health, financial, physical, interpersonal relationships) and the probability of being in a relationship or being married by the interview date on time since divorce (in months) using both the certificate and survey-reported divorce dates. I controlled for educational attainment, gender, age, and survey mode. These models are intended to be not substantively important but instructive of the possible effects of divorce date measurement error.

In analyzing well-being, the measure of time since divorce using the certificate divorce date (CDD) had strong effects on well-being (often of comparable size to education's effect). In contrast, with use of the time since divorce based on the survey divorce date (SDD), the results were always in the same direction but often 25% – 33% smaller in effect size—implying an attenuation of the effect of time since divorce on well-being. This attenuation sometimes produced nonsignificant effects for SDD that were significant for CDD. Similarly, CDD time since divorce had a strong effect on the probability of repartnering and remarriage, but the effects were diminished by 30% – 40% using SDD—a significant decrease in effect size, although still in the same direction.



If one expects that the SDD measure was the more realistic date to evaluate emotional and relationship outcomes, then the effect of time since divorce using the SDD should have a stronger relationship with the outcomes of interest compared to the CDD. In fact, however, the SDD measure has a consistently weaker and sometimes insignificant effect on the outcomes. Thus, the finding suggests that, because of the greater measurement error in the SDD measure, the effect of time since divorce is attenuated. It also implies that the legal divorce date may be a good indicator of the end of the relationship for some outcomes because of its formal finality.

### Predictors of Error

The second contribution of this article is to examine how the probability of producing the different errors varies by important covariates of divorce behavior. A major concern is that the people who commit the error and those who do not commit the error are different in important ways. Accordingly, the second phase of the analysis takes the conditional probabilities discussed previously and uses independent variables to estimate models of the error in divorce reporting. Using the conditional probabilities allows the independent variables to have unique effects for each type of error. Although the initial plan of analysis would call for a model of each of the six conditional probabilities (i.e., noncontact, unit and item nonresponse, not reporting the divorce, misreporting the date, and telescoping error), because so few respondents produced item nonresponse error, a model predicting that type of error was not estimated. Telescoping error appeared to be independent of all measures used here and is also not shown.

To account for the changing conditional probabilities, I used a sequential logistic model. As Powers and Xie (2000) explain, this model works with conditional probabilities such that  $y_i$  = failing to complete the current step given that they have already completed the previous steps. These are interpreted like standard logistic regressions, which are interconnected because each model is based on the reduced sample of people who completed the previous step. Running separate models for each step in the process of reporting allows for different mechanisms to work at each step. In this sequential logistic model, there are five possible sequential regression models, which corresponds to the five conditional probabilities just mentioned. The outcomes are coded so that if individuals committed the error (conditional on not committing the past error) they were coded as 1, whereas all those who not commit that error are coded as 0. Thus, a positive regression coefficient is interpreted as positively associated with that type of error.

To accomplish the goal of understanding how the different errors just described vary by different subgroups, this article uses several independent variables (see Table 1). These indicators are related to divorce, and many are also associated with survey error (Amato, 2000; Groves & Couper, 1998). This is important because when a measure is correlated with both an outcome of interest and its error, it is likely to bias the results. Although I am limited to the variables available from the survey and the divorce certificate, I attempt to provide a broad range of predictors.

Like the divorce date, the court clerk recorded the custodial assignments, and because of the legal implications of the custodial arrangements, I assumed this information to be highly accurate. Because the unidentified person filing for the divorce petition provided other information about both spouses, some personal information is subject to surveylike data collection error and therefore may be inaccurate. This may be especially true for information on the nonfiling spouse. Yet, despite their limitations, the data provide a unique opportunity to compare survey-reported divorce behavior to the same legal documents that produced the vital statistics.

Many indicators come from the divorce certificate, including the respondent's gender, the respondent's age, time since divorce (2 years vs. 6 years), marital duration at divorce, and whether the respondent had been previously divorced. Another important indicator from the divorce certificate was the presence of children. Information on the divorce certificate indicated that approximately 60% of the original sample had a child living in the home on the day of the separation. Around 46% of the sample received joint or sole custody of a child. As expected, this differed tremendously by gender. That is, 59% of women received custody, but only 32% of men received custody of a child.

Additional independent variables used to predict the different types of error come from answers on the survey. Unlike the previously discussed independent variables that had no missing data, at most, 1,358 respondents have data reported in the survey. Nevertheless, many of the variables are also very important to consider as predictors of error. Educational attainment is coded to approximate years of education as follows: 10 = *did not finish high school*, 12 = *graduated from high school or obtained a general education development (GED) certificate*, 14 = *received a vocational/technical or associate's degree*, 16 = *received a bachelors degree*, and 19 = *received a graduate degree*. Less parsimonious parameterizations of education yielded similar results. Income was reported as "total annual income for your family or household" and measured in thousands of dollars. For the regressions, the natural logarithm of income was used. I included the current marital status of the respondent (divorced, married, or cohabiting). I also included the mode of interview the respondent used to provide the survey data (mail, 47%; CATI, 29%; and CAPI, 25%). There are a greater number of responses by mail because respondents who did not reply to the CATI or CAPI after several contact attempts were mailed the questionnaire. Survey mode was included only in the response error models because I could not distinguish between no contact and unit nonresponse for those assigned to the mail survey.

Although there are several significant results in Table 2, because most of the results parallel the survey methods literature, we would not expect to them have any particularly unique effect on studies of divorce (Dykema & Schaeffer, 2000; Groves & Couper, 1998). Therefore, I limit my discussion to a few key results for divorce. For example, the gender coefficient estimate for all of the models is in line with previous research, where women were much less likely to contribute error than men during the divorce survey research process (Auriat, 1993; Bumpass et al., 1991; Groves & Couper, 1998; McCarthy et al., 1989; O'Connell, 2007; Preston & McDonald, 1979; U.S. Bureau of the Census, 1975). These results show that women's overall higher consistency with vital statistics results from men's lower response rates and worse memory on family dates, resulting in men being substantially less reliable respondents than women in divorce studies. Also, although there were response rate differences between modes, no other differences were significant, which implies that all three survey modes are similar in the production of divorce response error.

Respondents who had already had at least one previous divorce, in general, provide more error than those respondents for whom this was their first divorce. Compared to people for whom this was a first divorce, those people who had experienced at least one previous divorce have higher odds of not being contacted and misreporting the divorce. Previously divorced people appear to be particularly difficult to reach, which may be a result of being more like single persons in their at-home patterns (Groves & Couper, 1998). The lack of accuracy in their reports may be a result of greater interference as a result of multiple important events (Tourangeau, Rips, & Rasinski, 2000). Essentially too many important events, like marriage and divorce, make each event a little less salient and thus less likely to be remembered accurately.



Research on memory suggests that the longer the marriage, the more memorable is the divorce (Auriat, 1993; Brewer, 2000; Cannell & Henson, 1974; Fault & Herzog, 1995; Smith & Thomas, 2003; Sudman, Bradburn, & Schwarz, 1996). Although marriage duration shows no statistical relationship with the accuracy of the report, respondents with longer marriages were less likely to not report the divorce. Thus, even though the marital duration may not improve the measurement of the divorce date, it may be that the longer people are married, the greater the change it is in their lives to get divorced, thus improving the overall quality of the report.

In general, people who had children at the time of the divorce, regardless of the custodial arrangement, have lower odds of generating any kind of error during the survey process. In part, this may be attributable to a continuing relationship with the ex-spouse to see and/or jointly raise the children. This also requires that both partners remain stably located, thus decreasing nonresponse error. The reduction in divorce date error (Model 4) suggests that having children involved in the divorce increases the salience of the divorce.

The final variable of note in Table 2 is the respondent's current marital status. Compared to divorced respondents, those respondents in a union (i.e., marriage or cohabitation) have significantly higher odds of not reporting the divorce and misreporting the divorce date. The result that those who are currently in a relationship tend to be more likely to misreport their divorce date is, at best, another example of interference, in which multiple important dates are diminishing recall ability (Tourangeau et al., 2000). At worst, the result that currently married individuals are significantly more likely to not report the divorce may be evidence of deliberate misreporting of the past, possibly to hide information from the current spouse.

## Discussion

The majority of the error in survey gathered divorce information is a result of nonresponse errors, in particular noncontact and unit nonresponse. Therefore, researchers should compare their distributions of marital statuses to expected distributions and weight accordingly. Doing so should account for the divorce under-count attributable to nonresponse. The previously divorced and more recently divorced are more likely to commit nonresponse error. This suggests that studies of divorced individuals may have too few recent divorcees and multiple divorcees. If this result is also true of less formal relationship transitions such as cohabitation, researchers may be underestimating the number of family transitions occurring in the population. Further research into the nonresponse of multiple transition respondents is warranted but could be accounted for with weighting if the actual population distribution of transition was known (something far more difficult to know than marital status).

Few people fail to answer marital histories upon responding to the survey. Also, only a low percentage of people misreport the divorce event, which suggests that this very damaging error has little effect on studies of divorce. Even so, people who had shorter marriages or had already remarried have higher odds of not reporting the divorce. Thus, studies using divorce information may have too few respondents who remarried or had short marriages, and all of those respondents are reporting to be in another marital classification. Thus, researchers should be cognizant that this group is probably smaller than it should be. Also, if relationship history is essential to the study, researchers may want to consider improving the privacy of the respondent during that section to avoid possible bias if the respondent is avoiding reporting a relationship unknown to the current partner.

The vast majority of people report their divorce date within a year of the correct date, and about half are correct to the month—suggesting little bias on divorce rate estimates or for

temporal ordering. On average, divorced individuals backdate their divorce by about 3 months. Nevertheless, the measurement error caused by the misreporting the divorce date appears to attenuate the effects of time since divorce on some outcomes. More educated respondents, women, and respondents who shared legal custody of a child are more likely to provide the correct date, whereas those who had more than one divorce or had already repartnered have greater divorce date error.

This study's design limits its completeness because I cannot examine coverage error using the data. Coverage error, another type of nonresponse error, occurs when the target population and the sampling frame population are not identical. In this study, a systematic coverage error would occur only if a group of people were held out of the initial sample frame (i.e., the divorce decree lists), and there is no indication that this occurred. Because most studies have different sampling frames and different purposes, generalizing to every form of coverage error is very difficult. Thus, I argue that coverage error warrants its own careful examination. A good study of coverage error would need to examine differences in multiple types of sampling frames (e.g., block listing, telephone directories, listings), but this study used only administrative records.

In addition, limiting the study to only persons known to have experienced a divorce prevents the evaluation of errors of response and nonresponse among the continuously nondivorced population. Evidence suggests that divorced people have higher nonresponse rates than their married counterparts—if not higher than the entire nondivorced population (Groves & Couper, 1998; O'Connell et al., 2007; Tolonen et al., 2006). Future research could address this gap by using a similar follow-up design as this study to examine the nonresponse and response error rates for several marital groups, such as never married, currently married, divorced, never divorced, cohabiting, and so on. This information would aid researchers in properly weighting their samples to account for the differential error by marital history.

There are two major problems concerning divorce date that should be noted. First, although researchers attempted to distinguish the divorce date from the separation date by asking for both, we cannot be sure that some respondents did not report the separation date. In this study, only 25% of respondents had a separation date recorded on both divorce record and survey gathered marital history. Considering both dates are subject to recall error, it is interesting to note the distribution of the difference in the separation dates is almost identical to the distribution of the difference in the divorce dates. This suggests that the recall error is similar, and people may just be reporting all dates associated with the termination of the union similarly. It also suggests that the use of the separation date may have similar effects as the divorce date.

Second, it is not clear that researchers want the divorce date as their indicator of union dissolution depending on their outcome of interest. Certainly, divorce date may be of interest because of the legal constraints placed on an individual both before and after that specific date, but other dates, such as the separation date, may be useful in terms of emotional adjustments and even residential location. In contrast, relationships often suffer from multiple separation dates—which may make this date more difficult to recall. Further, I would argue that researchers really want a date by which their outcome of interest begins to be affected but are forced to use proxies of those dates. That is, for many outcomes, from remarriage to emotional well-being to personal finances, the date can be difficult to pin down and may be rather subjective. Further work in this area is needed—most likely qualitative work asking about when emotional, relationship, physical, and financial separation began (and some of those dates may occur before the date of separation). Also, greater attention to the theoretical connection between separation event and outcome is needed.

Using a sample of four Wisconsin counties diminishes the generalizability of this study. Of particular note is that, compared to the rest of the country, the counties are more highly educated, more rural, and primarily White. Although those differences may change the amount of error, there is no indication that the characteristics interact with nonresponse and response error. Wisconsin was one of the first so-called no-fault divorce states but instituted unilateral divorce later than most of the country in 1978 (Gruber, 2004). All of the divorces and almost all of the marriages took place under unilateral no-fault laws that have been prominent throughout the country since the mid-1980s. Also, despite the late acceptance of unilateral divorce laws, the spike in divorces due to those changes would have dissipated prior to the first set of divorces in our sample (Wolfers, 2006). Wisconsin is typically in the lowest 20% of states for the divorce rate, although this is often accounted for by racial, socioeconomic, and education differences (Brinig & Allen, 2000).

Researchers used fairly standard protocols and even used three survey modes to improve generalizability to multiple study types. Despite the different modes, there is little evidence that recall was different—which suggests that researchers can be more flexible with their collection strategy. Even so, specific study protocol may influence nonresponse and response rates, and therefore more work is needed to verify these results across multiple survey types. Of particular note is that this study used only one version of the marital history questions: It asked about the sequence of relationships, including dates of meeting, living together, marriage, separation, and divorce. Most surveys rely on the idea that bounded questions provide more accurate dates than unbounded questions. Taken to another level, the Life History Calendar attempts to use dates from several facets: fertility, employment, education, natural disasters, and politics in a respondent driven exercise to fill in and change dates to achieve the most accurate dates (Axinn, Pearce, & Ghimire, 1999; Freedman, Thornton, Camburn, Alwin, & Young-DeMarco, 1988). The increasing complexity of family transitions may require greater use of bounding and increased integration with other date collection modules. The cost and benefits of different levels of bounding has not been examined for relationship history data collection and would be useful for future studies.

Despite these limitations, this article is the first to systematically address several different sources of error for divorce data and to provide evidence for how those sources may be different for important covariates of divorce, such as income, gender, education, and marital history. To the extent that this knowledge is generalizable to other relationship data, such as that from dating, cohabitation, and marriage, we might expect that nonresponse would account for most differences in missing data and that not reporting cohabiting or being married would be low. The timing of entry into and out of the relationships, however, may not be as accurate as many would like. In fact, evidence shows that the dates in cohabitation may be more problematic because the beginning (and possibly the end as well) of cohabitation is more gradual and less formal (Knab & McLanahan, 2007; Manning & Smock, 2005; Pollard & Harris, 2007). A more focused research agenda on measuring timing of relationship entry and exit would be timely, considering the increased reliance on these data in studies of family change.

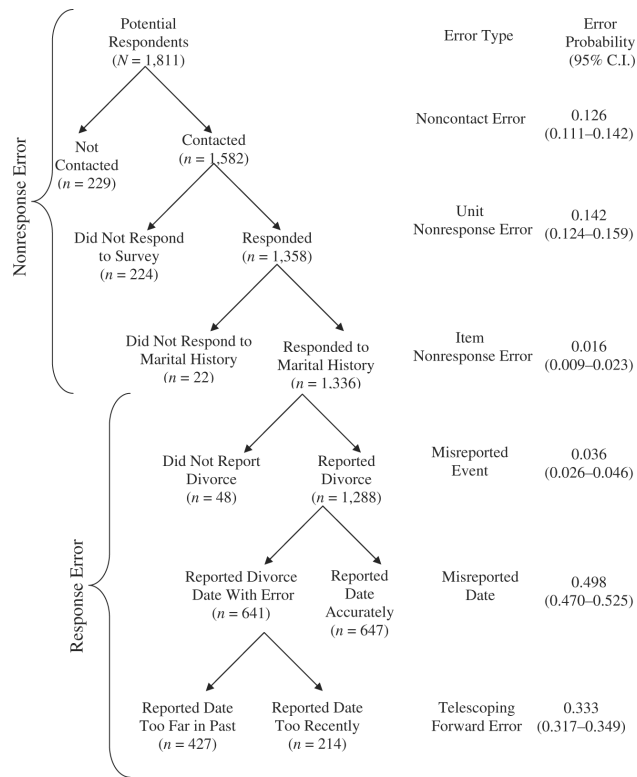
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**Figure 1.** Sequential Process of Divorced Respondents Reporting in a Divorce Study.



**Table 1**  
Descriptive Statistics for the 1995 Life Events and Satisfaction Study Sample and Subsamples of Divorced Men and Women

Variable	Total Sample			Women		Men	
	M	SD	Range	M	SD	M	SD
<i>Data From 1989/1993 Divorce Certificates (N = 1,811)</i>							
Years since divorce <sup>a</sup>	0.49	0.50	0-1	0.49	0.50	0.49	0.50
Age at divorce	40.2	8.97	21-74	39.1	8.57	41.3	9.22
Previously divorced	0.19	0.39	0-1	0.19	0.39	0.18	0.39
Marital duration (years)	11.2	8.2	0-52	11.2	8.01	11.2	8.40
Child/child custody							
No child in home at separation	0.39	0.38	0-1	0.38	0.38	0.40	0.39
Has child, but no custody	0.16	0.36	0-1	0.05	0.22	0.26	0.44
Shares custody of child	0.28	0.44	0-1	0.27	0.44	0.28	0.44
R received sole custody of child	0.18	0.38	0-1	0.31	0.46	0.05	0.21
<i>Data From the 1995 Life Events and Satisfaction Survey (N = 1,296)</i>							
Income (in thousands) <sup>b</sup>	41.6	26.3	1-300	37.5	24.5	46.2	27.4
R's educational attainment <sup>c</sup>	13.7	2.22	10-19	13.6	2.17	13.7	2.27
Current marital status							
Divorced	0.44	0.46	0-1	0.47	0.46	0.41	0.45
Married	0.34	0.47	0-1	0.32	0.47	0.37	0.48
Cohabiting	0.22	0.41	0-1	0.22	0.41	0.22	0.41

<sup>a</sup>Years since divorce: 0 = 2 years, 1 = 6 years (years used in analyses).

<sup>b</sup>Income is "total annual income for your family" in thousands (ln (income in thousands) used in analyses).

<sup>c</sup>Educational attainment: 10 = did not finish high school, 12 = graduated from high school or obtained a GED, 14 = received a vocational/technical or associate's degree, 16 = received a bachelor's degree, 19 = received a graduate degree.

**Table 2**  
Sequential Logistic Regression of Divorce Nonresponse and Response Error

Measure	(1) Noncontact Error			(2) Unit Nonresponse Error			(3) Did Not Report Divorce			(4) Misreported Divorce Date		
	<i>B</i>	<i>SE B</i>	<i>e<sup>B</sup></i>	<i>B</i>	<i>SE B</i>	<i>e<sup>B</sup></i>	<i>B</i>	<i>SE B</i>	<i>e<sup>B</sup></i>	<i>B</i>	<i>SE B</i>	<i>e<sup>B</sup></i>
Female	-0.140	0.164	0.87	-0.514**	0.163	0.60	-0.561	0.360	0.57	-0.716***	0.137	0.49
Years since divorce	-0.068	0.153	0.93	-0.390*	0.156	0.68	-0.047	0.087	0.95	0.083*	0.033	1.09
Age (at time of divorce)	-0.043**	0.016	0.96	0.019	0.015	1.02	0.058*	0.028	1.06	0.009	0.014	1.01
Previously divorced	0.620**	0.216	1.86	-0.216	0.245	0.81	0.424	0.453	1.53	0.766***	0.204	2.15
Marriage duration (years)	0.006	0.017	1.01	-0.016	0.015	0.98	-0.122***	0.036	0.89	-0.025	0.014	0.98
<i>Child/child custody</i>												
No child in home at separation	—	—	—	—	—	—	—	—	—	—	—	—
Has child, but no custody	0.054	0.208	1.06	-0.434	0.230	0.65	-0.682	0.557	0.51	0.057	0.195	1.06
Shares custody of child	-0.576**	0.199	0.56	-0.425*	0.187	0.65	-0.556	0.473	0.57	-0.316*	0.152	0.73
R received sole custody	-0.254	0.218	0.78	-0.315	0.235	0.73	0.103	0.466	1.11	-0.001	0.178	1.00
<i>Survey mode</i>												
Mail	—	—	—	—	—	—	—	—	—	—	—	—
CATI	—	—	—	—	—	—	0.352	0.358	1.42	-0.046	0.141	0.96
CAPI	—	—	—	—	—	—	0.295	0.450	1.35	-0.185	0.150	0.83
In (Income in thousands)	Measures only available for those who responded to the survey											
R's educational attainment							-0.640	0.358	0.53	-0.191	0.101	0.83
<i>Current marital status</i>												
Divorced							0.075	0.074	1.08	-0.099***	0.029	0.91
Married							1.715***	0.411	5.56	0.396*	0.157	1.48
Cohabiting							0.217	0.575	1.24	0.304*	0.152	1.36
<i>n</i>		1,811			1,582			1,296			1,250	
$\chi^2$		37.364			29.977			77.427			128.024	

Note: *e<sup>B</sup>* = exponentiated *B*.

\* *p* ≤ .05.

\*\*\*  
 $p \leq .01$ .  
\*\*\*  
 $p \leq .001$  (two tailed).