CONSISTENCY OF RETROSPECTIVE COHABITATION AND MARITAL HISTORIES

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Revised 9-18-09

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ABSTRACT

Retrospective data is one of the main sources of information on non-marital cohabitation, but the quality of these data has not been thoroughly evaluated. In this paper, I investigate the consistency of retrospective data on cohabitation and marriage using the 2000, 2003, and 2005 General Household Surveys (GHS), cross-sectional, probability samples of the British adults aged 16-59. I evaluate the extent of recall error by comparing rates of entry into cohabitation and marriage for the same age and period in each dataset. I examine differentials in recall error by socio-demographic factors and characteristics of the union such as duration. Finally, I investigate the *implications* of any recall error for substantive conclusions about the association between pre-marital cohabitation and subsequent marital disruption. Preliminary results show that cohabitations in the teenage and young adult years, as well as an individual's first cohabitation are more likely to be underreported.

One of the most notable recent changes in family life is the rise of non-marital cohabitation (Bumpass and Lu 2000, Haskey 2001, Murphy 2000). [1] In addition to restructuring the process of forming intimate unions, cohabitation affects the economic and psychological well-being of individuals, their relationships, and their children (see reviews by Seltzer 2000 and Smock 2000). For example, a number of studies find a positive association between cohabitation and subsequent marital instability, leading some to conclude that cohabitation is weakening further the institution of marriage and others to conclude that (Brines and Joyner, 1999; South, Trent, and Shen, 2001; Steele et al., 2005). Early research on cohabitation used cross-sectional data from Censuses (Casper and Cohen 2000) or sample surveys (Glick and Spanier 1980).

Subsequent research has exploited the advantages of retrospective data on of cohabitation: retrospective data provide an individual's entire history of relationships without the time or financial burden required to collect data prospectively (Freedman et al. 1988). A potential flaw of retrospective data, however, is that respondents may not recall the occurrence or timing of their unions (Hayford and Morgan 2008, Murphy 2000, Wu, Martin, and Long 2001). Recall errors may bias estimates of the levels and correlates of cohabitation, making evaluating the quality of retrospective data crucial. A handful of studies assess the quality of quantitative retrospective cohabitation data (Baughman, Dickert-Conlin, and Houser 2002, Brown and Manning 2009, Hayford and Morgan 2008, Murphy 2000, Teitler, Reichman, and Koball 2006), but a number of important issues have not been examined.

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[1] For brevity, I refer to non-marital cohabitation as "cohabitation."

This project examines the consistency of retrospective histories of cohabitation and marriage in the General Household Surveys (GHS). The present version of this paper uses data from the 2000 and 2005 GHS; future versions of this paper will include data from the 2003 GHS. The GHS data are taken from cross-sectional surveys of the British population and contain retrospective cohabitation and marital histories of women and men aged 16-59.

I pursue three goals in this paper. First, I evaluate the extent of recall error in reports of cohabitation and marriage. I do so by examining the consistency of rates of entry into cohabitation and marriage in a comparable age-period group from the 2000 and 2005 GHS. Second, I investigate variation in recall error by an individual's gender, education, previous union experience. I also examine associations between recall error and several characteristics of the cohabiting union, including duration, age of entry, union order, whether the cohabitation transitioned to marriage, and whether a child was born in the union. Third, I examine the implications of recall error. Statistically significant findings of recall error do not necessarily lead to substantive different conclusions about the levels or correlates of cohabitation (Swicegood, Morgan, and Rindfuss 1984). In this paper, I examine how recall error may bias estimates of the association between cohabitation and subsequent marital stability.

This paper is organized as follows. In the next section, I draw from the cognitive psychology and survey methodology literatures to formulate hypotheses about errors in retrospective cohabitation and marital histories. Next, I describe a study design used to assess the consistency of retrospective data on cohabitation and marriage (e.g., Bumpass 1984, Hayford and Morgan 2008, Swicegood, Morgan, and Rindfuss 1984). I conclude the background section by describing the contributions of this paper. The following sections describe the methods, preliminary results, and future work.

BACKGROUND

Supplying autobiographical histories in a survey interview is a cognitively demanding task.

Respondents are required to enumerate and retrieve details of events, integrate events into a

logical sequence, and answer in the form requested (Krosnick 1999, Sudman et al. 1996,

Tourangeau 1984). The cognitive demands of this process are magnified by the oftentimes long

recall period, high level of detail requested, and lack of incentives for respondents (Krosnick

1999, Schaeffer and Presser 2003). In light of this burden, respondents rarely directly retrieve

answers to autobiographical questions but instead *construct* answers using several pieces of

information, sometimes relating the event to another personal experience or external reference

point (Blair and Burton 1987, Bradburn, Rips, and Shevell 1987, Freedman et al. 1988). Recall

errors may occur if the respondent never encoded the information into memory, fails to retrieve

the appropriate information, or declines to spend the cognitive effort required to search fully her

memory (Bradburn, Rips, and Shevell 1987, Krosnick 1991).

Research from the cognitive psychology and survey methodology literatures identifies a number

of factors that contribute to recall error in autobiographical memory questions. In this paper, I

focus on three factors: recall duration, event characteristics, and respondent characteristics.[2] In

this section, I briefly review literature on these three topics and formulate hypotheses to be

tested.

Recall Duration and Recall Error

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[2] Other factors affect response errors, most notably the characteristics of the interview such as mode, context and
wording of questions, and quality of interviewer. The data I use, however, only contain variation in the interview
mode, but not other characteristics of the interview.

Memories about the occurrence or details of an event deteriorate over time, making retrieval of memories in the distant past more difficult (Ebbinghaus 1964, Sudman, Bradburn, and Schwarz 1996). Studies find that a long duration between the reference period and interview date increases the likelihood that respondents will omit events from their reports (Hayford and Morgan 2008, Peters 1988, Smith and Thomas 2003, Wu, Martin, and Long 2001). There may be a non-linear association between recall duration and recall error. One study, for example, finds that recall error increased in the first four years after the event but subsequently leveled off (Wu, Martin, and Long 2001). Other studies have detected similar patterns in which rates of recall error are highest after the event and then subsequently decline (Bradburn, Rips, and Shevell 1987, Ebbinghaus 1964, Smith and Thomas 2003).

I anticipate that the duration between the reference period and the interview date is positively associated with recall error. I do not have a hypothesis about the functional form of this recall error.

Event Characteristics and Recall Error

Characteristics of events, such as clarity, complexity, rehearsal of the memory, and salience, also affect recall error. Clarity refers to whether the experience of the event is unambiguously defined and distinguishable from other events. Clearly defined events are more likely to be reported accurately (Bradburn, Rips, and Shevell 1987). For example, in a study of economic transfers between divorced parents, Dykema and Schaeffer (2000) find that reports of child support payments were less accurate when other economic exchanges that may be confused with child support (e.g., alimony) were occurring. In addition, respondents provide lower quality

reports about complex events compared to simple events (Freedman et al. 1988). Dykema and

Schaeffer (2000) find that complex patterns of child support – transfers that are irregular, and

variable, and frequent – lead to less accurate reporting. "Rehearsing" events refers to regularly

remembering or talking about an event: birthdays, for example, are highly rehearsed events

(Sudman, Bradburn, and Schwarz 1996). Respondents have better recall about memories that are

rehearsed because the memory trace is strengthened each time the memory is recalled (Pezdek

2006).

Finally, salient events are less susceptible to recall errors because individuals are likely to encode

and retrieve the details of meaningful life experiences. Events involving high affect such as the

birth of children may have more accurate reporting than do events with low affect (Dyekma and

Schaeffer 2000). Events that mark symbolic or logistical transitions, such as marriage, long

distance moves, or long spells of unemployment are also likely to be recalled more accurately

compared to less consequential events (Auriat 1993, Dex and McCulloch 1998, Smith and

Thomas 2003).

Hypotheses

Cohabitation Versus Marriage. I hypothesize that recall error will be present in respondents'

reports about cohabitation, but not about marriage. This is because cohabitation is less clearly

defined, is more complex, has fewer opportunities for rehearsal, and is less salient compared to

marriage (see reviews by Seltzer 2000 and Smock 2000). Entry into cohabitation is not

formalized and may be gradual or incomplete; there is also no commonly accepted term for

cohabitation (Manning and Smock 2005, Sassler 2004). Exit from both marriage and

cohabitation is conceptually and empirically ambiguous. Defining the end of marriage is ambiguous because of vagueness in when separation occurs, as well as the time period between separation and divorce (Bumpass, Sweet, and Castro Martin 1990). The end of cohabitation is even more ambiguous, given the absence of legal formalities and the fact that cohabiting partners sometimes live together after the relationship ends (Cross-Barnet, Cherlin, and Burton 2008). Further, cohabitation is often a complex event in which partners sometimes transition in and out of living together (Binstock and Thornton 2003). Although the date of marriage is regularly rehearsed through anniversary celebrations, there is not a similar, widely-recognized ritual for cohabitation. Additionally, some cohabiters live together for convenience and may not regard it as a salient life event (Manning and Smock 2005, Smock, Manning, and Porter 2005). Indeed, women's retrospective reports of marriage are generally accurate, though not perfect (Bumpass and Raley 1992, Freedman et al. 1988, Raley and Bumpass 2003, Wu, Martin, Long 2001, Wu, Bumpass, and Musick 2000). For this reason, I focus on cohabitation for the remaining variables.

Age of Entry. I expect that reports of cohabitation in young adulthood will have more recall error than will reports of cohabitation in middle age. Hayford and Morgan (2008) speculate that cohabitations at young ages may be susceptible to recall errors because of their short duration and instability. In addition, cohabitations at young ages may be less clearly defined because they may occur in the context of labor force, educational, and geographic transitions. Cohabitations in youth also may be cohabitations of convenience involving less affect and investment (Manning and Smock 2005).

Union Order. I anticipate that an individual's report of their union will have less recall error than higher-order unions because a person's first union represents a salient life transition. This explanation is supported by evidence that remarriages are reported less accurately than are first marriages (Bumpass 1984, Bumpass, Sweet, and Castro Martin 1990).

Children. I expect that having a child in a union will decrease the likelihood of recall error compared to unions that do not produce a child. Having a child together increases the salience of the union and serves as a reminder of the union if the parents are no longer together. Further, the birth of a child may serve as a landmark event that improves the quality of cohabitation reports.[3]

Duration. I anticipate that short-term cohabitations will have more recall error than will long-term cohabitations. This is because shorter cohabitations may be a less salient and have fewer opportunities for the memory to be rehearsed. Indeed, short duration moves (Smith and Thomas, 2003) and unemployment spells (Mathiowetz and Duncan 1988) have more recall error than their longer-duration counterparts.

Non-Marital Cohabitation Versus Pre-Marital Cohabitation. It is unclear whether reports about cohabitations that do not transition to marriage ("non-marital cohabitation") will have more recall error compared to cohabitations that transition to marriage ("pre-marital cohabitation"). On one hand, transitioning to marriage is a proxy for the of the seriousness of the union, making pre-marital cohabitations perhaps more salient than non-marital cohabitations. But on the other

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[3] I do not include this variable in the current version of the paper but will in subsequent versions. Women (but not men) were asked to report the dates of all their children's births. This information allows me to identify whether a woman had a birth within the context of a cohabiting or marital union. I do not know for sure, however, that the child was fathered by the woman's cohabiting partner or spouse at the time.

hand, individuals who cohabited before marriage may regard their marriage as the more important marker of their union and fail to report the cohabitation.

Respondent Characteristics and Recall Error

Studies identify three characteristics of respondents associated with recall error: cognitive ability, motivation, and relationship of a person to the events. Individuals with high cognitive ability report events more accurately than those with less cognitive ability (Wu, Martin, and Long 2001) due to the cognitive demands of encoding information, retrieving detailed autobiographical memories, and integrating memories into a coherent framework (Bradburn, Rips, and Shevell 1987, Krosnick 1991).

Respondents who are motivated to devote the cognitive effort to search, retrieve, and integrate memories are more likely to provide high quality answers. Additionally, respondents who believe in the importance or utility of the survey (Krosnick 1999), as well as those who have a greater personal connection to the subject matter (Schaeffer and Presser 2003), are expected to supply more accurate responses.

Finally, a respondent's history of the event may affect recall error. Respondents with more complicated histories report less accurately than respondents with simple histories because of the complexity of their experiences (Blair and Burton 1987, Bradburn, Rips, and Shevell 1987, Mathiowetz and Duncan 1988, Schaeffer and Presser 2003, Smith and Thomas 2003).

Hypotheses

Gender. I anticipate that men's reports of cohabitation will have more recall error than will women's reports. Because of differences in gender socialization, women may view relationships as more salient and be more likely to devote the cognitive effort necessary to provide high quality reports. One diary study also found that women are better at encoding and retrieving exact dates than are men (Skowronski et al. 1994). Indeed, women's reports of marriage histories are more accurate than are men's reports (Cherlin and McCarthy 1984). Studies of gender differences in other domains such as migration also find that women provide more valid and reliable information than do men (Auriat 1993, Smith and Thomas 2003).

Education. Education is a proxy for cognitive ability. I expect a negative association between education and recall error because of the cognitive burden of supplying detailed autobiographical histories. In addition, education may provide practice in responding to questions about dates. Indeed, a number of studies find education to be negatively associated with reporting errors (Bumpass 1983, Hahn, Eaker, and Roker 1997, Peters 1988, Smith and Thomas 2003 – but see Dykema and Schaeffer 2000).

Complicated Union History. Individuals who have a complicated union history are expected to provide less accurate union histories compared to those with simple histories. In this study, I use the total number of unions an individual has ever experienced to define the complexity of the union history.[4]

Methods for Assessing the Quality of Retrospective Union Histories

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[4] I do not include this variable in the analysis in the present paper but will incorporate it in future versions of the paper.

The gold standard for evaluating the quality of retrospective data is to compare such data with external data such as marriage or birth registries that presumably have fewer errors compared to respondent reports (e.g., Auriat 1993, Dykema and Schaeffer 2000, Wu, Martin, and Long 2001). This study design is infeasible with non-marital cohabitation because there is not a household registration system in Britain.

Another study design compares the consistency of retrospective cohabitation histories collected from two independent cross-sectional samples in different periods (Bumpass 1983, Bumpass 1984, Bumpass and Lu 2000, Hayford and Morgan 2008, Swicegood, Morgan, and Rindfuss 1984).[5] A recent paper by Hayford and Morgan (2008) adopts this approach. The authors analyze retrospective cohabitation histories of women from four cross-sectional studies: the 1987-88 National Survey of Families and Households (NSFH) and the 1988, 1995, and 2002 waves of the National Survey of Family Growth (NSFG). Their analysis is restricted to women because union histories were not collected from men in the 1988 or 1995 NSFG. Hayford and Morgan (2008) select an age-period group that was observed in all four surveys. This age-period group is comprised of women born between 1960 and 1968 who reported union experiences between 1978 and 1987, up a maximum age of 27. Like other studies using this design, Hayford and Morgan (2008) assume that the only difference between the union histories from the four surveys is *when the histories were collected.* The authors reason that if no error is present, the four surveys should produce equal rates of entry into cohabitation because the surveys are assumed to observe the same cohorts of women reporting the same union histories, but at

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[5] There are other methods of assessing the quality of retrospective histories. These include repeated retrospective histories from the same individuals (Beckett et al. 2001), comparing reports from retrospective and panel observations on the same individuals (Freedman et al. 1988, Peters 1988, Teitler, Reichman, and Koball 2006), and comparing aggregate rates of cohabitation using retrospective versus and cross-sectional data (Murphy 2000).

different times. The authors reason that recall error would produce *lower* rates of entry into cohabitation in surveys further away from the reference period because of the longer recall duration. Conversely, social desirability bias may produce *higher* rates of entry into cohabitation in surveys further away from the observation period because of greater acceptance of cohabitation in later periods.

Hayford and Morgan (2008) find that the greater duration between the observation period and the survey, the lower the rates of entry into cohabitation among never-married women. For example, for the period 1978-1987, rates of entry into cohabitation are 23% lower in the 2002 NSFG relative to the 1988 NSFG. The authors find less underreporting in surveys spaced seven years apart. The authors interpret these inconsistencies as evidence of recall error and caution analysts about the quality of retrospective cohabitation data.

Contributions of the Present Study

I adopt Hayford and Morgan's (2008) analytic approach and extend their work in four ways. First, I examine variation by socio-demographic characteristics in the consistency of cohabitation and marital histories. Research has documented socio-demographic differences in the quality of retrospective data on marriage and divorce (Bumpass 1984, Bumpass and Raley 1992, Cherlin and McCarthy 1984, Wu, Martin, and Long 2001). But it is unclear whether these reporting errors also apply to cohabitation (for an exception, see Teitler, Reichmand, and Koball 2006). The lack of research on socio-demographic differences in retrospective cohabitation data is particularly true of gender: because of data constraints, most research has been conducted on women (Bumpass and Raley 1992, Bumpass 1984, Hayford and Morgan 2008, Peters 1988,

Teitler, Reichman, and Koball 2006). The GHS includes women and men, allowing me to examine whether men provide lower quality reports of cohabitation, as has been found in other domains (Auriat 1993, Cherlin and McCarthy 1984, Smith and Thomas 2003, Skowronski et al. 1994).

Second, I explore whether certain types of cohabitation suffer from recall error. I examine four characteristics: age of entry, duration, whether the cohabitation transitioned to marriage, and whether children were born in the union. My sample includes individuals aged 16-55, a much broader age range than the 18-27 range used by Hayford and Morgan (2008). The wider age range age range allows me to test Hayford and Morgan's (2008) speculation that cohabiting unions early in the life course are more susceptible to recall error compared to cohabiting unions in middle age. Additionally, the broader age range allows me to examine differences in reporting quality between a person's first and higher-order unions. Hayford and Morgan's (2008) sample also includes previously-married women, but problems in the administration of the 2002 NSFG and the young age range in this sample (18-27) make the interpretation of these findings less meaningful.

Third, I compare the quality of reports of cohabitation and marriage. This allows me to contrast the consistency of an event that is well defined and salient (marriage) with a less well-defined and less salient event (cohabitation). No study to my knowledge has compared recall error in retrospective data of cohabitation versus marriage.

Fourth, I study the implications of any recall error I identify. Underreporting of cohabitations may not bias associations between cohabitation and other phenomena because the magnitude of the error may not be large enough (Swicegood, Morgan, and Rindfuss 1984). I examine how any recall errors I identify bias conclusions about a substantively important subject, the association between pre-marital cohabitation and marital stability.

METHODS

Data and Measures

The General Household Surveys

I use data from the General Household Surveys (GHS), cross-sectional probability samples of the British population living in private households. The GHS have been conducted annually by the Office for National Statistics (ONS) since 1971 (except in 1997 and 1999). In the current paper, I use data from the 2000 and 2005 GHS, but will include the 2003 survey in future work. I cannot use any surveys before 2000 because 2000 was the first time the GHS collected information about ex-cohabitations that never transitioned to marriage. Although the 2001, 2002, 2004, and 2006 GHS are also available, I do not use these surveys because three time points may be sufficient to identify recall error, and also because including these survey years is computationally infeasible. If the patterns I observe in the 2000, 2003, and 2005 GHS are inconsistent, I may add the 2001, 2002, 2004, and 2006 GHS. The GHS use computer assisted, face-to-face interviews to collect information from all individuals aged 16 and older in the sampled household.[6] Surveys were conducted throughout the calendar year. The household response rates in 2000 and 2005 are 67% and 72%, respectively. I use weights provided by ONS

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[6] The GHS uses a multi-stage, stratified sample. The present analyses are not adjusted for the complex sample design of the GHS, so the standard errors are downwardly biased. I consider this bias when I interpret my results. Future versions of this paper will adjust for the complex sample design.

to adjust for non-response bias. The weights are at the household level; individual-level weights

are not available. The baseline sample consists of 9,446 respondents aged 16-59 from the 2000

GHS and 13,854 respondents from the 2005 GHS. These samples exclude 1,545 respondents

from the 2000 GHS and 3,268 respondents from the 2005 GHS due to missing data.

Respondents may have missing data for several reasons. There were some individuals who did

not participate in the survey, but lived in households in which at least one other person

participated in the survey (n = 46 in the 2000 GHS and 325 in the 2005 GHS). Additionally,

respondents whose data were collected by proxy reports (n = 758 in the 2000 GHS and n = 1,416

in the 2005 GHS) have missing data because the proxy interview did not collect union histories.

Individuals who refused to answer the union history questions have missing data (n = 86 in the

2000 and 331 in the 2005 GHS), as do individuals who provided incomplete responses to the

union history questions (n = 655 in the 2000 GHS and 1,196 in the 2005 GHS).[7] The vast

majority of incomplete responses to the union history questions are caused by missing months.

In future work, I will describe the patterns of missing data. Missing data due to refusal to answer

the union history questions or incomplete/invalid responses to the union history is substantively

important: respondents may have missing data because of difficulties recalling union histories.

Retrospective Union Histories in the GHS

The GHS is one of the primary sources of information about cohabitation and marriage in Britain

(Haskey 2001, Murphy 2000). Since 2000, the GHS has collected information from women and

men aged 16-59 about the occurrence and timing of all marriages, all pre-marital cohabitations,

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[7] The true number of incomplete responses to the union history questions is likely to be lower than these numbers; I
adopted a conservative approach and excluded some responses that may be valid; I am contacting ONS about
ambiguities in the data.

and up to three cohabitations that have ended and did not lead to marriage.[8, 9] A unique feature

of the GHS is that questions in the union history are worded exactly the same from 2000-2006.

The union history was included as a separate module within the GHS interview. Respondents

were given the option of completing the union history themselves on a laptop or providing verbal

answers to the interviewer. In future work, I will examine associations between interview mode

and reporting quality.

The union history contains three parts. First, respondents who had identified themselves earlier

in the survey as currently cohabiting ("living with someone in the household as a couple") are

asked to provide the month and year in which they moved in with their current partner. Second,

ever-married respondents are asked to provide the number of marriages, the month and year of

each marriage, and whether the respondent lived together "as a couple" before marriage, and the

start date of pre-marital cohabitation (if applicable). Ever-married respondents were also asked

whether previous marriages ended through death, divorce, or separation, as well as the month

and year of the end of the marriage.[10] Third, all respondents are asked whether they ever had a

previous cohabiting relationship that did not transition to marriage. Respondents who answer

affirmatively are asked to provide the number of such relationships. For the first three non-

marital cohabitations in the past, respondents were also asked to report the duration and dates of

each union and whether the union ended in death or separation. Further, respondents are also

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[8] Two percent of individuals in the 2000 and 2005 GHS reported more than three cohabitations in the past that did
not transition to marriage. Data were collected about the first three of these cohabitations. This limitation
downwardly biases rates of entry into high-order cohabitations. This bias is likely to be trivial because so few
individuals had four or more non-marital cohabitations. Further, this bias affects the 2000 and 2005 GHS equally.
[9] Marriages are assumed to be co-resident. Cohabitation between same-sex partners is not explicitly identified in the
data, but some respondents volunteered that they were currently cohabiting with a same-sex partner. In future
versions of this paper, I will exclude same-sex cohabitation because I am comparing cohabitation with marriage; sex
marriage between same-sex partners is illegal in Britain.
[10] Divorced respondents were asked when they stopped "living together as a couple" as well as when their divorce
decree was granted. I use the date of when the couple stopped "living together as a couple."

asked to provide the dates of the end of the relationship, as well as the dates of when the

respondent and ex-cohabiting partner stopped living in the same household. I use the date of the

relationship ending. See Murphy (2000) for additional information about retrospective union

histories in the GHS.

Using this information, I created a person-month file that tracks an individual's union status

(single, cohabiting, married) for each month from age 15 through age 55 and 11 months (or

censoring). The union history questions did not specify a lower age boundary. I arbitrarily use

age 15 because individuals in the data report unions at this age. In future work, I will investigate

the sensitivity of the parameters to different starting ages such as 14 and 16.[11]

Demographic Variables

I include several demographic variables to control for changes in the composition of the

population between 2000 and 2005 (Hayford and Morgan 2008),[12] as well as to examine whether

these characteristics are associated with reporting quality. These include gender, highest

educational qualification at the time of the survey, school enrollment, and whether the

respondent had previously married or cohabited (time varying).[13] Highest educational

qualification, a measure of educational attainment in Britain, consists of the following five

categories: A levels or above, O levels, other qualifications, no qualifications, and missing. "A

levels or above" represents the highest level of education and "no qualifications" represents the

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[11] Hayford and Morgan (2008) begin their "clock" at age 18. This is not appropriate in the British context because of the earlier age of school-leaving in Britain (age 16).
[12] In future work, I will consider including additional variables such as nativity status to control for changes in the composition of the population.
[13] In future versions of this paper, I will disaggregate the "any previous union" variable into whether the respondent was previously married or had cohabitation experience.

lowest level.[14] School enrollment is a dichotomous variable distinguishing people who are

currently enrolled in school from those who are not. I do not have a hypothesis about the

association between school enrollment and recall error. I include this variable to account for the

right censoring of the educational attainment measure.

Analysis

Analytic Sample

To select a comparable sample from the 2000 and 2005 GHS, I restrict the age, period, and

cohorts in the person-month file described above. First, I limit my sample to birth cohorts

observed in both surveys. This requires me to exclude individuals born before February 1945;

these individuals were observed in the 2000 but not the 2005 GHS. I also exclude respondents

born after April 1985; these individuals were observed in the 2005 but not the 2000 GHS.

Second, I restrict the person-month file to periods before May 2000.[15] Third, I limit the person-

month file to ages under 55 years and 3 months. This age restriction refers to the ages in the

union history, *not* the current age of the respondent. There are individuals between 55 and 60

years old from the 2005 GHS in the sample: they contribute histories until age 55 and 2 months,

but in order to have comparable ages in the 2000 and 2005 GHS, histories at later ages are not

included in the person-month file.

These restrictions yield a final analytic sample of 2,569,832 person-months from 8,606

respondents in the 2000 GHS and from 12,772 respondents in the 2005 GHS. The Lexis

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[14] Note that education could have been attained after cohabitation or marriage, but this is not intended to be a substantive model of union entry: I treat education as a proxy for an individual's cognitive ability at the time of the survey.

[15] In future versions of this paper, I need to determine whether this is the correct period restriction; this may need to be adjusted by several months.

Diagram below summarizes the union histories observed in my analytic sample. In a Lexis Diagram, period (calendar year) is indicated on the X axis and age is indicated on the Y axis. The dotted line indicates the oldest age that is represented in both surveys. Union histories in the 2000 GHS are represented in the left-hand triangle; union histories in the 2005 GHS are represented in the right-hand triangle. The shaded area describes the overlapping age-period segment.

Figure 1. Lexis Diagram Illustrating Analytic Sample
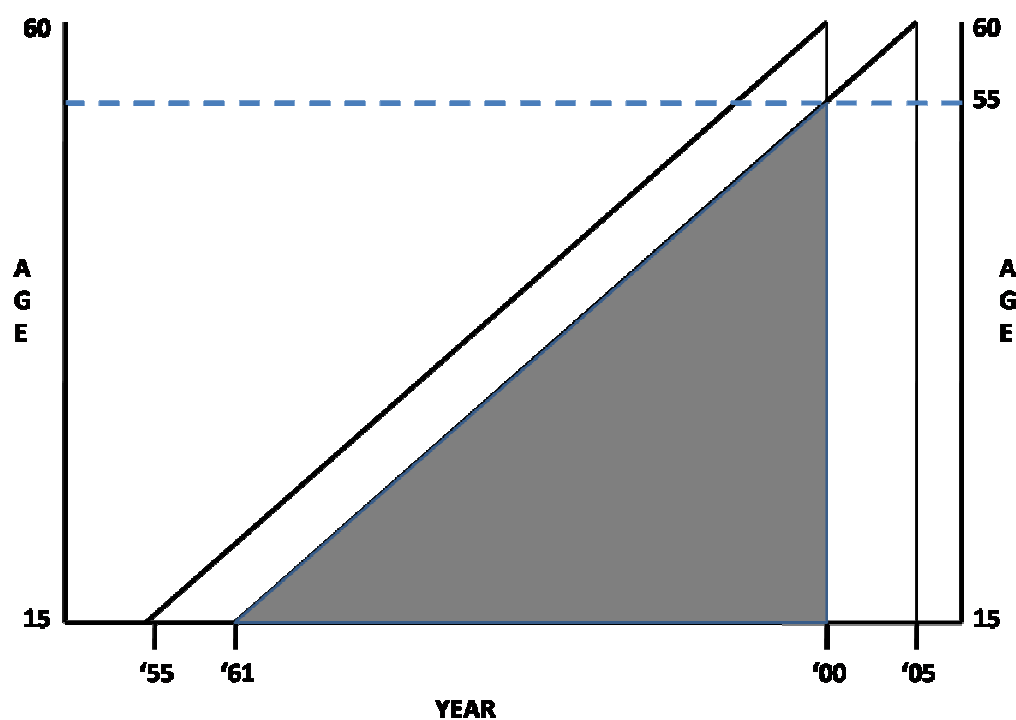
Analysis Plan

There are five stages in the analysis. The first stage examines whether rates of entry into cohabitation and marriage differ in the 2000 and 2005 GHS for the same age and period. In this stage, I begin by presenting life tables showing the cumulative probability of ever cohabiting or marrying at different ages – separately for the 2000 and 2005 GHS. Because the two surveys are

assumed to contain the same age-period group, they should produce similar life table estimates. Inconsistent results between the 2000 and 2005 GHS may indicate that error is present. Consistent results do not mean that error is absent, however because these life tables are limited to a person's first cohabitation and first marriage. In addition, I show plots of the smoothed hazard of entering first cohabitation and first marriage.

Next, I estimate a multinomial logistic regression of entry into cohabitation and marriage on the following variables: interview date, age, period, gender, highest educational qualification, school enrollment, and previous union experience. I use broad categories for the highest age group (35-55) and earliest period (1960-1979) because of the small numbers of entries into cohabitations. Note that these models observe entry into marriage from the single state, but not from the cohabiting state. In future work, I will add a section incorporating the transition from cohabiting to married. Individuals enter the risk set at age 15 and remain at risk until they enter a union or are censored. Individuals who enter unions that subsequently dissolve are returned to the risk set; all models are adjusted for the clustering of unions within individuals. In the current version of the paper, I do not adjust for the clustering of individuals within households. The GHS attempted to collect union histories from every individual aged 16-59 in the household, so a current marriage may be reported in the data twice if both members of the couple are successfully interviewed. In future work, I will consider whether adjusting for this type of clustering is necessary. I adopt a discrete-time approach due to the ease of modeling interactions between age (the "clock" variable) and the covariates. Age, period, and previous union are time-varying; the other variables are time-invariant.[16]

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[16] The covariates in this model are not intended to represent a substantive model of union entry (Hayford and Morgan 2008, Swicegood, Morgan, and Rindfuss 1984). For example, a substantive model of union entry would

The main parameter in the model is "interview date."[17] This parameter indicates whether rates of entry into cohabitation and marriage differ in the 2000 and 2005 GHS for the same age and period group. Lower rates of entry into cohabitation in the 2005 GHS relative to the 2000 GHS are consistent with recall error. This is because the six year longer time period between the observation period (1961-2000) and the interview may have led respondents in the 2005 GHS to underreport cohabitations relative to 2000 GHS respondents. This interpretation requires the assumption that the only difference between the 2000 and 2005 GHS is the year in which the data were collected. This is a reasonable assumption, given the exact same question wording and placement of the union history questions, the same sampling frame, and features of the data collection.

There are three threats to this assumption. First, differential survey non-response and item non-response across GHS years may bias the results. For example, if people who ever cohabited are disproportionately excluded in the 2005 relative to the 2000 survey, then lower rates of entry into cohabitation in the 2005 survey would be due to non-response, not recall error. In future work, I will document differences in survey and item non-response between surveys. Second, migration and mortality between 2000 and 2005 may lead to biased inferences. For example, if there were significant immigration to Britain between 2000 and 2005, and if immigrants are less likely to cohabit than non-immigrants, then lower rates of entry into cohabitation in the 2005 GHS relative to the 2000 GHS may be due to migration, not recall error. In future work, I will

require a time-varying indicator of education. But education in this model is a proxy for the respondent's cognitive ability *at the time of the interview*; a time-varying indicator is not appropriate.

[17] In future work, I will use interview month rather than interview year to get a more precise measure of when the survey occurred. Additionally, Wu, Martin, and Long (2000) note that over the course of data collection, interviewers may gain experience, leading to an improvement in data quality. In future work, I will add a variable indicating the number of months of data collection in a particular survey year.

examine the extent of migration and mortality in the British population between 2000 and 2005.

Third, I am inferring that differences between the 2000 and 2005 GHS are due to respondents

forgetting cohabitations in the approximately six year window. Another possibility is that

individual respondents redefined what constitutes cohabitation between 2000 and 2005 (Elias

1991). For example, an individual may have reported a short-term cohabitation in 2000, but due

to the passage of time or other experiences such as entering another union, the person no longer

considers that short-term cohabitation meaningful enough to report in 2005. One way to

examine this issue is to exclude 2005 GHS respondents who entered a union or had a child

between 2000 and 2005. The inclusion of the 2003 GHS in future work will provide more time

points to mitigate any errors specific to the 2000 or 2005 surveys.

The second stage of the analysis examines variation in recall error across sociodemographic

groups (gender, education, previous union experience) and types of cohabitation (age, period).

In this stage, I estimate the same model from the first stage, but include interaction terms

between the covariates in the model and interview date, one at a time. I include interaction terms

sequentially because there are too many interactions to estimate in one model, as well as to

facilitate interpretation of the parameters. In future versions of the paper, I will also include

three-way interaction terms to test, for example, gender differences in underreporting of

cohabitations at young ages.[18]

(Stages three through five are not included in this draft of the paper, but will be included in

future work.)

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[18] In future versions of this paper, I also will attempt to include measures of the duration of union to investigate
whether respondents' reports of cohabitations at young ages are of lower quality due to the shorter duration of
cohabitations at young ages.

The third stage of the analysis examines variation in recall error by the duration of the union. This stage requires me to estimate an event history model predicting the union *dissolution*. I begin by re-organizing the person-month file to consist of months in which individuals are partnered; individuals are at risk of dissolution until union dissolution or censoring. I estimate a discrete-time, logistic regression model of union dissolution on interview date, union type (cohabitation versus marriage), age of union entry, period, gender, education, school enrollment, previous union experience, and duration of union (the "clock" variable). I will also include interaction terms between interview date and duration. Period and duration are time-varying; all other variables are time-invariant. Individuals may contribute several union experiences to the analysis; this model is adjusted for the clustering of unions within individuals. The main variable in this model is the interaction term between interview date and short duration. This variable indicates whether, for the same age and period, the risk of dissolution for short durations is associated with when the data were collected. Further, I will include additional interaction terms between union type (cohabitation versus marriage) and interview date to examine whether short-term unions are underreported for both cohabitation and marriage.

The fourth stage of the analysis compares the consistency between the 2000 and 2005 GHS in reports of two types of cohabitation: cohabitations that transitioned to marriage ("pre-marital cohabitation") and cohabitations that never transitioned to marriage ("non-marital cohabitation"). In this stage of analysis, I estimate the model from the first stage of the analysis, but use a four category dependent variable: no entry, entry into pre-marital cohabitation, entry into non-marital cohabitation, and entry into marriage. The "interview date" variable indicates whether there are

differences in recall error of cohabitation depending on whether the cohabitation transitioned to marriage.

The fifth stage of the analysis examines the *implications* of recall error. In this stage, I evaluate whether recall error affects substantive conclusions about the association between pre-marital cohabitation and marital disruption. There are two approaches in this stage of the analysis. The first approach is simple, and involves estimating a model of marital dissolution separately for the 2000 and 2005 GHS. The main independent variables in this model are pre-marital cohabitation or previous cohabitation with another partner. If there are different associations between cohabitation and marital disruption between the two models, there is evidence that recall error leads to incorrect substantive conclusions.

The second approach is more complicated. In this approach, I treat the 2000 GHS data as the gold standard, and examine whether introducing recall error observed in the 2005 GHS affects substantive conclusions in the 2000 GHS. I will begin with the percent of all cohabitations and the percent of pre-marital cohabitations that are undercounted in the 2005 GHS relative to the 2000 GHS; these figures come from stages one and four.[19] I will estimate two models of *marital* dissolution using data from the 2000 GHS. The main independent variables are dichotomies distinguishing individuals with any previous cohabitation experience and individuals who pre-martially cohabited with their spouse. The first model will use the 2000 GHS data, as is. For the second model, I will randomly reclassify a certain percent of marriages with pre-marital cohabitation to marriages without pre-marital cohabitation. This reclassification should produce bias towards the null in the association between pre-marital cohabitation and marital disruption.

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[19] This analysis is only possible if there is underreporting of pre-marital cohabitations.

I will then compare the association between pre-marital cohabitation and marital disruption in the two models. If both models have statistically significant, positive associations with marital disruption of similar strengths, there is no evidence that underreporting of cohabitations has harmful implications of this type. On the other hand, if the first model (without error) has a positive, statistically significant association but the second model (with error injected) has no statistically significant association (or has a weaker magnitude), there is evidence that the underreporting of cohabitation has serious consequences.

PRELIMINARY RESULTS

Sample Characteristics

In Table 1, I describe the characteristics of individuals in the samples from the 2000 and 2005 GHS. The two surveys have a similar gender composition. Individuals in the 2000 GHS also have lower educational qualifications compared to 2005 GHS respondents; this difference is statistically significant ($p < .01$). For example, 51% of individuals in the 2005 GHS have attained A levels or above, compared to 45% in the 2000 GHS.[20]

[Inset Table 1]

Descriptive Analysis

Table 2 contains life table estimates of the cumulative probability of ever cohabiting or marrying, separately for the 2000 and 2005 GHS. The cumulative probabilities correspond very well in the 2000 and 2005 GHS. Further, figures 1 and 2 also show that the smoothed hazard of entering first cohabitation and marriage is very similar in the 2000 and 2005 GHS. These

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[20] To test whether this difference is due to educational attainment of the cohorts in the six years between the 2000 and 2005 GHS, I restricted the sample to cohorts born before 1960, who likely would have completed their education by the year 2000. The difference between the 2000 and 2005 GHS surveys remained, which suggests that other factors are responsible.

descriptive figures and plots are consistent with an absence of recall error in the 2005 survey. However, they are limited to first unions, do not distinguish by socio-demographic characteristics of individuals or types of cohabitation, and are confounded with other differences between the surveys. The multivariate analysis in the next section addresses these limitations.

[Insert Table 2, Figure 1, and Figure 2 here]

Multivariate Analysis

Stage 1: Consistency in the 2000 and 2005 GHS

Table 3 contains the results of a multinomial logistic regression predicting entry into cohabitation and marriage. The main independent variable is whether the union history was collected in the 2005 versus 2000 GHS. In the cohabiting versus single comparison, the parameter distinguishing the 2005 GHS from 2000 GHS is negative and statistically significant. The model implies that rates of entry into cohabitation are 5% lower ($1-e^{(-.054)}$) in the 2005 versus 2000 GHS. In other words, for the same age and period, the 2005 GHS yields 5% lower cohabitation rates than does the 2000 GHS. This finding is consistent with the hypothesis that due to recall error, union histories collected at later time points produce lower estimates of cohabitation than histories collected closer to the observation period. Although statistically significant, the association is weak and implies a small underreporting of cohabitation. In the married versus single comparison, the parameter for the interview date is not statistically significant. This suggests that reports of entries into marriage are similar in the 2000 and 2005 GHS, consistent with expectations.

[Insert Table 3]

Stage 2: Variation in Underreporting of Cohabitation

The model in Table 3 assumes that the differences between the 2000 and 2005 GHS are constant across socio-demographic groups and types of cohabitation. In the next set of models, I relax this assumption by interacting the interview date dichotomy with age, period, gender, education, and previous union. I estimate five models, including interaction term(s) between interview date and each independent variable, one at a time. There were statistically significant interactionns between interview date and three variables: age, education, and previous union.

Table 4 contains the parameters from a model that includes interaction terms between age and interview date. In the cohabiting versus single comparison, because the reference category for age is 35-55, the variable for interview date refers to the association between being interviewed in 2005 versus 2000 and rates of entry into cohabitation for individuals aged 35-55. This variable is not statistically significant, suggesting that rates of entry into cohabitation from ages 35-55 are the same in the 2000 and 2005 GHS.

[Insert Table 4]

Several of the interaction terms are statistically significant, however. These parameters indicate that the association between interview date and entry into cohabitation varies by age. A Wald test for the joint significance of interaction terms is statistically significant [$\chi^2(4) = 10.7$; $p = .03$]. This model implies that in the 16-19 age group, rates of entry into cohabitation are 10% lower ($1-e^{(0.121-.222)}$) in the 2005 versus 2000 GHS. The same figure is 10% ($1-e^{(0.121-.224)}$) for the 20-24 age group and 5% ($1-e^{(0.121-.174)}$) for the 25-29 age group. To test whether these numbers are themselves statistically significant, I re-estimated the interactive model three times, sequentially treating 16-19, 20-24, and 25-29 as the reference category. In these models, the parameter for the interview date represents the association between being interviewed in 2005 versus 2000 for

the age group in the reference category. This parameter was statistically significant when the reference groups were 16-19 and 20-24, but not when the reference group was 25-29.

In sum, (1) there is significant underreporting of cohabitation in the 2005 relative to the 2000 GHS for ages 16-24, (2) there is more underreporting of cohabitation in the 2005 versus 2000 GHS in the 25-29 age group than in the 35-55 age group, but I cannot reject the null hypothesis that there is no underreporting in the 25-29 age group, and (3) there is no evidence of underreporting of cohabitation at ages 30 and above.

In Table 5, I present the parameters from a model that examines variation by education in the consistency of reports of cohabitation and marriage in the 2000 and 2005 GHS. As the table indicates, the reference category for education is "A levels or above." This means that the parameter for the interview date represents the association between being interviewed in 2005 rather than in 2000 and entry into cohabitation among those with A level or higher qualifications. In the cohabiting versus single comparison, this parameter is negative and statistically significant. The parameter implies that rates of entry into cohabitation are 9% lower $(1-e^{(-0.089)})$ in the 2005 GHS relative to the 2000 GHS among this education group. The interaction terms between interview date and two other education categories (O levels and other qualifications) are not statistically significant.

[Insert Table 5]

The interaction term between no qualifications and interview date is positive and statistically significant in the cohabiting versus single comparison. The model implies that rates of entry into cohabitation are 12% higher $(1-e^{(-.089 + .216)})$ in the 2005 GHS than in the 2000 GHS. To determine whether this number significantly differs from zero, I re-estimated the interactive

model but treated "no qualifications" as the reference category. The parameter for interview date was indeed statistically significant. These models suggest that cohabitations are over-reported in the 2005 versus 2000 GHS for those with no educational qualifications. A Wald test of the joint significance of interaction terms was statistically significant $[\chi^2(3) = 12.4; p < .01]$.

Table 6 contains the results of a model that contains interaction terms between interview date and previous union experience. In the cohabiting versus single comparison, the parameter associated with interview date represents the association between being interviewed in 2005 relative to 2000 for individuals who had never cohabited or married. The parameter is statistically significant and negative, implying that for those without any union experience, being interviewed in 2005 rather than in 2000 is associated with a 8% lower $(1-e^{(-.088)})$ rate of entering cohabitation. The interaction term is positive and statistically significant: it indicates that rates of entry into cohabitation are 2% higher $(1-e^{(-.088 + .110)})$ in the 2005 versus 2000 GHS for individuals who had a previous union. I re-estimated the model with the dichotomy reversed; in this model, the parameter for interview date represents the association between being interviewed in 2005 versus 2000 for individuals with previous union experience. This parameter was not statistically significant (p = .592). In sum, these results suggest that recall error affects reports of entry into first unions, but not higher-order unions – even when age of entry into union is controlled. In future research, I will disaggregate the "higher-order" category further to distinguish between individuals with two, three, and four previous unions. I will also distinguish individuals who have previous cohabiting experiences from those who were previously married.

[Insert Table 6]

None of the interaction terms between interview date and period or gender is statistically significant in the cohabiting versus single comparison (results not shown).

DISCUSSION AND FUTURE WORK

Recall error may distort knowledge about the levels and correlates of non-marital cohabitation. In this paper, I have investigated the consistency of retrospective cohabitation and marital histories in the British General Household Surveys. My work extends previous research by examining variation by socio-demographic factors and types cohabitation, comparing consistency of cohabitation with that of marriage, and investigating the implications of recall error for substantive conclusions about cohabitation. Preliminary results from this paper point to several conclusions.

For the same age and period, rates of entry into cohabitation in the aggregate are 5% lower in the 2005 GHS than in the 2000 GHS. This difference, although statistically significant, is quite small. There are notable differences, however, by age, education, and previous union experience in the consistency of cohabitation reports in the two surveys. Cohabiting unions in the teenage and young adult years are more susceptible to underreporting than cohabitations later in the life course, a finding consistent with the speculation of Hayford and Morgan (2008). For example, rates of entry into cohabitation from ages 16-24 were 10% lower in the 2005 GHS compared to the 2000 GHS. Although the reasons for this underreporting in the 2005 survey cannot be explored in this paper, recall error is a likely candidate explanation. A reasonable question is whether this undercounting of cohabitation matters at all. It is possible, for example, that cohabitations before age 25 are not meaningful and may not have effects on the well-being of adults or children. Future versions of this paper will address this question by investigating

whether short-term cohabitations suffer from recall error and by investigating the implications of underreporting of cohabitation for marital stability.

For the same ages and period, individuals with no educational qualifications were more likely to report cohabitating unions in the 2005 GHS compared to the 2000 GHS. This is a surprisingly result, given research that shows that individuals with less education are more susceptible to recall errors (Bumpass 1983, Hahn, Eaker, and Roker 1997, Peters 1988, Smith and Thomas 2003). Hayford and Morgan (2008) suggest that higher rates of reported cohabitation in periods further away from the reference period are consistent with social desirability bias. Applying this logic, individuals with no educational qualifications may have felt more comfortable admitting a cohabiting union in 2005 than in 2000. Alternatively, childbearing may explain this finding, provided that low education is positively associated with childbearing and also positively associated with over-reporting cohabitation in 2005 versus 2000. The inclusion of children as a covariate of recall error in future versions of this paper will allow me to test this hypothesis. Individuals reported fewer first cohabiting unions in the 2005 than in the 2000 GHS. But there was no underreporting of cohabitations among individuals who had previous experience in a cohabiting or marital union. This is consistent with the claim that first cohabitations are more susceptible to recall error than are higher-order unions. Such findings were contrary to my hypothesis that first unions would have less from recall error because they are more salient than higher order unions.

The design of this study has two main limitations. First, the design compares the consistency of rates of entry into cohabitation on the aggregate level rather than at the individual level

(Swicegood, Morgan, and Rindfuss 1984). Inferences about the recall error are weaker when inconsistency is detected at the aggregate level. It would be more desirable to compare the consistency of retrospective cohabitation reports taken at two different time periods *on the same individuals* (e.g., Beckett et al. 2001) to eliminate the possibility of unobserved, aggregate-level differences. For example, Beckett et al. (2001) find a high degree of consistency in *aggregate* levels of employment among women across their two data sources, but only a fair degree of consistency across the data sources on the *individual* level.

Second, my design can only examine the consistency of reports six years apart. In this study design, however, there is a tradeoff between age range and the spacing of interview dates: the broader the age range, the more narrow the interview dates. Hayford and Morgan (2008) has at maximum a fourteen year gap between interviews (1988 and 2000), but the authors are forced to adopt a narrow age range (18-27). Conversely, studies concerned with the age patterning of underreporting (such as my study) are constrained to have more narrowly-spaced interview dates. To address methodological concerns about reporting errors, we need both studies that maximize the spacing of interviews (Hayford and Morgan 2008) and studies that maximize age range (my study).

I plan to extend this working paper in a number of ways, as I have mentioned throughout the paper. The major revisions to the paper are as follows. First, I will incorporate the 2003 GHS to provide an additional time point. Second, I will add the third and fourth stages of the analysis, which examine whether short-term and non-marital cohabitations are susceptible to recall error. Third, I will add the fifth stage of the analysis that investigates the implications of recall error for

marital stability. Fourth, in addition to reporting how the *rates* of entry into cohabitation are affected by recall bias, I will also report how other measures, such as the cumulative probability of ever cohabiting, are affected by recall bias. Fifth, I will examine how other factors – including survey and item non-response, migration and mortality, and redefinition of cohabitation – undermine my assumption that GHS surveys conducted in different years can represent the same source population. Sixth, I will examine how childbearing within a union affects recall error among women. Seventh, I will examine how mode of interview affects consistency in different surveys.

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TABLES

Table 1. Sample Characteristics (percentages unless noted)

	2000 GHS	**2005 GHS**	**Difference between 2000 and 2005***
Number of individuals (unweighted)	8,606	12,772	
Male	49	47	$F(1, 21377) = 8.47$ $p < .01$
Education			
A levels or above	45	51	$F(4, 85383) = 201$
O levels	23	25	$p < .01$
Other qualifications	15	6	
No qualifications	17	15	
Missing data	0	4	
Currently enrolled in school	7	4	$F(1, 21377) = 81.2$ $p < .01$

Sources: Weighted data from the 2000 and 2005 General Household Surveys.
Variables and sample described in text.

Table 2. Cumulative Probability of Ever Entering Cohabitation and Marriage, by Age and Survey Year

	Cohabitation		Marriage	
	2000 GHS	**2005 GHS**	**2000 GHS**	**2005 GHS**
20	.10	.10	.10	.09
25	.35	.33	.40	.38
30	.55	.53	.54	.53
35	.65	.63	.59	.59
40	.71	.71	.62	.61
45	.76	.75	.64	.63
50	.80	.79	.66	.63

Table 3. Parameters from Multinomial Logistic Regression of Union Entry

	Cohabiting Versus Single		Married Versus Single	
	β	β/s.e.	β	β/s.e.
2005 GHS (ref = 2000 GHS)	-0.054	-2.59	-0.015	-0.62
Age (ref = 35-55)				
16-19	0.026	0.52	-1.099	-14.90
20-24	1.138	25.71	0.673	9.65
25-29	1.220	28.26	0.727	10.32
30-34	0.796	16.95	0.396	4.96
Period (ref = 1960-1979)				
1980-1989	0.895	24.82	-0.769	-28.56
1990-2000	1.209	33.41	-1.616	-46.35
Male	-0.215	-10.14	-0.546	-23.31
Education (ref = A levels or higher)				
O levels	0.103	4.07	0.280	9.57
Other qualification	0.062	1.66	0.382	10.05
No qualifications	-0.145	-4.13	0.343	10.55
Missing	-0.304	-3.26	0.380	5.10
Currently enrolled in school	-0.660	-6.16	-0.670	-4.44
Had previous union	0.807	27.63	-0.864	-16.83
Intercept	-7.001	-118.66	-4.913	-64.30
Number of events	11,291		8,902	
Number of person months		2,569,832		
Pseudo log-likelihood		-120,395		

Sources: Weighted data from the 2000 and 2005 General Household Surveys.
Variables and sample described in text. Age, period, and previous union are time-varying
variables; other variables are time invariant.

Table 4. Parameters from Multinomial Logistic Regression of Union Entry (with interactions)

	Cohabiting Versus Single		Married Versus Single	
	β	β/s.e.	β	β/s.e.
2005 GHS (ref = 2000 GHS)	0.121	1.69	-0.197	-1.54
Age (ref = 35-55)				
16-19	0.129	1.84	-1.162	-11.64
20-24	1.242	19.95	0.602	6.36
25-29	1.300	20.48	0.619	6.33
30-34	0.840	11.94	0.259	2.24
Age x Interview Date				
16-19 x 2005 GHS	-0.222	-2.59	0.152	1.13
20-24 x 2005 GHS	-0.224	-2.87	0.171	1.32
25-29 x 2005 GHS	-0.174	-2.12	0.246	1.82
30-34 x 2005 GHS	-0.094	-1.02	0.306	1.95
Period (ref = 1960-1979)				
1980-1989	0.896	24.82	-0.769	-28.59
1990-2000	1.210	33.41	-1.616	-46.37
Male	-0.216	-10.18	-0.546	-23.33
Education (ref = A levels or higher)				
O levels	0.102	4.03	0.281	9.58
Other qualification	0.062	1.66	0.382	10.08
No qualifications	-0.145	-4.15	0.342	10.54
Missing	-0.306	-3.27	0.380	5.08
Currently enrolled in school	-0.666	-6.20	-0.673	-4.45
Had previous union	0.807	27.66	-0.863	-16.81
Intercept	-7.082	-100.07	-4.836	-49.57
Number of events	11,291		8,902	
Number of person months		2,569,832		
Pseudo log-likelihood		-120,386		

Sources: Weighted data from the 2000 and 2005 General Household Surveys.
Variables and sample described in text. Age, period, and previous union are time-varying variables; other variables are time invariant.

Table 5. Parameters from Multinomial Logistic Regression of Union Entry (with interactions)

	Cohabiting Versus Single		Married Versus Single	
	β	β/s.e.	β	β/s.e.
2005 GHS (ref = 2000 GHS)	-0.089	-3.16	0.005	0.16
Age (ref = 35-55)				
16-19	0.026	0.51	-1.099	-14.91
20-24	1.138	25.71	0.673	9.64
25-29	1.221	28.27	0.727	10.33
30-34	0.797	16.96	0.396	4.96
Period (ref = 1960-1979)				
1980-1989	0.896	24.86	-0.768	-28.53
1990-2000	1.211	33.46	-1.616	-46.35
Male	-0.216	-10.19	-0.547	-23.31
Education (ref = A levels or higher)				
O levels	0.110	2.81	0.307	6.64
Other qualification	0.028	0.61	0.375	7.73
No qualifications	-0.244	-4.74	0.368	7.58
Missing	-0.286	-3.05	0.370	4.93
Education x Interview Date				
O levels x 2005 GHS	-0.011	-0.21	-0.052	-0.88
Other qualification x 2005 GHS	0.104	1.36	0.046	0.60
No qualifications x 2005 GHS	0.216	3.21	-0.054	-0.84
Currently enrolled in school	-0.662	-6.18	-0.672	-4.45
Had previous union	0.806	27.60	-0.865	-16.83
Intercept	6.985	-116.67	-4.922	-63.29
Number of events	11,291		8,902	
Number of person months		2,569,832		
Pseudo log-likelihood		120,386		

Sources: Weighted data from the 2000 and 2005 General Household Surveys.
Variables and sample described in text. Age, period, and previous union are time-varying variables; other variables are time invariant.

Table 6. Parameters from Multinomial Logistic Regression of Union Entry (with interactions)

	Cohabiting Versus Single		Married Versus Single	
	β	β/s.e.	β	β/s.e.
2005 GHS (ref = 2000 GHS)	-0.088	-3.71	-0.011	-0.47
Age (ref = 35-55)				
16-19	0.028	0.55	-1.099	-14.90
20-24	1.140	25.76	0.673	9.64
25-29	1.221	28.33	0.726	10.32
30-34	0.796	16.98	0.396	4.95
Period (ref = 1960-1979)				
1980-1989	0.895	24.83	-0.769	-28.56
1990-2000	1.210	33.41	-1.616	-46.36
Male	-0.215	-10.15	-0.546	-23.30
Education (ref = A levels or higher)				
O levels	0.102	4.04	0.280	9.57
Other qualification	0.062	1.66	0.382	10.05
No qualifications	-0.145	-4.15	0.342	10.54
Missing	-0.305	-3.27	0.380	5.10
Currently enrolled in school	-0.664	-6.18	-0.670	-4.44
Had previous union	0.758	19.45	-0.839	-11.83
Had previous union x 2005 GHS	0.110	2.36	-0.059	-0.62
Intercept	-6.986	-118.03	-4.914	-64.34
Number of events	11,291		8,902	
Number of person months		2,569,832		
Pseudo log-likelihood		-120,392		

Sources: Weighted data from the 2000 and 2005 General Household Surveys.
Variables and sample described in text. Age, period, and previous union are time-varying variables; other variables are time invariant.

FIGURES
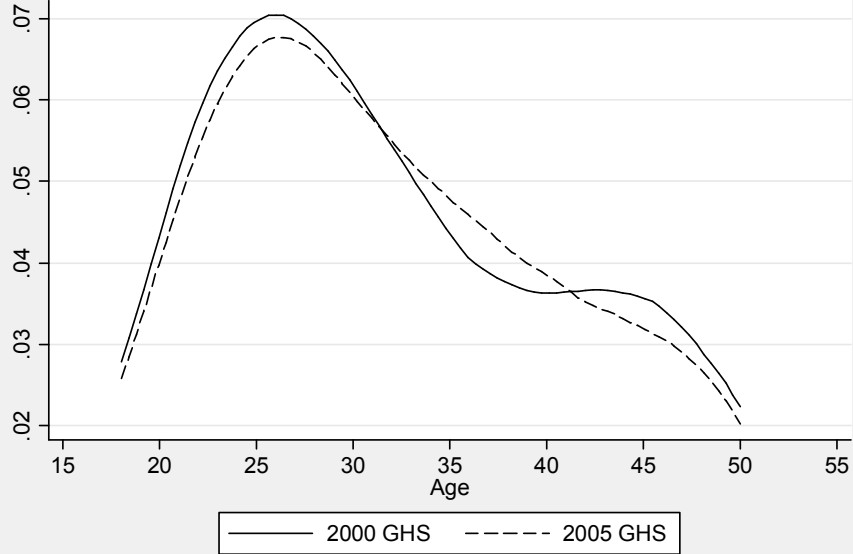

Figure 1: Rate of Entering First Cohabitation by Survey Year

Figure 2: Rate of Entering First Marriage by Survey Year