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The Protective Effect of Marriage for Survival: A Review and Update

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Abstract The theory that marriage has protective effects for survival has itself lived for more than 100 years since Durkheim's groundbreaking study of suicide (Durkheim 1951 [1897]). Investigations of differences in this protective effect by gender, by age, and in contrast to different unmarried statuses, however, have yielded inconsistent conclusions. These investigations typically either use data in which marital status and other covariates are observed in cross-sectional surveys up to 10 years before mortality exposure, or use data from panel surveys with much smaller sample sizes. Their conclusions are usually not based on formal statistical tests of contrasts between men and women or between never-married, divorced/separated, and widowed statuses. Using large-scale pooled panel survey data linked to death registrations and earnings histories for U.S. men and women aged 25 and older, and with appropriate contrast tests, we find a consistent survival advantage for married over unmarried men and women, and an additional survival "premium" for married men. We find little evidence of mortality differences between never-married, divorced/separated, and widowed statuses.

Keywords Mortality · Marital status · Gender · Age

Introduction

The lower mortality of married adults versus unmarried adults is a consistent empirical finding across populations (Hu and Goldman 1990), and the gap appears

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to be widening in most high-income countries (Jaffe et al. 2007; Murphy et al. 2007; Valkonen et al. 2004). While causal interpretations based on the protective effects of marriage for health have long been made (Durkheim 1951 [1897]; Gove 1973), they were difficult to substantiate with cross-sectional data against the alternative interpretation of positive selection into marriage of more healthy and wealthy individuals (Goldman 1993). The availability in recent decades in the United States and elsewhere of prospective, individual-level data sources has led to a much stronger empirical case for the “marriage protection” hypothesis. Mortality remains higher among unmarried individuals even after controlling for observed socio-economic and health variables that are believed to select people into and out of marriage (Murray 2000; and see reviews by Brown and McDaid 2003; Hummer et al. 1998; and Williams and Collins 1995). Protective effects of marriage have been found, at least for men, also when modeling the effects of unobserved characteristics believed to select individuals into marriage (Espinosa and Evans 2008; Lillard and Panis 1996). In summary, while there is a consensus that married individuals are positively selected on socioeconomic characteristics, evidence also points to substantial protective effects of marriage for survival.

Investigators have accordingly turned toward sharpening our understanding of how and for whom marriage is protective. We focus on three major questions in this literature: Do the protective effects of marriage differ between men and women? Do protective effects of marriage differ in their contrasts to the different categories of unmarried adults (never-married, widowed, and divorced)? And are the protective effects of marriage less at older ages? The many studies that have addressed these questions with prospective, individual-level data represent a major methodological advance over earlier, cross-sectional studies using aggregate data. They have, however, yielded inconsistent findings. While Zick and Smith (1991), for example, argued that the survival benefits of marriage may accrue only to men, Lillard and Waite (1995) also found protective effects for women, and Manzoli et al. (2007) found no difference between the protective effects of marriage for men and women in their meta-analysis of old-age mortality. Other conflicting findings across studies are seen in diverging claims about which unmarried marital status incurs the greatest mortality disadvantage compared to being currently married (see also Waldron et al. 1997). Claims are found in the literature that unmarried survival disadvantages are either limited to, or are greatest for, never-married (Cheung 2000; Kaplan and Kronick 2006), widowed (Ben-Shlomo et al. 1993; Goldman et al. 1995), or divorced/separated individuals (Ebrahim et al. 1995; Smith and Waitzman 1994; Waldron et al. 1997). It has also been argued that the benefits of marriage either diminish or disappear at older ages, but with various exceptions found in comparisons by gender and race/ethnicity across age groups (Johnson et al. 2000; Kallan 1997; Sorlie et al. 1995).

Through the use of better data and statistical practices, we aim in this study to provide more authoritative estimates of these mortality differences by marital status. We describe these differences after controlling for observed socioeconomic and disability variables as estimates of a “marriage protection effect” or “marriage survival advantage,” while acknowledging that positive selection on unobserved variables may account for some part of the estimated marital status differences. Our study’s principal aim is to resolve inconsistencies in conclusions across the many previous studies that have applied standard methods of multivariate analysis to individual-level, prospective data to estimate marriage protection effects.

The present study considers two potential explanations for the inconsistent conclusions across these previous studies. The first is that while there are differences in the marriage protection effect by gender, by age, and when contrasted to different unmarried marital statuses, data deficiencies have prevented their consistent emergence across studies. Most studies use "baseline covariate" data sets. These suffer from the problem that mortality exposure occurs up to a decade or more after "baseline" observation of marital status, health, and other socioeconomic predictor variables in a cross-sectional household survey. Estimated effects of these covariates will therefore be attenuated (Meinow et al. 2004). Because marital status is one of these covariates, "marriage protection" effects will be among those that may be underestimated. The coefficients for those unmarried marital statuses that experience the most changes after observation at baseline (i.e., "never-married" and "divorced/separated" at younger ages and "widowed" at older ages) will, in general, be the most attenuated. The relatively few studies using panel surveys and time-varying covariates, meanwhile, suffer from having much smaller sample sizes. Real effects may therefore go undetected due to lack of statistical power.

A second potential explanation is that while the protective effects of marriage are in reality general and of largely similar magnitudes across genders, across ages, and when contrasted to different unmarried marital statuses, investigators have confused differences occurring by statistical chance with real underlying mortality differences. Supporting this explanation is the dominant but incorrect practice of drawing conclusions about differences based on serial comparisons to the "married" group. Coefficient magnitudes relative to the married groups are compared, or the statistical significance of coefficients of different groups relative to the married group are compared, but formal statistical tests of difference between groups for whom a marriage protection difference is claimed are omitted. For a more general critique of the practice of drawing conclusions that group differences exist in the population based on significant versus nonsignificant coefficients for the respective groups in the sample, see Gelman and Stern (2006).

Our data consist of six pooled panels of the U.S. Survey of Income and Program Participation (SIPP), linked to administrative records of deaths, earnings, and disability. This data set combines the advantages of large sample sizes and linked death-record data of "baseline covariate" studies with the advantages of repeat observation of marital status and control variables of panel-survey studies. We use these data to estimate logistic regression models of the annual probability of death, and we conduct statistical tests of differences not only relative to the married group but also between ages, genders, and unmarried marital statuses. We are thus able to adjudicate between the two competing explanations proposed above to account for inconsistent findings in the literature: (1) there are real marital-status differences in mortality, but the data have not been good enough to find them consistently; and (2) there aren't real differences, but incorrect statistical practices have resulted in unfounded claims about their existence and character.

We find evidence supporting both of these explanations. We find a strong and consistent difference in mortality for married versus unmarried individuals for both men and women, and we find that its magnitude is greater for men than it is for women. We attribute the clarity of these findings to the superior data used in our estimation, together with our including only a married-unmarried distinction in our main models. Supporting the use of a simple married-unmarried distinction, we find little evidence of mortality differences between never-married, divorced/separated, and widowed statuses. We attribute previous conflicting claims about such differences to a combination of statistical chance and incorrect practices to account for this chance. Finally, we find statistically significant declines in marriage differences with age, but are cautious about interpreting this finding due to selection of frailer unmarried individuals into institutions.

Literature Review

Marriage has been theorized to have protective effects for longevity through the social pathways of social integration, social support, social control, and social role attainment as well as the material pathways of financial resources and economies of scale (Gove 1973; House et al. 1988; Kobrin and Hendershot 1977; Rogers et al. 2000; see also reviews by Kiecolt-Glaser and Newton 2001; Macintyre 1992; Ross et al. 1990; and Waite 1995). On social integration, Durkheim (1951 [1897]) highlighted both the risks of failing to enter into the institution of marriage (the "chronic domestic anomie" experienced by bachelors) and of exiting unexpectedly or prematurely from marriage (the "acute domestic anomie" experienced by widows). Through social integration into the family, community, schools, and religious institutions, marriage is claimed to provide people with a sense of meaning, of purpose, of obligations to others, and of belonging (Waite 1995). It also signals the attainment of an adult social role that, particularly when combined with associated adult roles like parent and provider, reduces the likelihood of health-harmful social deviance and risk-taking (Durkheim 1951 [1897]; Hibbard and Pope 1993).

Marriage provides socioemotional support (Ross et al. 1990), which in turn may enhance resilience to stress and even physiological disregulation (Pearlin et al. 1981; Robles and Kiecolt-Glaser 2003). A spouse may assist in the monitoring of health and health-related behaviors and may help to encourage a healthy lifestyle (Cockerham 2005; Ross 1995; Umberson 1987, 1992). Marriage also offers an expansion of social capital through which one may enhance both economic and social resources (Furstenberg 2005). Greater family income, wealth, and economies of household scale in marriage may facilitate purchases of better medical care, better diet, and safer surroundings (Becker 1991; Ross et al. 1990; Waite 1995).

Empirical evidence supports the hypothesis of the protective effect of marriage for survival, but this support is not overwhelming. Manzoli et al.'s (2007) meta-analysis of 53 recent studies of mortality at older ages found a relative risk of death 12% lower for married than otherwise similar unmarried individuals. In only half the studies they analyzed, however, was the married advantage statistically significant. The frequent nonsignificant findings have supported arguments that the protective effects of marriage on health and survival are not general.

Data Limitations of Existing Studies

One reason for weak and inconsistent findings across studies may be deficiencies in the data. The prospective data used in studies of marital status effects on mortality cited earlier all have significant limitations. Goldman described the ideal data set for understanding the effects of marriage on mortality as:

> …a prospective survey which follows a young unmarried sample through the adult life span, collecting repeated measures of marital status, health status, health-related risk factors and behaviors, and socioeconomic status. Such a survey would be based on a sample large enough to produce sufficient numbers of deaths at young and middle ages and to distinguish single, divorced, and widowed groups. (Goldman 1993:191)

The data in the studies to date deviate in two major ways from this ideal. First, "baseline covariate" data sets lack repeated measures of marital status and socioeconomic and health covariates (Ben-Shlomo et al. 1993; Cheung 2000; Espinosa and Evans 2008; Hemström 1996; Johnson et al. 2000; Kallan 1997; Kaplan and Kronick 2006; Lund et al. 2002; Martikainen 1995; Rogers 1996; Rogers et al. 2000; Smith and Waitzman 1994, 1997; Sorlie et al. 1995). The critical problem is that changes to marital status and other covariates are not observed after baseline, whereas mortality exposure extends between 5 and 12 years after baseline (Manzoli et al. 2007). Marital status at baseline will therefore become an increasingly poor measure of marital status at the time of mortality exposure. The standard result for measurement error in regression estimation applies (Cameron and Trivedi 2005): the magnitude of the estimated coefficient (for marital status) will be downwardly biased ("attenuated"). Intuitively, because some of those who were married at baseline will no longer be married at the time of mortality exposure and others who were unmarried (e.g., widowed) will have already remarried at the time of mortality exposure, any real effect of being married on reducing mortality will be muted in the data.

Meinow et al. (2004) evaluated the biasing effects of observing time-varying covariates only at baseline. As expected, they found "weaker predictive strength with longer follow-up . . . for those variables that can change rapidly" (p. S188), including health and living arrangement variables. We know of no evaluation of the magnitudes of this bias specifically for marital status (though, see Korenman et al. [1997] and Weaver [2000] for treatment of misclassification of survey self-reported marital status). Researchers using "baseline covariate" data frequently recognize the problem of unobserved marital-status changes as a limitation of their studies (e.g., Cheung 2000). Johnson et al. (2000) went some way toward solving it by using matched-spouse death records to incorporate change in marital status after baseline, but this introduces only widow(er)hood events among the different possible marital-status changes after baseline.

Panel surveys have included repeated measures of marital status and other covariates. They might not, however, have had large enough samples to estimate mortality differences simultaneously by age, gender, and groups of unmarried statuses. Data from the Panel Study of Income Dynamics (PSID) through the late 1980s have been used by Smith and Zick (Smith and Zick 1994; Zick and Smith

1991) and by Lillard and colleagues (Lillard and Panis 1996; Lillard and Waite 1995). Zick and Smith (1991) analyzed 24,111 person-years of mortality exposure and observed 919 deaths. Lillard and Waite (1995) analyzed up to seven years of exposure for each of 11,112 men and women aged 10 or older and observed 857 deaths to ever-married individuals. These are only a tenth the size of even the more specialized baseline-covariate samples (e.g., Rogers et al. 2000).

Statistical Testing for Differences in the "Marriage Protection" Effect

Claims of differences in the protective effects of marriage by gender, by age, and between different unmarried marital statuses are rarely supported by appropriate statistical tests. An appropriate test defines its null hypothesis to be of no difference in the marital status effect on mortality between different groups. The conclusion that intergroup differences exist requires that this null hypothesis be rejected (Gelman and Stern 2006). Conclusions of gender differences in the mechanism of the protective effect (social for men, economic for women) made by Lillard and Waite (1995) are instead based on the magnitude and statistical significance of coefficients respectively in men's and women's models. They reported (p. 1144) having conducted formal statistical tests of differences between women's and men's unmarried coefficient, but only for a model without controls for income and the presence of children or other adults in the household (the male and female coefficients were not statistically different). Zick and Smith's (1991) conclusions about gender differences, noted earlier, are based on comparisons of nonsignificant coefficients for women with significant coefficients for men, even while the signs of the coefficient are typically the same for both genders. Goldman et al. (1995) concluded that becoming widowed is associated with higher mortality for men but not women on the basis of the magnitudes and statistical significance of the coefficients in separate models for men and women. The effect of widowhood that they claimed to be observed only in the model for men, moreover, is based on a widowhood coefficient that is not significant itself once the full set of controls is included.

Rogers's (1995) study, matching administrative data on deaths with household survey data on population characteristics at ages 25 to 64, is the only multivariate population-representative study we are aware of that conducts and presents formal tests of difference between men and women in the effect of marital status on mortality for its main models. He found greater relative mortality risks from being unmarried for men than for women, but that unmarried women's mortality did not differ significantly (at the .05 level) from married women's mortality.

On differences by unmarried groups, neither Kaplan and Kronick's (2006) nor Cheung's (2000) conclusions that "never-married" individuals have the highest mortality risks are supported by tests for mortality differences between unmarried groups. In both studies, the confidence intervals of never-married and widowed and divorced coefficients or odds ratios (versus married) overlap. Neither Smith and Waitzman's (1994) nor Ebrahim et al.'s (1995) claims of higher mortality of divorced and separated individuals is supported by statistical tests of difference from other unmarried marital statuses (for a similar critique, see also Waldron et al. 1997). Goldman et al.'s (1995) argument for widowhood to be considered as an unmarried status distinct from divorced and never-married is similarly not based on a finding of statistically significant contrasts between widowed and other unmarried statuses (similarly, see Ben-Shlomo et al. 1993). Studies in the epidemiology literature often present confidence intervals about mortality odds ratios for married versus unmarried or versus different unmarried marital statuses, but the intervals of the groups being compared (men and women; widowed, divorced, and never-married), are usually overlapping (e.g., Cheung 2000; Johnson et al. 2000; Kaplan and Kronick 2006; Manzoli et al. 2007).

Data and Method

Our empirical analyses address mortality in a nationally representative sample of noninstitutionalized U.S. men and women aged 25 and older in the mid-1980s through the end of the 1990s. We use logistic regression to estimate discrete-time hazard models of the probability of death in the next year given covariates observed at the beginning of that year.

Data

Our data are from the 1984, 1990, 1991, 1992, 1993, and 1996 panels of the Survey of Income and Program Participation (SIPP), matched to the Social Security Administration's (SSA) Detailed Earnings Records (DER), Summary Earnings Records (SER), Master Beneficiary Records (MBR), Supplemental Security Record (SSR), and "Numident" file of deaths. Mortality exposure in the years 1984–1986 and 1990–1999 is covered by these six panels and linked administrative data. The SIPP is collected by the Census Bureau as a series of panels with survey interview "waves" four months apart (Westat 2001). It was conducted from 1984 through 1993 as a series of annual panels. Each panel ran for two to three years, and individual panel samples were of around 20,000 households. The sample sizes at any given year are typically twice this size, due to overlap between panels. In 1996, a longer-running panel with 40,000 households was initiated. While the shorter panels of the SIPP than the PSID have some disadvantages for observing histories of predictor variables, these are addressed here in part through links to histories of earnings and disability benefits in administrative records.

The SIPP-DER/SER/MBR/SSR/Numident includes links to administrative records for both the dependent variable (death) and for key predictor variables (earnings and disability benefit receipt). Social Security death data have been used in studies of old-age mortality by, among others, Duggan et al. (2007), Preston et al. (1996), Waldron (2007), and Zayatz (2005), and in studies of earnings and income relationships to mortality over broader adult age ranges by Duleep (1986, 1989) and Cristia (2009). To our knowledge, our study is the first to use these data to investigate the relationship between marital status and mortality.

The history of the Numident (our source for death data in this study) and its predecessor linkage files compiled by the SSA is reviewed in Aziz and Buckler (1980, 1992), including a discussion of the difference between the Numident and its public-use extract, the Deaths Master File (DMF). The Numident file we use here is expected to be more complete than the public-use DMF because all state death reports are included in the Numident, while some state and municipal death reports for nonbeneficiaries of Social Security may be legally excluded from the public-use DMF. The DMF has been evaluated against National Center for Health Statistics (NCHS) death records and death rates at a national level by Hill and Rosenwaike (2002), and in small clinical samples by Lash and Silliman (2001) and Schisterman and Whitcomb (2004). The most comprehensive of the evaluation studies of the DMF linkage in the period of our study comes from Hill and Rosenwaike (2002). They found that the linkage rates are high for ages 65 and older, but significantly lower between ages 25 and 64. They suggest that this difference is due at least in part to the greater efforts the SSA makes to collect and verify deaths to individuals with claims on Social Security benefits. With the exception of the years 1987 through 1992, linkage rates are between 93% and 96% for ages 65 and older, and between 76% and 86% for ages 55 to 64. For ages 25 to 54, linkage rates were between 70% and 77% in the years 1984–1986 and 1993–1997. While our own comparisons of the Numident version of the death data did not indicate a worse linkage rate in these years, we take the conservative approach here of excluding from our analyses the years 1987–1989, when linkage rates in the public-use DMF were the poorest. We found that estimates of differences by our study's control variables were generally consistent between our age 25-plus and age 65-plus samples (results not shown). This gives us additional confidence that the greater proportions of unlinked deaths among individuals younger than 65 are not systematically related to the covariates of our study.

Mortality Exposure and Selection of Predictor Variables

We include only periods of mortality exposure during the SIPP's panel years and in the year immediately after the end of the panel. Although many more deaths are available in the linked-record data, we impose this restriction so that all predictor variables describe the characteristics of the individuals at the beginning of each year of mortality exposure. The person-year sample sizes, observed numbers of deaths, and weighted descriptive statistics on sociodemographic and economic characteristics for married and unmarried men and women are shown in Table 1. A total of 582,211 person-years and 7,672 deaths are contributed by the 175,007 individuals observed in our 1984–1999 SIPP-DER/SER/MBR/SSR/Numident data. Men's higher mortality rates result in their contributing 4,060 deaths compared to women's 3,612. While most deaths are at older ages, 1,139 and 749 deaths are contributed, respectively, by men and women aged 25 to 64 (not shown). These sample sizes are at least as large as those in the more specialized baseline covariate samples with linked health data, such as in the National Health Interview Survey (NHIS; e.g., Rogers et al. 2000), but are still much smaller than those using the National Longitudinal Mortality Study (NLMS; Johnson et al. 2000).

We code marital status from the core interview data at the end of the year before exposure. To test the general hypothesis of the protective effect of marriage, we first code a simple married/unmarried variable. To evaluate hypotheses about mortality differences between unmarried groups, we follow most prior studies in coding three unmarried marital statuses: never-married, divorced/separated, and widowed.

Table 1 Sample characteristics, person-years ages 25 and older, by gender and marital status

	Women					Men				
	Married	Unmarried	Divorced or Separated	Widowed	Never-Married	Married	Unmarried	Divorced or Separated	Widowed	Never-Married
Age and Period										
Age (mean)	46.8	52.2	46.1	71.9	38.1	48.8	43.2	46.0	72.3	35.9
Year (mean)	1992.4	1992.6	1992.7	1992.3	1992.7	1992.4	1992.7	1992.8	1992.6	1992.7
Race/Ethnicity (proportions)										
White (non-Hispanic)	0.829	0.722	0.704	0.815	0.643	0.828	0.733	0.753	0.787	0.710
Black	0.064	0.174	0.177	0.116	0.234	0.065	0.152	0.147	0.144	0.157
Hispanic	0.070	0.079	0.096	0.047	0.092	0.074	0.085	0.079	0.053	0.095
Other	0.037	0.025	0.023	0.021	0.032	0.033	0.030	0.020	0.016	0.039
Nativity										
Proportion foreign-born	0.090	0.072	0.070	0.078	0.068	0.089	0.069	0.059	0.068	0.076
Education (proportions)										
< High school	0.236	0.356	0.298	0.509	0.261	0.266	0.315	0.317	0.528	0.275
High school graduate	0.340	0.278	0.306	0.282	0.240	0.280	0.278	0.313	0.240	0.261
Some college	0.230	0.209	0.261	0.140	0.221	0.212	0.211	0.216	0.122	0.224
College degree	0.194	0.157	0.135	0.069	0.279	0.241	0.196	0.154	0.110	0.240
Work and Disability (ages 25–64)										
Proportion worked last year	0.717	0.788	0.806	0.608	0.818	0.909	0.827	0.826	0.719	0.832
Proportion disabled last year	0.019	0.076	0.069	0.127	0.069	0.031	0.075	0.071	0.127	0.076
Proportion disabled <1 year	0.003	0.008	0.009	0.013	0.005	0.004	0.007	0.009	0.014	0.005

Table 1 (continued)

	Women					Men				
	Married	Unmarried	Divorced or Separated	Widowed	Never-Married	Married	Unmarried	Divorced or Separated	Widowed	Never-Married
Ratio of Earnings/Population Average										
Average of last 5 years, ages 25–40	0.55	0.58	0.55	0.40	0.60	1.20	0.74	0.91	0.81	0.68
Average of last 10 years, ages 41–61	0.55	0.68	0.71	0.46	0.81	1.61	1.03	1.11	1.11	0.85
Average of last 16 years, ages 62+	0.31	0.36	0.51	0.30	0.61	1.14	0.81	0.85	0.86	0.62
Sample *N* (person-years)	188,069	124,156	46,915	41,433	35,808	192,863	77,123	29,194	8,228	39,701
Sample Number of Deaths	1,190	2,422	369	1,838	215	2,614	1,446	458	649	339

Note: All statistics are weighted except sample numbers of deaths and person-years.

Source: Survey of Income and Program Participation (SIPP) 1984, 1990, 1991, 1992, 1993, and 1996 panels, matched to the Social Security Administration Detailed Earnings Record, Summary Earnings Record, Master Beneficiary Record, Supplemental Security Record, and Death Master File.

Age and calendar year are both included as predictor variables. We also include the sociodemographic characteristics of race/ethnicity, nativity, and educational attainment. The educational-attainment distributions, in particular, are expected to confer a mortality advantage to married men and women over unmarried men and women: one-quarter of married men and women, but one-third of unmarried men and women, did not graduate from high school.

In selecting predictor variables, we also take advantage of administrative linkages of work and earnings histories and current disability benefit status and history. Married men were much more likely to have worked (have positive earnings) in the previous year than were married women (91% versus 72%). The gender difference in proportions working was small, however, among unmarried individuals (83% of unmarried men and 79% of unmarried women). Only in comparison to the widowed group were married men and women substantially more likely to have worked in the previous year.

We take advantage of linked administrative data to code earnings and disability variables. Using earnings histories, we first calculate ratios of the individual's annual earnings to the SSA's Average Wage Index (AWI; Social Security Administration 2009) in order to establish individuals' relative positions in the earnings distribution in each year. We then average the individual's annual ratios over different numbers of years according to their life stage. For ages 25 to 40, we calculate a ratio of earnings in the last five years to average earnings; for ages 41 to 61, we calculate a ratio of earnings in the last 11 years to average earnings; and for ages 62 and older, we calculate a ratio of earnings over their ages 46 to 62 to average population earnings. These variables are constructed to best represent "permanent earnings" within the constraints of left-censoring of earnings histories that differ by life-course stage and panel year. In all three age groups, married men's earnings exceed those of unmarried men, while the opposite is true for the earnings of married versus unmarried women (see again Table 1).

We code disability based on benefit receipt, either through the Disability Insurance (DI) program or the Supplementary Social Insurance (SSI) program. (For a description of these programs, see Rupp et al. [2008]). Major advantages of the DI/SSI disability variables include their objective nature—they require medical certification of the respondent's health condition—and that they are continuously updated in the linked administrative data. The major disadvantage is that they do not allow us to code disability or health status after age 65, as individuals eligible for Old Age and Survivors Insurance (OASI) benefits are no longer eligible for new disability benefits from DI or SSI. In this respect, our study suffers from a similar limitation after age 65 that applies at all ages in the larger "baseline covariate" studies using the NLMS (Johnson et al. 2000; Sorlie et al. 1995). Lillard and Waite (1995) also omitted health predictor variables from their analyses due to their infrequent collection over their panel observation period. For individuals younger than 65, we specify regressors for current disability receipt and whether this is the first year of disability receipt. While longer claiming histories are available, we found no statistically significant increases in mortality risk for longer claiming periods and so excluded these regressors from the final models presented here. Disability receipt is more than twice as prevalent among unmarried men and women as among married men and women, but this unsurprisingly differs greatly by the mean age of the respective unmarried groups (see Table 1).

Statistical Model

We estimate logistic regression models of the probability of death in the next year given marital status and other sociodemographic and economic covariates. The discrete-time, person-year specification allows for time-varying characteristics, such as marital and work status and disability receipt, and for specifying age interactions with both fixed and time-varying covariates. Calendar year and age are both specified as linear in the logit. We tested for interactions of year (1984–1999) with marital status and for varying slopes in different age categories. In the equation for all ages 25 and older, the reference age is 50. This is approximately the mean age of our person-year observations and follows Lillard and Panis (1996). The use of age 50 makes for a more meaningful "main effect" interpretation of unmarried marital statuses, as there are substantial numbers of divorced/separated, widowed, and never-married individuals at age 50. Choice of reference age has no effect on the age or age-interaction coefficients.

We model and statistically test for mortality differences both in marital-status category main effects and for age and gender interactions with marital status. We also test for statistically significant differences in mortality between widowed, divorced/separated, and never-married categories, and in age interactions between these different marital statuses. In testing for gender differences in the marital status effects, we first run separate but identically specified models for men and women and then pool men and women using a full set of gender interactions with all regressors. This allows us to estimate final models for pooled male and female samples aged 25 and older and aged 65 and older.

All regressions are estimated without sample weights. This provides more efficient estimates than when using sample weights (Cameron and Trivedi 2005) and is broadly compatible with the structure of the SIPP sampling scheme. In four of the six panels, the SIPP uses an approximately equal probability sample design but includes low-income oversamples in the 1990 and 1996 panels (Weinberg 2002). Our final model includes earnings and race/ethnicity among other sociodemographic variables, however, allowing us to model the variables associated with low family income. The clustering and stratification sample-design characteristics relating to the SIPP individuals are not available to us across the six panels being pooled, and therefore our standard errors are not adjusted for survey-design features.

Results

We begin our multivariate logistic regression analyses using a marital-status dummy variable "unmarried" that distinguishes only between married and unmarried individuals. Marital status is defined at the beginning of each person-year of exposure to death (the outcome variable in the regressions). In the first set of models,

Table 2 Logistic regression of death in the next year on age, year, and marital status, and on fixed and time-varying sociodemographic and economic control variables, men and women aged 25 and older, 1984–1999

	Women			Men			
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3	*p* Value Men vs. Women[a]
Intercept (reference age 50)	−5.5018**	−4.9870**	−4.9130**	−4.8642**	−4.3754**	−4.0867**	.0000
	(0.0597)	(0.0639)	(0.0762)	(0.0482)	(0.0521)	(0.0783)	
Year	−0.0338**	−0.0181**	−0.0236**	−0.0478**	−0.0314**	−0.0387**	.0101
	(0.0041)	(0.0041)	(0.0042)	(0.0038)	(0.0039)	(0.0041)	
Age (centered at age 50)	0.0956**	0.0891**	0.0855**	0.0968**	0.0897**	0.0778**	.0492
	(0.0022)	(0.0023)	(0.0028)	(0.0016)	(0.0017)	(0.0027)	
Unmarried	0.6388**	0.5175**	0.3187**	0.9042**	0.7561**	0.5436**	.0704
	(0.0671)	(0.0685)	(0.0716)	(0.0539)	(0.0546)	(0.0586)	
Odds ratio	1.89	1.68	1.38	2.47	2.13	1.72	
Age 50 × Unmarried	−0.0172**	−0.0158**	−0.0093**	−0.0232**	−0.0203**	−0.0131**	.3078
	(0.0027)	(0.0027)	(0.0029)	(0.0022)	(0.0022)	(0.0024)	
Fixed Characteristics							
Race (ref. = non-Hispanic white)							
Black		0.1875*	0.0544		0.2201**	0.0626	.9986
		(0.0907)	(0.0931)		(0.0781)	(0.0803)	
Black × Age 50		−0.0132**	−0.0095**		−0.0146**	−0.0092**	.9507
		(0.0035)	(0.0037)		(0.0034)	(0.0035)	
Hispanic		−0.1534	−0.2354[†]		0.0870	0.0078	.2435
		(0.1375)	(0.1399)		(0.1049)	(0.1064)	
Hispanic × Age 50		−0.0063	−0.0039		−0.0122*	−0.0100*	.4203
		(0.0055)	(0.0057)		(0.0049)	(0.0050)	
Other		0.0480	0.0151		0.2470	0.1984	.3769
		(0.2108)	(0.2150)		(0.1569)	(0.1599)	
Other × Age 50		−0.0086	−0.0077		−0.0202**	−0.0185*	.3469
		(0.0086)	(0.0088)		(0.0071)	(0.0073)	
Nativity (ref. = U.S- born)							
Born outside U.S.		−0.5375**	−0.4814**		−0.8313**	−0.7679**	.2334
		(0.1578)	(0.1602)		(0.1425)	(0.1452)	
Born outside U.S. × Age 50		0.0089[†]	0.0074		0.0149**	0.0146**	.3424
		(0.0053)	(0.0054)		(0.0051)	(0.0052)	
Education (ref. = less than high school graduate)							
High school graduate		−0.8573**	−0.7932**		−0.7583**	−0.6779**	.0717
		(0.0458)	(0.0461)		(0.0443)	(0.0445)	
Some college		−0.9051**	−0.8079**		−0.9423**	−0.8051**	.9735
		(0.0597)	(0.0603)		(0.0579)	(0.0583)	
College graduate		−1.1358**	−0.9982**		−1.1696**	−0.9458**	.6067
		(0.0782)	(0.0800)		(0.0609)	(0.0627)	

Table 2 (continued)

	Women			Men			
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3	p Value Men vs. Women[a]
Time-Varying Characteristics							
Disability							
On disability (i.e., DI or SSI)			1.4699**			1.0961**	.0013
			(0.0894)			(0.0783)	
Disabled less than one year			0.6524**			0.3683*	.1576
			(0.1371)			(0.1470)	
On disability × Age 50			−0.0379**			−0.0213**	.0034
			(0.0043)			(0.0037)	
Employment and earnings							
Working last year			−0.3857**			−0.2395**	.0841
			(0.0690)			(0.0490)	
Working if age <55, else 0			0.2025†			−0.1686†	.0078
			(0.1094)			(0.0865)	
Earnings if age <41, else 0			−0.5152*			−0.1318	.4097
			(0.2203)			(0.0854)	
Earnings if 40 < age < 62, else 0			−0.0685			−0.1495**	.1063
			(0.0896)			(0.0403)	
Earnings if age > 61, else 0			0.0196			−0.1270**	.1637
			(0.0957)			(0.0439)	
Earnings if age > 61, else 0 × Age 50			0.0096			0.0126**	.6503
			(0.0058)			(0.0033)	
Log-Likelihood	−15,976.9	−15,644.7	−15,421.7	−17,049.3	−16,643.4	−16,379.0	
Sample N	312,225	312,225	312,225	269,986	269,986	269,986	

Notes: Numbers in parentheses are standard errors. Disability is identified by DI = Disability Insurance Program, and SSI = Supplemental Security Income Program.

Source: Survey of Income and Program Participation (SIPP) 1984, 1990, 1991, 1992, 1993, and 1996 panels, matched to the Social Security Administration Detailed Earnings Record, Summary Earnings Record, Master Beneficiary Record, Supplemental Security Record, and Death Master File.

[a] Statistical significance of difference between men and women reported for Model 3.

† $p < .10$; * $p < .05$; ** $p < .01$

presented in Table 2, we interact the unmarried dummy variable with age and assess how the unmarried "main effect" and the "age interaction" coefficients change across three model specifications. Because our reference age is set at 50 years old, the "main effect" for each variable with an age interaction expresses the magnitude of impact of that variable on mortality at age 50. The interaction coefficient associated with each of these variables has in most cases a negative sign, indicating by how much the impact of the variable is greater going back to ages younger than 50 and is lesser going forward to ages older than 50.

In the first, purely demographic model (Model 1), we include as controls only age and year variables. In Model 2, we additionally control for sociodemographic variables that are fixed (race, ethnicity, nativity) or approximately fixed (completed education) for adults aged 25 and older. These variables are typically available equally in baseline-covariate and panel-survey studies. In Model 3, we add controls for time-varying sociodemographic and economic variables (work status, earnings, receipt of disability benefits). The coefficients express the increase in the log odds of death in the next year that is associated with a one-unit change in the explanatory variable. Exponentiating the coefficients of binary explanatory variables results in an odds ratio relative to the omitted reference category (Liao 1994). In our case, this odds ratio provides an easily interpretable metric for evaluating the impact of being unmarried relative to married.

In all three models for both men and women, we find a strong and statistically significant positive association of being unmarried at the reference age 50 with death in the year ahead, and a statistically significant decrease in this association as age increases. In Model 1, where only age and year are controlled, the strength of the unmarried main-effect coefficient implies an odds of dying in the next year that is 2.47 times as high for unmarried men than for married men at age 50, and that is 1.89 times as high for unmarried women than for married women at age 50. These are in the approximate range of the ratios of age-standardized unmarried to married mortality rates for men and women aged 15 and older and for the age group 55–64, estimated for U.S. men and women by the NCHS in 2006 (Kung et al. 2008).

The size of the unmarried coefficient in Table 2 decreases substantially, consistent with previous studies (Johnson et al. 2000; Kaplan and Kronick 2006; Lillard and Waite 1995; Rogers 1995; Sorlie et al. 1995), when first fixed (in Model 2) and then time-varying (in Model 3) variables are added. This is consistent, moreover, with the theory that at least part of the positive association of marriage with survival is due to positive selection into marriage. The "unmarried" effect remains large, however, even after controlling for a range of relevant sociodemographic and economic variables.

We illustrate the joint outcome of the positive unmarried "main effect" (at age 50) and its negative interaction with age in Figs. 1a and b. These graphs present the ratio of the odds of death by age for unmarried relative to married individuals, respectively for women and men across the three models of Table 2. They illustrate clearly the value of the panel-data estimation strategy of the present study, allowing for time-varying characteristics not only for marital status itself but also for work, earnings, and disability. For both women and men at ages 25 through 64, the reduction in the odds ratio is typically much greater when adding time-varying characteristics than when including only the usual fixed sociodemographic controls. After age 65, Model 3, which includes the time-varying covariates, suggests a disappearance more than a reversal of the protective effect of marriage at the oldest ages. In this model, "marriage protection" ceases at age 84 for women and at age 89 for men.

Figures 1a and b also illustrate the lower estimated levels of the "marriage protection" effect for women than for men. For example, unmarried men's odds of dying in the next year are 2.40 times higher than the odds for married men at age 25, falling to 1.72 times higher at age 50, and 1.24 times higher at age 75. For unmarried

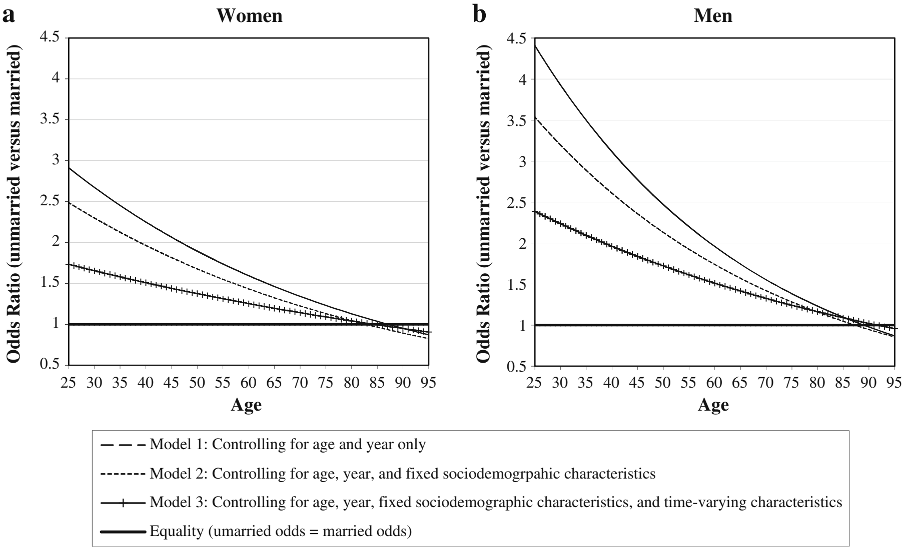

Fig. 1 Ratio of odds of dying for unmarried versus married men and women, by age and model specification

women versus married women, the corresponding odds are 1.73 at age 25, 1.38 at age 50, and 1.09 at age 75. We present in the far right column of Table 2 the *p* values expressing the probability that the estimated difference in magnitudes of each coefficient in the male versus female equation differs also between men and women in the U.S. population. The difference between men and women in the main-effect coefficient for the effect of being unmarried on mortality at age 50 is marginally significant at the .10 level ($p = .070$). The coefficient for the interaction of age with being unmarried does not differ significantly between men and women ($p = .308$).

The signs of the coefficients for the sociodemographic and economic control variables are consistent with the literature (e.g., Hummer et al. 1998) and are similar between men and women. Graduating from high school is associated with lower mortality risk, and this education premium over nongraduates is larger the higher the level of eventual educational attainment reaches. Receiving disability benefits has a strong mortality-increasing association, and working in the previous year has a strong mortality-decreasing association for both men and women. Higher own earnings tend to be mortality-decreasing for both men and women. Interestingly, the "main effect" of being black at age 50 is substantially positive (higher mortality) and statistically significant when fixed sociodemographic variables are controlled for (Model 2), but becomes near zero and nonsignificant after adding the time-varying variables, including work and earnings histories (Model 3).

In Table 3, we display the results of models that vary the marital status specification and that additionally include a separate estimation for the 65-plus age group alone. For our variants on the marital status specifications of Table 2, we first drop the age interaction with marital status to estimate an overall effect of being unmarried on mortality, and then expand the "unmarried" variable into never-married, divorced/separated, and widowed categories. Both variants are applied

Table 3 Logistic regression of death in the next year, with variants on the marital-status main effect and marital status by age interaction specifications, ages 25+ and ages 65+

	Women				Men				
	Estimate	Estimate	p Value Status vs. Divorced/Separated	p Value Status vs. Never-Married	Estimate	Estimate	p Value Status vs. Divorced/Separated	p Value Status vs. Never-Married	p Value Men vs. Women
1. All Ages 25 and Older									
a. No age interaction									
Unmarried	0.1271**				0.2858**				.003
	(0.0400)				(0.0358)				
Odds ratio	1.14				1.33				
Divorced or separated		0.1748**	—	.238		0.4063**	—	.199	.005
		(0.0628)				(0.0543)			
Odds ratio		1.19				1.50			
Widowed		0.1211**	.399	.511		0.1910**	.002	.122	.289
		(0.0437)				(0.0495)			
Odds ratio		1.13				1.21			
Never-married		0.0696	.238	—		0.3094**	.199	—	.018
		(0.0792)				(0.0638)			
Odds ratio		1.07				1.36			
b. Age interaction with unmarried (ref. = age 25)									
Divorced or separated		0.2401*	—	.891		0.5160**	—	.759	.170
		(0.0936)				(0.0764)			
Odds ratio		1.27				1.68			
Widowed		0.4314**	.100	.175		0.7502**	.129	.088	.113
		(0.0994)				(0.1445)			
Odds ratio		1.54				2.12			
Never-married		0.2563*	.891	—		0.4891**	.759	—	.176
		(0.1091)				(0.0745)			

Table 3 (continued)

	Women				Men				
	Estimate	Estimate	*p* Value Status vs. Divorced/Separated	*p* Value Status vs. Never-Married	Estimate	Estimate	*p* Value Status vs. Divorced/Separated	*p* Value Status vs. Never-Married	*p* Value Men vs. Women
Odds ratio		1.29				1.63			
Age × Divorced or separated		−0.0032	—	.122		−0.0057	—	.040	.666
		(0.0044)				(0.0038)			
Age × Widowed		−0.0129**	.039	.693		−0.0206**	.011	.315	.211
		(0.0036)				(0.0049)			
Age × Never-married		−0.0111*	.122	—		−0.0150**	.040	—	.488
		(0.0044)				(0.0035)			
2. Ages 65 and Older Only									
a. No age interaction with									
Unmarried	0.1456**				0.1885**				.498
	(0.0463)				(0.0430)				
Odds ratio	1.16				1.21				
Divorced or separated		0.2732**	—	.023		0.3649**	—	.029	.409
		(0.0816)				(0.0752)			
Odds ratio		1.31				1.44			
Widowed		0.1377**	.082	.170		0.1406**	.008	.811	.967
		(0.0480)				(0.0518)			
Odds ratio		1.15				1.15			
Never-married		0.0056	.023	—		0.1166	.029	—	.418
		(0.1010)				(0.0928)			
Odds ratio		1.01				1.12			

b. Age interaction with unmarried (ref. = age 65)									
Unmarried	0.3463**				0.3949**				.756
	(0.0859)				(0.0837)				
Odds ratio	1.41				1.48				
Age × Unmarried	−0.0177**				−0.0162**				.860
	(0.0063)				(0.0057)				
Divorced or Separated		0.4683**	—	.140		0.4988**	—	.220	.956
		(0.1472)				(0.1294)			
Odds ratio		1.60				1.65			
Widowed		0.3359**	.353	.302		0.3511**	.346	.621	.962
		(0.0905)				(0.1123)			
Odds ratio		1.40				1.42			
Never-married		0.1248	.140	—		0.2601	.220	—	.656
		(0.2075)				(0.1617)			
Odds ratio		1.13				1.30			
Age × Divorced or separated		−0.0188	—	.660		−0.0130	—	.996	.717
		(0.0115)				(0.0110)			
Age × Widowed		−0.0171**	.873	.678		−0.0148*	.885	.892	.804
		(0.0065)				(0.0069)			
Age × Never-married		−0.0121	.660	—		−0.0129	.996	—	.964
		(0.0127)				(0.0125)			
Sample N	68,358				47,938				
Number of Deaths	2,863				2,921				

Notes: Numbers in parentheses are standard errors. Control variables for the “age 25 and older” models are identical to those of Model 3 in Table 2; control variables for the “age 65 and older” models are as for Model 3 in Table 2, except that omitted are those variables not applying after age 65 (the disability variables, working < age 55, and earnings for ages 25–40 and 41–61).

Source: Survey of Income and Program Participation (SIPP) 1984, 1990, 1991, 1992, 1993, and 1996 panels, matched to the Social Security Administration Detailed Earnings Record, Summary Earnings Record, Master Beneficiary Record, Supplemental Security Record, and Death Master File.

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

alternately in the 25-plus and 65-plus age ranges. The same controls are used for the 25-plus group in Table 3 as those used for Model 3 of Table 2. For the 65-plus group, we drop disability and the work and earnings variables related to those younger than 65, but otherwise retain the same controls. The estimates of the coefficients for the control variables for the 65-plus age group were substantially similar to those for the age 25-plus sample. Accordingly, only the estimated coefficients for the main variables of interest for our study—those describing marital status effects—are presented in Table 3. The results of tests for gender differences in the coefficients are again shown by *p* values in the rightmost column of the table.

With no age interaction with marital status (see Panel 1a), the coefficient for the effect of being unmarried for men aged 25 and older is more than twice the magnitude of that for women in the same age group (0.2858 vs. 0.1271), a difference that is statistically significant at the .01 level ($p = .003$). The odds of death in the next year are 1.33 times greater for an unmarried man than for a married man with otherwise equivalent sociodemographic, economic, and health (disability) characteristics, but are only 1.14 times greater for unmarried women than for otherwise similar married women (both individually statistically significant). These results, when added to the evidence in Table 2, strengthen the conclusion that marriage is protective for survival for both men and women, and that a survival premium accrues to men.

For ages 65 and older (see Panel 2a), the “unmarried” coefficients are again both positive and statistically significant for men and women (0.1885 and 0.1456, respectively). The resulting odds of death in the next year are, respectively, 1.21 and 1.16 times greater for unmarried men and women than for married men and women. While these results are individually statistically significant relative to the “married” reference category, the gender difference is not statistically significant. We also estimate for the age 65-plus sample a specification including an age interaction with “unmarried” that is linear in the logit. Being unmarried at the reference age of 65 is associated with higher mortality for both men and women, but this association diminishes with age after 65. Both the main effect and age interaction coefficients are statistically significant. They are not statistically different, however, between men and women. Bringing together our findings from the 25-plus and 65-plus estimates, a declining strength of the unmarried mortality disadvantage with age is a consistent finding. The declining gender contrast in the marriage protection effect with age may therefore be partly attributable to an overall reduction in married-unmarried contrasts with increasing age.

Previous studies focusing on marital status and mortality divide the “unmarried” category into either three or four separate unmarried groups. We evaluate the statistical validity of doing this by expanding our “unmarried” variable into the usual never-married, divorced/separated, and widowed categories. Statistical tests are first presented in the standard way, with tests for mortality differences in each of these unmarried categories contrasted to the reference “married” category (see the “estimates” columns of Table 3). Additionally, we present, as we did for gender contrasts, the *p* values expressing the probabilities that the magnitudes of the estimated coefficients represent real contrasts in the population. These *p* values are presented in columns alternately contrasting widowed and never-married coefficients with divorced/separated coefficients and contrasting widowed with divorced/separated (and, redundantly, never-married with divorced/separated). We estimate and test these with and without age interactions and for both ages 25-plus and ages 65-plus.

We find no statistical evidence for the claim that mortality differs between never-married and widowed individuals. This finding holds across both the 25-plus and 65-plus age groups for both the specifications without and with age interactions with unmarried marital status. Our conclusion would be different, however, if we followed the practice of most other studies and either compared coefficient magnitudes alone or simply compared which unmarried categories were statistically significant (that is, different from the reference "married" group). For example, while the odds of death for widowed men aged 65 and older is 1.15 times that of married men aged 65 and older, and this is significant at the .01 level, the relative odds for never-married men aged 65 and older is 1.12 times, and this is not significant ($p = .21$). The difference between the 1.15 odds of widowed men and the 1.12 odds of never-married men, meanwhile, is far from being statistically significant ($p = .81$).

We find mixed evidence on whether divorced and separated men and women are distinct from the other two unmarried groups. Divorced and separated women and men aged 25 and older do not differ in their mortality risks from never-married women and men, and only for men does their mortality risk differ from that of widowed individuals. When age interactions for the 25-plus group are included for the three unmarried categories, however, the results indicate that for both men and women, the divorced/separated association with higher mortality persists into older ages more than does either the never-married or widowed association with mortality. Consistent with this are higher odds ratios for divorced/separated individuals aged 65 and older when contrasted with both widowed and never-married men and when contrasted with never-married women.

Discussion

Our finding that unmarried American men and women have significantly higher mortality than otherwise similar married men and women supports the long-standing theory that marriage is protective for survival. While this finding is not new, it is by no means a universal result in the studies we reviewed. Only half of the 53 studies of older populations included in a recent meta-analysis (Manzoli et al. 2007), for example, found a statistically significant mortality advantage for married over unmarried individuals. We found a married mortality advantage both in our complete sample of adults aged 25 and older and for the subsample of older adults aged 65 and older. Our results provide new support for the theory that marriage is protective for survival in part because our data cover a period later than that of most other population-representative data sources we reviewed. There has been a long-running tendency for investigators to assert a declining importance of marital status as marriage becomes less prevalent in society and as nonmarital cohabitation becomes more prevalent (House et al. 1982; Kotler and Wingard 1989; Rogers et al. 2000). Our results are instead more consistent with cross-national comparative findings of widening mortality differences between married and unmarried individuals in high-income countries over recent decades (Murphy et al. 2007; Valkonen et al. 2004). Positive selection on unobserved characteristics cannot be ruled out as at least a partial explanation for our findings, and further efforts to model such selection are encouraged. However, the consistently negative coefficients for the unmarried of our study after controlling for observed socioeconomic and disability variables are themselves stronger evidence in support of a marriage protection effect than are the sometimes-significant, sometimes-nonsignificant coefficients of previous studies.

Our results strengthen support for the "marriage protection" hypothesis also because of our study's data and statistical modeling advantages over previous studies. Unlike "baseline covariate" studies that observe marital status and other covariates only at the beginning of their longitudinal observation of mortality exposure, our linked panel survey and administrative data set allowed us to observe marital status in the year preceding mortality exposure and included both fixed and time-varying socio-demographic, economic, and health-related controls for selection into marriage. By pooling across six panels, moreover, we were able to achieve sample sizes on the scale of those of "baseline covariate" studies that link from cross-sectional surveys to the National Death Index. Our sample sizes are approximately 10 times those of previous panel-survey studies of marital status and mortality.

Our study also challenges the common practice of estimating contrasts in mortality between married individuals and three or four different unmarried groups (never-married, widowed, divorced and/or separated). In reviewing previous studies, we found that claims of differences between unmarried marital statuses are often based on weak statistical evidence, with explicit tests for differences between different unmarried marital statuses almost always absent. Little consistency, moreover, is seen in the character of the unmarried marital status differences claimed across studies. Added to this is a lack of strong and unambiguous theoretical arguments for treating different unmarried statuses distinctly. Many of the theoretical arguments advanced for the higher mortality of one unmarried category, such as loss or lack of economic or psycho-social support, are applied by different researchers to different unmarried categories.

Our own findings suggest that differentiating between unmarried groups may obscure more than it illuminates. When we simply contrasted married and unmarried individuals, our multivariate analyses provided robust estimates of a marriage survival advantage across ages and genders and in different equation specifications. When we separated the unmarried into never-married, widowed, and divorced/separated groups, the uniformity our findings of statistically significant differences from otherwise similar married individuals broke down, and did so in nonsystematic ways suggestive of sampling error rather than real differences. Statistically, breaking the unmarried group into different unmarried marital statuses reduces the sample sizes for each contrast with the married group. This results in reduced power to detect true protective effects of being married. We suggest this as the likely reason, for example, for Rogers' (1995) finding of no statistically significant unmarried mortality disadvantages for 25- to 64-year-old women, even when each of the unmarried marital status coefficients pointed in this direction.

In the absence of strong theoretical arguments for separating the unmarried groups, and without a consistent and statistically rigorous basis for such a separation from previous studies, our modeling recommendation to researchers is to begin with a single "unmarried" category and to model unmarried categories separately only after finding statistically significant contrasts between unmarried groups. Following this prescription, our empirical analyses revealed no statistically significant differences between the mortality of never-married and widowed men and women and evidence of higher excess mortality of divorced or separated men and women that was limited to older ages.

Turning to gender differences, we found a greater overall marriage advantage for men than for women, but a tapering off of the male marriage advantage into older ages. While this finding is broadly consistent with the conclusions of previous studies (see reviews by Brown and McDaid 2003; Hummer et al. 1998; and Williams and Collins 1995), our findings are again important for the stronger statistical evidence they provide. Overall we found a statistically significant survival advantage for both married men and married women, and additionally a statistically significant premium in the married survival advantage of men. This male marriage advantage is consistent with Rogers' (1995) statistically significant findings from matched death and exposure data of 25- to 64-year-olds in 1986, and with Zick and Smith's (1991) and Lillard and Waite's (1995) direction of findings from smaller panel-survey samples (without statistical tests substantiating estimated differences in magnitudes between men and women). In contrast to our findings, however, none of these studies found a consistent, statistically significant effect of marriage for both men and women. We suggest that the weaker statistical power of these studies, due to smaller sample sizes and to their having separated the unmarried into three groups, may be responsible for their not finding a statistically significant effect for women.

We also find the theoretical case for gender differences in the protective effects of marriage to be more persuasive, supported by evidence of both a greater tendency toward health-threatening behavior by unmarried men than by unmarried women (Preston 1976; Waldron 1990), greater monitoring of men's health-promoting behavior by wives than by husbands (Umberson 1992; Waldron 1990), and greater social support and social integration provided by wives to husbands than vice versa (Umberson 1987). Martikainen (1995) and Johnson et al. (2000) argued that the combination of positive effects of own employment on survival and an opposite effect of marriage on employment between men (marriage increases their employment) and women (marriage reduces their employment) may explain why working-age men gain more from marriage than do working-age women. Consistent with this argument, we found strong positive effects of working on survival for both men and women. But we also found a greater survival advantage of marriage for men than for women even after controlling for both employment and earnings.

Our study also addresses differences in the protective effect of marriage for survival by age. As in many previous studies (e.g., Sorlie et al. 1995), we found a tendency toward attenuation with age. This finding has consequences too for detecting differences by gender and between unmarried groups into older ages. In particular, the greater survival advantage for men than for women that we found in our age 25-plus sample did not hold when we restricted our sample to ages 65 and older. This is consistent with Manzoli et al.'s (2007) finding of no gender difference in their meta-analysis of mortality among elders. In one way, this makes more tentative our conclusion of greater protective effects of marriage for men than for women. Another interpretation, however, is that real differences are more difficult to detect with increasing age due to increasing sample selection biases. As with all prospective studies of mortality using household survey data, our study's findings may be biased by the omission of older individuals in institutions (typically nursing homes). Because marriage differentially keeps frailer married individuals in the household population, the effect will be to bias studies against finding a marriage protection effect at older ages (Murphy et al. 2007). Research that explicitly addresses the dual risks of exit from the household population due to death and to institutionalization (see, e.g., Henretta 2007) is suggested to understand better why estimated mortality differences between married and unmarried individuals are smaller at older ages.

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