

# **Marital Dissolution and Women's School Investment**

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## **Abstract**

This study examines the impact of marital dissolution on women's school investment. Using the National Longitudinal Survey of Youth 1979 Cohort, I use a semiparametric model to estimate both short-term and long-term effects of marital dissolution on women's school enrollment and educational attainment. The results indicate that women's school enrollment increases by 28% three years after marital dissolution and that the impact of marital dissolution persists 8 years after marital disruption. The impact of marital dissolution is largest for women with an education of a high school diploma or less. Furthermore, the share of income generated by the husband during marriage is positively associated with the magnitude of the marital dissolution effect. This suggests that the dissolution of household specialization tends to lead to school enrollment in the event of marital disruption. Finally, and perhaps most importantly, the results suggest that divorced women are successful in converting school enrollment into completed years of education. I show that divorced women begin to experience an increase in completed years of education 6 years after marital dissolution, primarily because many of these women are part-time students.

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## 1 Introduction

This study examines the impact of marital dissolution on women's school investment. Marital dissolution continues to be a destabilizing event for many women. As of 2007, more than 40% of marriages were expected to end in divorce (Wilcox, 2009). In general, the economic consequences of marital dissolution are severe, with women bearing much of the hardship.<sup>1</sup> On average, women who remain single after a divorce experience a 14% decline in per capita income (McKeever and Wolfinger, 2001). This decline likely exerts pressure on women to increase their income in order to maintain their pre-divorce standard of living. In addition, if intra-marriage specialization is important, one should expect adjustments to human capital investment for women after their marriage dissolves. Numerous studies address changes in income and labor supply following divorce, but changes in education have been ignored.

In this study, I use the National Longitudinal Survey of Youth 1979 (NLSY79) Cohort to investigate the impact of marital dissolution on women's school investment.<sup>2</sup> First, I estimate difference-in-difference models with nonparametric leads and lags that allow me to examine the time pattern of the impact of marital dissolution on school investment. In each round, NLSY79 respondents report changes in their marital and school enrollment status. Therefore, I am able to trace out the time pattern of marital dissolution effects on school enrollment in a manner that puts little structure on this pattern. In addition, by examining pre-divorce trends, I can determine whether there is any evidence that the timing of marital dissolution is endogenous to the school enrollment decision.

Second, I examine changes in completed years of schooling following marital dissolution. Previous studies suggest that nontraditional students are more likely to quit school

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<sup>1</sup> There are a few studies that have not concluded that women, on average, suffer economic hardship following marital dissolution. For example, Bedard and Deschenes (2005) use the sex of the first born child as an instrumental variable for divorce and data from the 1980 U.S. Census to show that ever-divorced women have significantly higher levels of adjusted household income. Nevertheless, using Quantile Treatment Effect methodology and the same instrument for marital disruption as Bedard and Eschenes, Ananat and Michaels (2008) show that marital dissolution increases the variance of income. In other words, marital disruption leads some women to have higher incomes as well as some women to be poor, which exacerbates poverty and inequality (Ananat and Michaels, 2008).

<sup>2</sup> In this paper, marital dissolution is defined as divorce or marital separation.

than their traditional counterparts.<sup>3</sup> Therefore, I investigate whether post-divorce school enrollment translates to completed years of school.

Finally, I estimate a more parsimonious model in order to examine how the impact of marital dissolution differs by the woman's characteristics. For instance, in the event of divorce, women with education of high school diploma or less may find it optimal to obtain more education rather than increase hours of work. Therefore, to estimate differential effects, the marital dissolution indicator is interacted with individual and household characteristics.

My findings provide evidence that marital dissolution leads to substantial increases in women's school investment. In particular, the results indicate that marital dissolution increases the probability that women enroll in school by 2.5 percentage points (or 28%) 3 years after marital dissolution and that the impact of marital dissolution remains positive 8 years after marital disruption. I find that the impact of marital dissolution is largest for women with an education of a high school diploma or less. Furthermore, the share of income generated by the husband during marriage is positively associated with the magnitude of the marital dissolution effect. This suggests that the elimination of household specialization tends to lead to school enrollment in the event of marital disruption.

I also find that divorced women are successful in converting school enrollment into completed years of education. I show that divorce has a positive impact on completed years of education that grows until it becomes statistically significant 6 years after marital dissolution, with the slow growth primarily because many of these women are part-time students.

The large majority of studies that examine the economic consequences of marital dissolution find that women experience serious reductions in post-divorce income. For example, Burkhauser et al. (1991) compare the United States with Germany using data from the Panel Study of Income Dynamics (PSID) and the German Socio-Economic Panel, respectively. They find that after divorce women and children suffer greater income losses than men in both countries. Bianchi et al. (1999) use Survey of Income and Program Participation (SIPP) data from the 1980s and early 1990s and find that divorced women who are the custodial parent have needs adjusted income levels at 56% of their former spouse's income levels. More recently, McKeever and Wolfinger (2001) use data from the National Survey of Families and Households

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<sup>3</sup> A traditional college student is a young person who enrolls in college immediately following high school graduation, enrolls as a full-time student, relies on parental support to finance college costs, and plans to complete a baccalaureate degree in four years (Seftor and Turner, 2002).

(NSFH) to show that women who remain single after a divorce, on average, experience a 14% decline in per capita income. Thus, although the labor force participation rate of married women has increased in recent years, it remains true that women tend to experience a decline in economic wellbeing following divorce.

This decline in economic well-being likely exerts pressure on women to increase their income. One clear way to do so is to increase their labor supply. Indeed, numerous studies examine changes in women's labor supply following divorce. Johnson and Skinner use the PSID to investigate the effect of marital dissolution on the labor supply of women. They find that women's average labor supply increases from 1024 hours one year prior to marital dissolution to 1551 hours four years following the separation. Haurin (1989) uses the National Longitudinal Survey's mature women's cohort to measure reactions of a married woman's labor supply to shocks to her household, and finds in a dynamic choice model that divorced and separated women increase their work hours shortly after marital dissolution. Finally, Couch et al. (2011) use the 2004 SIPP panel and records from the Social Security Administration to assess the implications of divorce for women's earnings. Consistent with previous studies, they find positive effects of marital dissolution on women's earnings.

Human capital investment is an alternative, or possibly complementary, channel for raising earnings after divorce. Women lacking sufficient labor market skills or experience may find it difficult to acquire employment that compensates for the loss of the husband's income. For example, Bianchi et al. (1999) show that the post-dissolution gender gap in earnings is lower if the wife was a full-time worker and above-average earner during marriage.

The loss of income that follows marital dissolution can be somewhat offset by child support and remarriage. Nevertheless, in many cases, these alternative sources of income fail to compensate for the loss of income that follows divorce (Bartfield, 2000; Waller and Plotnick, 2001). For divorced women with children, the child support program provides services to assure they have the resources needed to support their children. Nevertheless, it is often the case that poor women who are eligible for child support fail to receive it. For example, Sorensen and Zibman (2000) report that in 1996, only 29% of poor children who had a parent living elsewhere received child support. Moreover, Peters et al. (1993) find that compliance with child support orders varies from month to month, and that informal modifications change in response to economic circumstances.

Remarriage is yet another way to alter the economic consequences of marital dissolution for women.<sup>4</sup> Bianchi and McArthur (1991) find that children whose mothers remarried or reconciled had nearly twice the income-to-needs ratio as those who remained in mother-only families. However, remarriage is a less successful strategy for low-educated divorced women. Smock (1990) uses data from the National Survey of Families and Households and finds that educational attainment is positively associated with remarriage among black women. For white women, education has minimal or no systematic relationship to the likelihood of remarriage (Smock, 1990).

The literature has mostly been silent on the impact of marital dissolution on women's school investment.<sup>5</sup> This is surprising because nontraditional students now occupy a considerable portion of college classrooms. The percent of enrolled college students over the age of 30 increased from 15% in 1970 to 29% in 1997 (Lalumia 2011). In 2007, students age 35 and older accounted for nearly 17% of students enrolled in degree granting institutions and about 32% of part-time students (U.S. Department of Education, 2009). Perhaps more significantly, adult school enrollment is associated with an increase in wages. Leigh and Gill (1997) find that the average community college student (with no four-year college experience), who enrolls but does not attain a degree earns 9 to 13% more than the average high school graduate with similar high school grades and/or test scores between the age of 29 and 38. Other studies find that each year of credit at a community college is associated with a 5-8% increase in annual earnings—the same as the estimated value of a year's worth of credit at a four-year college (Monk-Turner, 1994; Grubb, 1995; Kane and Rouse, 1995). In a more recent study, Jacobson et al. (2005) utilize administrative data on displaced workers from the state of Washington to estimate the wage effects of community college enrollment. They find that community college retraining increases the wages of older women by 10%.

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<sup>4</sup> Cohabitation is perhaps a less reliable avenue for divorced women to increase their standard of living. Winkler (1997) finds that, as a group, cohabitators do not pool income. Income pooling is more likely for couples that have a biological child and for those in longer-term relationships (Winkler 1997). In contrast, Kenney (2004) finds that cohabiting couples generally pool resources. Nevertheless, cohabitation appears to be negatively related to education. For example, Lichter and Qian (2008) find that serial cohabitations, which are less likely to end in marriage, are more likely among women with low education.

<sup>5</sup> Stevenson (2007) considers how divorce laws affect the incentives for couples to invest in their marriage by focusing on the impact of unilateral divorce laws on investment decisions of couples in their first 2 years of marriage, using the 1970 and 1980 censuses. The results show that newlywed couples in states that allow unilateral divorce are about 10% less likely to be supporting a spouse through school.

The remainder of the paper is organized as follows: Section 2 provides a theoretical framework for analyzing the relationship between divorce and women's school investment. The data are described in Section 3. Section 4 contains the empirical model as well as assumptions underlying the identification of the parameter estimates. Results are presented in Section 5, and Section 6 concludes.

## **2 Theoretical Framework**

A priori, it is unclear how marital dissolution will affect the school investment of divorced women. Economic models of divorce developed by Becker et al. (1977), Landes (1978), and Peters (1986) argue that divorce occurs when joint marital satisfaction is less than the joint level attainable by each partner separated. In the event of divorce, the loss of specialization in the household increases time devoted to the market and would then increase returns to investing in marketable human capital. Thus, women's returns to education may increase due to marital dissolution.

Even if the returns to education were not higher in the divorced state, marital dissolution may affect school enrollment by changing women's ability to pool risk. For example, in a marriage the husband may increase labor supply if the wife is ill or unemployed.<sup>6</sup> Thus, divorce may cause some women to seek jobs less affected by unemployment because they no longer have the husband's entire earnings as insurance. This is a real concern for less educated individuals, who are more likely to be dismissed from their job than more educated individuals (Campbell III, 1997).

Marital dissolution may also have a nonpositive effect on women's school enrollment. Because men are unable to control the allocation of resources by women if they live apart, men are likely to contribute less income to the woman's household when divorced as compared to the when married (Weiss & Willis, 1985). Additionally, the economies of scale that women experience in marriage is no longer realized in the divorced state. Therefore, women may have less income that is available to apply to schooling costs. The loss of income and economies of scale following a divorce changes the woman's budget constraint and disposable income, which

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<sup>6</sup>Good medical or unemployment insurance makes this less of a concern (Weiss, 1997).

may have a negative impact on school enrollment in the presence of credit constraints. Several studies provide evidence that credit access is a determinant of college enrollment. For example, Belley and Lochner (2007) use the NLSY79 and NLSY97 and show an increase over time in the effects of family income on college attendance. Their theoretical model suggests that borrowing constraints are responsible for the growing effects of family income on college attendance. In addition, Lovenheim (2011) finds evidence that housing wealth influences school enrollment of 18-22 year olds, which he suggests is consistent with a credit constraint explanation.<sup>7</sup>

These predictions of the impact of divorce on women's school investment are not mutually exclusive. Divorce could simultaneously increase returns to education, alter risk pooling, and reduce the amount of income. To the extent that these outcomes have differential effects on school enrollment, simple models that examine divorce and schooling do not yield unique predictions about the impact of divorce on women's school investment. Consequently, it is necessary to analyze empirically the effect that divorce has on women's school enrollment.

### **3 Data**

The empirical analysis uses data on women drawn from the 1981-2008 panels of the NLSY79. The NLSY79 is a nationally representative sample of 12,686 men and women who were 14-22 years old during the first round of interviews in 1979. The sample members were interviewed annually until 1994 and are currently interviewed biannually.<sup>8</sup> The NLSY79 cohort is comprised of three subsamples: a cross-sectional sample of 6,111 respondents designed to represent the non-institutionalized United States population; a supplemental sample of 5,295 Hispanic, black, and economically disadvantaged nonblack/non-Hispanic respondents; and a sample of 1,280 respondents constructed to represent the population serving in the United States military as of September 30, 1978. Because approximately 1,100 military sample members and all economically disadvantaged, nonblack/non-Hispanic sample members were ineligible for interview after 1984 and 1990, respectively, these subsamples are excluded from the analysis.

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<sup>7</sup> In contrast, Carneiro and Heckman (2002) find that the long run factors that shape ability are the major determinants of the family income-schooling relationship.

<sup>8</sup> Following Ahituv and Lerman (2005), I obtain information for odd-post-1994 years by designating a "quasi-interview" month, 12 months prior to actual interview month in the succeeding year. Fortunately, the NLSY79 includes school enrollment history on a monthly basis that makes it possible to recover the necessary information for odd-post-1994 years. A complete description of variables for post-1994 years is contained in the appendix.

The longitudinal nature of this survey and its detailed marriage, educational attainment, and school enrollment questions make it well suited for my analysis.

The NLSY79 questionnaire contains comprehensive information on the timing of past changes in marital status, which allows the creation of a complete marital history for each person. In this study, marital dissolution is defined as being either a divorce or marital separation. For individuals who report dates for marital separation and divorce, the date of marital dissolution corresponds to the date of marital separation. The longitudinal nature of the survey enables researchers to follow the same respondents over time, which makes it possible to examine the respondent's schooling decisions before and after marital dissolution. For this analysis, I focus on changes in school enrollment behavior due to the disruption of the first marriage.<sup>9</sup> Therefore, I exclude respondents who had experienced more than one marriage before round one of the survey.

Information on the respondent's schooling experiences is collected during each survey round. After round one, respondents are asked if they have attended or been enrolled in regular school since the date of last interview. Regular school is defined as a school that provides credit toward an academic degree or diploma. For survey years 1981-2008, respondents are asked the months that they attended school since the since the last interview. This analysis utilizes two dependent variables: number of months enrolled in the prior calendar year and an indicator for school participation during the prior calendar year. The respondents are considered to be enrolled if they have attended school for at least one month during the previous calendar year. School measures are obtained for the prior calendar year, as opposed to the time since the last interview date, because the interview schedule changed after 1986.<sup>10</sup> Additionally, the analysis sample is restricted to person-year observations for women who are at least 20 years of age. This is done to avoid years of mandatory high school enrollment.

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<sup>9</sup> The effects of divorce from remarriages likely reflect difference properties than first marriages (Couch et al., 2011).

<sup>10</sup> The first NLSY79 interviews were administered between late January and mid-August 1979. Before survey year 1987, the interviews were conducted during the first six months of year, which allowed all respondents still in school to be interviewed before leaving for summer jobs. The fielding period varies after 1987.



### **3.1 Demographic Characteristics**

The NLSY79 also contains detailed demographic information about each respondent and household. I create measures of the woman's age, race, education, number of children under 18 living in the household, and an indicator for having a child under age 1. These variables are taken directly from the NLSY79 and are measured as of the current year of the survey.

### **3.2 Labor Market Measures**

In a manner similar to Murphy and Welch (1992), I use the wage differential for 25-34 year old, full-time workers (at least 35 hours per week) in the respondent's state of residence as a proxy for the education premium. This differential is computed as the log difference between the average wage of individuals whose education is one level above the respondent and the average wage of individuals who share the respondent's education level, where the education levels are less than high school diploma, high school diploma, some college (no B.A.), and college graduate (B.A. or higher). For example, the earnings differential for a respondent who has a high school diploma is the log difference between the average wage of individuals with some college and those who are high school graduates. Low program completion rates for nontraditional students motivate the construction of the average premiums for individuals in the state. The measures are constructed using the March Current Population Survey (CPS). Three-year moving averages are used because the March CPS is a nationally representative sample, but not representative of the state's population.

### **3.3 Analysis Sample**

In order to identify changes in schooling trends that are due to marital dissolution, Separated and Intact samples are constructed. To be included in the Separated Sample, women must be observed in their first marriage for at least three survey rounds and at least one survey round after divorce. The Intact Sample includes women who are observed in their first marriage for at least four survey rounds of the NLSY79. These criteria ensure that a detailed history is available for every respondent. Also, focusing on marriages that remain intact for at least 3 survey rounds focuses the analysis on women who have had substantial time to invest in their marriage.

Furthermore, since the dates of marriage and marital dissolution are central to the analysis, dropped from the analysis are women whose marriage or marital disruption dates are missing. After inspecting for coding errors, the Intact Sample consisted of 1,713 and the Separated Sample included 1,280 women.

Table 1 compares selected variables for the Intact and Separated Sample. Aside from the portions who are Hispanic, there are few similarities between the samples. For example, the average AFQT score percentiles are 56 and 47 for women in the Intact and Separated Samples, respectively. The average age at marriage for women in the Separated sample is 21.7, which is approximately 2.8 years less than the average age for women in the Intact Sample. Women from the Intact Sample are also closer in age to their first husband than are women in the Separated Sample. The average age differences from their spouses are 2.47 and 3.03 years for the Intact and Separated Sample, respectively. In addition, women in the Separated Sample are more likely to begin the marriage without a high school diploma and less likely to begin marriage with a bachelor's degree. In general, the Separated Sample appears to be less advantaged than the Intact Sample.

### **3.4 Trends in School Enrollment Outcomes**

It is useful to examine trends in women's school enrollment by divorce status before launching an empirical examination of the effect of marital dissolution on women's schooling. Figure 1 shows women's average number of months enrolled according to the number of years preceding or following divorce. These averages, which include women who did not attend school, show a decrease in the average number of months enrolled from the fifth year before the divorce (.74 months) to the year of divorce (.54 months). Following divorce, there is an increase in the average number of months enrolled to about .76 months in the fourth year after dissolution. Thus, from the year of dissolution to the fourth year after dissolution, the average number of months enrolled in school increased by 40.7%. A substantial portion of this increase can be accounted for by increased school enrollment. Figure 2 shows that the percent of Separated Sample members enrolled in school at any time during the previous calendar year increased from 9.4% during the year of divorce to 11.9% in the fourth year after dissolution, an increase of 26.6%.

It is common for individuals to make schooling investments early in life so as to maximize the number of years they have access to the returns to their schooling decision. Therefore, the number of months enrolled in school and the enrollment rate are expected to decrease as the sample ages. Therefore, it is instructive to compare trends in schooling outcomes for the Separated and Intact Samples. Following Johnson and Skinner (1986), Intact Sample averages are modified to reflect the calendar year composition of school outcomes for women in the Separated Sample. For example, if half the observations for 2 years before the divorce came from 1984 and half from 1997, then the appropriate comparison for the Intact Sample is the average of the school outcome in 1984 and 1997. Looking at Figures 1 and 2, across both samples, there is a persistent downward trend in school outcomes prior to dissolution. However, in contrast to the Separated Sample, the Intact Sample does not experience an increase in the enrollment rate or average number of months during the survey period. Thus, Figures 1 and 2 provide descriptive evidence that women increase school investment following divorce.

Although serious consideration of the effect of marital dissolution on men is beyond the scope of this paper, for purposes of comparison, men's average number of months enrolled is presented in Figure 3. Men in both the Intact and Separated Samples experience a decrease in the average number of months enrolled during the period of marriage. In addition, the Intact Sample has higher rates of enrollment than do men in the Separated sample at each period. Unlike the Separated Sample for women, men in the Separated Sample do not experience an increase in enrollment following dissolution. This suggests that post-divorce changes in men's preferences and/or income does not promote school enrollment of divorced men.

Although Figures 1 and 2 provide descriptive evidence that divorce may have a positive impact on women's school enrollment, interview nonresponse provides an alternative explanation for the increase in school enrollment that follows divorce. More specifically, a recent divorce may increase the likelihood that women are unable to complete an interview. If women who are absent from the survey following a divorce are also less likely to enroll in school, their absence from the sample during the survey rounds immediately following divorce would inflate post-divorce schooling outcomes in the Separated Sample. For example, some respondents exit and reenter the sample after a temporary residential move, which is more likely

following a divorce.<sup>11</sup> However, there is some evidence that this is not an issue. For example, Fitzgerald et al. (1998) use the PSID to analyze sample attrition and find that while men who moved recently or who show a high average propensity to move are more likely to attrite, no significant effects appear for women. To investigate this issue using the NLSY79, I present the nonresponse rates by when the individual experienced a divorce. In Figure 4, the dependent variable takes on a value of one if the respondent did not complete the survey, where time is measured relative to the date of divorce. As before, the averages for the Intact Sample are modified to reflect the calendar year composition of the Separated Sample. Surprisingly, the nonresponse rates for the Separated Sample are lower than for Intact Sample. Most importantly, the trend in the nonresponse rate for the Separated Sample closely tracks that of the Intact Sample. In addition, the nonresponse rate of the Separated Sample does not experience an increase following divorce, which suggests that sample attrition does not account for the observed increase in the probability of school enrollment following divorce.

Another explanation for trends presented in Figures 1 and 2 is that the anticipation of divorce may limit women's education opportunities. For example, the respondent's husband may be less likely to invest in her schooling if he suspects divorce is inevitable. In this case, the woman may file for divorce in order to attend school. Thus, we would expect to see a decrease in the probability of school enrollment shortly before marital dissolution.<sup>12</sup> Indeed, Stevenson (2007) finds that new couples are less likely to support each other through school when unilateral dissolution laws are enacted. However, Figures 1 and 2 does not provide evidence that selection based on previous enrollment is a key determinant of the increase in schooling following divorce. The school outcomes for the Separated Sample does not exhibit breaks from the trend prior to divorce. In fact, the averages move similarly across the Separated and Intact Sample prior to dissolution, which provides descriptive evidence that selection on school outcomes is not driving the increase in schooling after divorce. Nonetheless, a more formal test is provided later.

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<sup>11</sup> Women may exit the survey during stressful periods. If the amount of dissolution related stress is correlated with the propensity to enroll, the marital dissolution effect could be biased. However, using the British Household Panel Survey, Oswald (2006) finds that women reap psychological gains due to marital separation—in other words, they are happier after dissolution.

<sup>12</sup> In the training literature it has been highlighted that, in many instances, a decrease in earnings precedes enrollment in the program because program managers usually enroll those individuals with recent labor market problems (Ashenfelter, 1978; Ashenfelter and Card, 1985; Heckman and Smith, 1994).

While Figures 1 and 2 yield descriptive evidence that marital dissolution has a positive impact on women’s school investment, it is difficult to interpret the sharp increase in schooling after marital dissolution as causal because the samples may experience different secular variation in school enrollment. Identification of the treatment effect of interest is confounded if the Intact and Separated Samples experience different secular variation in school enrollment. Although it does not appear the case that the two samples have differing secular trends in enrollment, a more valid approach is to use Intact Sample members in each state to control for counterfactual trends. The remainder of this paper uses methods that control for individual fixed effects to identify the effect of marital dissolution on women’s schooling.

#### 4 Empirical Strategy

To examine the effect of marital dissolution on women’s school enrollment, I estimate the following equation on the Intact and Separated Samples:

$$Y_{ist} = \beta_0 + \sum_{j=-5}^{10} \gamma_j I(t - year_d = j) + \delta X_{ist} + \theta Z_{ist} + \tau_i + \eta_{st} + \varepsilon_{ist}, \quad (1)$$

where  $Y_{ist}$  represents the school enrollment measure of interest in the previous calendar year,  $X_{it}$  is a vector of time -varying individual characteristics and  $Z_{ist}$  is a vector of time-varying state characteristics that are listed in Section 4.2,  $\tau_i$  are individual fixed effects,  $\eta_{st}$  are state-specific year effects, and  $\varepsilon_{ist}$  is an error term.  $I(t - year_d = j)$  is a binary variable that equals one if person  $i$  is  $j$  years from divorce and zero otherwise, where  $year_d$  indicates the year in which person  $i$  divorced.<sup>13</sup> For individuals in the Intact Sample and observations for Separated Sample members for which relative year to divorce is outside the event window, these indicator variables are assigned zero. The event window is from 5 years prior to dissolution to 10 years post dissolution because sample sizes decline beyond this range and because any meaningful relationship between marital dissolution and educational attainment should be expected to take

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<sup>13</sup> In an influential study, Jacobson et.al (1993) use this method to examine the earnings losses of displaced workers. More recently, Lovenheim (2009) uses this methodology to estimate the effect of teacher’s unions on student educational attainment.

place within 10 years of the date of marital dissolution. Excluded from this analysis are observations from the Separated Sample for which time since divorce is greater than 10 years.

Equation (1) semiparametrically estimates both short-term and long-term effects of the independent variable of interest, marital dissolution, and is more general than using a single binary variable for dissolution. The inclusion of dummy variables for each year relative to marital dissolution places no structure on the pattern of time trends either before or after dissolution. Moreover, marital dissolution may have nonlinear effects on women over time that will be obscured by imposing the parametric assumption that the impacts are equal, which makes the flexibility of equation (1) important.<sup>14</sup> It is theoretically possible that the effects of marital dissolution might diminish over time—mainly due to the completion of the respondent’s desired level of education.

Another benefit of equation (1) is that it includes individual fixed effects. If marital dissolution depends on time-invariant unobservable characteristics that are correlated with both the decision to divorce and school enrollment outcomes, cross-sectional estimates will be biased. However, the fixed effects model compares the same woman at different times relative to the year of divorce and controls for any unobservable (and time invariant) effects.

The principal identifying assumption is

$$E(\varepsilon_{ist} | I(t - year_c = j) \forall j \in [-5, 10], X_{ist}, Z_{ist}, \tau_i, \eta_{st}) = 0. \quad (2)$$

Satisfaction of (2) requires, conditional on time-varying individual and state variables and fixed effects, the timing of divorce is uncorrelated with potential outcomes. Estimates of the  $\gamma_j$  parameters from equation (1) will be biased if there is selection into divorce based on pre-divorce school enrollment trends. Further, if women’s school enrollment is affected by the anticipation of divorce, it will become evident in the estimates for years preceding divorce. To test for any selection on the outcome variable that may be a determinant of the dissolution decision, I estimate  $\gamma$ ’s prior to dissolution ( $j < 0$ ). Instead of controlling for differential pre-treatment trends across the Intact and Separated Sample, this difference-in-difference approach enables me to test directly for the existence of such trends.<sup>15</sup> In addition, it is plausible that

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<sup>14</sup> For example, the college admission and financial aid process may force recently divorced women to delay admissions by at least a year.

<sup>15</sup> The NLSY79 panel is unbalanced with respect to relative year to separation. Consequently, each  $\gamma_j$  is identified off of a likely different set of women. An Appendix table lists the number of observations.

divorce is preceded by years of marital conflict. Thus we might observe changes in enrollment patterns several years prior to marital dissolution.

Given the observed differences between the Separated and Intact samples, what effect can one expect these differences to have on estimates from equation (1) given that the parameter of interest in this study is the average treatment effect on the treated (ATT)? NOTE that selection into divorce based on perceived or actual gains from marital dissolution will not bias identification of the ATT; however, such selection will bias identification of the average treatment effect. Since individual fixed effects control for any time-invariant differences in enrollment among women in the analysis sample, what is required to identify the ATT is for the state-specific year effects to reflect counterfactual trends in enrollment for the Separated Sample. For that reason, correctly identifying  $\eta_{st}$  is the main difficulty in estimating the treatment effect on the Separated Sample using equation (1).

State-specific yearly variation in the dependent variable from the Intact Sample identifies the state-specific year effects,  $\eta_{st}$ . The estimates from equation (1) use the Intact Sample combined with all observations for which relative year to divorce is less than or equal to 10 and are presented in Appendix Table B3. The Intact Sample and those observations for which relative year to dissolution is less than -5 make up the control group. This sample is appealing because it uses all observations that conceivably are unaffected by marital dissolution, which allows for the most power in identifying all parameters of equation (1). In Section 6.2, I provide a series of robustness checks that illustrate that my estimates are not sensitive to the control group used.

Although equation (1) is a more flexible model, a model with fewer parameters is preferred when examining how the impact of divorce differs by various characteristics. Equation (3) is a more parsimonious model than equation (1), containing two mutually exclusive terms: (1)  $SEP_{it}$ , which equals 1 if the woman's first marriage has dissolved by period  $t$  and 0 otherwise, and (2)  $PRE_{it}$ , which takes a value of 1 during the 3 years prior to divorce:

$$Y_{ist} = \beta_0 + \beta^{PRE} PRE_{it} + \beta^{POST} SEP_{it} + \delta X_{it} + \theta Z_{ist} + \tau_i + \eta_{st} + \varepsilon_{ist}, \quad (3)$$

In order to estimate differential effects, the divorced indicator is interacted with individual and household characteristics. For example, the magnitude of the divorce effect may

depend on the share of household income generated by the husband during the last year of marriage. An obvious reason for this assertion is that women who earned significantly less than their former spouse may obtain additional education in order to be less dependent on their former spouse's transfer payments—especially since child support receipt is a function of the economic circumstances of the noncustodial parent. In addition, the impact of marital dissolution may differ by education—for instance, the impact of marital dissolution on school enrollment may decline as the woman's level of education increases because of the positive association between wages and education.

## 5 Empirical Results

Figures 5 and 6 plot the estimates of  $\gamma_j$  from equation (1) for school enrollment and number of months enrolled, respectively. The points depict the estimates of the  $\gamma$  coefficients from each relative-year-to-dissolution binary variable, while the height of the bars stretching from each point represent the bounds of the 95% confidence interval from the standard errors that are clustered at the individual level.<sup>16</sup> Regression estimates for the results in Figures 5 and 6 are presented in Appendix Tables A3 and A4.

Consistent with the trends in Figures 1 and 2, the results provide evidence that marital dissolution has a positive impact on school enrollment. Focusing on Figure 5, school enrollment is comparable for the treatment and control groups three years prior to marital dissolution and remains similar until one year after marital dissolution. However, the probability of school enrollment increases by approximately 2.5 percentage points (or 27%) 3 years after marital dissolution ( $\gamma_j = 3$ ). This is a sizable effect. For example, it is equal to the effect of a 13% increase in women's school wage premium.

In Figure 6, the results indicate that the average number of months enrolled in school in the prior calendar year increases by .164 months (or 28%). In both Figures 4 and 5, the point estimates for post-dissolution years 3 through 7 are statistically distinguishable from zero at the 10% level, or better. Moreover, using an F-test, I fail to reject the joint hypothesis that  $\gamma_j =$

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<sup>16</sup> The  $\gamma_j$  coefficients identify treatment effects relative to the effect for the year before separation,  $\gamma_j$ . I include a zero for the point estimates in relative year  $j = 1$ , but omit standard errors bars due to the fact that this zero is imposed rather than estimated.



$0, \forall j < 0$ . Thus, there is no evidence that there is selection into divorce timing based on recent school enrollment patterns.<sup>17</sup>

While Figures 5 and 6 provide evidence that marital dissolution has a positive impact on the probability of school enrollment, the figures do not address the question of whether these women are completing additional years of education. Divorced women are classified as nontraditional students, and these students are more likely to quit school than their traditional counterparts (Horn and Carroll, 1996). For example, 38% of nontraditional students leave school in their first year (Horn and Carroll, 1996). Therefore, in order to examine the impact of marital dissolution on completed years of education, I estimate equation (1) using highest grade completed as the dependent variable.

Figure 7 plots the point estimates from equation (1) for highest grade completed and regression estimates are in Appendix Table A5. I find that marital dissolution has a positive impact on the highest grade completed in the first 5 years following marital dissolution, but that it then increases by .06 years in the 6<sup>th</sup> year after marital dissolution and remains at this level or higher for the remainder of the event window. This is consistent with the fact that enrollment does not peak until four years after marital dissolution and that most nontraditional students attend school part-time. Consequently, it takes several years before marital dissolution has a positive impact on educational attainment.

## 5.1 Robustness Checks

As discussed in Section 5, the critical assumption underlying identification of the  $\gamma$  coefficients in equation (1) is the use of a suitable control group to account for secular variation in school enrollment outcomes. Recall that figures 1 and 2 provide evidence that my estimates should not be overly sensitive to the within-state Intact Sample that I use as a control group. Nevertheless, I assess the frailty of my results to the choice of estimation sample by estimating (1) using additional samples that each suggests a different control group. First, I restrict the estimation sample to include only the Intact Sample and the person-year observations for which the relative time to marital dissolution falls within the event window. Thus the control group is composed of only the Intact Sample. This control group is attractive relative to the control group used to

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produce the main estimates because it will be unaffected by the effects of marital dissolution on the dependent variable more than 5 years prior to marital dissolution. In addition, I obtain estimates using only those person-year observations for which the relative time to marital dissolution is less than or equal to 10. This sample is the same as the one used to estimate the parameters displayed in figures 4 and 5, but it omits the Intact Sample.

As expected, estimates from these robustness checks are similar in both magnitude and quality to those shown in Appendix Table A4, where the dependent variable is the number of months enrolled in school. Once again, the evidence suggests that marital dissolution has a positive impact on women's school enrollment. While the estimated effects reported in column 2 of table A6 are less precise than the estimates in table A4, they are qualitatively similar. In addition, there is little evidence that trends in enrollment are correlated with the timing of marital dissolution. I fail to reject an F-test that  $\gamma_k = 0, \forall k < 0$ , at any reasonable level of statistical significance.

## **5.2 Impact by Individual and Household Characteristics**

Thus far the analysis has focused on the average effect of marital dissolution, but the impact of marital dissolution may differ by characteristics such as education or race. Table 5, column (2), shows the race interaction terms are not statistically significant. The post-dissolution effect is .099 and .152 for white and black women, respectively. The magnitude of the post-dissolution effect for Hispanic women is .45. However, none of these effects are significant.

Table 5 shows that the impact of marital dissolution on enrollment decisions differ by the woman's education, where education is captured from the last survey round in which the respondent is observed married. Table 5, column (3) shows that the post-dissolution effect is largest for women who had an education of high school diploma or less when the marriage ended. The post-dissolution effect is .33 and .25 for women who had less than a high school diploma and high school diploma, respectively. For women who had some college and a bachelor's degree or higher, the post-dissolution effect is .07 and -.74 (significant at 99% confidence), respectively. These results suggests that returning to school after divorce could be more costly for women with some college than for women with high school diploma or less—especially if community college is no longer an option. Further, women with some college face higher opportunity costs of enrolling than women with high school diploma or less due to higher

wage offers. In regards to skill retooling, women with a high school diploma or less are more likely than women with some college to need further schooling in achieve occupation mobility.

The share of the household income that the husband earned during the last round the marriage was observed intact is positively associated with marital dissolution impacts (Table 5 column 4). The coefficient on the interaction term of dissolution and the share of household income contribute by the husband is positive and significant, but small in magnitude. For example, women whose husband generated 100% of household income increase the number of months enrolled by an additional 4%.

## **6 Conclusion**

In this paper, I add to the sizable literature on the consequences of divorce by examining the impact of divorce on women's school enrollment and educational attainment. I use an empirical strategy that imposes minimal structure on the dynamic response of school enrollment, and I then estimate a more parsimonious model in order to determine how the impact of divorce differs by individual characteristics.

Evidence from the NLSY79 suggests that divorce has a positive impact on women's school enrollment. More specifically, estimates show that the probability of school enrollment increases by 28% three years after marital dissolution. The effect of marital dissolution on women's school participation is the same as a 13% increase in the school wage premium. Moreover, the results indicate that the positive impact of marital dissolution on school enrollment persists 8 years after marital dissolution. Perhaps most importantly, marital dissolution has a positive effect on highest grade completed that begins 6 years after marital dissolution and remains at this level or higher for the remainder of the event window. I also find that the impact is larger for women whose husband generated a larger portion of household income and for women who with an education of high school diploma or less when the marriage dissolved.

Understanding the consequences of divorce for women is a key agenda because a significant portion of divorces involves children. Since divorced women with children are likely to suffer a reduction in income following divorce, they are likely to invest in human capital in order to increase their income. My results suggest that policy makers should be sensitive to the school investment decisions of divorced women. The impact of divorce on school enrollment is

largest for women with a high school diploma or less education—a group vulnerable to poverty and unemployment. Previous studies suggest that lowering the price of college through need-based grants and subsidized loans is more effective than tax credits aimed at increasing the school enrollment of disadvantaged groups. For example, LaLumia (2011) investigates how eligibility for an education tax credit affects the college attendance decision, and finds no effects of tax credits on women’s decision to attend college. In contrast, Seftor and Turner (2002) find that changes in federal financial policy have a significant impact on the enrollment behavior of non-traditional students. Thus, reductions in need-based aid may affect women’s ability to retool after marital dissolution.

The central implication of this work is that policies aiming to increase the success of nontraditional students will likely benefit divorced women. In particular, because dropout rates are especially high for students attending community college, policies that increase persistence in community colleges are a reasonable focus if the goal is to increase the income and well-being of divorced women and their children.

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### Table 1. Descriptive Statistics for Intact and Separated Samples

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Weiss, Y. (1971). Investment in graduate education. *The American Economic Review*, 61(5), pp. 833-852.

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	Intact Sample	Separated Sample	Two-sided t-test for equality of means
Number of Individuals	1713	1280	
Black = 1	0.08	0.13	**
Hispanic = 1	0.05	0.07	**
Age at marriage	24.54 (5.71)	21.92 (4.39)	**
AFQT percentile in 1980	56.70 (29.19)	45.03 (28.63)	**
Wife's education at			
Less than high school	0.08	0.18	**
High school diploma	0.38	0.46	**
Some college	0.25	0.22	
Bachelor's or above	0.29	0.14	**
Spouse's age at marriage	26.97 (6.81)	24.91 (5.84)	**
Husband's education at			
Less than high school	0.09	0.19	**
High school diploma	0.40	0.45	*
Some college	0.21	0.21	
Bachelor's degree or	0.30	0.15	**
Marriage duration		10.24 (6.06)	

NOTE: The Intact Sample is comprised of women who were observed for at least 4 survey rounds while married and who did not experience marital dissolution. The Separated Sample consists of women who were observed married with complete data for at least 4 survey rounds before divorce and who were observed for at least 1 survey round after divorce. Standard errors for continuous variables are in parentheses. Sampling weights are used to construct descriptive statistics. \*  $p < .05$ ; \*\*  $p < .01$

**Table 2. Comparison of Separated and Excluded Samples**

	Separated Sample	Excluded Sample	Two-sided t-test for equality of means
Number of Individuals	1280	921	
Black	0.13	0.17	**
Hispanic	0.07	0.07	
Urban residence	0.75	0.78	
AFQT in percentile in 1980	45.03 (28.23)	40.00 (27.86)	**
Lived with biological parents at age 14	0.72	0.67	*
Raised Catholic	0.31	0.27	*

Summary statistics are based on data from round 1 of survey. The full sample includes all women from NLSY79 sample except military sample. The Separated Sample consists of women who were observed married with complete data for at least 3 survey rounds before divorce and who were observed for at least 1 survey round after divorce. The Excluded Sample includes women from full sample who are observed being married, but are not included in the analysis sample. Standard errors for continuous variables are in parentheses. Sampling weights are used in estimation of descriptive statistics. \*  $p < .05$ ; \*\*  $p < .01$

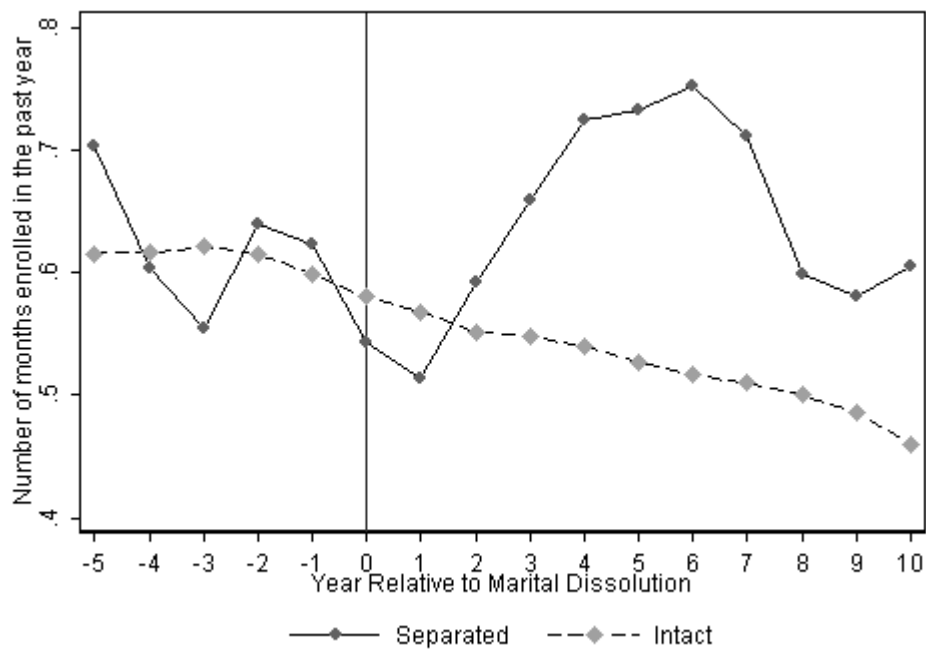


Figure 1.--Women's average months enrolled in school. The Intact Sample is comprised of women who were observed for at least 4 survey rounds while married and who did not experience marital dissolution. The Separated Sample consists of women who were observed married with complete data for at least 3 survey rounds before divorce and who were observed for at least 1 survey round after divorce. The vertical line marks the year when the divorce is initiated.



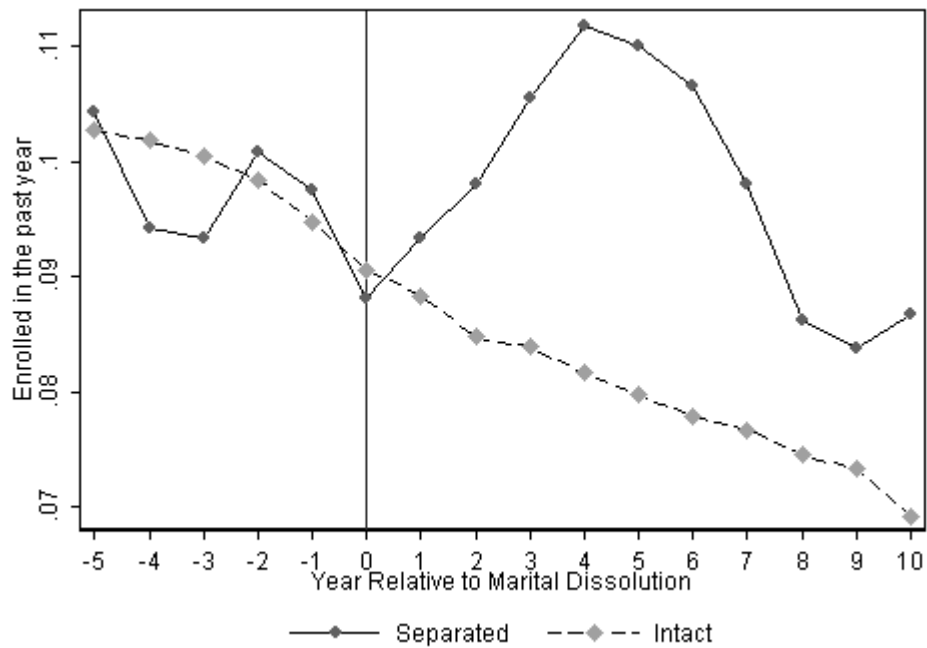


Figure 2.-- Women's school enrollment rate. The Intact Sample is comprised of women who were observed for at least 4 survey rounds while married and who did not experience marital dissolution. The Separated Sample consists of women who were observed married with complete data for at least 3 survey rounds before divorce and who were observed for at least 1 survey round after divorce. The vertical line marks the year when the divorce is initiated.

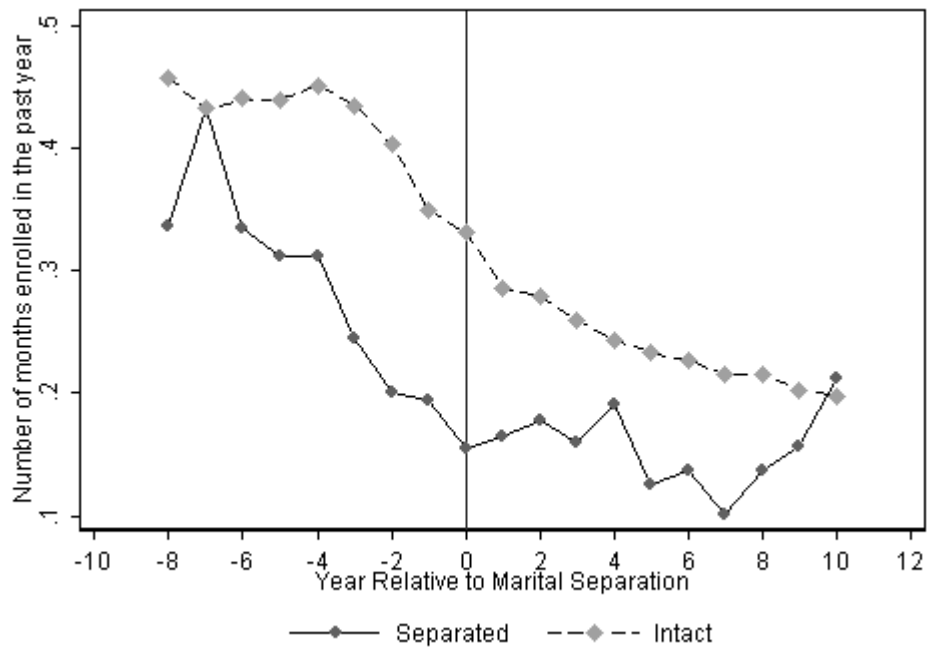


Figure 3.-- Mens's average months enrolled in school. The Intact Sample is comprised of men who were observed for at least 4 survey rounds with while married and who did not experience marital dissolution. The Separated Sample consists of men who were observed married with complete data for at least 3 survey rounds before divorce and who were observed for at least 1 survey round after divorce. The vertical line marks the year when the divorce is initiated.



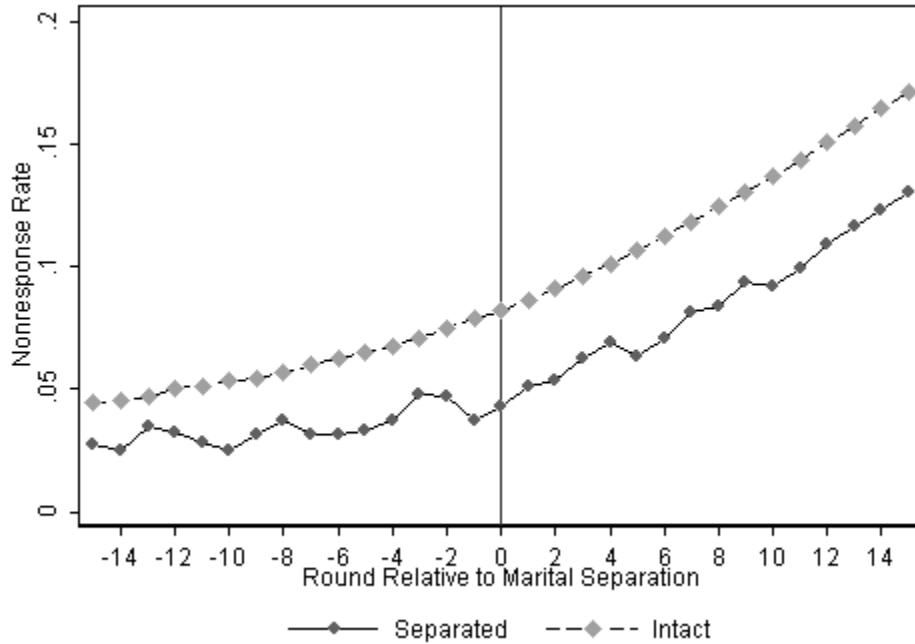


Figure 4.-- Women's nonresponse rates relative to year of dissolution. The Intact Sample is comprised of women who were observed for at least 4 survey rounds while married and who did not experience marital dissolution. The Separated Sample consists of women who were observed married with complete data for at least 3 survey rounds before divorce and who were observed for at least 1 survey round after divorce. The vertical line marks the year when the divorce is initiated.

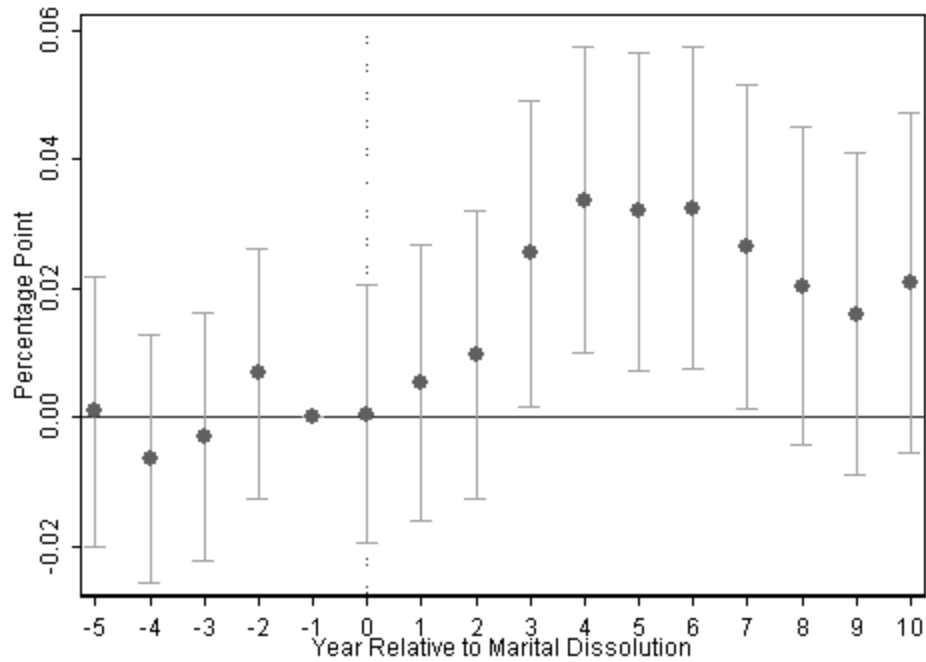


Figure 5. The effect of marital dissolution on the school participation rate. The points depict coefficient estimates from estimation of eq. (2) on the analysis sample, as explained in the text. The bars extending from each point represent the bounds of the 95% confidence interval calculated from standard errors that are clustered at the individual level. Relative year -1 is excluded in order to make all estimates relative to the year prior to dissolution. The exclusion of standard error bars for the point estimate in relative year  $j = -1$  reflect that the estimate of zero is imposed rather than estimated.

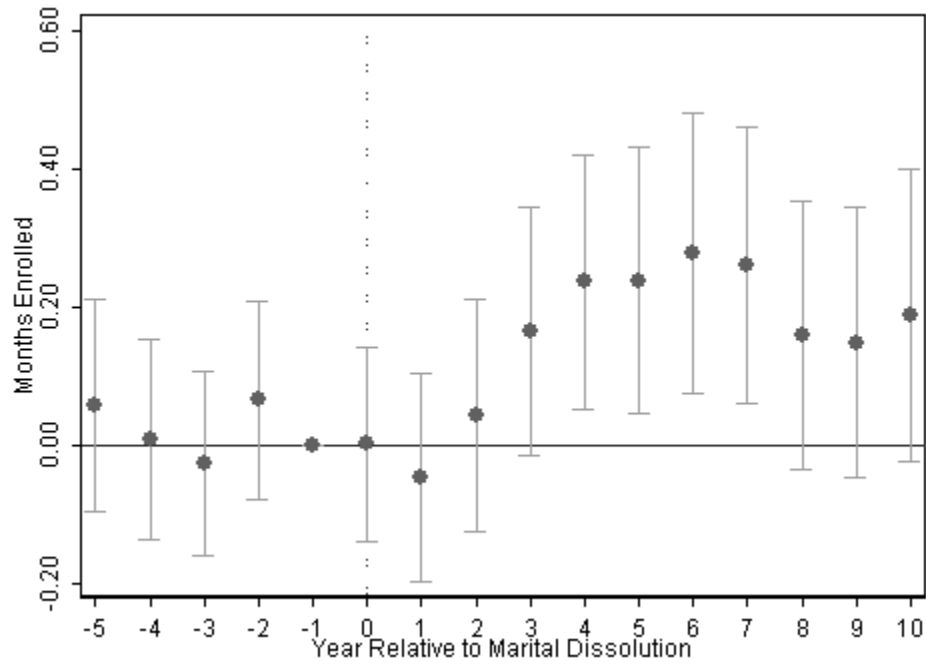


Figure 6. The effect of marital dissolution on months enrolled in regular school. The points depict coefficient estimates from estimation of eq. (2) on the analysis sample, as explained in the text. The bars extending from each point represent the bounds of the 95% confidence interval calculated from standard errors that are clustered at the individual level. Relative year -1 is excluded in order to make all estimates relative to the year prior to dissolution. The exclusion of standard error bars for the point estimate in relative year  $j = -1$  reflect that the estimate of zero is imposed rather than estimated.

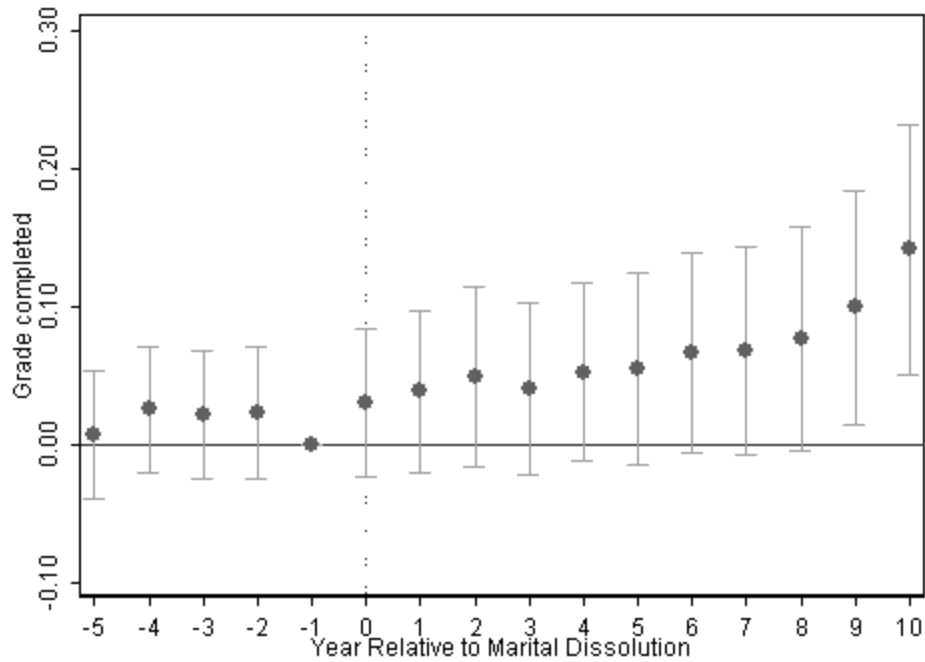


Figure 7. The effect of marital dissolution on highest grade completed. The points depict coefficient estimates from estimation of eq. (2) on the analysis sample, as explained in the text. The bars extending from each point represent the bounds of the 95% confidence interval calculated from standard errors that are clustered at the individual level. Relative year -1 is excluded in order to make all estimates relative to the year prior to dissolution. The exclusion of standard error bars for the point estimate in relative year  $j = -1$  reflect that the estimate of zero is imposed rather than estimated.

**Table 5. Ordinary Least-Squares Regression Coefficients Predicting the Impact of Marital Dissolution on Number of Months Enrolled in School, by Race, Education, and Husband's Income Share**

Variable	(1)	(2)	(3)	(4)	(5)
BD (1-3 years)	0.036 (0.060)	0.057 (0.084)	-0.006 (0.0495)	0.038 (0.060)	0.019 (0.070)
AD	0.099* (0.060)	0.099 (0.082)	0.248*** (0.057)	0.072 (0.060)	0.260*** (0.078)
Black x BD (1-3 years)		0.001 (0.134)			-0.031 (0.136)
Black x AD		0.053 (0.132)			0.034 (0.134)
Hispanic x BD (1-3 years)		-0.054 (0.152)			-0.104 (0.152)
Hispanic x AD		-0.092 (0.129)			-0.171 (0.128)
Less than high school x BD (1-3 years)			0.093 (0.076)		0.120 (0.082)
Less than high school diploma x AD			0.083 (0.081)		0.123 (0.086)
Some college x BD (1-3 years)			0.350** (0.166)		0.350** (0.168)
Some college x AD			-0.175 (0.157)		-0.198 (0.156)
Bachelor's Degree x BD (1-3 years)			-0.299 (0.274)		-0.308 (0.273)
Bachelor's Degree x AD			-0.980*** (0.252)		-0.987*** (0.252)
Husband's income share x BD (1-3				0.002*** (0.0001)	0.001 (0.001)
Husband's income share x AD				0.017*** (0.0001)	0.018*** (0.001)
Observations	56180	56180	56180	56180	56180
Individuals	2993	2993	2993	2993	2993
Mean (SD) of Dependent Variable	0.585 (2.126)	0.585 (2.126)	0.585 (2.126)	0.585 (2.126)	0.585 (2.126)
R <sup>2</sup>	1.747	1.747	1.747	1.747	1.747

NOTES: Standard errors, shown in parentheses, are clustered at the individual level. BD = 1 if respondent is married and within 3 years of marital dissolution, and AD = 1 if respondent has experience marital dissolution as of the respondent's interview date. Unreported controls include the state unemployment rate, number of children, binary variable indicating the presence of a child less than age 1, state-year fixed effects, and individual fixed effects. \*  $p < .10$ ; \*\*  $p < .05$ ; \*\*\*  $p < .01$



## Appendix A

### Supplemental Figures and Tables

**Table A1.—Distributions of Months Enrolled**

Months Enrolled	Intact Sample	Separated Sample		
		All	Before Dissolution	After Dissolution
1	4.9	4.21	3.48	4.96
2	5.75	6.95	6.77	7.14
3	8.71	8.72	9.86	7.54
4	23.25	25.07	24.76	25.4
5	18.26	16.85	15.09	18.65
6	6.47	5.39	5.22	5.56
7	4.86	6.17	6.96	5.36
8	4.44	4.7	5.22	4.17
9	6.34	5.88	7.35	4.37
10	5.79	5.48	6.58	4.37
11	2.03	2.94	2.9	2.98
12	9.21	7.64	5.8	9.52
Observations (% of sample)	2366 (9.6)	1021 (11.2)	517 (10.8)	504 (11.5)

NOTE: The Intact Sample is comprised of women who were observed for at least 4 survey rounds and who did not experience marital dissolution. The Separated Sample consists of women who were observed married with complete data for at least 3 survey rounds before divorce and who were observed for at least 1 survey round after divorce.

**Table A2.—Enrollment Rate and Average Months Enrolled of Separated Sample**

Year relative to dissolution	Separated Sample			Intact Average Months Enrolled
	Enrollment Rate	Average Months Enrolled	N	
-8	0.090	0.438	570	0.447
-7	0.101	0.547	642	0.462
-6	0.091	0.515	765	0.434
-5	0.114	0.620	876	0.439
-4	0.102	0.526	1012	0.454
-3	0.101	0.508	1102	0.464
-2	0.097	0.529	1102	0.443
-1	0.089	0.457	1080	0.416
0	0.080	0.390	1118	0.410
1	0.080	0.394	1127	0.383
2	0.088	0.441	1111	0.380
3	0.093	0.452	1085	0.367
4	0.090	0.484	1048	0.364
5	0.093	0.513	1022	0.357
6	0.082	0.515	936	0.363
7	0.090	0.530	925	0.357
8	0.083	0.467	858	0.370
9	0.074	0.424	813	0.356
10	0.085	0.499	792	0.357

NOTE: The Intact Sample is comprised of women who were observed for at least 4 survey rounds with while married and who did not experience marital dissolution. The Separated Sample consists of women who were observed married with complete data for at least 3 survey rounds before divorce and who were observed for at least 1 survey round after divorce.

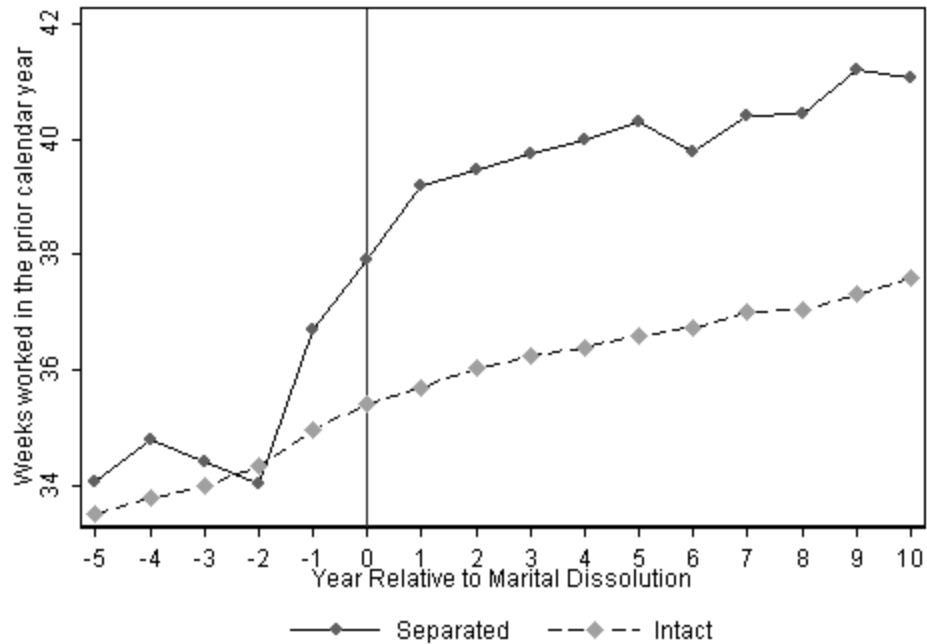


Figure A1.-- Women's average weeks worked relative to year of dissolution. The Intact Sample is comprised of women who were observed for at least 4 survey rounds while married and who did not experience marital dissolution. The Separated Sample consists of women who were observed married with complete data for at least 3 survey rounds before divorce and who were observed for at least 1 survey round after divorce. The vertical line marks the year when the divorce is initiated.

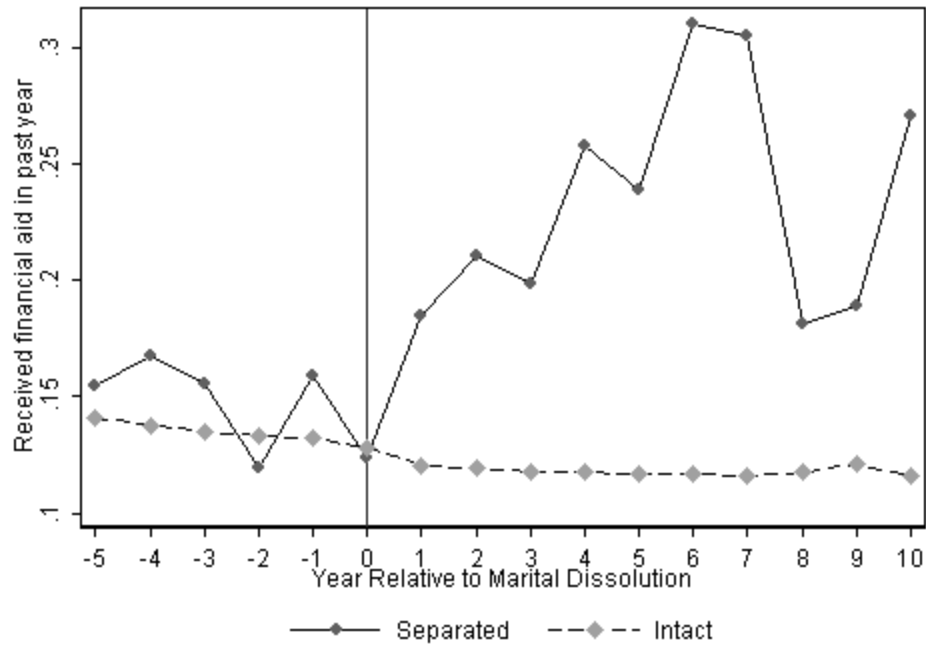


Figure A2.--Women's propensity to receive financial aid. Percentages are based on sample who reported attending college in the previous year. The Intact Sample is comprised of women who were observed for at least 4 survey rounds while married and who did not experience marital dissolution. The Separated Sample consists of women who were observed married with complete data for at least 3 survey rounds before divorce and who were observed for at least 1 survey round after divorce. The vertical line marks the year when divorce is initiated.

**Table A3. Fixed Effects Estimates of the Impact of Marital Dissolution on Women's School Investment**

Variable	School Participation	Number of Months Enrolled
	(1)	(2)
Age	-0.018** (0.008)	-0.150** (0.058)
Age, Squared	0.0005** (0.000)	0.001** (0.001)
School Wage Premium	0.191*** (0.027)	1.633*** (0.210)
Total Children	-0.015*** (0.003)	-0.097*** (0.022)
Presence of child age less than 1	-0.050*** (0.004)	-0.368*** (0.031)
Observations	56180	56180
Individuals	2993	2993
Mean (SD) of Dependent Variable	0.091 (0.287)	0.585 (2.126)
$R^2$	0.279	0.283
F-test $\rho H_0: \gamma_j = 0, \forall j < 0$	0.792	0.529

Models include state-specific year and individual fixed effects. All standard errors are clustered at the individual level and are shown in parentheses. The analysis sample includes the Intact Sample and observations with relative years to divorce less than 10. Relative year -1 is excluded to make all estimates relative to the year prior to dissolution.

**Table A4. Fixed Effects Estimates of the Impact of Marital Dissolution on Women's School Investment**

Relative Years to Marital Dissolution	School Participation	Number of Months Enrolled
	(1)	(2)
-5 years	0.001 (0.011)	0.057 (0.079)
-4 years	-0.006 (0.010)	0.009 (0.074)
-3 years	-0.003 (0.010)	-0.027 (0.068)
-2 years	0.007 (0.010)	0.065 (0.074)
0 years	0.001 (0.010)	0.001 (0.072)
1 year	0.005 (0.011)	-0.047 (0.077)
2 years	0.010 (0.011)	0.043 (0.086)
3 years	0.025** (0.012)	0.164* (0.091)
4 years	0.034*** (0.012)	0.237*** (0.094)
5 years	0.032** (0.013)	0.239** (0.099)
6 years	0.032** (0.013)	0.277*** (0.103)
7 years	0.026** (0.013)	0.261** (0.103)
8 years	0.020 (0.013)	0.159 (0.099)
9 years	0.016 (0.013)	0.148 (0.100)
10 years	0.021 (0.013)	0.188* (0.108)
Observations	56180	56180
Individuals	2993	2993
Mean (SD) of Dependent	0.091 (0.287)	0.585 (2.126)
$R^2$	0.279	0.283
F-test $\rho H_0: \gamma_j = 0, \forall j < 0$	0.792	0.529

NOTE.--Regressions control for age, age squared, number of children, an indicator for presence of child under age 1, a measure of the wage premium, and state-specific year and individual fixed effects. All standard errors are clustered at the individual level and are shown in parentheses. The analysis sample includes the Intact Sample and observations with relative years to divorce less than 10. Relative year -1 is excluded to make all estimates relative to the year prior to dissolution.

**Table A5. Fixed Effects Estimates of the Impact of Marital Dissolution on Highest**

Relative Years to Dissolution	
-5 years	0.007 (0.024)
-4 years	0.025 (0.023)
-3 years	0.021 (0.024)
-2 years	0.023 (0.024)
0 years	0.030 (0.028)
1 year	0.038 (0.030)
2 years	0.049 (0.033)
3 years	0.040 (0.032)
4 years	0.052 (0.033)
5 years	0.055 (0.036)
6 years	0.066* (0.037)
7 years	0.068* (0.039)
8 years	0.077* (0.041)
9 years	0.099** (0.043)
10 years	0.141*** (0.046)
Observations	56180
Individuals	2993
Mean (SD) of	13.232 (2.47)
$R^2$	0.964
F-test $\rho H_0: \gamma_j =$	0.595

NOTE.--The dependent variable is number of years of education. Regressions control for age, age squared, number of children, an indicator for presence of child under age 1, a measure of the wage premium, and state-specific year and individual fixed effects. All standard errors are clustered at the individual level and are shown in parentheses. The analysis sample includes the Intact Sample and observations with relative years to divorce less than 10. Relative year -1 is excluded to make all estimates relative to the year

**Table A6. Fixed Effects Estimates of the Impact of Marital Dissolution on Number of Months Enrolled in School**

Relative Years to	Intact and Separated	Intact	Separated
	(1)	(2)	(3)
-5 years	0.057 (0.079)	0.003 (0.099)	0.042 (0.081)
-4 years	0.009 (0.074)	-0.066 (0.086)	-0.028 (0.076)
-3 years	-0.027 (0.068)	-0.092 (0.072)	-0.07 (0.069)
-2 years	0.065 (0.074)	-0.002 (0.058)	0.024 (0.072)
0 years	0.001 (0.072)	-0.075 (0.059)	-0.039 (0.082)
1 year	-0.047 (0.077)	-0.114 (0.073)	-0.099 (0.095)
2 years	0.043 (0.086)	-0.024 (0.087)	0.017 (0.110)
3 years	0.164* (0.091)	0.112 (0.092)	0.126 (0.120)
4 years	0.237** (0.094)	0.184* (0.094)	0.170 (0.127)
5 years	0.239** (0.099)	0.185* (0.098)	0.178 (0.139)
6 years	0.277*** (0.103)	0.209** (0.102)	0.192 (0.147)
7 years	0.261** (0.103)	0.195* (0.108)	0.186 (0.156)
8 years	0.159 (0.099)	0.099 (0.104)	0.072 (0.160)
9 years	0.148 (0.100)	0.086 (0.103)	0.056 (0.168)
10 years	0.188* (0.108)	0.117 (0.111)	0.076 (0.180)
Observations	56180	50100	22491
Individuals	2993	2993	1280
Mean (SD) of	0.585 (2.126)	0.575 (2.114)	0.643 (2.223)
$R^2$	0.283	0.303	0.322
F-test $\rho H_0: \gamma_j = 0, \forall j <$	0.529	0.543	0.458

NOTE.--Regressions control for age, age squared, number of children, an indicator for presence of child under age 1, a measure of the wage premium, and state-specific year and individual fixed effects. All standard errors are clustered at the individual level and are shown in parentheses. The analysis sample includes the Intact Sample and observations with relative years to divorce less than 10. Relative year -1 is excluded to make all estimates relative to the year prior to dissolution.