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Marriage Advantage in Subjective Well-Being: Causal Effect or Unmeasured Heterogeneity?

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Abstract

I investigate whether the marriage advantage in subjective well-being is a true protective effect vs. being attributable to self-selection into (or out of) marriage based on pre-existing mental health. I utilize 1,240 respondents from the GSS panel, a three-wave longitudinal survey collected from 2010–2014. I use a pseudo-treatment approach to informally test for the presence of self-selection. This is followed by a fixed-effect regression analysis to eliminate its influence when estimating the marriage effect. Results support the existence of self-selection: the currently married who in later waves will be exiting marriage are already more distressed than other married respondents in wave 1. And the currently not married who in later waves will be entering marriage are not more distressed in wave 1 than those remaining continuously married. A protective effect is also supported: at any given time, net of self-selection, the currently married are less distressed than the unmarried.

Keywords

fixed effect regression; general social survey; longitudinal study; marriage; marriage advantage; subjective well-being

That married people have greater average subjective well-being compared to those in other marital statuses has been documented in several studies (Kiecolt-Glaser & Newton, 2001, and Lee, 2015 for reviews of this literature). This finding is robust enough to be referred to as the “marriage advantage” to health (Hughes & Waite, 2002). And although there are many mechanisms by which marriage can indeed foster better health (reviewed below), any such association of marriage with superior health status can also be due to unmeasured heterogeneity. Also referred to as a selection effect, this references the idea that healthier persons may be especially likely to self-select into marriage. Or, unhealthier persons may be especially likely to self-select out of marriage. If this is the case, any supposedly causal effect of marriage in bringing about an improvement in health would be spurious. There is yet a third possibility. Associations observed between current marital status and subjective well-being may reflect *processes* rather than either state or trait phenomena. That is, if, say, divorced people are unhappier than married people this might be due to the adversarial

process of uncoupling, rather than the absence of a spouse, *per se*, or the selection of unhappier people out of marriage.

Normally, separating selection from processual effects in nonexperimental studies is not possible. However, there are several approaches that have been used in prior research for teasing out a protective effect of marriage from other scenarios. For example, the most common approach is simply to control for other predictors of subjective well-being, i.e., measured heterogeneity, that would also be associated with marital status (Barrett, 2000; Lee & Bulanda, 2005). Another option is to use an autoregressive modeling strategy in a two-wave study such that wave 2 mental health is the response and wave 1 mental health is a control variable (Horwitz & White, 1998). A third possibility is to use a prospective specification in which changes in marital status are predicted based on prior health condition (Horwitz, White, & Howell-White, 1996; Joung, Van De Mheen, Stronks, Van Poppel, & Mackenbach, 1997; Lamb, Lee, & DeMaris, 2003). This is especially efficacious for examining how individuals may self-select into either marriage or divorce. A fourth tactic is to rely on econometric modeling to control for unmeasured heterogeneity. Most researchers utilizing this method have used fixed-effect regression with longitudinal data (Amato & Kane, 2011; Musick & Bumpass, 2012), although cross-sectional strategies have also been used (DeMaris, 2014).

In the current study, I re-examine the possible protective effect of marriage utilizing econometric modeling to control for unmeasured heterogeneity. This is a sequel to my previous work (DeMaris, 2014) using the same data source, the general social survey. Previously I utilized pooled data from the 2006, 2008, and 2010 cross-sectional samples along with the Heckman selection model and instrumental variable regression to explore the marriage advantage in cross-sectional data. In this study, I use the 2010–2014 GSS panel dataset to again investigate a potential marriage advantage in subjective well-being. This time I use longitudinal data and fixed-effect regression modeling to eliminate unmeasured heterogeneity. However, I also test for the presence of said heterogeneity using an informal test based on the assignment of “pseudo-treatment groups” to those in different marital statuses (Cotten, Ford, Ford, & Hale, 2014; Imbens & Wooldridge, 2009). By relying on the same basic data source but tackling the problem from two different modeling strategies, I hope to illuminate any possible protective effect of marriage should it be at play.

Theoretical background

Marriage advantage as protection

Being married is held to be advantageous to health for many reasons. Marriage entails what Cherlin (2009, p. 138) refers to as “enforceable trust.” It involves a public commitment to enter into a potentially lifelong, caring relationship with one’s partner. As it is a legally enforceable commitment, it requires substantially more effort to sunder than, say, a cohabiting relationship. Therefore the married can be relatively secure in their spouse’s obligation to support them both financially and emotionally in times of need. They also have regular access to a confidant, a companion, and a sexual partner. All of these elements may contribute to a feeling of subjective well-being (Horwitz et al., 1996; Kim & McKenry, 2002; Marks, 1996). Married people also appear to embrace healthier and less risky

lifestyles than, say, the unmarried (Pienta, Hayward, & Jenkins, 2000). This can also spill over as improvements in subjective health. Several more subtle aspects of married life are expected to enhance psychological functioning (Ross, 1995). Marriage provides social integration, nesting inside a network of social obligation, which also protects against isolation. Married people also enjoy more emotional support than singles, which reduces depression and anxiety. Due to the pooling of resources, marrieds enjoy greater economic support than others. Finally, married people have higher levels of attachment to a significant other than the unmarried (Ross, 1995).

Marriage advantage as selection

At the same time, those who are in an intact marriage at any given time are likely to be a select group. The unhealthy, whether suffering from physical or mental maladies, are unlikely to be attractive marital partners (Pienta et al., 2000). Similarly, married individuals with physical or mental health issues are more likely to be abandoned by their spouses (Blekesaune, 2008). One complication in untangling causation from selection effects in the marriage advantage inheres in the *process* of either forming or dissolving a union (Joung et al., 1997). This can manifest as a selection effect. For example, dating partners who are moving toward marriage are likely to display anticipatory health benefits by virtue of their commitment to becoming a couple. This, however, can make it appear that those who are healthier are self-selecting into marriage (Stutzer & Frey, 2006). In a similar vein, divorcing couples are likely to be experiencing considerable stress due to the conflict surrounding breakups. This can make it appear as though the mentally distressed are self-selecting out of marriage (Simon & Marcussen, 1999). With nonexperimental data, it is not possible to untangle selection from process effects (Joung et al., 1997). On the other hand, process effects would tend to dilute any apparent marriage advantage in data. This occurs because committed unmarried couples will have relatively high subjective well-being, compared to intact married couples. And couples on a dissolution trajectory will have relatively low subjective well-being among all married couples. Both of these trends would minimize differences between the unmarried and married. These caveats need to be kept in mind as this study unfolds.

Evidence to date

The literature to date provides evidence for both selection and protective effects of marriage at work. However, the findings conflict regarding certain nuances, such as whether cohabitation provides the same benefits as formal marriage, and whether women reap the same advantages as men. Several studies find that married people are happier and more satisfied with life in general, compared to those who are unmarried, including cohabitators (Brown, 2000; Diener, Gohm, Suh, & Oishi, 2000; Horwitz & White, 1998; Kim & McKenry, 2002; Lamb et al., 2003; Lee & Ono, 2012; Marcussen, 2005; Soons & Kalmijn, 2009; Soons, Liefbroer, & Kalmijn, 2009; Stack & Eshleman, 1998; Stutzer & Frey, 2006). On the other hand, some studies find no difference in the well-being advantage accruing to marriage vs. cohabitation (Musick & Bumpass, 2012; Ross, 1995; Zimmerman & Easterlin, 2006). Unfortunately, due to small cell sizes (explained below), I cannot address the benefits of cohabitation vs. marriage in the current study. Instead, I treat all unmarried individuals as belonging to the same category. As the preponderance of studies find that cohabitation does

not provide the same health advantage as marriage, this should be a reasonable approach. On the other hand, at the worst, it results in a conservative test of the marriage advantage.

In a similar vein, some studies claimed to have found a gender gap, such that men benefit more in mental health from marriage than women (Brown, Bulanda, & Lee, 2005; Gove, Hughes, & Style, 1983; Mastekaasa, 1994). However, a number of other studies have found no significant gender differences in the marriage advantage (Bierman, Fazio, & Milkie, 2006; Diener et al., 2000; Dush & Amato, 2005; Hughes & Waite, 2002; Lee & Bulanda, 2005; Simon, 2002; Soons et al., 2009; Stack & Eshleman, 1998; Williams, 2003). In the current study, I reassess whether there is a gender difference in the potential protective effect of marriage for subjective well-being.

Several studies have attempted to tackle the selection vs. protection conundrum using innovative modeling strategies. For example, Wu, Penning, Pollard, and Hart (2003) used cross-sectional data combined with the Heckman self-selection model to control for selection into cohabitation and marriage. They found both marriage and cohabitation to be equally protective for both physical and mental health. In a similar vein, I examined the pooled 2006–2010 GSS, also controlling for selection into marriage using the Heckman self-selection model. I found evidence of inverted selectivity: those likely to marry were also more prone to subjective distress. Nevertheless, a significant marriage advantage remained after controlling for unmeasured heterogeneity (DeMaris, 2014).

Some have used an autoregressive approach, in which an earlier measure of, say, depression or alcohol abuse is held constant while looking at the effect of a marital status transition on the later measure of the same outcome (Frech & Williams, 2007; Hawkins & Booth, 2005; Horwitz & White, 1998). Several of the findings reinforced the protective effect of marriage: cohabitators reported significantly more alcohol problems than the married (Horwitz & White, 1998); marriage or remarriage enhanced well-being, compared to remaining divorced (Hawkins & Booth, 2005); and transitioning to marriage resulted in a boost in well-being, compared to remaining unmarried, although this effect was greatest for those in happy marriages (Frech & Williams, 2007).

Although focused on the marital wage premium rather than the health premium, Antonovics and Town (2004) used a sample of monozygotic twin pairs in tandem with fixed-effect modeling to rule out selection effects. They found no evidence for selection effects and claimed that the marital wage premium was strictly a protective effect of marriage. Several other studies have used fixed-effect regression modeling with longitudinal data to examine the marriage advantage in well-being (Amato & Kane, 2011; Blekesaune, 2008; Musick & Bumpass, 2012; Soons et al., 2009; Stutzer & Frey, 2006; Wade & Pevalin, 2004). After accounting for selection by eliminating unmeasured heterogeneity, all of these studies found some protection due to entering into or being in a union for well-being.

Yet another large group of studies used longitudinal data to assess the extent to which pre-existing mental health determined transitions into marital statuses (Brown, 2000; Horwitz & White, 1991; Horwitz et al., 1996; Joung et al., 1997; Lamb et al., 2003; Marks, 1996) or the extent to which change in marital status was associated with health trajectories (Kim &

McKenry, 2002; Lorenz, Wickrama, Conger, & Elder, 2006; Lucas & Clark, 2006; Lucas, Clark, Georgellis, & Diener, 2003; Simon, 2002; Simon & Marcussen, 1999; Williams, 2003; Williams & Umberson, 2004; Zimmerman & Easterlin, 2006). Although most of these studies find protective effects of marriage, there are notable exceptions. For example, Joung et al. (1997) suggest that the majority of the differences in subjective health and all of the differences in chronic conditions between married and divorced people may be due to selection of unhealthier persons out of marriage through divorce. Similarly, Horwitz and White (1991) report that a portion of the association between marital status and reduced alcoholism reflects the selection of less problematic drinkers into marriage.

Current study

In sum, although several studies using various methodologies have supported a protective effect of marriage, the jury is still out on the extent to which selection plays a part. In the current study, I revisit the issue using a nationally representative longitudinal dataset and refined statistical modeling. Given that the preponderance of the evidence suggests that there is a marriage advantage in subjective well-being, I tender one primary hypothesis: *Net of unmeasured heterogeneity (or self-selection into marital statuses), being married is associated with better subjective well-being compared to being unmarried.*

Methods

Data source

For the current study, I draw on the GSS panel. This dataset includes three waves of interviews. The first wave sampled 2,044 respondents and interviewed them in 2010. A total of 1,551 cases were reinterviewed in 2012 and 1,304 cases were reinterviewed in 2014. I use only the 1,304 cases who were interviewed at all three times. Moreover, I focus on respondents who fell into one of six mutually exclusive marital status categories: married in all three waves, never married in all three waves, widowed/separated/divorced in all three waves, marrieds in the first wave who transitioned to widowed/divorced/separated status in a later wave, never marrieds in the first wave who got married in a later wave, and widowed/separated/divorced respondents in the first wave who got married in a later wave. These marital status “trajectories” are those having sufficient cell sizes for analysis (minimum n in any group was 37 respondents). Several cases were dropped for various reasons. Twenty-six cases changed gender across waves. Whether these were miscodes or actual instances of people undergoing gender transition is not known. Either way, they were not suitable for analysis and were dropped. Another 16 cases were dropped due to nonsensical marital status transitions, e.g., never married in 2010, then married in 2012, then never married again in 2014. Finally, 22 cases were dropped due to falling into a marital status trajectory different from the six focus trajectories enumerated above. The final sample size for the study is 1,240 respondents.

Measures

Outcome variable—To maintain consistency with my earlier study of the same topic using pooled GSS cross-sections, I used the same measure of subjective impairment as used in DeMaris (2014). The outcome variable was the sum of three items from the GSS tapping

well-being. The first was “Taken all together, how would you say things are these days—would you say that you are very happy, pretty happy, or not too happy?” Responses of very, pretty, and not too happy were coded 1–3, respectively. The second question was “In general, do you find life exciting, pretty routine, or dull?” Responses of exciting, pretty routine, and dull were coded 1–3, respectively. The third question was “Would you say your own health, in general is excellent, good, fair, or poor?” Responses of excellent, good, fair, and poor were coded 1–4, respectively. Because these items were in different metrics, they were standardized first, and then summed, with higher scores reflecting greater subjective life distress. Cronbach’s alpha for the scale was 0.47, 0.50, and 0.55 for waves one to three, respectively. This is admittedly low. However, the scale showed good variability and little skewness across waves and allows me to triangulate findings with my earlier study using the same scale (DeMaris). A thorough scan of the GSS panel dataset did not uncover any superior instruments for tapping subjective well-being that were measured in all three waves.

Focal predictor—The focal independent variable in the analyses was marital status. This was measured in three different ways, depending on the analysis used. One form of the variable was simply marital status at wave 1 (2010), which was coded as married, widowed, divorced, separated, or never married. A second form of marital status was used to mine for evidence of possible selection into or out of marriage (see below for details). For this purpose, respondents whose status is widowed, divorced, or separated will just be referred to as “single” for economy of expression. This version of marital status distinguished among the always married (in all waves), always single, always never married, and transition categories of married to single, single to married, and never married to married. The third version of marital status, used in fixed-effect regression, was just a time-varying dummy for being married in any given wave, as opposed to being single or never married.

Control variables—Control variables were included that were consistent with those used in similar analyses using GSS data (DeMaris, 2014; Glenn & Weaver, 1988; Lee & Bulanda, 2005; Lee, Seccombe, & Shehan, 1991). Time-varying covariates were age in years, family income in ten-thousands and religiosity. The latter was a three-item scale consisting of frequency of church attendance (coded from 0 for “never” to 8 for “several times per week”), frequency of prayer (coded from 1 for “several times per day” to 6 for “never,” but reverse-coded for the scale), and degree of belief in God (coded from 1 for “don’t believe” to 6 for “no doubts”). Because the items were in different metrics, each was standardized first, prior to summing. Alpha reliabilities for the scales were 0.79, 0.80, and 0.80 for waves 1–3, respectively. Time-invariant controls were gender (with a dummy for being female), race (with dummies for being Black or for being of another race vs. being White), and education in years of schooling at wave 1. Although education is a time-varying variable, it exhibited so little variation between 2010 and 2014 that it was unsuitable for use in time-varying form in the fixed-effect analyses. Descriptive statistics for all study variables are shown in Table 1.

Statistical analysis—The first question of interest was whether any evidence could be marshaled for selection into particular marital statuses on the basis of pre-existing levels of life distress. Toward this end, I used the pseudo-treatment approach used by other

researchers (Cotten et al., 2014; Imbens & Wooldridge, 2009) to fashion an informal test of selection effects. Called a test of unconfoundedness (Cotton et al., 2014), the idea is to create artificial or “pseudo” treatment groups of respondents prior to the “application” of a treatment. At this point, groups are distinguished from each other based on the different treatments that they will receive in the future. If there are already pseudo-treatment-group differences on the response variable prior to application of the treatment, then this constitutes evidence for selection into treatment status. In the current case, I simply divide up all respondents in wave 1 based on their *future* trajectories in marital status. I then perform an analysis of covariance to examine whether there are group differences in wave 1 life distress based on these future trajectories, controlling for relevant covariates. If so, this suggests that people are self-selecting into marital status trajectories based on pre-existing subjective well-being. For the second question of whether marriage has protective value for life distress, I use fixed-effect regression modeling to eliminate the influence of selection—aka unmeasured heterogeneity—while examining the effect of being married on life distress. All regression modeling was done using unweighted data, following the recommendations of Winship and Radbill (1994).

Missing data—There was one missing value for life distress in each of the first two waves. Otherwise, the greatest amount of missing data was for family income in wave 1, with 9.4% of the values missing. Missing data on all study variables were addressed using multiple imputation with five replications of the dataset to replace missing values. Reported regression estimates therefore represent averages across five regression models, along with appropriate adjustments to the standard errors. All analyses were accomplished using SAS, version 9.3.

Results

Table 2 presents ordinary least square estimates for the regression models of life distress as a function of marital status and covariates. Model 1 uses wave 1 marital status as the focal predictor. The purpose of this model is to examine whether there is a cross-sectional marriage advantage in subjective well-being once key demographic characteristics are held constant. We see that there is. Net of covariates, each of the unmarried groups has a significantly higher mean level of distress than the currently married category (the reference group). Controlling for marital status, we also see that Blacks are significantly higher in life distress than Whites, although this is only a marginally significant effect. On the other hand, those with more education, greater family income, and higher religiosity are all significantly less distressed than other respondents.

Model 2 uses the aforementioned pseudo-treatment marital status groups to check for evidence of selection into or out of marriage. The reference group for these comparisons is the always-married group, i.e., those who are continuously married in all three survey waves. As before, the response is wave 1 life distress. We see now that those who are either always single or always never married are significantly higher in life distress than the always married. More importantly, we see that those who will transition from married to single status over time are significantly more distressed in wave 1, compared to the married subjects who will not make a transition. This effect suggests differential selection out of

marriage, based on pre-existing life distress. However, as formerly noted, it may also reflect the stress of the process of uncoupling. In contrast, both single and never-married respondents who will transition into marriage over time are not significantly different in average life distress compared to those who are currently, and will remain, married over time. This suggests differential selection into marriage among those who are currently single or never married. That is, those unmarried individuals who will be entering marriage in subsequent waves are already evincing levels of well-being that are comparable to those of married respondents. Once again, however, this effect may simply be because people on a marriage trajectory are already benefiting in the mental health arena by virtue of having a committed life partner. Regardless of the explanation for these effects, it is clearly important to control for unmeasured heterogeneity when examining any potential marriage advantage in well-being.

Table 3 presents the results of running fixed-effect regression models to eliminate any unmeasured heterogeneity influencing the analysis. Shown are the fixed-effect estimators for time-varying covariates representing being married, age, family income, religiosity, and time period. Fixed effect estimation automatically controls for all possible time-invariant regressors.

So although the time-invariant covariates (e.g., race, education) are not in the equation, they are nevertheless controlled. Model 1 presents estimates for the additive model. It is evident that being married at any given time reduces life distress by about four-tenths of a unit, on average. The only other significant factor in the model is religiosity; the more religious are also lower in life distress, compared to others. Model 2 tests for an interaction between being married and gender to explore whether the marriage advantage applies equally well to women and men. Although gender is a time-invariant factor and cannot therefore be in the model, its interaction with marital status is time-varying, and hence can be included (Allison, 2009). The interaction effect is very nonsignificant ($p > 0.8$), suggesting that there is no gender difference in the marital advantage, net of measured and unmeasured factors. Although the marriage effect, which represents the effect for men, is only marginally significant in Model 2, its coefficient has increased slightly. The decline in significance is due primarily to the unavoidable collinearity induced by cross product terms, which in this case has increased the standard error of the coefficient by about 45%. (None of the other standard errors show any change across models, since the collinearity only affects the variables involved in the interaction.) At any rate, Model 1 is to be preferred, and, as noted, it shows a significant marriage advantage.

Discussion

In this paper, I have attempted to untangle a possible protective effect of marriage on subjective well-being from the confounding influence of unmeasured heterogeneity. Indeed there is evidence of the latter at work in that those who would soon be exiting marriage were already more distressed than their continuously married counterparts. And those who would soon be entering marriage were no different in their subjective well-being than the continuously married. These findings suggest potential selectivity into and out of marriage based on pre-existing well-being, consistent with other research (Amato & Kane, 2011;

Horwitz & White, 1991; Joung et al., 1997; Wade & Pevalin, 2004). Nevertheless, controlling for measured covariates as well as unmeasured fixed characteristics of individuals, I still find a significant marriage advantage in subjective well-being. To the extent that selection into and out of marriage has been controlled, this suggests that marriage serves to protect people from subjective distress. This is consistent with a wealth of previous studies that have found marriage to be associated with greater mental health (Brown, 2000; Diener et al., 2000; Horwitz & White, 1998; Kim & McKenry, 2002; Lamb et al., 2003; Lee & Ono, 2012; Marcussen, 2005; Soons & Kalmijn, 2009; Soons et al., 2009; Stack & Eshleman, 1998; Stutzer & Frey, 2006).

These findings, however, need to be tempered with a view toward several limitations of the current study. First, the measure of subjective well-being used, a life distress scale, exhibited poor reliability. It may be that a more refined measure of subjective distress or well-being could engender different results. I leave it to future research to explore other measures of mental health to see if the robustness of the marriage advantage persists across different instruments for the outcome. Second, fixed effect analyses only control for time-stable characteristics of respondents with time-stable effects on the response. To the extent that there are time-varying unmeasured covariates affecting subjective distress, they have not been controlled in the current study. Third, the aforementioned difficulty also rules out controlling for the processes involved in transitions into and out of marriage. That is, instead of traits of individuals representing selection into marital statuses, drivers of the supposed marriage advantage could potentially be processes associated with becoming married or leaving a marriage. As these reflect physical and emotional states that likely change over time as the relevant process unfolds, they cannot be elided from the regression equation through a fixed effect formulation (Joung et al., 1997; Simon & Marcussen, 1999; Stutzer & Frey, 2006). Future researchers may be able to tease out processual factors from selection characteristics by designing studies with innovative measurement strategies. One possibility is to use multiple waves with short time intervals to capture potentially changing relational factors as process measures that can affect well-being.

Despite these limitations, the current study is a step forward. I was able to test—albeit informally—and find evidence for unmeasured heterogeneity influencing marital status groupings. I was then able to control for such heterogeneity in testing for and confirming a marriage advantage in well-being. All else equal, if marriage has a true protective effect for well-being, it will tend to be underestimated in a study like this one. The reason, as mentioned earlier, is that processes associated with trajectories into and out of marriage have the effect of diluting differences in well-being between the currently married and currently unmarried. Only a more fine-grained study that simultaneously controls for selection characteristics as well as processual factors will be able to answer definitively whether marriage is really beneficial for mental health.

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Table 1.

Descriptive statistics for study variables.

Variable	<i>M</i>	<i>SD</i>	Min	Max
Life distress W1	0.002	2.444	−4.253	6.665
Life distress W2	−0.015	2.50	−4.174	6.739
Life distress W3	−0.014	2.50	−3.960	6.592
Widowed W1 ^a	0.075	0.263	0	1
Divorced W1 ^a	0.181	0.385	0	1
Separated W1 ^a	0.026	0.159	0	1
Never married W1 ^a	0.263	0.440	0	1
Always single ^b	0.252	0.434	0	1
Always never married ^b	0.224	0.417	0	1
Married to single ^b	0.048	0.215	0	1
Single to married ^b	0.030	0.170	0	1
Never married to married ^b	0.039	0.193	0	1
Female ^c	0.560	0.500	0	1
Black ^d	0.147	0.354	0	1
Other race ^d	0.068	0.251	0	1
Education W1	13.828	2.874	2.000	20.000
Age W1	47.753	16.300	18.000	89.000
Age W2	49.700	16.295	20.000	89.000
Age W3	51.723	16.192	22.000	89.000
Family income W1	48,436.180	40,049.520	401.500	152,927.230
Family income W2	53,496.400	48,335.350	383.000	178,712.460
Family income W3	51,670.250	44,708.190	369.500	160,742.220
Religiosity W1	−0.0007	2.515	−5.753	3.339
Religiosity W2	−0.001	2.540	−5.592	3.312
Religiosity W3	−0.001	2.540	−5.546	3.397

N varies from 1,124 to 1,240.^aMarried W1 is the reference group.^bAlways married is the reference group.^cMale is the reference group.^dWhite is the reference group.

Table 2.

Ordinary least square coefficient estimates (standard errors) for regression of wave 1 life distress on wave 1 marital status and wave 1 marital status transition categories.

Predictor	Model 1	Model 2
Intercept	1.186 ^{**} (0.423)	1.143 ^{**} (0.423)
Widowed W1	0.772 ^{**} (0.292)	
Divorced W1	0.850 ^{***} (0.195)	
Separated W1	1.311 ^{**} (0.424)	
Never married W1	0.617 ^{**} (0.194)	
Always single		1.032 ^{***} (0.187)
Always never married		0.747 ^{***} (0.205)
Married to single		0.716 [*] (0.317)
Single to married		0.613 (0.396)
Never married to married		0.411 (0.363)
Female	-0.147 (0.138)	-0.157 (0.136)
Black	0.336 [†] (0.197)	0.304 (0.197)
Other race	0.176 (0.267)	0.182 (0.266)
Education W1	-0.106 ^{***} (0.026)	-0.104 ^{***} (0.026)
Age W1	0.008 (0.005)	0.006 (0.005)
Family income W1	-0.103 ^{***} (0.021)	-0.097 ^{***} (0.021)
Religiosity W1	-0.087 ^{**} (0.028)	-0.084 ^{**} (0.028)
R^2	0.125	0.129

$N = 1,240$.

[†] $p > 0.10$.

^{*} $p < 0.05$.

^{**} $p < 0.01$.

 $p < 0.001$.

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Table 3.

Fixed effect coefficient estimates (standard errors) for the regression of life distress on marital status.

Predictor	Model 1	Model 2
Intercept	−0.255 (1.604)	−0.255 (1.604)
Married	−0.406 [*] (0.178)	−0.452 [†] (0.258)
Married [*] Female		0.086 (0.352)
Age	0.018 (0.037)	0.018 (0.037)
Family income	−0.025 (0.018)	−0.025 (0.018)
Religiosity	−0.082 [*] (0.036)	−0.082 [*] (0.036)
Year 2010 ^a	0.073 (0.159)	0.073 (0.159)
Year 2012 ^a	0.035 (0.100)	0.036 (0.100)
<i>R</i> ²	0.680	0.680

N = 1,240.

^aYear 2014 is the reference year.

[†]*p* < 0.10.

^{*}*p* < 0.05.