

More Evidence for Trends in the Intergenerational Transmission of Divorce: A  
Completed Cohort Approach using Data from the General Social Survey\*

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Forthcoming in *Demography* (MS 2009-164)

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11/22/09

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ABSTRACT

Many studies have demonstrated that the children of divorce are disproportionately likely to end their own marriages. Wolfinger (1999) showed that the transmission of divorce between generations weakened substantially for General Social Survey (GSS) respondents interviewed between 1973 and 1996; Li and Wu (2008) contend that Wolfinger's finding is a methodological artifact of the GSS's lack of marriage duration data. This article presents a completed-cohort approach to studying divorce using the GSS. The results confirm a decline in the probability of divorce transmission that cannot be explained by the right censoring bias alleged by Li and Wu. This finding contributes to an ongoing debate about trends in the negative consequences of parental divorce, as well as demonstrating a useful approach to right-censored phenomena when event history data are not available.

Social scientists have been writing about the divorce cycle, the propensity to end one's own marriage as a result of growing up in a divorced family, at least since the 1930s (for an overview see Wolfinger 2005). At least 25 studies have demonstrated that marital instability runs in families, *prima facie* evidence that divorce transmission is of widespread concern to social scientists. The possibility that some of the negative consequences of growing up in a divorced family have abated adds a whole new level of interest. For several years around the end of the twentieth century there was considerable support for rolling back the clock on easy divorce laws in order to preserve two-parent families (Nock, Sanchez, and Wright 2008). It would undercut the critics of no-fault divorce if ending a marriage no longer hurt children as much as it used to.

Ten years ago Wolfinger (1999) used data from the 1973-1996 General Social Surveys (GSS) to show that the intergenerational transmission of divorce had weakened substantially over time. Li and Wu (2008) contested this finding on methodological grounds: the ostensible decline in the probability of divorce transmission, they aver, is nothing more than a methodological artifact resulting from the failure to properly model right censoring. Using data from the National Survey of Families and Households, they found no evidence of a trend in the divorce cycle after accounting for right censoring via event history analysis.

In this paper I develop a strategy for studying divorce using GSS data based on completed marriage cohorts that obviates concerns about right censoring. This approach reveals a trend in the divorce cycle that is consistent with Wolfinger (1999) and contrary to Li and Wu (2008), thus contributing new evidence to an ongoing debate about whether the consequences of growing up in a disrupted family abated during the years that divorce became more common in America and other western nations.

## RIGHT CENSORING, THE GENERAL SOCIAL SURVEY, AND COMPLETED COHORTS

Li and Wu (2008) contend that limitations of the GSS produced a spurious decline in the divorce cycle reported by Wolfinger (1999). The GSS lacks adequate information on marriage timing to conduct event history analysis, the preferred statistical technique for right-censored phenomena like divorce (Allison 1982, 1995). Wolfinger therefore analyzed divorce transmission via logistic regression. Li and Wu claim that

. . . if divorce rates are identical across marriage cohorts, more respondents in earlier marriage cohorts would be observed to divorce relative to respondents in later marriage cohorts simply by virtue of longer exposures to the risk of divorce. If Wolfinger's controls for exposure to risk are inadequate, his conclusion that divorce transmission has declined could be a methodological artifact (p. 875).

It may well be true that divorce is more fully observed for earlier marriage cohorts, but this by itself would not produce a spurious decline in the intergenerational transmission of divorce because people from both divorced and intact families of origin would be affected. For Wolfinger (1999) to be incorrect, the probability of right censoring would had to have disproportionately increased for the children of divorce. Li and Wu do not consider this issue. Instead, they reproduce Wolfinger's finding using data from the National Survey of Families and Households—albeit without Wolfinger's controls for duration dependence—then explain it away as an artifact of improper adjustment for right censoring. The lesson to be learned from Li and Wu is that right censoring can induce bias if not properly accounted for (though demonstrating

right censoring bias using the National Survey of Families and Households does not prove it exists for the GSS data analyzed by Wolfinger.)

Li and Wu do establish the need for an analytic strategy that can rule out any suspicion of bias. The solution is an analysis of completed marriage cohorts. After about four years of marriage the hazard rate for divorce declines monotonically (Diekmann and Mitter 1984; Goldstein 1999). After 30 years the hazard is negligible.<sup>1</sup> By this point most couples inclined to call it quits will have done so. Since the GSS measures age at first marriage it is possible to identify respondents for whom 30 years have passed since the time they first wed. At this point a marriage cohort is essentially completed—either first marriages have long since dissolved or their participants are probably together for life—allowing researchers to study trends in the intergenerational transmission of divorce without worrying whether right censoring is affecting the results.

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<sup>1</sup> The probability of divorce increases during the first four years of marriage, presumably the time when spouses determine whether they are compatible. Thereafter the exit costs steadily mount as spouses accumulate personal, familial, social, and economic reasons for staying together. Although strictly speaking marriage cohorts are only fully completed upon the death of all involved parties, few couples consider divorce after thirty years of marriage. In support of this point I analyzed event history data on first marriage duration from the 1995 Current Population Survey's Marriage and Fertility Supplement (N = 34,698). At thirty years of marriage the monthly hazard rate for dissolution is .004.

## METHODS

### Data

This research uses data from the General Social Survey (GSS) (Davis and Smith 2007). The GSS, a national probability sample of English-speaking households within the continental United States, has been conducted annually or biennially since 1972. Within each household an adult ages 18-89 is randomly selected as the respondent. I use data for the years 1973-1994, excluding the Black oversamples in 1982 and 1987. After 1994 the GSS ceased inquiring about age at first marriage, and did not do so again until 2006. The 1972 survey did not adequately measure family structure of origin.

Analysis is limited to respondents for whom at least thirty years has passed since they first married ( $N = 7,226$ ). Cases with missing data are deleted listwise except for parental education (an additional dummy is coded for missing data) and occupational status (missing data are set to the sample mean with a dummy for missing data).

### Variables

The dependent measure in all analyses is whether a respondent reports ever having been divorced (summary statistics for all variables appear in Table 1). A single dichotomous measure was formed by merging information from two questions, one inquiring whether respondents have ever been divorced and the other querying respondents about current marital status. Never-married respondents are excluded from the analysis; previous research suggests that differential

selection into marriage cannot explain trends in the divorce cycle (Wolfinger 2005: Appendix B). Unfortunately the GSS does not have adequate data for event history analysis of divorce (including event history analysis with time-varying covariates).

Table 1 Here

The GSS includes two items that measure the structure of respondents' families of origin. Respondents were first queried about household composition at age 16. If respondents were not living with both biological parents, a second item ascertained the reason. Following Wolfinger (1999), my analysis is based on GSS respondents who reported the three most common varieties of family structure: intact two-parent families, mother-only families resulting from divorce or separation, and mother/step-father families resulting from divorce or separation. Respondents reporting other living arrangements are omitted from the sample, as were those whose living arrangements at age 16 were the product of parental military service, parental incarceration, or parental death. The family structure items are recoded as a single dummy variable measuring whether a respondent hailed from a divorced family (including stepfamilies).

Analyses include continuous variables measuring three different dimensions of time: marriage cohort, birth cohort, and survey year. Wolfinger (1999) uses survey year as the temporal index for studying trends in the divorce cycle; Li and Wu (2008) use marriage cohort. I present regression results based on different combinations of these three variables. None of the three are mean-centered. Doing so produces virtually identical results. No model contains both birth cohort and marriage cohort given their high correlation ( $r = .92$ ). Alternate model specifications are discussed in greater detail in the appendix.

On average, adults reared in nonintact households complete fewer years of schooling (McLanahan and Sandefur 1994) and do less well vocationally (Biblarz and Raftery 1993). To

ascertain whether trends in the divorce cycle are the result of diminished socioeconomic well-being, I use three variables, occupational prestige for respondents and education for both respondents and their parents. For respondents reared in intact families and step-families the higher level of education between the two parents is used. For people from mother-only families I use mothers' education. Measures of income or occupational status for respondents' parents would be helpful but are not available. An item that asks respondents to recall their families' economic well-being almost certainly fails to provide accurate recollections.

Researchers have shown that various other factors may affect the relationship between parental divorce and respondent divorce. I ascertain whether the following affect trends in the probability of divorce transmission: race (Bumpass, Martin, and Sweet 1991; Glenn and Kramer 1987; McLanahan and Bumpass 1988), presence of siblings (Mueller and Pope 1977), Catholicism (McLanahan and Bumpass 1988), rural origins (Pope and Mueller 1976), age at marriage (Wolfinger 2003a, 2005), and gender (Amato 1996; Glenn and Kramer 1987; Kulka and Weingarten 1979). Controlling for gender is especially important because men often fail to report their own divorces (Bumpass, Martin, and Sweet 1991).

## Analysis

I estimate logistic regression models assuming the following general form:

$$\log(p / 1 - p) = \beta_0 + \beta_1 \text{DIV} + \beta_2 \text{TIME} + \beta_3 \text{TIME} * \text{DIV} + \beta_4 \text{CONTROL}, \quad (1)$$



where  $p$  is the probability of respondent divorce, DIV is the dummy measuring whether respondents hail from divorced families, TIME is survey year, marriage cohort, or birth cohort, and CONTROL represents miscellaneous control variables. The interaction between family background and survey year, marriage cohort, or birth cohort allows for exploration of trends in the divorce cycle; previous research indicates that the functional form of the decline in divorce transmission is linear (Wolfinger 1999, 2005). Robust standard errors based on primary sampling units are reported to account for the cluster-sample design of the GSS.

## RESULTS

The logistic regression analysis of completed marriage cohorts appears in Table 2. Model 1 follows the lead of Wolfinger (1999) by using survey year as the temporal index measuring trends in the divorce cycle. All variables in this model are statistically significant, most notably the interaction between parental divorce and survey year. As in Wolfinger, the negative coefficient for this interaction indicates that divorce transmission has declined over time for GSS respondents in completed marriage cohorts. The magnitude of the decline can be obtained by substituting values for the year variable into the following equation, derived from the parameter estimates shown for Model 1:

$$\text{odds of divorce transmission} = \exp(72.421 - .036 * \text{SURVEY YEAR}) \quad (2)$$

For 1973, the equation yields an odds ratio of 4.03. This indicates that GSS respondents from divorced families interviewed in 1973 were about four times more likely to report a personal

divorce than were respondents who lived with both biological parents at age 16. By 1994, this ratio had declined to 1.89. These figures represent a larger decline in the intergenerational transmission of divorce than was reported by Wolfinger (1999). *Based on completed marriage cohorts, this result cannot be an artifact of the right censoring bias alleged by Li and Wu (2008).* Furthermore, the data span many years: birth cohorts from 1884 to 1948, marriage cohorts from 1901 to 1964, and GSS waves from 1973 to 1994.

Table 2 Here

Survey year is not the best index for trends in divorce transmission in an analysis of completed cohorts, given that the period of high divorce risk in a marriage has long since passed (Diekmann and Mitter 1984; Goldstein 1999). Model 2 offers results based on the interaction between marriage cohort and parental divorce. Survey year is also included in order to account for survey-specific change. Model 3 omits survey year, while Model 4 shows results based on the interaction between birth cohort and parental divorce. In all three cases the interaction term measuring trends in the intergenerational transmission of divorce is negative and statistically significant at the  $p < .10$  level. These results show that the trend in the divorce cycle is robust to alternative model specifications.

It might be argued that statistical significance at .10 is less than impressive, but the point of the analysis of completed cohorts is not to produce the definitive assessment of trends in divorce transmission based on the GSS; this has already been accomplished by Wolfinger (1999). Instead, my objective is to rule out the possibility that the trend in the divorce cycle is a product of improper controls for right censoring as alleged by Li and Wu (2008). This is amply demonstrated by the significance tests in Models 1-4: these tests should have been nowhere near significance if Li and Wu (2008) had been correct in their criticism of Wolfinger (1999).

Moreover, it could be argued that one-tailed tests are appropriate for my analysis given my directional hypothesis: divorce transmission has declined over time. If one-tailed tests are used, my results in Models 1-4 are all significant at the .05 level.

Model 5 introduces a variety of social and demographic variables into the analysis. These variables attenuate the effect of parental divorce on offspring marital stability, as the interaction between marriage cohort and parental divorce becomes nonsignificant. This finding is consistent with the argument that the etiology of divorce transmission is partially attributable to social differences between respondents (Amato 1996; Wolfinger 2005).

## DISCUSSION

Although my analysis of completed cohorts provides compelling evidence that the divorce cycle abated, it is important to acknowledge what others have found. Four American studies (Amato and Cheadle 2005; Li and Wu 2008; McLanahan and Bumpass 1988; Teachman 2002) show that divorce transmission has remained stable over time.<sup>2</sup> Teachman provided the strongest test, analyzing multiple waves of the National Survey of Family Growth; McLanahan and Bumpass employed a single wave of this survey. Amato and Cheadle used a sample of parents and children from the Marital Instability Over the Life Course survey. On the other hand, Engelhardt, Trappe, and Dronkers (2002) found equivocal evidence of a decline in divorce

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<sup>2</sup> A cross-national study of mainly European countries also failed to find evidence of a trend in divorce transmission (Dronkers and Härkönen 2008). This finding was relegated to a single sentence, so it is difficult to evaluate fully. The authors combine data for eighteen countries with different divorce rates, which may obscure possible trends in the divorce cycle.

transmission in Germany. Diekmann and Engelhardt (1999) provide somewhat stronger evidence of a decline, also in Germany.

Additional evidence of the weakening of the negative consequences of parental divorce exists for outcomes besides its transmission between generations. Kulka and Weingarten (1979) found that parental divorce had fewer negative effects on survey respondents interviewed in 1976 than it did for a comparable sample from 1957. Amato and Keith's (1991) meta-analysis of almost 100 studies found that the average negative effect of parental divorce on offspring well-being has weakened over time.<sup>3</sup> More recently, Wolfinger (2003b, 2005) showed that rates of teenage marriage for the children of divorce declined disproportionately faster than they did for offspring from two-parent families. This by itself should prolong the marriages of people from divorced families. Teenage marriage is strongly correlated with divorce; in turn, age at marriage can itself account for a portion of the divorce cycle (Wolfinger 2003a, 2005). The final piece of evidence is indirect. Dronkers and Härkönen (2008) recently found that the probability of divorce transmission varied inversely by the level of parental divorce across eighteen European countries. In other words, the divorce cycle was strongest when parental divorce was least common. This is analogous to Wolfinger (1999), with the prevalence of parental divorce varying by country instead of historical time.

Taken together, existing studies reveal profound dissensus about the possibility that divorce transmission has abated over time. At the same time, there are enough studies suggesting a decline in the negative consequences of parental divorce that it is hard to deny that no such decline has taken place. My disagreement with Li and Wu (2008) should be read in this

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<sup>3</sup> A later meta-analysis found that effect sizes have since increased (Amato 2001). Wolfinger (2005) proposes an explanation of why Amato's two meta-analyses produced conflicting results on this point.

context. Based on the present study, there is little doubt that the GSS is showing a decline in the negative consequences of growing up with divorced parents. Other studies using GSS data also provide evidence of a decline (Wolfinger 1999, 2003b, 2005). However, studies based on most other data sets suggest that the divorce cycle has not abated.

How can this discrepancy be explained? Although the nature of the differences between data sets cannot be known with certainty, one possibility concerns the extremely long time frame covered by the GSS. As noted earlier, my sample contains 63 years of marriage cohorts; Wolfinger's (1999) sample spanned 92 years of marriages. Many 19<sup>th</sup> century births are represented in these data. Perhaps this is the difference from other data sets that fail to evince the decline in divorce transmission reported by this paper and Wolfinger (1999).

The literature on divorce is replete with conflicts, controversies, and unresolved issues. For instance, over twenty-five studies have produced conflicting evidence as to whether parental divorce leads to earlier or later marriage among offspring, or has no effect at all (Wolfinger 2003b, 2005). Fifteen studies suggest that parental divorce leads to earlier marriage, while seven others find that parental divorce delays marriage; still others find no relationship. The question of trends in the divorce cycle is similarly conflicted.

The ideal solution is more research with new data that combines the best features of existing data sets. A repeated survey, like the General Social Survey, allows analysts to better separate age, period, and cohort effects in the intergenerational transmission of divorce; retrospective data based on a single cross-section, like the sample analyzed by Li and Wu (2008), allow researchers less insight into the temporal dynamics of divorce transmission (Mason and Wolfinger 2001). The ideal data set should have sufficient information for event history analysis. It should also offer details on family structure backgrounds of married couples, not just

individual respondents. The family histories of both spouses contribute equally to the likelihood of divorce transmission (Amato 1996; Wolfinger 2003a, 2005). Furthermore, the divorce cycle is strongest for people who experience multiple family structure transitions in their families of origin (Wolfinger 2000, 2005). An optimal exploration of trends in the intergenerational transmission of divorce would take all these factors into account.

## CONCLUSION

The jury is still out on the extent to which the consequences of parental divorce have diminished over time. Nonetheless, my analysis provides incontrovertible evidence of a decline in divorce transmission according to General Social Survey data. Based on completed cohorts, this analysis establishes that trends in the divorce cycle cannot be attributed to the absence of proper event history data (*pace* Li and Wu 2008). In addition, the GSS shows that the effect of parental divorce on offspring marriage timing has also weakened (Wolfinger 2003b, 2005).

This paper has also demonstrated a completed cohorts approach to studying marital stability. In the absence of event history data, this technique can be used for exploring how individual characteristics affect the probability of divorce—and potentially other right-censored phenomena—without having to take right censoring bias into account. The completed cohorts approach indeed has a long-standing precedent in the demographic literature, the study of completed fertility (e.g., Bumpass and Westoff 1969). It may also be applicable to studies of marriage timing, given that the probability of getting married for the first time asymptotically approaches zero past a certain age (Goldstein and Kenney 2001).

There are at least two obvious shortcomings to the completed cohort approach to divorce. First, it offers no insight into how trends in the divorce cycle have changed in recent years. Second, the results may be influenced by selective mortality. The work of Linda Waite and others (e.g., Waite and Gallagher 2000) demonstrates that divorced people die younger; it has also been shown that parental divorce decreases life expectancy (Schwartz *et al.* 1995). However, selective mortality is a generic problem that affects much social research.

## Appendix. Alternate Analytic Specifications.

The difference between age and age at first marriage, employed by Wolfinger (1999, 2005) as a control variable, is omitted given that all respondents are at least thirty years removed from the time they first married. Also, this variable is highly correlated ( $r = -.81$ ) with marriage cohort.

Age is highly correlated with marriage cohort ( $r = -.73$ ) and accordingly omitted. Its inclusion does not substantially affect results. I experimented with other alternate specifications involving temporal variables: Omitting marriage cohort in lieu of survey year, including time since first marriage and survey year instead of marriage cohort, including two-way interactions between marriage cohort, parental divorce and survey year in an attempt to further control for duration dependence (see Wolfinger 2005: Appendix A), and varying the definition of a completed marriage cohort from 25 to 40 years. In each case a decline in divorce transmission persisted.

It could conceivably be argued that GSS respondents from divorced families are somehow over- or under-represented in the sample of completed cohorts in a way that produces a spurious decline in divorce transmission. The time elapsed since the date of first marriage is two years greater for people from intact families, perhaps reflecting trends in marriage timing for the children of divorce (Wolfinger 2003b, 2005). Could this have any effect on my results? It seems unlikely given the miniscule hazard of marital dissolution after thirty years—two fewer years of exposure at this point could not substantially affect the trend in divorce transmission. Nevertheless, I explored this possibility by selectively redefining the definition of a completed marriage cohort for people from divorced and intact families by up to five years in both



directions (i.e., alternately higher and lower entry ages for people from divorced and intact families). In all cases, the trend in the divorce cycle persisted.

In a working paper version of their *Demography* article, Li and Wu (2006: 37) present a reanalysis of Wolfinger (1999) that supposedly establishes that any trend in divorce transmission based on the GSS is a methodological artifact. They report the results of sixteen logistic regression models using Wolfinger's (1999) GSS sample and methods. The models analyze successively smaller intervals of exposure to the risk of divorce:

<u>Model</u>	<u>Exposure time (in years)</u>
1	0-32
2	2-32
3	4-32
...	
14	26-32
15	28-32
16	30-32

The first eight models reveal a trend in the divorce cycle consistent with my paper and Wolfinger (1999). After Model 8 (exposure time = 16-32 years), the negative regression coefficient denoting a trend in divorce transmission loses statistical significance. Is this evidence against my finding of a trend in the divorce cycle based on the GSS? In response, I point out that I agree with Li and Wu (2006: 9) about the crudity of their analysis: it is arbitrary in its choice of

intervals of exposure time. Why should Model 16 use 30-32 years as opposed to 28-30 years, 32-34 years, or any other two year window of data?

I reanalyzed Wolfinger's (1999) sample, replicating Li and Wu's Model 16 for all two year intervals between zero and 60 years. According to the logic of Li and Wu's (2006) Table 4, two year intervals should not depict trends in the divorce cycle because such trends are assumed to be artifacts of longer exposure times. Yet four of the intervals I analyzed indeed suggest large and statistically significant trends. The trends approach significance in two other models.

More broadly, my analysis of two year intervals is not evidence for or against a trend in the divorce cycle based on the entire GSS sample. Many of these intervals have sample sizes too small for trends in divorce transmission to be emergent. My point here is simply that shortening the exposure duration for GSS data does not necessarily make observed trends in divorce transmission go away. As Li and Wu (2006: 9) seem to concede, "The[se] analyses . . . using the GSS [General Social Survey] are relatively crude because assessing the sensitivity of results to different durations of exposure required relying on different GSS subsamples."

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Table 1. Summary Statistics.

Variable	Mean/ percentage	S.D.	Min.	Max.
Respondent has ever divorced	.233	.423	0	1
Respondent from divorced family	.053	.223	0	1
Survey year	1983.413	6.413	1973	1994
Marriage cohort	1940.364	10.754	1901	1964
Birth cohort	1918.358	11.213	1884	1948
Non-urban at age 16	.711	.453	0	1
Only child	.049	.217	0	1
Male	.376	.484	0	1
Black	.082	.274	0	1
Catholic	.214	.410	0	1
Occupational prestige	39.413	12.106	12	82
Occupational prestige missing	.229	.420	0	1
Age at first marriage	22.005	4.377	12	49
Parent education				
Less than H.S.	65%	--	--	--
H.S. graduate	22	--	--	--
Junior college	1	--	--	--
College graduate	3	--	--	--
Post graduate	2	--	--	--
Data missing	7	--	--	--
Respondent education				
Less than H.S.	43%	--	--	--
H.S. graduate	44	--	--	--
Junior college	2	--	--	--
College graduate	7	--	--	--
Post graduate	4	--	--	--

Notes: N is 7,226.

Source: General Social Surveys, 1973-1994.

Table 2. Logit Analysis of Respondent Divorce on Parental Divorce, Survey Year, and Marriage Cohort for Completed Cohorts.

Variables	Model 1	Model 2	Model 3	Model 4	Model 5
Respondent from divorced family	72.421* (32.662)	32.744 <sup>†</sup> (19.068)	32.333 <sup>†</sup> (19.049)	33.836 <sup>†</sup> (18.604)	31.456 (19.988)
Survey year	.016* (.007)	.013 <sup>†</sup> (.007)	--	.001 (.007)	.017* (.008)
Divorced family*survey year	-.036* (.016)	--	--	--	-- (.017)
Marriage cohort	.025*** (.003)	.026*** (.010)	.026*** (.003)	--	.030*** (.003)
Marriage cohort*survey year	--	-.016 <sup>†</sup> (.010)	-.016 <sup>†</sup> (.010)	--	.016 (.010)
Birth cohort	--	--	--	.042*** (.003)	--
Birth cohort*survey year	--	--	--	-.017 <sup>†</sup> (.010)	--
Parent education					
Less than H.S.	--	--	--	--	--
H.S. graduate	--	--	--	--	.183* (.076)
Junior college	--	--	--	--	.017 (.325)
College graduate	--	--	--	--	.150 (.165)
Post graduate	--	--	--	--	.610** (.216)
Data missing	--	--	--	--	.292** (.114)
Non-urban at age 16	--	--	--	--	-.183** (.064)
Only child	--	--	--	--	.132 (.124)
Male	--	--	--	--	.172** (.063)
Black	--	--	--	--	.045 (.117)
Catholic	--	--	--	--	-.406*** (.081)
Respondent education					
Less than H.S.	--	--	--	--	--
H.S. graduate	--	--	--	--	-.124 <sup>†</sup> (.068)
Junior college	--	--	--	--	.370 <sup>†</sup> (.193)
College graduate	--	--	--	--	-.165 (.140)
Post graduate	--	--	--	--	-.353 <sup>†</sup> (.182)
Occupational prestige	--	--	--	--	-.003 (.003)
Occupational prestige missing	--	--	--	--	-.245** (.087)
Age at first marriage	--	--	--	--	-.111*** (.009)
Constant	-80.588** (12.437)	-77.735*** (12.444)	-59.626*** (6.000)	-79.581*** (12.473)	-92.706*** (14.371)
Log likelihood	-3824.05	-3825.07	-3828.34	-3765.48	-3674.86

Notes: N is 7,226.

Cluster-adjusted robust standard errors are in parentheses.

Source: General Social Surveys, 1973-1994.

<sup>†</sup>p < .10; \*p < .05; \*\*p < .01; \*\*\*p < .001 (two-tailed tests)