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REFINING ESTIMATES OF MARITAL STATUS DIFFERENCES IN MORTALITY AT OLDER AGES

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ABSTRACT

The main objective of this analysis is to demonstrate that some of the limitations that have characterized recent studies of the relationship between marital status and health outcomes may result in biased estimates of marital status differences in mortality among the elderly. A secondary goal is to evaluate the strength of evidence in support of the excess risks of mortality associated with widowhood, once we are able to eliminate or mitigate many of the limitations experienced by other studies.

Our results, based on the 1984-1990 Longitudinal Study of Aging, demonstrate that the estimated marital status effects in logit and hazard models of survival are very sensitive to whether and how marital status information is updated after the baseline interview. Refined measures of marital status that capture prospectively transitions from the married to the widowhood state result in substantially increased estimates of the relative risk of dying in the early durations of widowhood (bereavement).

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Introduction

Since the early 1980s, there has been a marked increase in the availability of prospective data suitable for examining the relationship between social factors and health outcomes. In particular, several new surveys were fielded to study the association between life-course transitions and health status among the elderly (e.g., the Longitudinal Study of Aging, LSOA, and the Yale Health and Aging Project, YHAP). In addition, death records were linked to existing longitudinal social surveys such as the National Longitudinal Surveys of Labor Market Experience (NLS) and the Panel Study of Income Dynamics (PSID), making possible the study of mortality in large national prospective samples for which detailed and repeated social and economic information is available.

One consequence of this trend has been a new generation of prospective studies of marital status differences in health and mortality. Some studies have examined overall differences across the marital states with regard to health and survival status, typically with the objective of establishing the protective effects of marriage relative to the single, widowed, and divorced states; other analyses have focussed more specifically on the timing of death relative to the timing of widowhood. Many of the latter efforts have been targeted towards the identification of a "bereavement effect," namely an increase in the risk of mortality that is experienced shortly after a person becomes widowed.[1]

These studies typically have been based on one of the two following types of data: (1) prospective community surveys, generally based on small samples; and (2) large samples or populations from censuses or registries that are linked to death records. While surveys

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[1] We consider any increased mortality risk soon after a transition from the married to a widowed state as a bereavement effect, whether or not the increased risk is causally related to grief or to other psychological processes associated with bereavement.

generally have the advantage of more detailed health, marital status and socio-economic information than the other types of data, estimates based on census and register information are typically more precise (because of larger sample sizes) and are more readily generalizable to a national population than those derived from community surveys.

Despite the increased availability of data suitable for studying the relationship between marital status and health outcomes, many of the resulting studies suffer from limitations that compromise the accuracy of their findings. As described in more detail below, these limitations include inadequate control variables for health, socioeconomic status, and age; failure to update changes in marital status that occur after the onset of the study; loss-to-follow-up; and restricted sampling frames. The main objective of the analysis presented here is to demonstrate that some of these limitations may have substantial effects on estimates of marital status differences in mortality among the elderly. Our focus on the elderly permits us to pay particular attention to the measurement of potential bereavement effects associated with widowhood.

As part of the analysis, we use statistical models estimated in a previous study (Goldman, Korenman and Weinstein, 1995) in order to consider potential biases in the context of a realistic set of health, social and economic variables that are known to be related to both marital status and to health outcomes. An indirect goal of our analysis is to evaluate the strength of evidence in support of excess mortality associated with being widowed, once we are able to eliminate or mitigate the influence of many of the limitations that characterize other studies. Our estimates are based on a nationally representative sample of non-institutionalized persons aged 70 and older from the Longitudinal Study of Aging (LSOA), 1984-1990.

In the subsequent section of the paper, we review some of the findings from recent analyses of mortality differences by marital status, including studies that evaluate the

bereavement effect. We also consider in more detail the limitations that characterize many of these research efforts. We then describe the data set and methods that we use to refine estimates of marital status differences in mortality and to evaluate the potential consequences of analyses that fail to or are unable to include such refinements. In the remainder of the paper, we present our results and discuss their implications for studies of marital status differences in survival that are based on prospective data.

Recent Studies

Whereas earlier research, based largely on cross-sectional data, indicated that unmarried persons — whether they are single, divorced or widowed — experienced higher mortality rates than their married counterparts (e.g., Gove, 1993; Hu and Goldman, 1990; Mastekasa, 1992; Ross, 1990), more recent studies based on prospective data offer somewhat less clearcut results. With regard to single and divorced persons the results for men confirm earlier findings: single men experience excess mortality relative to married men (Ben-Shlomo et al., 1993; Lillard and Waite, 1993), as do divorced men (Zick and Smith, 1991; Lillard and Waite, 1993; Rogers, 1993). However, the evidence for divorced and single women is mixed. Lillard and Waite (1993) find substantially elevated mortality rates for divorced and single women; Zick and Smith (1991) and Goldman et al., (1995) do not.

Studies of the widowed population generally find higher risks of death associated with the widowed state, mainly for men (Goldman et al., 1995; Menchik 1993; Zick and Smith 1991; Lillard and Waite 1993). Compared to married persons of the same sex, widowers generally have higher relative mortality rates than widows (Jones and Goldblatt, 1987, older

sample; Mellstrom et al., 1982; Goldman et al., 1995; Zick and Smith, 1991; Helsing and Szklo, 1981; Helsing et al., 1981), although there are exceptions in the early durations of widowhood (Jones and Goldblatt, 1987; Jagger and Sutton, 1991). The higher relative mortality risk associated with the widowed state diminishes with time spent widowed (Jones and Goldblatt, 1987; Mellstrom et al., 1982; Jagger and Sutton, 1991; Helsing and Szklo, 1981; Helsing et al., 1981; an exception is Wolinsky and Johnson, 1992). Excess risk of death among widows and widowers remains sizable in the presence of controls for health status (Ben-Shlomo et al., 1993; Mendes de Leon, Kasal and Jacobs, 1993; but not in Goldman et al. 1995) or socioeconomic status (Ben-Shlomo et al., 1993; Menchik, 1993; Lillard and Waite, 1993, for men but not for women). Like earlier studies, more recent studies document elevated death rates shortly following transitions to widowhood among men (Jones and Goldblatt, 1987; Mellstrom et al., 1982; Helsing and Szklo, 1981; Helsing et al., 1981), and, less frequently, among women (Jones and Goldblatt, 1987, younger ages; Mellstrom et al., 1982; Jagger and Sutton, 1991).

Despite the fact that studies based on prospective data offer more convincing evidence relating marital status to survival than do studies based on cross-sectional data, even the prospective studies have several limitations, many of which also characterized earlier research efforts (Susser, 1981). Indeed, it is possible that these limitations may account for the discrepant findings noted above. These problems include: geographically limited sampling frames; lack of detailed age controls; insufficient health controls; insufficient controls for socio-economic status; failure to update marital status for changes after the onset of the survey; and loss-to-follow-up.

As noted earlier, since most of the prospective studies obtain samples from community rather than national populations,[2] researchers are faced with uncertainties about the extent to which variation in findings across studies is the consequence of specific characteristics of the communities under investigation. A second problem relates to inadequate age controls, such as the use of 10-year age groups (Helsing and Szklo, 1981; Helsing et al., 1981) and five-year age groups (Jones and Goldblatt, 1987; Kaprio, Koskenvuo and Rita, 1987) to assess marital status differences in mortality. These broad age groups fail to eliminate age variation across marital states (e.g., widowed persons continue to be older than married persons within a five-year age group) and are likely to give rise to biases for older samples since mortality rates increase sharply with age.

The third problem noted above results from a failure to include health measures, or sufficiently detailed health measures, at baseline (e.g., Jones and Goldblatt, 1987; Mellstrom et al. 1982; Kaprio et al. 1987; Menchik 1993; Zick and Smith 1992; Lillard and Waite 1993; and Rogers 1993.) Since married persons may differ from their unmarried counterparts as a result of selection factors affecting entry into and exit from marriage and as a result of protection factors associated with marriage in earlier years, evaluation of the effects of marital status on health during a specified stage of life necessitates controlling for health status at the start of the relevant period. The fourth problem also pertains to insufficient control variables, in this case for social and economic factors (e.g., Jones and Goldblatt 1987; Mellstrom et al. 1982; Ben-Shlomo et al. 1993; Kaprio et al. 1987). These measures are necessary to understand how social and economic characteristics, such as the extent of social support received from friends and relatives, income and assets, and participation in social and

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[2] These include samples from Melton Mowbry, England (Jagger and Sutton, 1991); New Haven, Connecticut (Mendes de Leon et al., 1993); Baltimore County, Maryland (Helsing et al., 1981).

religious activities, intervene in the relationship between marital status and health (Goldman et al., 1995).

The last two problems are inter-related and will be the focus of the analysis in this paper. The first is that most studies fail to pay attention to the dynamics of marital status. That is, while analyses typically include a variable denoting marital status at the start of the survey (i.e., baseline), few studies update marital status in subsequent years even though some of the follow-up periods are very long (e.g., 18 years in Ben-Shlomo et al. and 17 years in Menchik et al., 1993). Even in cases where data on marital status are collected subsequent to baseline and are used in the analysis, determination of marital status often depends on information collected at the last available interview which may not adequately reflect marital status at the time of death. Such failure to update marital status (or to update it sufficiently frequently) suggests that many transitions from the married to the widowed state among the elderly population are likely to be missed and, as a consequence, that exposure to the risk of dying by marital status as well as marital status at the time of death are apt to be misclassified. We hypothesize that failure to update marital status (e.g., because of reliance on infrequent interview reports of marital status) will typically lead to underestimates of the level of excess mortality associated with being widowed.

A related problem is loss-to-follow-up, which by its very nature is unique to prospective data. Loss-to-follow-up — i.e., failure to obtain responses at interviews subsequent to the baseline interview from respondents or suitable proxies — is a serious problem in longitudinal studies, particularly among the elderly population, and is known to vary by both marital and health status (Goldman et al., 1995). A particularly insidious problem associated with loss-to-follow-up is that it may lead to mismeasurement of both vital status and marital status, because of two related issues. The first is that widows are more likely to be lost to survey

follow-up than married persons. The second is that we are more likely to find out that married persons have become widowed if they survive to a subsequent interview. A problem related to loss-to-follow-up is increased reliance on proxy respondents subsequent to the baseline interview, as the sampled persons become more difficult to locate, too ill to respond, etc. While virtually all prospective surveys are plagued by loss-to-follow-up and reliance upon proxy respondents, most studies provide no or limited analysis of the associated problems in the determination of vital status, date of death or marital status. As demonstrated later, measurement problems resulting from loss-to-follow-up, as well as from more general reasons for failure to update marital status, can create severe biases in estimates of marital status differentials in mortality, especially of bereavement effects.

In the following section we describe how the data set used for this analysis enables us to avoid some of the problems described above and permits us to assess the potential extent of bias created by such limitations. We pay particular attention to misclassification of marital status that results largely from two limitations that characterize the vast majority of existing studies and that appear to be especially serious for the older population: namely, loss-to-follow-up and failure to update adequately marital status after the baseline interview.

Data

The main source of data for this analysis is the Longitudinal Study of Aging 1984 to 1990 (LSOA), a subsample of the 1984 National Health Interview Survey Supplement on Aging (SOA). The SOA was administered to persons aged 55 and older; the LSOA followed a subsample of approximately 7,500 persons aged 70 years and older. Follow-up interviews were originally scheduled for 1986, 1988, and 1990 for the entire LSOA subsample.

Unfortunately, due to budget cuts, 1986 reinterviews were not attempted for half of white LSOA members who were aged 70 to 79 in 1984. However, funding for reinterviews with the entire LSOA sample was restored for the 1988 and 1990 rounds. As of the baseline interview, only about 9 percent of responses were supplied by proxies, but this percentage increased over the period of follow-up reaching a level of about one-third of responses in the final interview in 1990.

Data for sample persons in the LSOA are linked to Medicare hospitalization records and to the National Death Index (NDI). The baseline survey contains extensive information about health and disability status, as well as data related to social support, living arrangements, participation in social and religious activities, sources of income, assets and other financial information. Data on marital status are obtained at baseline, while information on any change in marital status — including the date of a marital transition — is obtained at subsequent interviews. Detailed disability status information is also obtained at each reinterview, along with information on living arrangements; data on social support and economic status from the follow-up surveys are more limited.

The LSOA has several advantages over other data sources that have been used to study the relation between marital status and mortality. First, it is based on a nationally representative sample. Second, as noted above, it contains fairly detailed health status information as well as a set of social and economic variables that permit inclusion of necessary control variables in statistical analyses.

Third, it contains multiple sources of vital status information that appear to be of high quality. Nearly all (15,938 of 16,148) SOA respondents provided information for matching to death records. Moreover, interview information (including interviews with proxies or contact persons) and follow-back attempts were used to refine determination of vital status.

In addition, information from linked Medicare hospitalization records was used to refine further vital status for LSOA sample members (Kovar, Fitti and Chyba, 1992).

Fourth, we can identify and match records for spouses in the LSOA and thereby further refine our estimates. Because the LSOA and SOA are subsets of a household survey (the NHIS, 1984), there are a large number of married, cohabiting couples, both members of which are included in the LSOA (or one member is in the LSOA and the spouse is in the SOA) and are linked to death records.[3] By linking spouses' death records to one another, comparing their dates of death, and linking both to interview records, we are able to determine widowhood status at the time of death (as well as date of widowhood) for LSOA members more accurately than we would be able to based on interview information alone. This determination can be made for persons who are lost-to-follow-up from the LSOA, even when both members die in the intervals between scheduled interviews, which vary from between 2 to 4 years.

Fifth, we can verify the reliability of reports from various sources (e.g., marital status reported in interviews and dates of spouses' deaths) by incorporating information on marital status at the time of death recorded on the death certificates. Although the LSOA public-use

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[3] The spouses of some married LSOA members are not included in the LSOA, but we were able to match 86% of married males and 92% of married females to spouses in the SOA (or LSOA). The reason we are not able to match all married LSOA members to a spouse in the SOA is that, although the SOA includes all persons aged 65 and older, it includes only a random half-sample of persons aged 55 to 64. Thus, the cause of failure to match among the vast majority of cases is the result of this random selection of persons aged 55 to 64, and the greater proportion unmatched among married male as compared to female LSOA members (14 percent versus 8 percent) is due to a combination of the SOA sampling scheme and the tendency of men to marry younger women. Note that even though only the LSOA subsample is followed prospectively, the entire SOA sample is eligible for matching to the National Death Index (NDI).

data files do not include marital status information recorded on the death certificates, the CDC made this information available to us, for decedents matched to the NDI.

Methods

In the results presented below, we examine the extent to which reliance on interview information in the LSOA could result in misclassification of marital status. Our primary mechanism for assessing the potential degree of such misclassification is to (1) focus on the subset of respondents who are married and cohabiting at baseline and whose interview records can be matched to their spouse's records; and (2) compare the assessment of marital status from these matched records with two other sources: reported marital status in the most recently available LSOA interview, and, for the subset of decedents, marital status at the time of death recorded on death certificates. A total of 68 sample persons who did not supply information for matching to the NDI were dropped from the entire analysis.[4]

After examining the consistency of reports of marital status across different sources of data, we assess the potential effects of misclassification of marital status on the two types of multivariate models most frequently used to study the relation between marital status and survival status: logit and hazard models of survival. Although logit models have been frequently used to model survival outcomes for a defined follow-up period, they virtually always suffer from misclassification of marital status since marital status is typically determined at baseline and not updated for changes such as becoming widowed during the

[4] Tabulations not presented here suggest that the baseline marital status distribution of these 68 persons does not differ markedly from the distribution for the overall sample. We also dropped nine sample members who did not respond to the question about marital status at the time of the 1984 baseline interview.

follow-up period. Thus, we also estimate Cox proportional hazard models (Cox and Oakes, 1984) in which marital status is treated as a time varying covariate. In these hazard models, the most frequent change in marital status is from married to widowed, but transitions between other states (such as widowed or divorced to married) are also taken into account.

In both the logit and the hazard models, we avoid some of the drawbacks of previous analyses by incorporating detailed age controls (e.g., single year of age in the logit models) and by including variables to measure baseline health status. The latter measures serve two purposes: (1) to reduce potential biases which may arise from the restriction (at baseline) of the LSOA sample to the non-institutionalized population[5]; and (2) to examine the extent to which marital status offers health protection in later life, independent of possible selection and protection effects at younger ages. As described later, some models include additional control variables to describe the social and economic environment.

[5] Since the risk of institutionalization is greater for persons in poorer health and for unmarried persons (Feinstein, Josephy and Wells, 1986), it is likely that unmarried persons who appear in the LSOA are disproportionately selected for good health.

Results

Potential Misclassification of Widowhood Status

In the first four tables, we examine distributions of marital status obtained from various sources: interviews in the LSOA, survival status of spouses gleaned from matched spouses' death records, and reports of marital status on death records. In these tables, we include only the subsample of married LSOA respondents who are cohabiting with a spouse and who can be matched with their spouse in the LSOA or the SOA (see footnote 3).[6] As a result, *marital status* in these tables essentially refers to *widowhood status* — that is, whether a person is married[7] or widowed.

In the first table, we present basic descriptive information pertaining to the classification of marital status of all sample persons (in the first panel) and of decedents (in the second panel), for men and women respectively. Decedents are defined as persons who died between the baseline interview in 1984 and January 1, 1991.[8] These counts of sample

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[6] Tabulations not presented here indicate that the variables of interest in this analysis — health status, social contacts, and socioeconomic status — are distributed similarly for married respondents who can be matched to a spouse in the LSOA or SOA and for those who cannot.

[7] Although all persons in these tabulations are married at baseline and most transitions reflect a change to the widowed state, a few individuals are reported as separated or divorced at last interview and are included in the married category in Tables 1 through 4.

[8] Telephone interviews were conducted between July and September 1990. Mail questionnaires were sent from October to December 1990 to persons who did not have information for telephones calls and to those with no response to telephone calls (Kovar et al., 1992, pp. 11-14). As a result, sample members could have been reinterviewed (and could have recorded an "interview" marital status transition) through December of 1990. However, marital status at death for persons who experienced a marital status change after their 1990 interview and before January 1, 1991 would necessarily be incorrect if based on marital status at last interview (1990). In Table 1, only six of the 97 decedents who would be

members and of decedents are based on two different sources of information on marital status, labelled "last interview" and "matched spouse death records." Marital status measured by interview data refers to the marital status recorded in the LSOA at the time of the last available interview. Sample members responded for themselves whenever possible, but interviews were conducted with proxy respondents if the sample member was unable to respond. Short interviews were conducted with proxy respondents if the sample member died in the interval since the previous interview. No attempt was made to collect from proxy respondents information about marital status transitions prior to death for decedents. Thus, the last interview refers to the most recent interview conducted while the sample member was alive, although the interview may have been carried out with a proxy respondent.

Marital status measured by spouses' death records refers to the determination of widowhood status on the basis of whether death records for either or both spouses exist and, if so, their (relative) dates of death.[9] For example, if both spouses die in the follow-up period but the husband's date of death preceded his wife's, the husband would be classified as married at the time of death, while his wife would be classified as widowed; similarly, if neither spouse died in the follow-up period, both would be classified as married. An additional column, labelled "tied," refers to the 11 sample members who died in the same calendar year and month as their spouse. We are unable to determine marital status at death from matched

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erroneously classified as married (rather than widowed) on the basis of interview marital status died between July 1, 1990 and January 1, 1991. Thus, the extension of the study period through December 1990 can account for at most six of the 97 misclassified decedents.

[9] For SOA sample members who are not included in the LSOA, vital status and dates are death are determined from only death record information. However, for LSOA sample members, all available information — which includes interviews, death records and Medicare files — is used to determine vital status and date of death in order to obtain the most reliable classification possible.

spouses' death records for these cases because the NDI match lacks information on the day of the month of death.[10]

The figures in Table 1 suggest that relying on interview information alone could lead to substantial misclassification of marital status, especially for men. For example, the first column indicates that 40 percent (100/253) of transitions to widowhood among married men 70 and older would be missed by relying on the most recent LSOA interview information available. Misclassification of marital status is potentially even more severe for decedents. For example, the first column of the second panel indicates that two-thirds (52/76) of men who became widows (according to matched spouses' death records) and subsequently died would have been misclassified as married at the time of their death if an analyst relied solely upon survey information to determine marital status. About 28 percent of women (136/480) and more than half (45/85) of female decedents would be misclassified as married by relying on interview information.

It is important to keep in mind that the discrepancies in Table 1 may arise from several sources. Since interview information is updated only at two-year intervals (and, for some respondents not interviewed in 1986, at four-year intervals), interviews will fail to capture a substantial number of transitions to widowhood that occur within the same survey interval as a death. Loss-to-follow-up exacerbates this problem, since it results in a failure to capture marital transitions subsequent to the last available interview.

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[10] Of these 11 cases, nine had marital status recorded on their death certificates (although in two of these cases marital status was recorded as "unknown"); in the remaining two cases, vital status and date of death were determined from survey information without a match to the NDI. In the hazard models presented below, we use death certificate marital status information to determine which member of the couple died as a widow. For the four "tied" persons with missing or unknown marital status at death, we used a random number generator to assign marital status at death with probability 0.5 (each) of being assigned to the widowed or married state.

It is also possible that the interview information is misreported. However, interviews were conducted with sample members ("self response") for 90 percent of cases in 1984 and approximately 64 percent of each of the follow-up interviews (1986, 1988, 1990). An additional 30 percent of the follow-up interviews were conducted with a proxy respondent who resided in the sample person's household. Only four to nine percent of follow-up interviews were conducted with proxy respondents who did not reside in the sample person's household (Kovar et al., 1992, Tables D and J).

Although less likely than misreports in interviews, some discrepancies in Table 1 may arise from imperfect matching of the death certificate information or inaccurate dates of death. For example, according to Table 1, one man (a decedent) and one woman (also a decedent) who would not have been classified as widowed according to matched death record information were reported to be widowed in a follow-up interview.[11]

The magnitude of the potential misclassification bias for estimated mortality differentials is illustrated by the simplistic calculations presented in Table 2. The proportion dying among new widowers is understated by nearly one half (.162 versus .300) if one uses interviews rather than matched death records to determine marital status at death. The odds ratio (widowed/married) of death (corresponding to exponentiated coefficients from logit models) would be understated by more than one half (.27 versus .64).[12, 13] The proportion

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[11] When we estimate models of mortality later in the analysis, we give priority to the interview information in these few cases.

[12] While the comparison between these odds ratios is useful for demonstrating the potential for misclassification bias, the magnitude of these survival probabilities and odds ratios should be interpreted carefully because differences in exposure have not been taken into account. In particular, for a married person, all else the same, the cumulative probability of becoming a widow increases the longer one survives. In effect, in these simple comparisons, becoming widowed may be a proxy for survival (i.e., outliving one's spouse). Below we estimate hazard models to take differences in exposure into account.

dying among new widows would also be understated (.119 versus .177), as would the odds ratio (widowed/married) of death (.38 versus .65) by relying on interview information alone to determine marital status.

In Table 3 we present the distribution of the sample by marital status reported in the last interview, for those decedents who died as widows according to matched spouses' death records. Widows are classified into those who died within 12 months of their spouse's death and those who died a year or more after becoming widows. This sample excludes the 11 people who died in the same calendar month and year as their spouse. The results demonstrate that potential misclassification is particularly severe for those who died within a year of their spouse. That is, the vast majority of men and women who died as recent widowers and widows (according to matched spouse death records) are classified as married in the most recent LSOA interview. These numbers highlight the potential difficulties in assessing bereavement effects when marital status information is obtained from interviews.

In Tables 1 and 3, determination of widowhood status on the basis of spouses' death records relied upon the matching of these records for the determination of the relative timing of death of husband and wife. In Table 4, we compare this assessment of widowhood status with that obtained by looking at actual reports of *marital status* on an individual's death certificate. The high degree of consistency between the two sources confirms the reliability

[13] One might suspect that the misclassification problem is exacerbated by the inclusion of persons who were not included in the 1986 reinterview sample (although marital status was updated at 1988 and 1990 interviews). To address this concern we repeated the analyses for the subsample of persons who were scheduled for follow-up interviews in 1986, 1988, and 1990. The results were similar to those in Tables 1 and 2.

of the matching procedure (as well as the reported dates of death) and also suggests that reports of marital status on death certificates are fairly accurate.[14]

Models of Mortality

The remaining tables present marital status effects estimated from logit and hazard models of mortality. The models estimated here are similar to those used in several different recent research efforts (e.g., Rogers, 1993; Wolinsky and Johnson, 1992; Lillard and Waite, 1993), although they rely most heavily upon those estimated by Goldman et al. (1995) in a recent analysis of marital status differences in health among the elderly.

For both the logit and the hazard analyses, two models are estimated. The first [model (1)] includes controls for age and race (black identification), in addition to baseline health measures. Baseline health status is measured by the following sets of variables: functional limitations (four categories), self-assessed health status (three categories), and seven medical conditions, all measured at the time of the baseline survey. The second [model (2)] adds social and socioeconomic measures to the first model, in order to determine the extent to which the social environment and socioeconomic status potentially account for the observed

[14] We also cross-tabulated marital status at last interview with marital status recorded on the death certificate for all decedents (not shown). As suggested by the results in Table 4, the largest discrepancy occurs for persons reported to be married at last interview but recorded as widowed on the death certificate. Interestingly, there is also a tendency (more prominent among women than men) for persons reported as separated/divorced at last interview to be recorded as widowed on the death certificate and for persons reported as widowed at last interview to be reported as separated/divorced on the death certificate. These inconsistencies, which would be of substantial importance in analyses focussing on the divorced population, may be partly the result of uncertainty surrounding the classification of the marital status of a divorced or separated person whose ex-spouse has died.

associations between marital status and health. Socioeconomic controls are years of schooling, an income-to-needs ratio (family income divided by the U.S. Census poverty line), home-ownership status (three categories), and whether the respondent is covered by private health insurance. The social contacts variables are indicators of recent contact with friends, recent participation in church or religious activities, and recent participation in social events.[15] (Lists of the variables included in models (1) and (2) are shown in Table 8.) Each model is estimated separately by gender.

Since the variable of major interest in this analysis is marital status, only marital status effects are presented in Tables 5-7. In each model, married persons form the reference category. In order to study bereavement effects and their sensitivity to the estimation strategy and sample used, the widowed category is divided into recent widows (< 1 year) and widows of longer duration.[16]

In Table 5, all of the models described above are estimated for the entire LSOA sample. In Table 6, a modified form of the hazard model (i.e., one which includes only the married and widowed states) is estimated for the subsample of married persons matched with their spouses at baseline. As indicated earlier, use of the spouse subsample for estimation allows us to take advantage of death record information to assess the timing of widowhood and marital status at death and results in a much more accurate classification of marital status. On the other hand, inclusion of the remaining LSOA respondents (in Table 5) permits us to

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[15] See Goldman et al. (1995) for further discussion of the choice of control variables and specifications of the final models.

[16] We also estimated models where we defined the bereavement period to be, alternatively, two years or six months rather than one year. The results were similar to those presented in the tables, although the magnitude of the bereavement effect is smaller when the bereavement period is lengthened to two years, and slightly larger when it is shortened to six months.

estimate marital status effects for single and divorced persons as well as for persons in longer durations of widowhood, estimates which cannot be obtained from the restricted sample of married couples at baseline.

The first panel of Table 5 presents odds ratios (exponentiated coefficients) from logit models in which the dependent variable is a dichotomous variable equal to one if the sample member died between baseline (1984) interview and January 1, 1991, and zero otherwise. The logit estimates for men suggest that there are elevated odds of death associated with widowhood (compared to married men), both recent widowhood and longer-term, although the effects of widowhood are not statistically significant. There appears to be a much more modest effect of widowhood for women. There is some evidence that never married women have lower odds of death, although this difference is not significant.

As noted earlier, an obvious drawback of the logit estimates in the first panel is that reliance on marital status at baseline implicitly leads to misclassification of marital status at death. A naive solution (that permits the analyst to retain a logit specification) is to use reinterview information to update marital status. In order to demonstrate the biases associated with this type of adjustment, in the second panel of Table 5 we present the results from logit models in which we remove from the married group individuals reported to be widowed in any follow-up interview. The estimates demonstrate clearly that persons who are reported as widows in later interviews have *lower* odds of death. This effect reflects "survival bias" since only persons who survive to subsequent interviews can be reported as widowed at that time.[17]

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[17] This type of bias may underlie the conclusion of Mendes de Leon et al. (1993) that married persons who became widowed in their sample had better health profiles at baseline than those who did not (p. 526).

The third and fourth panels of Table 5 present relative risks of dying or hazard ratios (exponentiated coefficients) from Cox proportional hazard models (Cox and Oakes, 1984). As noted earlier, the advantage of hazard models over logit models is the ability to update marital status in the former. Estimates in the third panel result from models in which marital status has been updated using interview information only — that is, marital status is treated as a time varying covariate that shifts at the date of a marital status transition reported in a follow-up interview. It is important, however, to recognize that while this adjustment takes into account differences in exposure by marital status, estimates in the third panel continue to suffer from the marital status misclassification problem demonstrated in Table 1 because reinterview information alone is used to update marital status for changes after baseline interviews. The estimates also suffer from survival bias because in most cases sample persons must survive to the next scheduled interview in order to record a marital status transition, yet their marital status is updated at the *retrospectively* reported date of a marital status transition. With these caveats in mind, we note that the estimates in the third panel indicate a *protective* effect associated with bereavement, unlike results from the logit model.

In the last panel of Table 5 we present results from models in which we have refined the classification of marital status by using information from matched spouses' death records whenever possible (i.e., for the subsample of married persons with a spouse in the LSOA or SOA). For "unmatched" married persons (about 14 percent of married men and 8 percent of married women) and for unmarried persons at baseline, we continue to use interview information to update marital status. The estimates here indicate that the relative risk of death among recent widows increases markedly compared to results in the previous panel (from 0.27 to 1.20 for model (1) for women; and from 0.36 to 1.23 for model (1) for men), but is not statistically significant. Recall that even these estimates are not unbiased,

however, since there may be problems of misclassification and survival bias for persons who were not matched to a spouse at baseline.

The estimates in Table 5 suggest considerable variability in the estimated effects of marital status on mortality, depending on the nature of the calculation. Just as troubling is the realization that, in spite of several attempts to improve the classification of marital status in Table 5, none of these estimates is without bias. Indeed, it is not possible to eliminate misclassification and survival bias if we continue to rely upon interview information for the classification of marital status, even though in the last panel of Table 5 we do so for a relatively small portion of the entire sample.

To demonstrate more clearly the potential biases resulting from reliance on interview information and to obtain more satisfactory estimates of bereavement effects, we re-estimate the hazard models in Table 6 for the subsample of married persons at baseline, matched to a spouse in the SOA or LSOA. The first panel presents results based on the use of only interview information to update marital status for changes after the baseline interview. The second panel uses spouses' death records to refine the classification of marital status.

A comparison of estimates between the two panels confirms the results in Tables 1 and 2 that relying on interview information alone biases downward widowhood effects. As suggested by the hazard model estimates in Table 5, the biases are so severe that there appears to be a substantial *protective* effect of widowhood in the first panel. However, according to matched spouses' death records (second panel), there is a significant adverse *bereavement* effect for both women (model (1) only) and men. These results, together with previous findings in Tables 1 and 2, suggest that estimates based on the full LSOA sample are problematic.

This conclusion is confirmed by the results in Table 7 which presents estimates for the same set of models as in Table 5, but in this case with the unmatched married persons removed from the full LSOA sample used in Table 5. The chief advantage of this sample over the sample of spouses used in Table 6 is that people who were not married at baseline are included, especially widows and widowers, yielding more precise estimates of the effects of widowhood. Because only a small proportion of persons who were married at baseline were "unmatched" we would expect these results to be similar to the corresponding results presented in Table 5. Surprisingly, however, results from the hazard models in the last panel of Table 5 differ from the corresponding estimates in Table 7. For both men and women, the excess risk of death associated with recent widowhood is substantially larger (and significant) when unmatched married persons are dropped from the sample, despite the fact that unmatched persons comprise only 14 percent of married men and 8 percent of married women.

The estimates in the last panel of Table 7 are the preferred set of estimates in this analysis, since they are based on a fairly complete report of marital transitions after baseline and they represent a greater range of marital states than do the estimates in Table 6 for the matched spouse subsample. The full set of hazard ratios associated with models (1) and (2) for this sample are presented in Table 8. The relative risks presented in the last panel of Table 7 (and Table 8) indicate a significant and substantial excess risk of mortality for recently widowed men and for both categories of widowed women. Neither divorced nor single persons experience significantly different risks of mortality from their married counterparts.[18]

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[18] It is interesting to note that the coefficient for single women increased substantially from the logit model to the final hazard model in Table 7. The apparent survival advantage of single women in the logit model (although not significant) appears to result from the misclassification of relatively high risk widowed persons as married, which results in an overestimate of the mortality risk of the married.

The estimates also indicate that the excess mortality risks experienced by widowed men and women are reduced only slightly in the presence of control variables for social and economic factors, a result previously reported by Goldman et al. (1995).

Discussion

In this paper, we have explored potential problems associated with the estimation of marital status differences in mortality for a sample of older Americans. By using data from a national study with fairly extensive health, social, and socioeconomic information, by comparing estimates based on only interview information with those gleaned from the matching of death records of spouses, and by constructing appropriate statistical models, we are able to avoid or mitigate many of the problems characterizing earlier studies. In particular, we are able to obtain estimates that are not biased (or are only minimally biased) by loss-to-follow-up and misclassification of marital status.

By examining a sample of persons aged 70 and older who were married at baseline and followed for up to six years, we noted that the most common problem was the classification of widows and widowers as married in the most recent interview. The potential consequences of this inconsistency for estimates of differential mortality by marital status are substantial. In particular, hazard models indicate that marital status information obtained from matched spouses' records leads to substantially increased risks of death associated with recent widowhood (bereavement), as compared with marital status information reported in interviews. Use of interview information alone to update marital status for changes after baseline interviews appears to make matters considerably worse (i.e., to bias downward the estimate associated with widowhood).

A troubling outcome of this analysis is the sensitivity of the estimates of marital status effects — particularly bereavement effects — to whether and how marital status information is updated after baseline. Updating marital status solely on the basis of interview information (whether in a logit or a hazard model) indicates a *protective* effect of widowhood, while the more scientifically justifiable procedure for classifying widowhood on the basis of matched spouses' death records (Table 6 and the last panel of Table 7) indicates a substantial and significant *bereavement* effect (in the range between 1.3 and 1.5). The wide range of estimates in Tables 5 and 7 suggest that some of the discordant findings across other studies may be due to problems of misclassification of marital status, which in turn may stem from the use of inappropriate statistical models, reliance upon (infrequent) interview data for reports of marital status, or loss-to-follow-up. The results presented here raise concerns about the findings of many previous investigations and highlight the need for collection of prospective records of marital status transitions, particularly when the objective is to estimate the association between widowhood and the risk of mortality.

Although this study clearly demonstrates that misclassification of marital status biases the estimates of marital status effects in multivariate models, estimates of the effects of other covariates that are typically included in models of mortality — baseline health status, social contacts, socioeconomic status — do not appear to be materially affected by the mismeasurement of marital status.

The existence of a sizeable bereavement effect for women and men in our preferred hazard models suggests that spouses may be dying from related causes. This is an interesting hypothesis that could be explored further in this sample with cause-of-death information that has recently been made available.

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Table 1: Distributions of All Persons[1] and Decedents According to Widowhood Status and Source of Marital Status Information

Last Interview	Matched Spouse Death Records: Widowed		Married		"Tied"[2]		Total	
	Number	%	Number	%	Number	%	Number	%
Men								
All persons								
Widowed	153	(60)	1	(0)	0	(0)	154	(8)
Married	100	(40)	1586	(100)	6	(100)	1692	(92)
Total	253	(100)	1587	(100)	6	(100)	1846	(100)
Decedents								
Widowed	24	(32)	1	(0)	0	(0)	25	(3)
Married	52	(68)	639	(100)	6	(100)	697	(97)
Total	76	(100)	640	(100)	6	(100)	722	(100)
Women								
All persons								
Widowed	344	(72)	1	(0)	0	(0)	345	(26)
Married	136	(28)	860	(100)	5	(100)	1001	(74)
Total	480	(100)	861	(100)	5	(100)	1346	(100)
Decedents								
Widowed	40	(47)	1	(0)	0	(0)	41	(13)
Married	45	(53)	214	(100)	5	(100)	264	(87)
Total	85	(100)	215	(100)	5	(100)	305	(100)

[1] Includes all LSOA sample persons cohabiting with a spouse in the LSOA or SOA at the baseline interview.

[2] "Tied" refers to spouses who died in the same calendar month and year as one another.

Table 2: Proportions Dying and Odds Ratios by Marital Status According to Source of Marital Status Information[1]

Source of marital status information	Proportions Dying			Odds Ratio
	Total	Married	Widowed	Widowed/Married
Men				
Last interview	.389	.410	.162	.27
Matched spouse death records	.389	.403	.300	.64
Women				
Last interview	.224	.260	.119	.38
Matched spouse death records	.224	.250	.177	.65

[1] Includes all LSOA sample persons cohabiting with a spouse in the LSOA or SOA at the baseline interview.

Table 3: Number of Decedents[1] Who Died as Widows According to Matched Spouse Death Records, By Recency of Widowhood and Marital Status Reported in Last Interview

	Matched Spouse Death Records	
Last Interview	Women	Men
Widowed ≤12 mos.		
Widowed	6	3
Married	23	26
Subtotal	29	29
Widowed >12 mos.		
Widowed	34	21
Married	22	26
Subtotal	56	47

[1] Includes all LSOA sample persons cohabiting with a spouse in the LSOA or SOA at the baseline interview, who died as widows. Excludes sample persons who died in the same calendar month and year as their spouse.

Table 4: Number of Decedents[1] According to Widowhood Status and Source of Marital Status Information

	Matched Spouse Death Records		
Death Certificates	Widowed	Married	"Tied"[2]
Men			
Widowed	64	5	2
Married	3	617	2
Unknown	0	1	1
Women			
Widowed	76	1	1
Married	0	205	2
Unknown	0	0	1

[1] Based on LSOA sample persons cohabiting with a spouse in the LSOA or SOA at the baseline interview. Sample excludes one "tied" couple for which there was no marital status information on the death certificate.

[2] "Tied" refers to spouses who died in the same calendar month and year as one another.

Table 5: Marital Status Effects Estimated from Logit and Hazard Models of Mortality, Entire LSOA Sample

	Odds Ratios or Hazard Ratios			
	Women		Men	
	(1)	(2)	(1)	(2)
Logits (Dead by 1/1/91)				
Widowed < 1 year	1.12	1.10	1.44	1.37
Widowed ≥ 1 year	1.16	1.12	1.24	1.20
Divorced/Separated	1.04	0.93	1.24	1.12
Never married	0.83	0.81	1.09	1.00
Logits (Dead by 1/1/91)				
Widowed < 1 year	0.83	0.80	1.26	1.19
Widowed ≥ 1 year	0.85	0.81	1.09	1.04
Became widowed	0.24**	0.24**	0.19**	0.18**
Divorced/Separated	0.78	0.68	1.12	0.99
Never married	0.61*	0.58**	0.98	0.88
Hazard models, "Interview" marital status[1]				
Widowed < 1 year	0.27**	0.26**	0.36**	0.35**
Widowed ≥ 1 year	0.99	0.96	1.00	0.98
Divorced/Separated	0.92	0.86	1.09	1.02
Never married	0.81	0.80	0.97	0.89
Hazard models, "Matched" marital status[2]				
Widowed < 1 year	1.20	1.16	1.23	1.20
Widowed ≥ 1 year	1.15*	1.11	1.08	1.06
Divorced/Separated	1.05	0.98	1.13	1.06
Never married	0.92	0.92	0.99	0.92

[1] "Interview" marital status is marital status as determined from available baseline and follow-up interviews. See text for further details.

[2] "Matched" marital status is determined by refining interview marital status using information from death records of spouses who are matched in the sample. This sample also included married persons who are not "matched" to a spouse in the sample. For these cases, interview information is used to update marital status for changes after the baseline interviews.

** $p \leq .01$; * $.01 < p \leq .05$

Model specifications:
Model (1) includes dummy variables for functional status (3), self-assessment health status (3), medical conditions (7), and black racial identification, and, in logit models, single year of age.

Model (2) includes, in addition to controls in (1): dummy variables for private health insurance, housing tenure (2), social contacts (3), and controls for years of schooling and income. See text for details.

Table 6: Marital Status Effects Estimated from Hazard Models of Mortality, Subsample of "Matched" Married Persons[1]

	Hazard Ratios			
	Women		Men	
	(1)	(2)	(1)	(2)
Matched subsample, "Interview" marital status[2]				
Widowed < 1 year	0.23**	0.22**	0.28**	0.28**
Widowed ≥ 1 year	0.67**	0.66**	0.51**	0.51**
Widowed	0.54**	0.53**	0.45**	0.44**
Matched subsample, "Matched" marital status[3]				
Widowed < 1 year	1.39*	1.35	1.47**	1.44*
Widowed ≥ 1 year	1.12	1.10	0.86	0.84
Widowed	1.21	1.18	1.05	1.03

[1] The subsample of "matched" married persons are LSOA sample members who were cohabiting with a spouse in the SOA or LSOA at the baseline interview.

[2] "Interview" marital status is marital status as determined from available baseline and follow-up interviews. See text for further details.

[3] "Matched" marital status is determined by refining interview marital status using information from death records of spouses who are matched in the sample.

** $p \leq .01$; * $.01 < p \leq .05$

Model specifications:
Model (1) includes dummy variables for functional status (3), self-assessment health status (3), medical conditions (7), and black racial identification.

Model (2) includes, in addition to controls in (1): dummy variables for private health insurance, housing tenure (2), social contacts (3), and controls for years of schooling and income. See text for details.

Table 7: Marital Status Effects Estimated from Logit and Hazard Models of Mortality, Entire LSOA Sample Less Unmatched Married Persons

	Odds Ratios or Hazard Ratios			
	Women		Men	
	(1)	(2)	(1)	(2)
Logits (Dead by 1/1/91)				
Widowed < 1 year	1.13	1.11	1.44	1.35
Widowed ≥ 1 year	1.17	1.13	1.26*	1.20
Divorced/Separated	1.06	0.94	1.27	1.13
Never married	0.85	0.81	1.11	0.99
Logits (Dead by 1/1/91)				
Widowed < 1 year	0.86	0.83	1.27	1.18
Widowed ≥ 1 year	0.88	0.83	1.11	1.05
Became widowed	0.26**	0.25**	0.20**	0.19**
Divorced/Separated	0.80	0.70	1.14	1.00
Never married	0.64*	0.60**	0.99	0.87
Hazard models, "Interview" marital status[1]				
Widowed < 1 year	0.30**	0.29**	0.44**	0.39**
Widowed ≥ 1 year	1.02	0.98	1.02	1.00
Divorced/Separated	0.94	0.87	1.14	1.06
Never married	0.82	0.81	0.98	0.90
Hazard models, "Matched" marital status[2]				
Widowed < 1 year	1.34*	1.28	1.38*	1.34*
Widowed ≥ 1 year	1.20**	1.15*	1.12	1.08
Divorced/Separated	1.09	1.01	1.18	1.10
Never married	0.96	0.94	1.02	0.93

[1] "Interview" marital status is marital status as determined from available baseline and follow-up interviews. See text for further details.

[2] "Matched" marital status is determined by refining interview marital status using information from death records of spouses who are matched in the sample.

** $p \leq .01$; * $.01 < p \leq .05$

Model specifications:
Model (1) includes dummy variables for functional status (3), self-assessment health status (3), medical conditions (7), and black racial identification, and in logit models, a control for single year of age.

Model (1) includes, in addition to controls in (1): dummy variables for private health insurance, housing tenure (2), social contacts (3), and controls for years of schooling and income. See text for details.

Table 8: Full Set of Effects Estimated from Hazard Models of Mortality, Entire LSOA Sample Less Unmatched Married Persons

	Hazard Ratios			
	Women		Men	
	(1)	(2)	(1)	(2)
Married				
Widowed < 1 year	1.34*	1.28	1.38*	1.34*
Widowed ≥ 1 year	1.20**	1.15*	1.12	1.08
Divorced	1.09	1.01	1.18	1.10
Never married	0.96	0.94	1.02	0.93
Black	1.03	1.05	0.69**	0.73**
Functional Limitations				
Disability free				
ADL, help	2.59**	2.20**	2.01**	1.78**
ADL, no help	1.82**	1.71**	1.30**	1.23**
Others, not dis. free	1.48**	1.44*	1.23**	1.21**
Self-assessment Health Status				
Very good/excellent				
Good	1.14*	1.11	1.17**	1.15*
Fair/poor	1.51**	1.42**	1.60**	1.52**
Medical Conditions				
Hip fracture, ever	1.02	0.99	1.39**	1.35**
Hardened arteries, ever	0.93	0.92	1.12	1.08
Heart attack, ever	1.39**	1.43**	1.19**	1.21**
Hypertension, ever	1.05	1.09	0.99	1.00
Stroke, ever	1.30**	1.24**	1.30**	1.32**
Cancer, ever	1.49**	1.47**	1.32**	1.35**
Diabetes, last 12 mos.	1.49**	1.46**	1.17*	1.18**
Socioeconomic				
No private health ins.		1.09		0.95
Income reported		0.99		0.92
Income/needs ratio		0.99		1.00
Own home				
Rent home		1.18**		0.99
Other		0.97		1.05
Schooling (years)		1.02**		1.01
Social contacts, last 2 weeks				
No contact w/friends		1.37**		1.40**
No church		1.30**		1.34**
No social events		1.34**		1.25**

** $p \leq .01$; * $.01 < p \leq .05$

Reference categories are indicated in italics