ESSAYS ON THE CONSEQUENCES OF DEMOGRAPHIC AND FAMILY CHANGE: THE CASES OF TEEN PARENTHOOD AND RELATIONSHIP DISSOLUTION

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ESSAYS ON THE CONSEQUENCES OF DEMOGRAPHIC AND FAMILY CHANGE: THE CASES OF TEEN PARENTHOOD AND RELATIONSHIP DISSOLUTION

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Chapter 1 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. I also 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.

Chapter 2 uses variation in the effectiveness of child support enforcement to identify the effect of child support income on paid employment of single mothers. Employing data from the Survey of Income and Program Participation, I find that child support income has a positive effect on the paid employment of previously married single mothers; each $1,000 increase in child support income increases the likelihood of paid employment by 6.7 percentage points. The effect is localized to lower-educated single mothers, for whom a $1,000 increase in child support income increases the likelihood of paid employment by 18.2 percentage points.

Chapter 3 uses data drawn from two cohorts of youth from the National Longitudinal Survey of Youth 1979 and 1997 (NLSY79 and NLSY97), we examine the relationship between teen parenthood and various socioeconomic indicators, with careful attention to the role of family- and individual-level unmeasured heterogeneity. We find that teen mothers in the NLSY79 had a larger employment penalty than teen mothers in the NLSY97, but teen mothers in the NLSY97 had a larger poverty penalty than teen mothers in the NLSY79. The results for men suggest that much of the observed correlation between teen fatherhood and the socioeconomic outcomes studied dissipate when controlling for unobserved family-level characteristics.

BIOGRAPHICAL SKETCH

Reginald Covington earned his Bachelor of Science degree in Mathematics from University of Maryland in 2006. During the summer of 2006 he was a research assistant in the Department of Agriculture's Economic Research Service (ERS). Following his tenure at ERS, he enrolled in the economics Ph.D. program at Cornell University in August 2006.

While pursing his degree, Reginald worked as a teaching assistant for courses such as Intermediate Microeconomics and the Economics of Crime. In addition, Reginald taught an introductory microeconomics course at the Auburn Correctional Facility. From 2008-20011, he was a research assistant for Professor H. Elizabeth Peters.

I dedicate this dissertation to my Lord and Savior, Jesus Christ, who is wonderful beyond imagine. To my fiancé, Danielle Turner, I thank you for your support and for crossing my path during my journey at Cornell. Your love, at times, carried me and I know that you prayed for me always. Finally, to my mother, I thank you for offering encouraging words during the tenuous seasons of my life. You are the best mother in the world.

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CHAPTER 1

MARITAL DISSOLUTION AND WOMEN'S SCHOOL INVESTMENT

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.[1] 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.[2] 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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[1] 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).

[2] In this paper, marital dissolution is defined as divorce or marital separation.

than their traditional counterparts.[3] 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

[3] 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 martial dissolution for women.[4] 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.[5] 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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[4] Cohabitation is perhaps a less reliable avenue for divorced women to increase their standard of living. Winkler (1997) finds that, as a group, cohabitors 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 cohabitions, which are less likely to end in marriage, are more likely among women with low education.

[5] 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.[6] 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

[6]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.[7]

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.[8] 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.

[7] 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.

[8] 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.[9] 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.[10] 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.

[9] The effects of divorce from remarriages likely reflect difference properties than first marriages (Couch et al., 2011).

[10] 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 1.1 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 1.1 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 1.2 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 1.1 and 1.2, 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 1.1 and 1.2 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 1.3. 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.[11] 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 1.4, 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 1.1 and 1.2 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.[12] Indeed, Stevenson (2007) finds that new couples are less likely to support each other through school when unilateral dissolution laws are enacted. However, Figures 1.1 and 1.2 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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[11] 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.

[12] 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 1.1 and 1.2 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:

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

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, τ_i are individual fixed effects, η_{st} are state-specific year effects, and ε_{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.[13] 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

[13] 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.[14] 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

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

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 γ_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.[15] In addition, it is plausible that

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

[15] The NLSY79 panel is unbalanced with respect to relative year to separation. Consequently, each γ_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 η_{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, η_{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 B1.3. 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:

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

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 1.5 and 1.6 plot the estimates of γ_j from equation (1) for school enrollment and number of months enrolled, respectively. The points depict the estimates of the γ 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. [16] Regression estimates for the results in Figures 1.5 and 1.6 are presented in Appendix Tables A1.3 and A1.4.

Consistent with the trends in Figures 1.1 and 1.2, the results provide evidence that marital dissolution has a positive impact on school enrollment. Focusing on Figure 1.5, 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 1.6, 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 1.4 and 1.5, 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

[16] The γ_j coefficients identify treatment effects relative to the effect for the year before separation, γ_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.

$\gamma_j = 0, \forall j < 0$. Thus, there is no evidence that there is selection into divorce timing based on recent school enrollment patterns.[17]

While Figures 1.5 and 1.6 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 1.7 plots the point estimates from equation (1) for highest grade completed and regression estimates are in Appendix Table A1.5. 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 6th 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 γ coefficients in equation (1) is the use of a suitable control group to account for secular variation in school enrollment outcomes. Recall that figures 1.1 and 1.2 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 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 1.4 and 1.5, 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 A1.4, 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 A1.6 are less precise than the estimates in table A1.4, 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 1.5, 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 1.5 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 1.5, 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 1.5 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.

Table 1.1 Descriptive Statistics for Intact and Separated Samples

	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	21.92	**
	(5.71)	(4.39)	
AFQT percentile in 1980	56.70	45.03	**
	(29.19)	(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	24.91	**
	(6.81)	(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 1.2 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	40.00	**
	(28.23)	(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. $*p < .05$; $**p < .01$

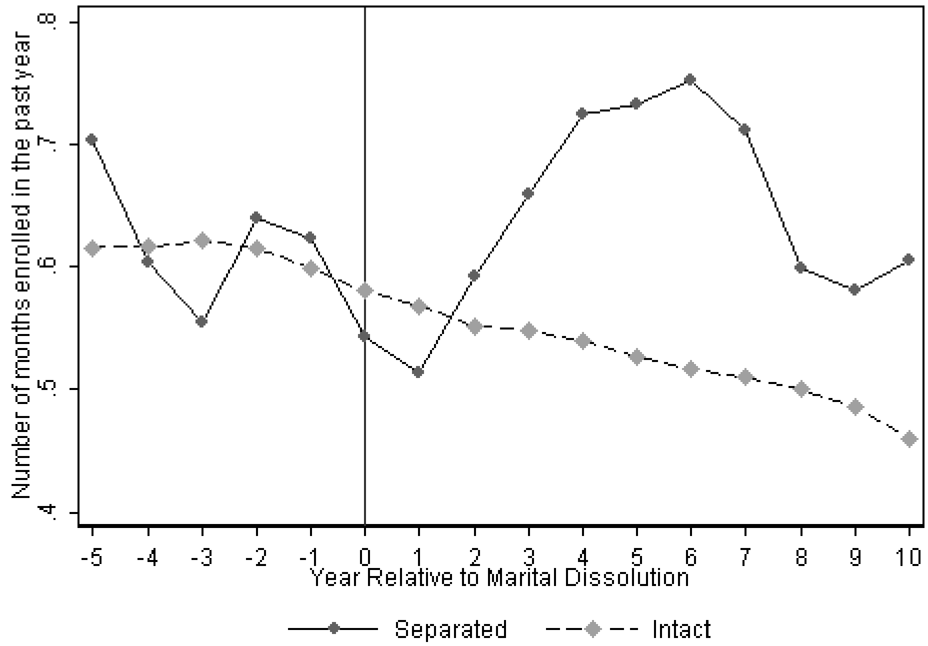

Figure 1.1--Women's average months enrolled in school. The Intact Sample is comprised of women who were observed for at least 4 survey rounds while married and who did not experience marital dissolution. The Separated Sample consists of women who were observed married with complete data for at least 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.

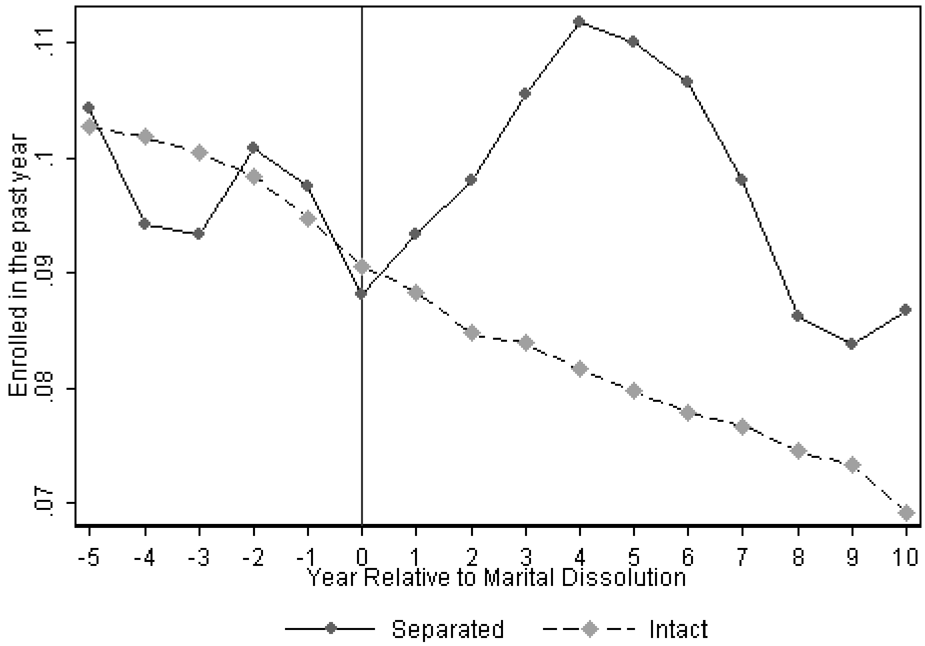

Figure 1.2.-- Women's school enrollment rate. The Intact Sample is comprised of women who were observed for at least 4 survey rounds with while married and who did not experience marital dissolution. The Separated Sample consists of women who were observed married with complete data for at least 3 survey rounds before divorce and who were observed for at least 1 survey round after divorce. The vertical line marks the year when the divorce is initiated.

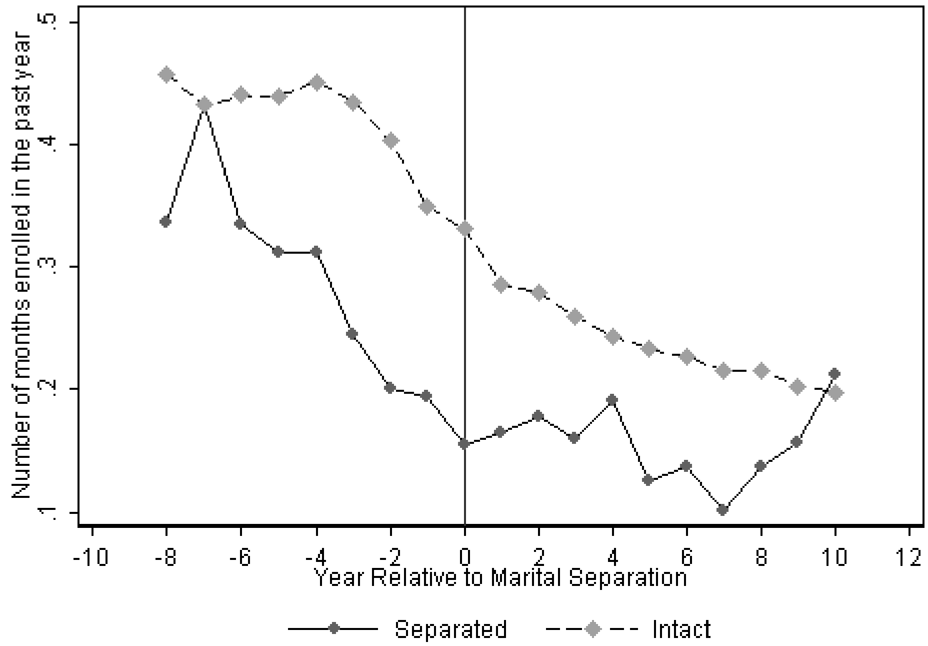

Figure 1.3.-- Mens's average months enrolled in school. The Intact Sample is comprised of men who were observed for at least 4 survey rounds with while married and who did not experience marital dissolution. The Separated Sample consists of men who were observed married with complete data for at least 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.

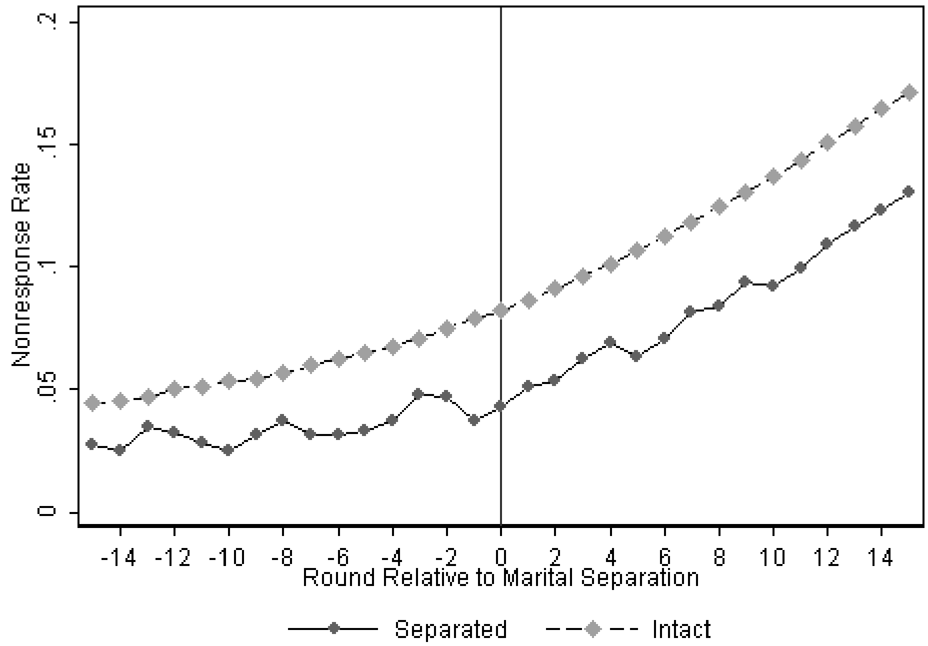

Figure 1.4.-- Women's nonresponse rates relative to year of dissolution. The Intact Sample is comprised of women who were observed for at least 4 survey rounds while married and who did not experience marital dissolution. The Separated Sample consists of women who were observed married with complete 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.

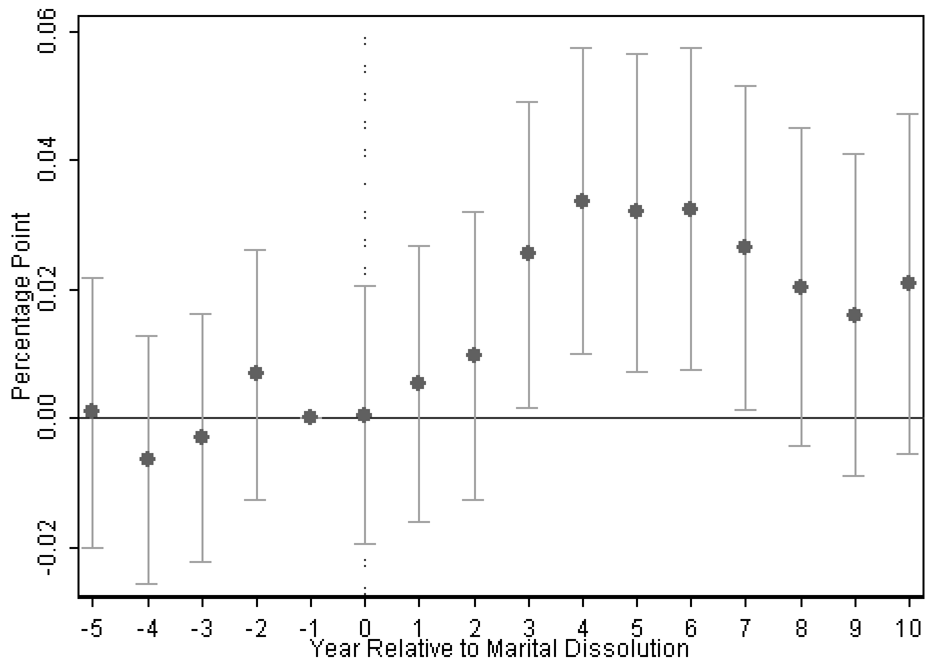

Figure 1.5. The effect of marital dissolution on the school participation rate. The points depict coefficient estimates from estimation of eq. (2) on the analysis sample, as explained in the text. The bars extending from each point represent the bounds of the 95% confidence interval calculated from standard 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.

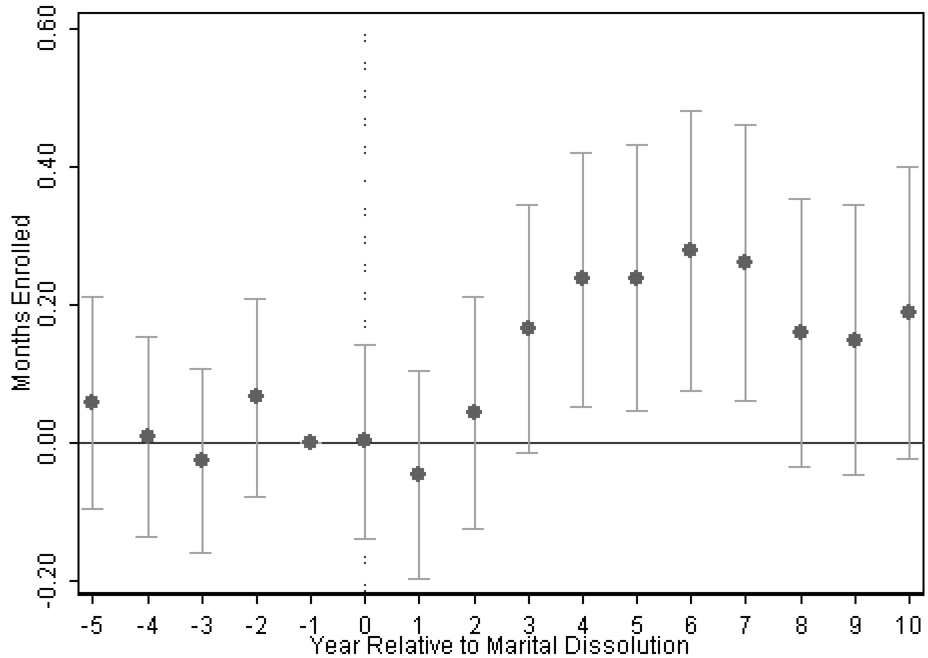

Figure 1.6. The effect of marital dissolution on months enrolled in regular school. The points depict coefficient estimates from estimation of eq. (2) on the analysis sample, as explained in the text. The bars extending from each point represent the bounds of the 95% confidence interval calculated from 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.

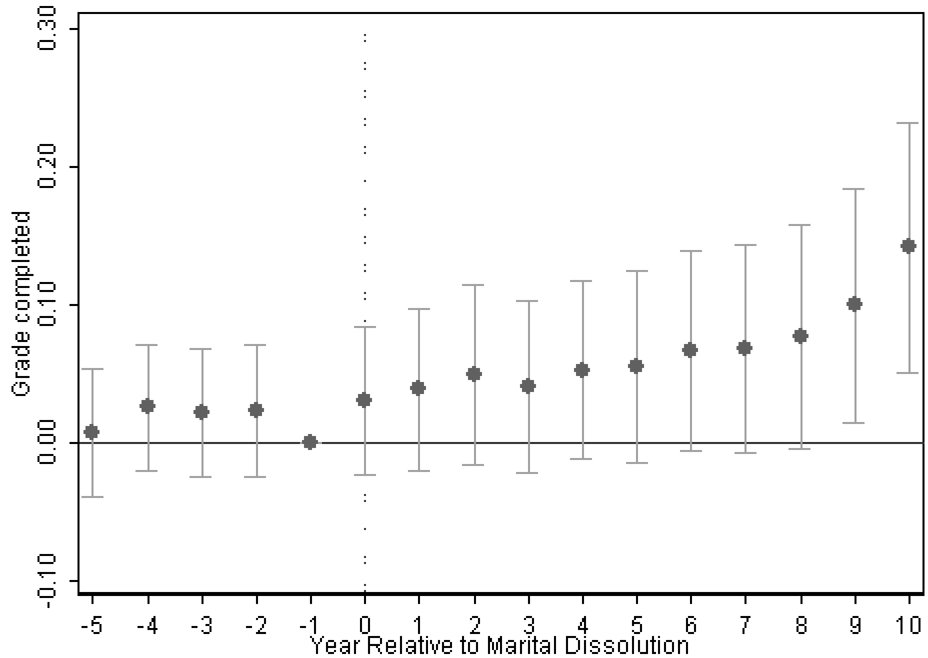

Figure 1.7. The effect of marital dissolution on highest grade completed. The points depict coefficient estimates from estimation of eq. (2) on the analysis sample, as explained in the text. The bars extending from each point represent the bounds of the 95% confidence interval calculated from standard 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 1.5. 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	0.057	-0.006	0.038	0.019
	(0.060)	(0.084)	(0.0495)	(0.060)	(0.070)
AD	0.099*	0.099	0.248***	0.072	0.260***
	(0.060)	(0.082)	(0.057)	(0.060)	(0.078)
Black x BD (1-3 years)		0.001			-0.031
		(0.134)			(0.136)
Black x AD		0.053			0.034
		(0.132)			(0.134)
Hispanic x BD (1-3 years)		-0.054			-0.104
		(0.152)			(0.152)
Hispanic x AD		-0.092			-0.171
		(0.129)			(0.128)
Less than high school x BD (1-3			0.093		0.120
			(0.076)		(0.082)
Less than high school diploma x			0.083		0.123
			(0.081)		(0.086)
Some college x BD (1-3 years)			0.350**		0.350**
			(0.166)		(0.168)
Some college x AD			-0.175		-0.198
			(0.157)		(0.156)
Bachelor's Degree x BD (1-3			-0.299		-0.308
			(0.274)		(0.273)
Bachelor's Degree x AD			-0.980***		-0.987***
			(0.252)		(0.252)
Husband's income share x BD				0.002***	0.001
				(0.0001)	(0.001)
Husband's income share x AD				0.017***	0.018***
				(0.0001)	(0.001)
Observations	56180	56180	56180	56180	56180
Individuals	2993	2993	2993	2993	2993
Mean (SD) of Dependent Variable	0.585	0.585	0.585	0.585	0.585
	(2.126)	(2.126)	(2.126)	(2.126)	(2.126)
R^2	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

Table A1.1.—Distributions of Months Enrolled

	Intact Sample	Separated Sample		
Months Enrolled		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 A1.2.—Enrollment Rate and Average Months Enrolled of Separated Sample

Year relative to dissolution	Separated Sample: Enrollment Rate	Separated Sample: Average Months Enrolled	Separated Sample: N	Intact Average Months Enrolled
-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.

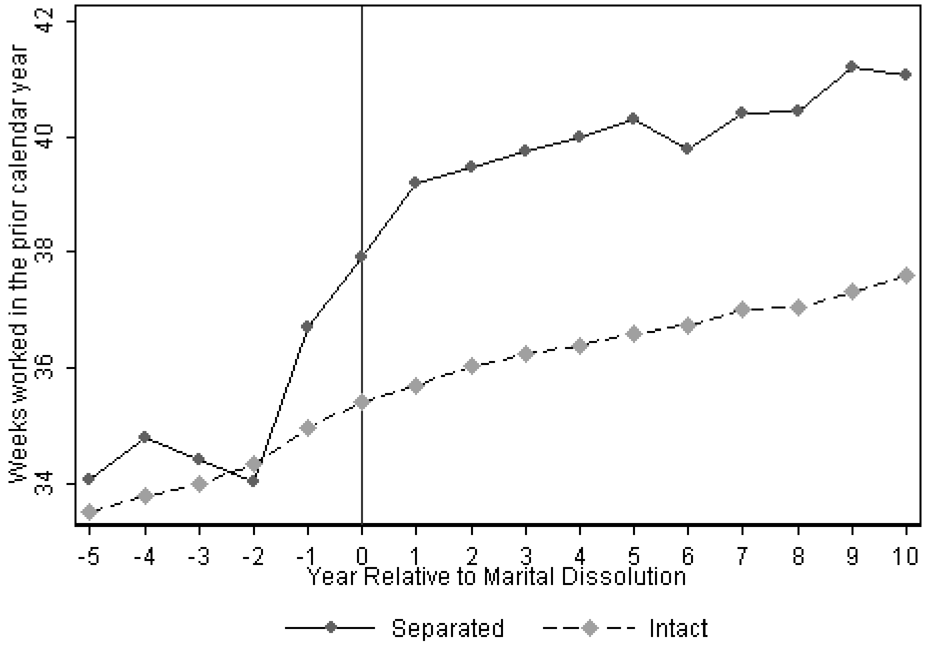

Figure A1.1-- Women's average weeks worked relative to year of dissolution. The Intact Sample is comprised of wo men who were observed for at least 4 survey rounds while married and who did not experience marital dissolution. The Separated Sample consists of women who were observed married with complete 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.

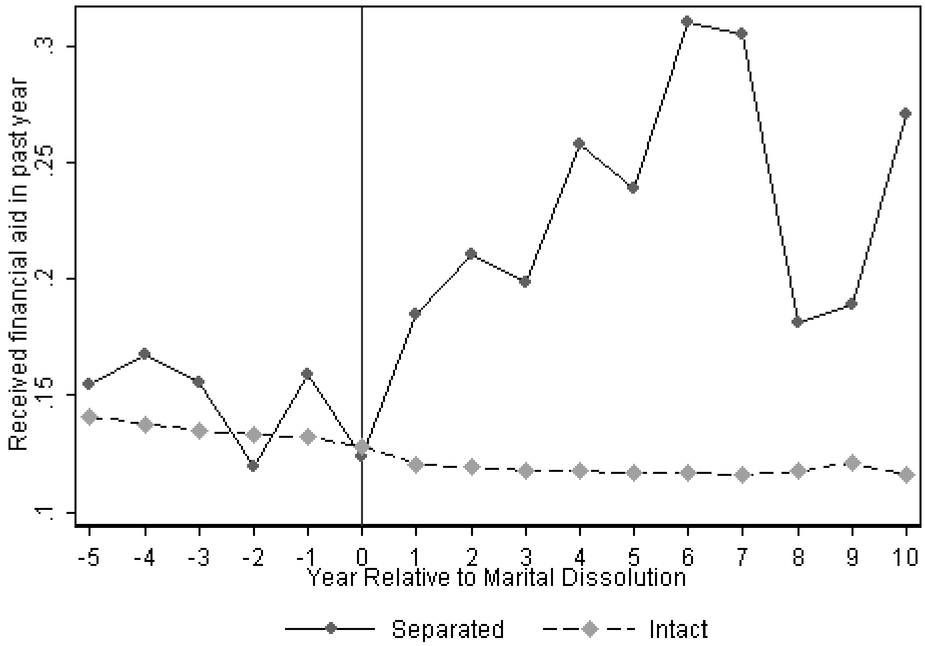

Figure A1.2.--Women's propensity to receive financial aid. Percentages are based on sample who reported attending college in the previous year. The Intact Sample is comprised of women who were observed for at least 4 survey rounds while married and who did not experience marital dissolution. The Separated 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 A1.3. Fixed Effects Estimates of the Impact of Marital Dissolution on Women's School Investment

	School Participation	Number of Months Enrolled
Variable	(1)	(2)
Age	-0.018**	-0.150**
	(0.008)	(0.058)
Age, Squared	0.0005**	0.001**
	(0.000)	(0.001)
School Wage Premium	0.191***	1.633***
	(0.027)	(0.210)
Total Children	-0.015***	-0.097***
	(0.003)	(0.022)
Presence of child age less than 1	-0.050***	-0.368***
	(0.004)	(0.031)
Observations	56180	56180
Individuals	2993	2993
Mean (SD) of Dependent Variable	0.091	0.585
	(0.287)	(2.126)
R^2	0.279	0.283
F-test ρ $H_0: \gamma_j = 0, \forall j < 0$	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 A1.4. Fixed Effects Estimates of the Impact of Marital Dissolution on Women's School Investment

Relative Years to Marital Dissolution	School Participation (1)	Number of Months Enrolled (2)
-5 years	0.001	0.057
	(0.011)	(0.079)
-4 years	-0.006	0.009
	(0.010)	(0.074)
-3 years	-0.003	-0.027
	(0.010)	(0.068)
-2 years	0.007	0.065
	(0.010)	(0.074)
0 years	0.001	0.001
	(0.010)	(0.072)
1 year	0.005	-0.047
	(0.011)	(0.077)
2 years	0.010	0.043
	(0.011)	(0.086)
3 years	0.025**	0.164*
	(0.012)	(0.091)
4 years	0.034***	0.237**
	(0.012)	(0.094)
5 years	0.032**	0.239**
	(0.013)	(0.099)
6 years	0.032**	0.277***
	(0.013)	(0.103)
7 years	0.026**	0.261**
	(0.013)	(0.103)
8 years	0.020	0.159
	(0.013)	(0.099)
9 years	0.016	0.148
	(0.013)	(0.100)
10 years	0.021	0.188*
	(0.013)	(0.108)
Observations	56180	56180
Individuals	2993	2993
Mean (SD) of Dependent	0.091	0.585
	(0.287)	(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 A1.5. 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 p	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 A1.6. Fixed Effects Estimates of the Impact of Marital Dissolution on Number of Months Enrolled in School

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

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CHAPTER 2

I. Introduction

An important issue underlying recent social policy debates has been the extent to which providing for low-income children is the responsibility of parents or the public. The relevance of this question is underscored by the large differences across family types in the poverty rate. In the United States in 2009, about 70 percent of children living with a single mother were poor or low income, compared to 32 percent of children living in other types of families (Mather, 2010).[18] Policy efforts to mitigate the high poverty rate among single-mother families include defining and enforcing child support obligations as well as providing income support through an assortment of government-funded welfare programs including Temporary Assistance for Needy Families (TANF), Food Stamps, and the Earned Income Tax Credit. However, with the passage of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996, single mothers have been expected to rely less on public assistance and more on their own capacity to earn income as well as transfers from the noncustodial parent.

Economic theory suggests that child support reform may both reduce public welfare expenditures and improve work incentives of poor single mothers. In the canonical static model of labor supply, the development of public assistance programs leads to a reduction in labor supply unambiguously among the eligible population because of the high benefit reduction or implicit tax rate (Burtless and Hausman, 1978; Moffit, 1983, 1986). The high implicit tax rate of the TANF program implies that a woman on welfare earning an extra dollar of income can typically expect her post-welfare income to rise by a small fraction of the extra dollar.

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[18] Families are characterized as poor if their total income is less than 100 percent of the official poverty threshold and "low income" if income is less than 200 percent of the poverty threshold.

However, because the benefit reduction rate creates a nonlinear budget constraint for eligible recipients, an increase in nonwage income does not necessarily reduce hours work (Moffit, 1986). More specifically, if child support income is sufficiently high to keep a woman off welfare, her earnings are not implicitly taxed at such high rates (Hu, 1999). Thus, reforms to increase child support receipt are particularly desirable because it may both reduce public welfare expenditures and increase labor supply of single mothers. Thus, it is important to identify whether child support income has a causal effect on the decision to work.

Many previous studies have documented the positive association between child support and child well-being indicators, such as educational attainment, schooling, and cognitive outcomes (Baydor and Brooks-Gunn 1994; Graham, Beller, and Hernandez 1994; Knox and Bane, 1994; Knox, 1996; Argys et al., 1998), but the positive association between child support receipt and the labor supply of single-mothers mothers has received less attention.[19] Earlier studies (e.g., Beller and Graham, 1985; O'Neil, 1985, Robins and Dickinson, 1985) use multiple regression techniques to examine the relationship between child support receipt and women's labor supply and show that women with child support are more likely to work and work longer hours than women without it. However, estimates from earlier studies measure the correlation of child support receipt with, not the causal effect of child support receipt on, women's labor supply. The correlation is an overestimate of the causal effect if, for example, men are more willing to pay child support to women who possess characteristics that are associated with a greater desire to work in the formal labor market (which is unlikely to be observed the econometrician). Conversely, the correlation is an underestimate of the causal effect if women who receive child support possess unobserved characteristics that are associated with a greater

[19] Grossman and Hayghe (1982) were the first to document the positive correlation between the receipt of child support income and labor supply.

desire to work at home. Several studies have accounted for the possibility that child support receipt and women's labor supply are jointly determined (See Graham and Beller 1989, Graham 1990, and Hu, 1999) but focus only on divorced/separated mothers, and the group of mothers with non-marital births is likely to be very different.

This paper paper builds on previous research by examining the relationship between the labor supply of previously-married and never-married single mothers and child support receipt. Endogeneity of child support receipt is addressed by estimating instrumental variables models, where child support policy variables are used as instruments. Specifically, I argue that the ratio of child support enforcement (CSE) collections to administrative expenditures is a valid instrument for child support income levels, and I use variation within states over time to identify the effect of child support income on paid employment. Further, I show that the instrument is a strong predictor of child support income for previously-married single mothers, but not for never-married single mothers. Consequently, I estimate IV models using only the sample of previously-married single mothers.

My main findings show a sizable positive relationship between child support income and paid employment for previously married mothers: a $1,000 increase in child support income increases the likelihood of paid employment by 6.7 percentage points, or 8.1 percent. These estimates are the strongest for previously married single mothers with an education level of high school diploma or less, for whom a $1,000 increase in child support income leads to a 18.2 percentage point increase in the probability of paid employment

The central finding of this analysis is that an increase in child support income has a positive effect on the employment status of previously-married single mothers. Moreover, the positive impact of an increase in child support income is largest for previously-married single mothers with relatively low education levels, an economically disadvantaged population. This result has important policy implications in light of the 2006 TANF reauthorization bill that includes a provision to allow single-mother families to receive both child support and TANF, which may increase the probability that noncustodial fathers pay support. My estimates imply that policies that are successful at increasing the amount of child support received by single mothers will have a positive effect on their labor supply.

II. History of Child Support Enforcement

The goal of child support is to mitigate the economic loss that children face as a result of living with just one parent. Prior to 1975, child-support enforcement was managed largely by family law in each state and enforced by the court system, while the federal government limited its child support efforts to children receiving Aid to Families with Dependent Children who were abandoned by one of her parents. Consequently, during this period, any family that wanted to receive child support had to hire an attorney and go to court. During this period, the court decided the amount of child support to be paid by the noncustodial parent on an individualized basis. Once the amount of support was decided, the noncustodial parent paid the support directly to the parent caring for the children.

In 1975, Congress added Part D to Title IV of the Social Security Act, establishing the Child Support Enforcement (IV-D) program. This legislation created the public system to enforce child support obligations. As part of legislation enacted in 1984, states were required to adopt expedited procedures for obtaining support orders either through the judicial system or in an administrative agency. In an effort to increase the number of awards to never-married mothers, states were required to extend the period during which paternity establishment can be initiated to any time up to the child's eighteenth birthday. Further, in an attempt to ensure award levels are consistent, states were required to establish by October 1987 child support guidelines to enable judges and others determining the sizes of awards to set equitable and adequate support payments—though, at the time, these guidelines were not binding on the judiciary.[20] Moreover, the amendments mandated that all states adopt automatic income withholding for child support to take effect after one month's delinquency. Wage withholding was then greatly expanded with the passing of the Family Support Act of 1988 by requiring immediate wage withholding to begin by November 1990 for all new or modified orders being enforced by the states. Most federal reforms have been extended to the private system, though it is still judicially based and complaint-driven (Sorensen and Hill, 2004).[21]

To this day, congress has never required families who are not on welfare to use the IV-D program. Nevertheless, the IV-D program must accommodate anyone who requests services. In contrast, welfare families are required to participate in this program and assign their rights to child support to the government as a prerequisite to receiving aid. Prior to 1984, any child support collected on behalf of welfare families was kept by the government and used to negate the costs of providing welfare to the family. These obligations of welfare recipients reflect the original goal of the IV-D program, which was to recover welfare costs.

Although using the collections of welfare recipients to offset administrative costs (what is termed "zero disregard") was considered cost effective, the policy had some major disadvantages. For example, why should custodial parents cooperate with the child support

[20] However, the Family Support Act of 1988 required states pass legislation making state child support guidelines a rebuttable presumption in any judicial or administrative proceeding and establishing the child support order that is established from the state guidelines the correct amount to be rewarded.

[21] For example, states were required, with some exceptions, to implement immediate wage withholding in all support orders first issued on or after January 1, 1994, regardless of whether the parent applied for child support services.

system if the support paid did not benefit them? Moreover, why should noncustodial parents pay formal child support if their children are not going to receive the money? With these concerns in mind, legislation enacted in 1984 required that the first $50 per month of current child support collected would go to the family, with the remainder retained by the government.

In 1996, there was another wave of child support reforms when PRWORA dramatically changed the nature of the welfare system by replacing AFDC with TANF. Perhaps most notable for this study, PRWORA abolished the requirement for a $50 per month disregard of child support and allowed states ample flexibility in determining how to handle child support paid on behalf of families receiving TANF. A majority of states now keep all child support paid on behalf of TANF families, whereas most of the remaining states continue to have a $50 per month disregard (Justice, 2007). As a consequence, mothers who receive welfare in a post-PRWORA environment, as is the case in this paper, have less of an incentive to corporate with child support officials than single mothers on welfare in the pre-PRWORA era.[22] There is some evidence that changing the disregard policy has the largest effect on never married mothers. For example, Sorensen and Hill (2004) find some evidence that never-married mothers have benefitted from the $50 pass-through, which was rescinded in 1996. More recently, Cancian, Meyer,and Casper (2008) find that when custodial parents keep all child support paid on their behalf, paternity establishment occurs more quickly, noncustodial fathers are more likely to support, and custodial families receive more income.

[22] Federal law requires applicants for, and recipients of TANF assign their rights to the state in order to receive benefits. In addition, each applicant or recipient must cooperate with the State to establish paternity of a child born outside of marriage and to collect child support payments. However, many states still allow mothers to simply attest to a lack of information about the father, and all states have a "good cause" exemption to protect mothers who may suffer injury from cooperation due to factors violence. Finally, states generally allow mothers who have not identified the father to do so as circumstances change (Turetsky 1998; Roberts 2000; Roff 2010).

Under Title IV-D of the Social Security Act, states are mandated to provide child services such as facilitating paternity establishment, locating absent parents, establishing child-support orders, and enforcing and adjusting child-support orders. As previously discussed, I use two-state level measures to indicate a state's relative effectiveness in these practices. First, I measure the average amount of administrative expenditures by the Child Support Enforcement office per single woman age 15 to 44. To measure the effectiveness of these expenditures, I include the ratio of the amount of child-support collections by the CSE office to the amount of administrative expenditures. Summary statistics for these measures are reported in Appendix Table 2.2.1 for the full sample, and by child support receipt and marital status (For a detailed description of these two variables, see Argys and Peters, 2003).[23]

III. The Effect of Child Support on Labor Supply

Economic theory predicts that changes in the level of child support income can influence the labor supply behavior of single mothers, where the predicted effect largely depends on the welfare status of the single mother. The standard consumption-leisure diagram is a convenient tool to demonstrate the influence of an increase in child support income on women's decisions regarding welfare and work. To illustrate the effects of interest, it is assumed the woman has a well-behaved preference function over leisure and consumption.

Following the theoretical exposition of Hu (1999), figure 2.1 shows the effects on an increase in child support income on a single mother's budget constraint. For simplicity, the woman is assumed to have no nonlabor income initially and the TANF earnings disregards for work expenses and child care are ignored. In the absence of TANF, her opportunities are

[23] Argys and Peters (2003) measure the average amount of administrative expenditures by the CSE office made on behalf of each family in the CSE caseload. As a result, the measure in their paper utilizes a different denominator than in this study.

represented by line OBX, the slope of which is equal to minus the wage rate. The TANF budget constraint is represented by OGBX. The length of OG is the guarantee level and point B is the break-even point. The slope between O and B is $-(1-\tau)W$, where τ is the TANF benefit reduction rate. Suppose the woman receives an increase in child support payment equal to the length of OC, so that the new budget constraint is $OGBY$. The woman who is initially at Point G, on welfare and working zero hours, will jump discontinuously from point G to Point 2 if the child support payments are large enough. For such a woman, child support income has three effects: it increase her income opportunities, leads her to leave the welfare rolls, and induces her to start working in the labor market. A woman who initially works positive hours and receives TANF benefits starts at point 2. This woman can increase her total income without changing her work hours because the new budget constraint lies above the TANF budget constraint at those hours of work. However, the increase in her effective wage rate induces substitution away from leisure, so that the woman may choose to increase her hours of work to Point 2, depending on the sizes of the income and substitution effects. Like the case in which a woman moves from Point G to Point 1, this increase in hours of work is simultaneous with the woman's exit from the TANF program. For single mothers who do not receive welfare, standard theory predicts that an increase in child support income would have a negative effect on labor supply.

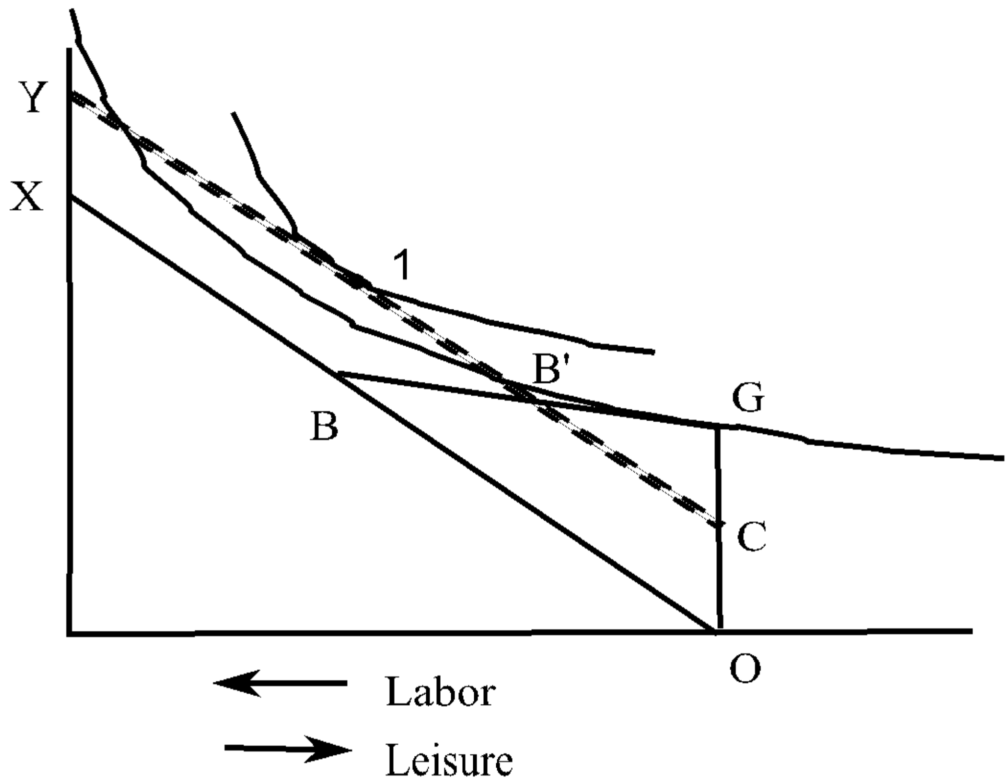

Figure 2.1. Effect of an increase in child support income

Although economic models lend theoretically ambiguous predictions, empirical evidence tend to show a positive association between child support income and labor supply of single mothers . For example, evidence from NLSY 1988 data suggests custodial mothers who received child support were more likely to be in the labor market and to work longer hours compared to mothers who did not receive child support (Veum, 1992). This study also finds that custodial mothers who received child support have higher educational attainment and wages compared to those who received no support, which suggests women who do and do not receive child support are likely to differ from each other in ways that are correlated with support receipt and labor supply and that are unobserved by the econometrician. In other words, child support receipt may be endogenous.

Several studies have extended earlier work by accounting for endogeneity problems, but the results have been mixed. These studies have examined the effects of child support payments on the labor supply of previously married mothers only (Graham and Beller, 1989; Hu, 1999). The dependent variables were typically welfare participation, labor force participation, and hours of work. Graham and Beller (1989) use data from a sample of divorced or separated women in the 1979 and 1982 March/April Match files of the Current Population Survey (CPS) to examine the relationship between hours worked and child support. They address the endogeneity of child support income by using and instrumental variable approach and find evidence that child support income deters hours of work, but that the effect is modest. For example, they find that a $1,000 increase in child support income reduces work by just two hours per year. Graham (1990), using the 1979 and 1982 March/April CPS, restricts the sample to women due child support awards and finds that a $1000 increase in child support income reduces the labor supply of single mothers by 54 hours. Hu (1999) extends the previous studies by using an instrumental variables procedure that incorporates child support policy measures that vary across state and time. Using the Panel Study of Income Dynamics, Hu finds evidence that an increase in child support income would lead to a modest increase in the proportion of divorced/separated mothers who work. Garfinkel, Heintze, and Huang (2001) examine the effect of child support income on the income of single mothers by using child support expenditures per single mother and a legislative index which accounts for the probability of receiving a formal obligation, the level of the obligation, and the probability of receiving the full obligation. Using the CPS from1979–1999, they find that the income of single mothers increased by two dollars for each dollar of child support received. Differences in sample composition could be the reasons why the effect of child support on mother's labor supply is unclear. Theory predicts the effect of child support income will differ by welfare status of the single mothers, yet the previous studies did not split samples by welfare status or eligibility. Consequently, examining the effect of child support income on the labor supply of a disadvantaged sample may yield different results than when analyzing a more advantaged sample. To examine how the labor supply effects of child support income differ by welfare status, this study will present results by education level under the assumption that single mothers with lower levels of education are more likely than their counterparts with more education to be on welfare. It follows that the effect of child support income on the labor supply of single mothers with relatively lower levels of education is expected to be nonnegative while the effect on single mothers with higher education levels is expected to be negative.

IV. Empirical Methodology

A. Empirical Model

The central difficulty in identifying how variation in child support income affects employment decisions is that child support income is not randomly assigned across female-headed households: unobserved factors that affect a woman's propensity to work may be correlated with the amount of child support she receives. I attempt to overcome this problem by using a measure of child support enforcement that varies across state and over time to generate exogenous variation in child support income. Specifically, I measure child support enforcement as the ratio of child support collections to child support enforcement (CSE) administrative expenditures. Measuring child support enforcement as a ratio of child collections to CSE administrative expenditures is an improvement over prior measures because this measure encompasses not only the interaction of the strength of child support laws and effort to enforce the laws but also the efficacy of state practices in implementing laws. For example, a state may be a leader in passing child support legislation, but its laws may not be enforced. Expenditures may be a good measure of a state's commitment to enforcement, but states with the worst collection rates may need to spend more to improve their outcomes. Moreover, past studies have shown that child support awards and receipt are more likely in states with high ratios of CSE collections to expenditures (Peters et.al, 2004). Therefore, I estimate the following model, instrumenting child support income with the ratio of the amount of child support collections by the CSE office to the amount of administrative:

$$Employ_i = \beta_0 + \beta_1 CS_i + \gamma X_i + s_i + t_i + \epsilon_i \tag{1}$$

where $Employ$ is a dummy variable equal to 1 if individual i had a paid job during the reference period and CS refers to child support income. The model also includes a vector of individual characteristics as well as state labor market measures (X). Finally, I include state fixed effects (s) and year fixed effects (t).

The motivation for instrumenting child support income with child support policy measures is two-fold. First, child support income level is likely endogenous because child support income is not randomly assigned among all eligible recipients. In other words, I am concerned about the presence of unmeasured attributes associated with the level of child support income that also partially determine decisions related to human capital investment. For example, women who desire to work at home may pursue child support receipt more aggressively than women who have less of a desire to specialize in home production, imparting a negative correlation between the error term and the level of child support income. Second, within-state variation in child support policy measures are an arguably exogenous source of child support variation. Using child support policy measures in the year of their first child's birth for never-married single mothers or the year of separation for previously-married single mothers as an instrument for child support income levels allows me to identify β_1 in equation (1) under the conditions that child support policy measures provide a significant source of child support income variation and that this child support income variation is exogenous, i.e., is uncorrelated with the error term conditional on the observables in the model.

One important factor that would cause child support policy variables to be endogenous is the existence of unobserved state characteristics that are correlated with both the employment of single mothers and child support income levels, the most likely of which is the generosity of the state's welfare programs. For example, if states that have less generous welfare programs tend to have higher levels of employment among single mothers and greater levels of child support enforcement, I may find spuriously positive results.

Equation (1) contains several controls for such selection. I control for the maximum level of Temporary Assistance to Needy Family (TANF) benefits for a single mother with 3 children to capture the generosity of the state's welfare programs. I also include state –level fixed effects, to proxy for differences in welfare generosity across states.

Conditional on these controls, the child support income variation I use to identify equation (1) comes from several sources. First, there is a strong time- and area-specific component to child support policy variables. For example, the ratio of CSE collections to administrative expenditures in New York increased from 3.39 in 1994 to 4.76 in 2001. In contrast, in Maryland, the ratio of CSE collections to administrative expenditures decreased during the same period from 4.71 to 3.91. Thus, this state-level variation incorporates both differences in within-state measures of child support policy variables over time and differences across states in a given year in the magnitude of the child support policy variable. This source of variation is ideal because it allows one to compare employment decisions of single-mothers with the same observable characteristics but who experience different child support income levels based on their location and the timing of becoming eligible to receive child support.

The final potential identification concern with equation (1) is that both child support policy measures and employment may be correlated with local labor market conditions. It is conceivable that labor demand shocks could increase the ratio of child support collections to expenditures by increasing child support transfers due to an increase in the employment of noncustodial fathers. If local labor demand shocks increase the ratio of child support collections per administrative expenditures and increase employment rates, my estimates of β_1 will be biased upward due to this spurious correlation. To mitigate this concern, I control directly for yearly variation in unemployment rates and real income per capita. In Section #, I also estimate the model only for childless single women, who experience the local macroeconomic fluctuations but not the financial gains from child support income increases. I find childless single women do not respond to changes in child support policy variables, which suggests my estimates are not being biased by unobserved concomitant macroeconomic shocks.

Throughout the analysis, I estimate separate models for never-married mother and previously married mothers because the process of establishing child support are different for these populations. For example, a never-married mother must legally establish paternity of her children prior to child support being awarded. For previously married mothers, paternity is not an issue. Nevertheless, estimating separate models for previously married and never-married mothers assumes that the child support enforcement variables do not affect the decision to divorce or have a child outside of marriage (Sorensen and Hill, 2004). Yet previous studies providence evidence that child support enforcement reduces divorce (Nixon 1997) and reduces nonmarital childbearing (Plotnick et al., 1999). Therefore, if women who are deterred from nonmarital childbearing or divorce are more responsive to these policies than other women, then the IV estimates of the effect of child support receipt are biased downward.

V. Data

The individual-level data in this analysis come from the 1996, 2001, and 2004 panels of the Survey of Income and Program Participation (SIPP). Each panel of SIPP includes a nationally representative sample ranging from 40,000 to 51,000 households, where each household is interview every 4 months. In both the 1996 and 2004 panels, interviews span a period of 4 years. For the 2001 panel, interviews span a period of 3 years. These data are particularly suited to address the central research question in this paper because they contain information on employment, school enrollment, child support arrangements, and a rich set of demographic characteristics. Each interview (or wave) gathers core labor force, income, household and family composition, and program participation data. In contrast, topical questions are not repeated in each interview and are designed to gather specific information on a wide variety of subjects.

Most of the data used in this paper come from the child support topical module (wave 5). This module identifies women who are eligible to receive child support and asks detailed questions about the characteristics of the child support agreement. Specifically, the survey asks about the type of agreement, frequency of payments and annual amount. The survey also asks about other forms of financial assistance, such as provisions for health care costs, which are not employed in this analysis.

The sample used in this analysis is single women who are eligible to receive child support. Women who are 15 years old or older and are biological or adoptive parents of children less than 21 years old whose other parent is not in the household are considered eligible to receive child support. The analysis focuses on single mothers, and does not include married women who are eligible for child support, because this group is more likely than married women to be on public assistance.[24]

Characteristics of the analysis sample are presented in first column of Appendix Table 2.1. Most of the variables are measured during the child support module.[25] Slightly fewer 40 percent of the sample received child support during the previous 12 months. The average respondent has slightly fewer than two children who are eligible to receive child support and for 16 percent sample the age of the youngest child is less than five years of age. In regards to race, 32 percent of the sample is black and 16 percent is Hispanic. About 65 percent of the mothers have been previously married. Only 19 percent of the sample has a bachelor's degree or more while 19 percent have not obtained a high school diploma.

Columns 2 and 3 report means by child support status for the full sample. In general, respondents who have received child support appear less disadvantaged than respondents who have not received child support. For example, 70 percent of respondents who have received child support were previously married as compared to only 47 percent of the respondents who have not received child support. Respondents who have not received child support are also less

[24] I begin with a sample of 9201 single women who are 15 years old or older and are biological or adoptive parents less than children less than 21 year old whose other parent is not in the household. Exclusions from the sample are made if the mother was not in the sample or did not interview during wave 2 (927). During the wave 2 interview, respondents are asked questions about their migration history such as their place of birth, where they lived, and how long they lived there. Obtaining information pertaining to migration history is vital to merging child support enforcement data. Further, I exclude an additional 521 mothers for whom information on child support enforcement is unavailable. The resulting analysis sample includes 7,843 mothers.

[25] The age of the youngest child was collected from the fertility topical module, which is conducted during wave 2. The respondent's education is from wave 4.

likely to have obtained at least a high school diploma than respondents who received child support. Most notably, respondents who received child support are 17 percent more likely to have had paid employment during the reference period (previous 4 months) than respondents who have not received child support.

I also report means by marital status. Not surprisingly, never-married respondents are less likely to receive child support than previously married respondents. Previously married mothers are 84 percent more likely than never-married mothers to receive some child support. Among respondents who report receiving child support, the average amount received for never-married mothers and previously married mothers is approximately $1,840 and $3,290, respectively. For both groups, child support receipt and employment are positively correlated. Never-married (previously married) mothers who received child support are 18 (10) percent more likely to be employed than then never-married (previously married) who did not receive child support.

To address the possible endogeneity of the level of child support income and employment status, child support policy measures likely to affect the level of child support but not employment status directly are used as instrumental variables. To link the respondent-level data with state-level policy variables, I use the migration history data to identify the state of residence at the time of separation and at the time of the birth of their first child for previously married and never-married mothers, respectively. For the full sample and samples stratified by marital status, child support receipt is associated with a higher level of CSE collections per administrative expenditures. Interestingly, child support receipt is also negatively associated with the state unemployment level. Thus, the positive relationship between child support policy variables and child support receipt could be the result of local labor market shocks. This underscores the importance of controlling for state-level unemployment and real income per capita in all models.

VI. Results

A. Estimates by Marital Status

2.1 presents OLS estimates of equation (1) using the SIPP data described in Section # for the 1996, 2001, and 2004 panels. In the first column, I present OLS estimates of the relationship between child support income levels and paid employment. Conditional on the demographic controls in the model, child support income levels are only modestly associated with the probability of paid employment: a $1000 increase in child support income is associated with a 0.84 of a percentage point change in the likelihood of paid employment.

In the next two columns of Table 2.1, I present OLS estimates of the relationship between child support income levels and paid employment by previous marital status. In column (2), I find that a $1,000 change in child support income leads to 1.7 of a percentage point change in the likelihood of paid employment for never-married mothers. For previously married mothers, a $1,000 increase in child support income is associated with a 0.71 percentage point increase in the probability of paid employment. There is reason to believe that these estimates are biased, however, if both the probability of paid employment and child support income level are, in part, determined by unobservable characteristics of the mothers.

In Table 2.2, I instrument child support income levels with the natural log of CSE collections per administrative expenditures. In column (1), I find that when the CSE effectiveness measure is used as an instrument for child support income level, a $1,000 change in child support income leads to a 5.4 percentage point change in the likelihood of paid employment. However, this estimate is not statistically distinguishable from zero at any conventional level. For never-married mothers, the bottom of column (2) shows that natural log of CSE collections per expenditure is not strongly associated with child support income levels. For previously married mothers, the second stage estimates in column (3) show that a $1,000 increase in child support income increases the likelihood of paid employment by 6.7 percentage points, which is statistically distinguishable from zero at the 10% level. These estimates are suggestive that, for previously married mothers, an increase in child support income increases the probability of paid employment.

B. Sample Stratified by Education

The results thus far have demonstrated a positive and statistically significant relationship between child support income and paid employment of previously married single mothers. I now turn to an analysis of whether these effects are larger or small for those with lower levels of education. Understanding how child support income variation influences labor supply across the education distribution is important because single mothers with less education tend to have less attachment to the labor market. So, to the extent this group is highly responsive, it points to one source of variation that can induce those with less education to obtain employment.

Table 2.3 shows IV estimates of equation (1) for the sample of previously married single mothers by different education levels, using the natural log of CSE collections per administrative expenditures as an instrument for child support income. The estimates in Table 2.3 show strong evidence that the effects in Table 2.2 are being identified off of single mothers with relatively less education. For single mothers with a high school diploma or less education, a $1,000 change in child support income leads to a 18.2 percentage point increase in the likelihood of paid employment. This estimate is statistically different from zero at the 5% level. The employment rate for previously married single mothers with an education level of high school diploma or less is 47.8%, implying a 38.1 percent increase in the employment probability from a $1,000 increase in child support income for this group. Thus, for single mothers with an education level of high school or less, an increase in child support income has a large, positive effect on paid employment. For the other education groups, the estimated effect of child support income is statistically insignificant at conventional levels, as shown in columns (2) and (3) in Table 2.3.

There are several explanations for why the employment status of previously married single mothers with at least some college is less sensitive to child support income. For instance, these women may find it easier than their less educated counterparts to find jobs that pay enough for them to cover the high cost of child care. Thus, for less educated single mothers, child support income levels may help cover the fixed costs of employment. Another possible explanation is the fact that higher educated single mothers are less likely than less educated single mothers to receive TANF. For less educated single mothers, an increase in child support income may render paid employment more attractive than welfare participation. It therefore is not surprising that higher education single mothers would exhibit less sensitivity of employment to child support income variation.

VII. Conclusion

This paper uses data from the SIPP on single mothers in Panels 1996, 2001, and 2004 to investigate the relationship between child support income and the labor supply of single mothers. OLS estimates, which fail to adjust for the endogeneity of child support income, show a positive and significant correlation between child support income and the probability of paid employment. Further, the positive association remains even after controlling for an extensive set of individual and state-level characteristics. Even when analyzing various subgroups, the OLS estimate remains positive and significant.

Do changes in child support income have a causal effect on the labor supply of single mothers? OLS estimates do not provide the answer. To address the causal claim, I estimate IV models that utilize the ratio of the state child support collections per administrative expenditures an instrument. The IV model relies on the assumption that the state level child support policy measure is significantly correlated to the level of child support income received by single mothers in the sample but is uncorrelated with the unobserved component of the employment equation.

The results suggest that child support income does impact the employment decision of single mothers. Specifically, a $1,000 increase in child support income increases the probability of paid employment by 6.7 percentage points for previously married mothers. I find that the effect of child support income is concentrated among previously married single mothers with a high school diploma or less education. For this group, a $1,000 increase in child support income leads to a 18.2 percentage points increase in the probability of paid employment.

Although the OLS estimates suggest a positive relationship between child support income and paid employment of never-married single mothers, the results were because the instrument was a weak predictor of child support income. There are several reasons why the child support policy variable may have been a weak predictor. First, since TANF allowed states to abolish the $50 pass-through, it is questionable how much incentive single mothers had to cooperate with the OCSE to establish paternity. Second, never-married mothers are likely to pursue child support when the relationship with the noncustodial parent dissolves and this information is unavailable in the SIPP. Thus, the alternative strategy of measuring the child support variable during the year of their first child's birth may be inappropriate. Last, child support enforcement may reduce the amount of informal child support provided by the noncustodial father by forcing him to make payments that are used to offset the public assistance afforded to the single mother or by forcing him to participate in the underground economy. As never-married mothers comprise an increasing share of single parent households and are most likely to be impoverished compared to other family types, more work needs to be done to examine the determinants of child support receipt for this group.

Table 2.1 OLS estimates of the Probability of Paid Employment as a Function of Child Support Income

	Dependent Variable: Dummy = 1 if Paid Employment		
	Marital Status		
	All	Never-married	Previously-married
Independent Variable	(1)	(2)	(3)
Real Child Support Income ($1,000)	0.0084***	0.0171***	0.0071**
	(0.0025)	(0.0059)	(0.0027)
Black	-0.0211	-0.0443*	0.0042
	(0.0141)	(0.0237)	(0.0183)
Hispanic	0.0016	-0.0282	0.0227
	(0.0322)	(0.0468)	(0.0249)
Age	0.0377***	0.0543***	0.0201***
	(0.0030)	(0.0041)	(0.0060)
Age Squared	-0.0005***	-0.0008***	-0.0003***
	(0.0000)	(0.0001)	(0.0001)
Number of kids eligible	-0.0411***	-0.0465***	-0.0375***
	(0.0058)	(0.0105)	(0.0072)
Age youngest child < 6	-0.0580***	-0.0287	-0.1083***
	(0.0118)	(0.0215)	(0.0204)
Separated	0.0353**		-0.0384***
	(0.0145)		(0.0138)
Divorced	0.0619***		
	(0.0118)		
Less than HS diploma	-0.1978***	-0.2006***	-0.1803***
	(0.0145)	(0.0231)	(0.0178)
Some College	0.0488***	0.0603***	0.0420***
	(0.0107)	(0.0177)	(0.0131)
Bachelor's degree or more	0.0866***	0.0752**	0.0916***
	(0.0144)	(0.0334)	(0.0145)
Unemployment Rate	-0.0067	-0.0044	-0.0076
	(0.0077)	(0.0160)	(0.0063)
Log TANF	-0.0848	0.1453	-0.2684***
	(0.1191)	(0.2152)	(0.0997)
Constant	0.7152	-0.7653	1.7749***
	(0.6264)	(1.1543)	(0.4841)
R^2	0.131	0.1349	0.1007
Number of Observations	8082	3519	4563

Source.—Author's estimation of eq. (1) using the 1996, 2001, and 2004 panels of the Survey of Income and Program Participation.

Note.--All monetary variables are measure in real 1995 dollars, adjusted using the CPI. All models include year and state fixed effects. Standard errors clustered at the state level are in parentheses.

Table 2.2. IV estimates of the Probability of Paid Employment as a Function of Child Support Income

	Dependent Variable: Dummy = 1 if Paid Employment	
	All	Previously-married
Independent Variable	(1)	(2)
Real Child Support Income ($1,000)	0.0542	0.0671*
	(0.0508)	(0.0389)
Number of kids eligible	-0.0542***	-0.0631***
	(0.0154)	(0.0161)
Age youngest child < 6	-0.0484***	-0.0912***
	(0.0145)	(0.0249)
Separated	0.0173	-0.0192
	(0.0179)	(0.0221)
Divorced	0.027	
	(0.0369)	
Less than HS diploma	-0.1849***	-0.1489***
	(0.0231)	(0.0283)
Some College	0.0312	0.0123
	(0.0226)	(0.0233)
Bachelor's degree or more	0.054	0.039
	(0.0439)	(0.0421)
Unemployment Rate	-0.0084	-0.0101
	(0.0069)	(0.0087)
Log TANF	-0.1209	-0.3343***
	(0.1211)	(0.1137)
Constant	0.9058	2.1966***
	(0.6315)	(0.5757)
R^2	0.0863	.
Number of Observations	8082	4563
First-Stage CSE Collections Per Expenditure Estimates:	0.4114	0.5457
	(0.1118)	(0.1539)
First-Stage F-Statistic:	13.5353	12.5744

Source.—Author's estimation of eq. (1) using the 1996, 2001, and 2004 panels of the Survey of Income and Program Participation.
Note.--All monetary variables are measure in real 1995 dollars, adjusted using the CPI. All models include year and state fixed effects. Controls for respondent's age, age squared, and race. Standard errors clustered at the state level are in parentheses.

Table 2.3. IV estimates of the Probability of Paid Employment as a Function of Child Support Income, Sample Stratified by Education

	Dependent Variable: Dummy = 1 if Paid Employment	
		Education Level
	HS Diploma or Less	Some College
Independent Variable	(1)	(2)
Real Child Support Income ($1k)	0.1824**	-0.0336
	(0.0905)	(0.0421)
Number of kids eligible	-0.1010***	-0.0188
	(0.0283)	(0.0193)
Age youngest child < 6	-0.0737*	-0.1165***
	(0.0431)	(0.0364)
Unemployment Rate	-0.0112	-0.0149
	(0.0207)	(0.0122)
Log TANF	-0.4375*	-0.2804
	(0.2492)	(0.2406)
Number of Observations	2397	1489
First-Stage CSE ratio:	0.4139	0.8665
	(0.1349)	(0.2765)
First-Stage F-Statistic:	9.4201	9.8227

Source.—Author's estimation of eq. (1) using the 1996, 2001, and 2004 panels of the Survey of Income and Program Participation.

Table A2.1. Means and Standard Deviations of Analysis Variables

	Full Sample			Never-married			Previously-married		
	All	Received CS Income		All	Received CS Income		All	Received CS Income	
Variable		Yes	No		Yes	No		Yes	No
CS Support Received	0.38	1.00	--	0.26	1.00	--	0.48	1.00	--
Real CS Income ($1K)	1.08	2.85	--	0.48	1.84	--	1.57	3.29	--
	(2.17)	(2.72)	--	(1.29)	(1.96)	--	(2.58)	(2.89)	--
Enrolled	0.17	0.17	0.17	0.21	0.21	0.21	0.14	0.15	0.13
Paid Job	0.76	0.83	0.71	0.68	0.77	0.65	0.82	0.86	0.78
Black	0.32	0.22	0.37	0.48	0.46	0.49	0.18	0.12	0.24
Hispanic	0.16	0.12	0.19	0.17	0.12	0.19	0.15	0.11	0.18
Age	34.69	35.74	34.04	29.76	30.28	29.58	38.63	38.14	39.08
	(9.13)	(8.24)	(9.58)	(8.22)	(7.47)	(8.46)	(7.81)	(7.37)	(8.17)
Number of Kids	1.72	1.79	1.68	1.66	1.73	1.64	1.77	1.81	1.73
	(0.95)	(0.92)	(0.97)	(0.96)	(0.96)	(0.96)	(0.94)	(0.90)	(0.98)
Age youngest kid < 5	0.16	0.14	0.16	0.20	0.20	0.20	0.12	0.12	0.13
Separated	0.15	0.15	0.16	0.00	0.00	0.00	0.28	0.21	0.34
Divorced	0.40	0.55	0.31	0.00	0.00	0.00	0.72	0.79	0.66
No HS Diploma	0.19	0.12	0.24	0.26	0.19	0.28	0.14	0.08	0.18
Some College	0.30	0.36	0.27	0.26	0.32	0.24	0.33	0.37	0.30
Bachelor's Degree	0.11	0.14	0.10	0.07	0.07	0.07	0.15	0.17	0.14
State UE Rate	5.25	5.16	5.31	5.27	5.17	5.31	5.24	5.15	5.32
	(1.00)	(1.00)	(1.00)	(1.00)	(0.98)	(1.00)	(1.00)	(1.01)	(1.00)
Ln(TANF payment)	5.78	5.77	5.78	5.78	5.75	5.79	5.77	5.77	5.77
	(0.42)	(0.41)	(0.43)	(0.42)	(0.41)	(0.43)	(0.42)	(0.40)	(0.43)
Ln(CSE ratio)	1.27	1.31	1.25	1.32	1.37	1.31	1.23	1.28	1.19
	0.40	0.39	0.40	0.37	0.35	0.38	0.41	0.40	0.41
Observations	8082	3064	5018	3519	931	2588	4563	2133	2430

Note.--The table shows the means and standard deviations (for only continuous variables) from the SIPP sample discussed in the text. All financial variables are in real 1995 dollars, adjusted using the CPI.

Table A2.2. First Stage OLS estimates of CS income as a Function of State OCSE collections per expenditures.

	Dependent Variable: Real CS Income ($1,000)		
		Marital Status	
	All	Never-married	Previously-married
Independent Variable	(1)	(2)	(3)
Log OCSE collections per expendure	0.4114***	0.0858	0.5457***
	(0.1118)	(0.0870)	(0.1539)
Black	-0.5962***	-0.1984**	-0.9631***
	(0.0668)	(0.0835)	(0.0855)
Hispanic	-0.4352***	-0.135	-0.5405***
	(0.0844)	(0.0967)	(0.1229)
Age	0.0344*	0.0401**	0.0901***
	(0.0171)	(0.0181)	(0.0284)
Age Squared	-0.0004	-0.0005*	-0.0011***
	(0.0003)	(0.0003)	(0.0004)
Number of kids eligible	0.2861***	0.0498**	0.4238***
	(0.0221)	(0.0211)	(0.0366)
Age youngest child < 6	-0.2166***	0.0021	-0.2880***
	(0.0551)	(0.0466)	(0.0990)
Separated	0.3844***		-0.3602***
	(0.0901)		(0.0883)
Divorced	0.7814***		
	(0.0597)		
Less than HS diploma	-0.2738***	-0.1130**	-0.5142***
	(0.0432)	(0.0439)	(0.0940)
Some College	0.3730***	0.1828***	0.4889***
	(0.0673)	(0.0615)	(0.1082)
Bachelor's degree or more	0.6966***	0.2026	0.8641***
	(0.0902)	(0.1356)	(0.1321)
Unemployment Rate	0.0398	0.0534	0.046
	(0.0418)	(0.0381)	(0.0729)
Log TANF	0.6388*	0.3333	0.9185
	(0.3777)	(0.3583)	(0.6300)
Constant	-3.8834*	-2.2807	-6.8972**
	(2.1237)	(1.8871)	(3.2024)
R^2	0.1269	0.0496	0.1109
Number of Observations	8082	3519	4563
F-Statistic	13.5353	0.9734	12.5744

Source.—Author's estimation of eq. (1) using the 1996, 2001, and 2004 panels of the Survey of Income and Program Participation.

Note.--All monetary variables are measure in real 1995 dollars, adjusted using the CPI. All models include year and state fixed effects. Standard errors clustered at the state level are in parentheses.

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CHAPTER 3

THE EFFECTS OF TEEN PARENTHOOD ON YOUNG ADULT OUTCOMES: EVIDENCE FROM TWO COHORTS OF YOUTH

I. Introduction

A 2006 report from the National Campaign to Prevent Teen Pregnancy (Hoffman, 2006) concluded that the public cost of teen births—including costs of children's health care, foster care, and incarceration, and tax revenue lost due to lower earnings by parents—reached $9.1 billion in 2004. The substantial costs imposed on taxpayers have led many policymakers to conclude that stronger disincentives for teen childbearing are needed. However, many of the policies crafted to achieve this goal—such as time limits or caps on welfare benefits, welfare requirements for teen mothers to stay in school, and school-based programs that teach adolescents sex resistance skills—are designed to affect the sexual and fertility behaviors of females. Moreover, despite the fact that teenage childbearing is the result of joint sexual, contraceptive, and pregnancy resolution decisions of males and females, much of the scholarly literature examining the causes and consequences of early childbearing, as well as the policies that affect teen pregnancy, has focused on women (Greene and Biddlecom, 2000). Much less attention has been paid to the role of men, and, in particular, the socioeconomic effects of teen fatherhood (Hoffman, 2006; Bachrach, 2007). Further, although the US labor market has experienced dramatic shifts in the structure of wages since the 1980s and significant reforms to various social programs, few studies have examined differences over time in the effect of teen parenthood.

There are a number of reasons why we might expect the processes that relate early parenthood with labor market outcomes to differ for men and women. First, the consequences of early fatherhood may be less severe for men than for women. Men are much less likely to be the primary caretaker of children, so their opportunity costs of parenthood are smaller. Moreover, many young fathers are non-resident, so many may be able to avoid the responsibilities (and subsequent consequences) of parenthood altogether. On the other hand, some research suggests that marriage and fatherhood may act as a 'civilizing force' for some young men and may lead them to adopt more mature adult roles such as increased work effort, but less educational investment (Popenoe, 1996, Nock, 1998a,b). Finally, the selection of teen fathers and teen mothers may be quite different; men are on average about two to three years older than women at the time of their first birth, and the proportion of all births to teen fathers is much lower than for teen mothers.

There is also reason to expect that the effects of teen parenthood on labor market outcomes may have changed over time due to a number of social and economic changes. The opportunity cost of early childbearing has increased over time as rates of return to education have increased. Indeed, a vast literature has documented the shift in the US labor market that began in the 1980s that resulted in a dramatic rise in education related wage gaps for younger workers (Bound and Johnson, 1992; Juhn, Murphy and Pierce, 1993; Katz and Murphy, 1992; Autor, Katz, and Krueger, 1999). In contrast, institutional changes such as increases in the availability of formal child care over time have made it easier for women to combine work and family. Additionally, the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996 encouraged work by ending welfare as an entitlement program, requiring recipients to work after two years, and placing a lifetime limit of five years on benefits paid with federal funds.

For fathers, increases in non-marital childbearing over time increase the likelihood that men can avoid the responsibilities of fatherhood, but stricter child support enforcement over time will mitigate that possibility. However, it is plausible that if increased child support requirements reduce the return to higher earnings, work incentives may be smaller.[26] No study of which we are aware has explored how gender differences in the labor market effects of teenage parenthood have changed over time.

Using data drawn from two cohorts of youths from the National Longitudinal Surveys of Youth 1979 and 1997 (NLSY79 and NLSY97) we estimate the effect of teen parenthood on labor market outcomes. Across a number of econometric strategies designed to control for measured and unmeasured heterogeneity—including family fixed effects and simultaneous equation modeling—we find that teen motherhood is associated with a lower probability of employment and an increased likelihood of welfare participation. Moreover, the teen motherhood employment penalty is larger for women in the NLSY79 than for women in the NLSY97. At ages 23 and 25, the teen motherhood poverty penalty is larger for women in the NLSY97 than for women in the NLSY79. For men, our results suggest that the observed correlation between teen fatherhood and the socioeconomic outcomes studied are diminished when controlling for unobserved family-level characteristics. We also find that our conclusions remain the same after controlling for education.

[26] Freeman and Waldfogel (1998) find negligible effects of child support enforcement on male labor supply, which is consistent with the general finding in the literature that male labor supply is inelastic with respect to taxes. In contrast, Holzer, Offner, and Sorensen (2005) find that child support enforcement contributed to a significant portion of the decline in employment among black men aged 25-34 that occurred in the 1980s and 1990s.

II. Background

There is a sizable literature on the consequences of teen parenthood for women. Most studies in the economics and sociology literature find evidence of a negative relationship between teenage childbearing and educational attainment for women, but estimates of the size of this relationship vary widely across studies. There are several explanations for this negative correlation. Early childbearing may have a negative causal effect on schooling for women because childrearing is time intensive and raises the opportunity costs of investing in human capital. Alternatively, selection may explain part, or all or the association, if observed and unobserved factors are associated with both early childbearing and schooling.

Early studies addressed the role of selection by controlling for observable characteristics such as parental education, income, test scores, and mother's age at first birth (Card & Wise, 1978; Hoffereth & Moore, 1979; McElroy, 1996; Upchurch & McCarthy, 1990; Blackburn et al., 1993; Waite & Moore, 1978). These studies generally found that conditioning on observables reduced the estimated effect of teenage motherhood on schooling, but the negative effect that remained was significant and substantial (see Hoffereth, 1987 for a review).[27] However, as Hoffman (1998) notes, these findings are upper-bound estimates of the economic consequences of teen motherhood given that other hard-to-measure factors—for example, personal discount rates, family attitudes, or peer group characteristics—may explain some, or all, of the estimated association.

A second set of studies restrict their samples to sisters and estimate family fixed effect that controls for family characteristics that are common to siblings and that may be associated with both teen motherhood and schooling outcomes (Geronimus & Korenman, 1992, 1993;

[27] Similar results are found using propensity score matching methods that ensure that the teen and non-teen birth groups are comparable with respect to the distribution of background characteristics (Levine & Painter, 2003).

Hoffman et al 1993; Bennett et al.,1995; Levine & Painter, 2003; and Ribar,1999). The results of these studies suggest that OLS estimates overstate the adverse effects of early motherhood, but the magnitude of the estimated bias differs across studies and might be explained, in part, by differences in the composition of sibling pairs across datasets.

While controlling for family-level heterogeneity is informative, concerns about individual heterogeneity (or reverse causality) remain. To overcome the shortcomings of family fixed effects models, some studies have used instrumental variables techniques (Ribar, 1994; Rindfuss et al., 1980; Klepinger et al., 1995, 1999; Marini, 1984; and Olsen & Farkas, 1989). This method requires identifying instruments that provide exogenous variation in fertility that is unrelated to unmeasured determinants of schooling or labor market outcomes. Instruments used in the literature include age of menarche, local abortion rate, and the availability of family planning and abortion services. Results using instrumental variables also differ across studies, though they generally point to modest adverse effects of teenage childbearing. One recent study by Hotz et al. (2005) uses a natural experiment that compares the outcomes of teenage mothers with the outcomes of females who became pregnant as teens, but miscarried. They find that any effect is small and only exists in the short-run. More recently, Fletcher and Wolfe (2009) and Ashcraft and Lang (2006) also use miscarriages as natural experiments and find some evidence of modest negative schooling effects.

Methods that have been used to examine the consequences of teen motherhood on educational attainment have also been utilized to examine the effect of teen motherhood on labor market outcomes. For example, Klepinger et al. (1999) use state and county-level costs of abortion as instruments for teen fertility. They find that teen motherhood reduces teenage work experience, and for white women only, adult work experience. Moreover, they find that through reductions in human capital, teen fertility reduces market wages at age 25. Hotz, McElroy, and Sanders compare those who gave birth as a teen to those who miscarry and find that teen motherhood appears to increase work experience and labor market earnings, a finding that is not consistent with standard theory. They also find that teen childbearing reduces the chances of living in poverty and participating in various social welfare programs. More recently, Fletcher and Wolfe (2009) provide evidence that miscarriages are unlikely to be random events. Using the timing of miscarriages as well birth control choices prior to pregnancy, they construct more relevant control groups and find that teen motherhood reduces annual income as a young adult by $1000 to $2400 and may increase the probability of receiving cash assistance.

Empirical evidence on the economic effects of early fatherhood is much less developed than in the literature for women, and the results for men are more variable than are the results for women. Most studies have treated teen fatherhood as exogenously determined and the results have been mixed. One study finds a positive relationship between early fatherhood and employment (Lerman, 1993), some studies document negative relationships, especially for schooling and earnings (Robbins & Streetman, 1994; Brien & Willis, 1997; Card & Wise, 1978), and still other studies find insignificant or mixed results (Marsiglio, 1987; Berrington et al, 2005; Sigle-Ruston, 2005).

Nock (1998) provided the first innovation in the fatherhood literature to address the role of family-level heterogeneity by comparing brother pairs from the NLSY79. He finds some weak evidence of a negative relationship between early fatherhood and school attainment, though much of the effect appears to be driven by non-marital fatherhood. Fletcher and Wolfe (2010) use data from the Add Health to compare labor market outcomes of teenage males whose female partners had miscarried to teenage fathers. Despite small sample sizes, they find that teen fatherhood is increases full-time employment probabilities as well as military employment, but find no statistically significant effects on overall employment status, idleness, total family income, or labor income.

Our work contributes to the literature on teen fatherhood in a number of ways. First, rather than rely on a single empirical approach, our work will use multiple methods across multiple data sources to examine robustness of the relationship between teenage parenthood and labor market outcomes. We use a number of identification strategies designed to control for family- and individual-level heterogeneity, including OLS, propensity score matching, family fixed effects, and simultaneous equation modeling. Second, we will do parallel analyses for men and women within datasets, using the same methodological strategies to better isolate heterogeneity in effects of early childbearing by gender. Finally, we explore how the labor market consequences of teenage parenthood have changed over time by exploiting data drawn from two cohorts of youth. Few studies in the existing literature have examined changes in the effects of teen motherhood and fatherhood over time.

III. Data and Measures

Data. This paper relies on data from two sources: the 1979 National Longitudinal Survey of Youth (NLSY79) and the 1997 National Longitudinal Survey of Youth (NLSY97). Each provides panel data on fertility and labor market outcomes for men and women from large, nationally representative samples, and collects detailed information about respondents' family and other background characteristics. Comparing estimates across the data sets will enable us to examine changes over time in the effect of teen parenthood on labor market outcomes, because respondents were ages 15 to 19 in different years, spanning 1972-83 and 1995-2003.

The NLSY79 is a large U.S. sample of 12,686 men and women born between 1957 and 1964. The data include an oversample of blacks, Hispanics, low-income whites, and youth enrolled in the military. Respondents were first interviewed in 1979 when they were 14-22 and were re-interviewed annually until 1994 and biannually after that. In 2002, respondents were ages 38-45 and had largely completed their childbearing years. We find that 1,597 women and 709 men report having had a birth (or fathering a child) before the age of 20. One advantage of the NLSY79 is the relatively low-attrition rate. The total sample retention rate through 1994, the year that all respondents were age 29 and older, is 89 percent.

The structure of the NLSY79 provides another analytic advantage: the household sampling frame resulted in 2,448 households with one or more siblings included in the data set, which will allow for the estimation of family fixed effects models. One disadvantage, however, is that many of the respondents were already in their late teens or early 20s at the first survey. For this group, fertility information is retrospective rather than prospective, and there are fewer individual and family background characteristics in the data that were measured prior to age 15, the age that we use to define the beginning of the risk period for teen fertility.

The NLSY97 samples 8,984 men and women born between 1980 and 1984 and, like the NLSY79, includes an oversample of blacks and Hispanics. Respondents were first interviewed in 1997 when they were 12-17 and have been interviewed annually since that time. By 2005 (round 9), the year that all respondents were age 20 and older, 464 males and 916 females had reported having a teen birth. Similar to the NLSY79, these data also employ a household sampling frame that includes in the sample all age appropriate members of the household. This results in 1862 households with one or more siblings included in the data set. The NLSY97 also have reasonably high retention rates: 82% of the original sample was interviewed in round 9.

Although the respondents are still relatively young—in round 12, the most recent round available, respondents are only ages 24 to 28—this age is sufficient to measure our labor market outcomes of interest, employment and poverty status, SNAP participation, and earnings.

The longitudinal nature of the NLSY datasets allows us to examine the effect of teen parenthood on labor market outcomes at different ages. For each measure, we analyze the effect of teen parenthood at ages 20, 23, and 25. The first, $Employed_r$, is an indicator set equal to 1 if the respondent was employed during week 42 of the year in which the respondent turned age r, for $r = 20, 23, 25$; the second, $poverty_r$, is an indicator set equal to 1 if the respondent's household income during the year prior to the respondent turning age r was below poverty line; the third, Supplemental Nutrition Assistance Program ($SNAP_r$), formerly known as the food stamp program, is an indicator set equal to 1 if the respondent received SNAP benefits during previous calendar year that respondent turned age r .; the last, $earn_r$, is the amount of income the respondent earned from during prior to the year when the respondent turned age r. Following Klepinger, Lunderberg, and Plotnick (1999), outcomes were measured at age $r + 1$ if a missing value was encountered for measure at age r. For example, if data on earnings are missing for respondent at age 25, we use earnings for calendar year prior to year when individual turned age 26. Appendix Table 3.1 shows, at each age, the share of males employed is consistently higher in both cohorts than the share of female respondents who are employed. In addition, at each age, males are less likely than females in both cohorts to participate in the SNAP program and to be in poverty. It is also the case that, at each age, males also tend to earn more than females. However, the male/female earnings gap appears to be smaller in the later cohort than in the earlier cohort. Interestingly, for males and females and at each age, SNAP participation and poverty incidence has increased from the earlier to the later cohort.

Our primary independent variable of interest, *Teen birth,* is a dummy variable equal to 1 if the respondent mothered or fathered a child as a teenager; and zero otherwise.

A potential threat to the validity of the results involves the reporting of births. For example, previous research has found both under-reporting of fathering and under-representation of fathers in a number of national data sets (Rendall et al. 1999; Cherlin and Griffith 1998). These studies also suggest that underreporting is more likely for those who fathered children outside of marriage. Our results will be biased if underreporting is correlated with the outcome of interest. Joyner et al. (2012) has conducted a number of analyses assessing the quality of male fertility reports comparing reports from survey data with rates estimated from vital statistics. Results show that while there is some underreporting, the degree of underreporting of fertility data in the NLSY79 and NLSY97 for men is not large. There are several reasons why data quality issues may be less of a problem in the NLSY. First, these are panel data, allowing for the collection of fertility data shortly after births occur. Rendall et al. (1999) suggest that panel data are likely to capture a higher proportion of male fertility than retrospective data. In addition, considerable efforts have made in cleaning up the male fertility date in the NLSY79 (Mott and Gyrn 2001).[28]

In Appendix Table 3.2, we show the mean proportion of the dependent variables by nonmarital birth status. We find consistent evidence across gender and cohorts that having a teenage birth is significantly and negatively related to socioeconomic status. For example, women in the NLSY79 and the NLSY97 who have experienced a nonmarital teen birth are 34.8 and 21.2 percentage points more likely than women in their respective cohorts who have not experienced a nonmarital teen birth to be in poverty at age 25. Additionally, women in both cohorts who experienced a nonmarital teen birth are more likely than women in their respective

[28] We use variables available in the public-use NLYS79 data to create best estimates of women's age at first birth and data compiled by Mott and Gyrn (2001) for male fertility that includes his best estimate of the date of each birth.

cohorts who did not experience a nonmarital teen birth to be SNAP participants. In both cohorts, the socioeconomic costs of nonmarital childbearing appear to be larger for women than for men. For men in both cohorts, having a nonmarital birth is associated with a reduced likelihood of employment and greater likelihood of SNAP participation. However, a number of patterns also become apparent. The employment rate for teen fathers at ages 23 and 25 has decreased in the later cohort and SNAP participation has increased for all men in the later cohort at all ages under consideration. Further, the percent of teen fathers in poverty at ages 20 or 23 is smaller in the NLSY97 than in the NLSY79.

IV. OLS Estimation Results

While the cross-tabulations in Table 3.1 provide some descriptive evidence that the socioeconomic costs of teen parenthood may have increased changed time, these correlations may be due to a number of individual or family background characteristics. Thus, we begin by estimating a parsimonious ordinary least squares regression of the following form:

$$(1) \mathrm{E}ir = \beta_0{}^{r} + \boldsymbol{\beta_1}^{r\prime}\mathbf{X}\boldsymbol{i} + \beta_2^{r}\, Teen\ Birth_i + \varepsilon_{ir}, \mathrm{r} = 20, 23, 25$$

where E_{ir} is a measure of respondent *i*'s socioeconomic measure at age r; $\boldsymbol{X_i}$ is a vector of controls; and $Teen\ Birth_i$ is the indicator variable defined above. Controls in $\boldsymbol{X_i}$ are measured at age 14 and include dummies for black, Hispanic, mother's education (less than high school and greater then high school with high school grad being the omitted group), type of family (step family, single family, and no parent family, with two biological parents being the omitted group), urban, oldest child, and whether the mom was working full-time.

Our focus is on the estimate of β_2. If β_2 is less than zero this could be interpreted as evidence that, as hypothesized, teenage parenthood is associated with a lower measure of socioeconomic status. We estimate equation (1) for each of our datasets and correct the standard errors for heteroskedasticity.

Tables 3.1, 3.2, and 3.3 present OLS estimates of the effect of teen parenthood on adult outcomes at age 20, 23, and 25, respectively by sex and cohort.[29] For women, OLS estimates in Table 3.3.1 suggest that the nonmarital birth penalty is larger at 20 for women in the earlier cohort than women in late cohort, but tend to converge at later ages. For instance, a nonmarital birth reduces the likelihood of employment at age 20 by 19.6 percentage points for women in NLSY79, but reduces the probability of employment by only 7.7 percentage points for women in the NLSY97. Similarly, OLS estimates suggest that a nonmarital birth reduces the earnings of women age 20 in the NLSY79 cohort by 39 percent[30], but reduces the earnings for women from the NLSY97 cohort by only 5 percent. However, Table 3.3 shows that teen childbearing reduces employment by approximately 12-13 percentage points at age 25 for women in both cohorts. The convergence of the OLS teen birth estimates is witnessed for all outcomes.

The OLS estimates suggest the impacts of a teen birth on adult outcomes are smaller for men than for women. For males, a teen birth is associated with a decrease in the probability of employment of between 1 and 7 percentage points. However, the effect of teen childbearing on employment is significant for men only at age 25. Interestingly, the positive association between poverty and teen childbearing is statistically insignificant for men in the NLSY79 at ages 23 and 25, but remains statistically significant and positive for men in the NLSY97. The results also suggest that teen childbearing has a larger effect on men's likelihood of food stamp receipt in later cohorts. For example, at age 20, a teen birth increases the

[29] Weighted regression results produce qualitatively similar results. Moreover, the marginal effects produced by a logistic regression as opposed to a linear probability model are similar to those shown in Tables 3.2 and 3.3.
[30] Since the dependent variable is log earnings, the coefficient reflects a percent change in earnings.

likelihood of SNAP participation by 11 percentage points for men in NLSY97, but only by 1 percentage points for men in the NLSY79.

V. Family Fixed Effects

While suggestive, one particular concern with OLS estimates is that the teen birth indicator is endogenous. In other words, teen childbearing may be related to an unobserved determinant of socioeconomic outcomes such as motivation to work in formal sector. Thus, we cannot rule out the possibility that unobservable family-level characteristics can explain, in part or in whole, the relationship between teen parenthood and adult socioeconomic outcomes. To address the issue of family-level unobservables, we restrict our sample to children who have the same mother, *j*, and estimate a family fixed effects model of the following form:

$$(2)\mathrm{E}ijr = \beta_0{}^{r} + \boldsymbol{\beta}^{\mathbf{r}}{}_{\mathbf{1}}\ '\mathbf{X}\boldsymbol{i} + \beta_2^{r}\, Teen\ Birth_{ij} + \boldsymbol{\kappa}_j + \varepsilon_{ijr}, \mathrm{r} = 20, 23, 25$$

where $\boldsymbol{\kappa}_j$ is a vector of family fixed effects and the vector $\boldsymbol{X_i}$ includes a set of individual-level characteristics that differ across siblings.

Family fixed effects estimates appear in rows 3 and 7 in Tables 3.1, 3.2, and 3.3, with rows 2 and 6 showing OLS estimates on the comparable sample.[31] For the sample of sisters (Panel I), our results suggest that the inclusion of family fixed effects does not eliminate the negative relationship between teen motherhood and the probability of employment for women in the NLSY79. Controlling for family fixed effects, teen motherhood is associated with a 15.8, 13.7, and 13.5 percentage point decline in the probability of employment at ages 20, 23, and 25, respectively for women in NLSY79, where the result at age 25 is statistically insignificant. For

[31] Family fixed effects models can only be estimated for a sample with more than one adolescent in a family. Therefore, to assess whether any differences in results are due to the empirical technique or to the different sample, we also report OLS results from the same sample as the family fixed effects models.

women in the NLSY97, the fixed-effects estimate of teen childbearing on the probability of employment is diminished and no longer statistically significant. Nevertheless, for women in both cohorts and all ages, the fixed-effects estimate of teen childbearing on probability of SNAP participation remains positive and statistically significant at conventional levels. For men, fixed-effects estimates of teen childbearing on all the outcomes studied are insignificant. This suggests that, for men, much of the observed correlation between teen childbearing and adult socioeconomic outcomes are due to unobserved heterogeneity.

VI. Simultaneous Equation Modeling

While fixed-effects estimates control for family-level unobservables, it does so at the expense of excluding a large fraction of the sample. Simultaneous equation modeling addresses the unobserved family-level heterogeneity without excluding respondents without sibling present in sample. The simultaneous equation framework consists of two components: a hazards model for teen childbearing and a probit model or OLS model for the adult outcome of interest. Each component includes a family specific random effect which allows for the influence of unmeasured time-invariant characteristics of the family on each outcome. These residuals may be correlated across processes, allowing for the possibility that the risk of teen childbearing and labor outcomes may be influenced by a common set of unobserved characteristics. The direction and magnitude of this residual correlation provides information on the nature and extent of selection on time-invariant family characteristics. For example, when analyzing SNAP participation, a positive correlation would suggest that children from families with an above average risk of having a teen birth tend to be more likely to participate in the SNAP program.

In addition to controlling for time-invariant unobserved characteristics at the family-level, the model controls for characteristics of the family or respondent measured prior to age 15. The model also assumes that the family-specific effect is the same for all respondents in the same family. Thus, to the extent that children have different attitudes and/or parents treat children differently, failure to allow for unmeasured family-specific characteristics means that some bias will likely remain.

Model for Teen Birth

Let $h_i(t)$ denote the teen birth hazard for individual i in year t. The model allowing for unobserved heterogeneity between respondents may be written

$$\text{(3)} \qquad log h_i(t) = f(t) + \alpha w_i + v_i$$

The teen birth log-hazard is assumed to depend on age minus 10 at year t through a function $f(t)$, the baseline log-hazard rate. We assume that $f(t)$ is a piecewise-linear spline with nodes dispersed at ages 16, 17 and 18. Covariates w_i are time-invariant individual and family characteristics. For each respondent, v_i represents the value of a collection of unobserved traits drawn from a normal distribution with variance σ_v^2 at age 10, which affect the respondent's risk of teen birth.

Binary outcomes are modeled using a probit model.[32] The probit model is defined in terms of a continuous latent variable or propensity y^{r*} underlying the observed binary response y_{ij}^r, where $y_{ij}^r = 1$ if $y^{r*} > 0$ and $y_{ij}^r = 0$ otherwise for $r = 20, 23, 25$. A multilevel model that allows for unobserved heterogeneity at the family level can be written:

$$\text{(4)} \qquad y_{ij}^{(r)*} = \gamma^r z_{ij} + \mathrm{B}^r x_{ij} + \lambda_j^{(r)} \mathrm{u_j} + e_{ij}^r, \; r = 20, 23, 25$$

[32] Log earnings are estimated using an OLS model.

where z_{ij}^{r} is the endogenous teen birth indicator with coefficient γ^{r}, and x_{ij} is a vector of background characteristics of the family and the respondent with coefficients B^{r}. Instead of focusing on adult outcomes at one point in time, this framework allows us to examine the effect of teen parenthood on adult outcomes at different ages.

In a similar manner to Eq. (3), we include a family specific random effect u_j, which here represents the time-invariant characteristics of the family that affect the education decision at each for each respondent in family j. We assume that the u_j follow a normal distribution with mean zero and variance σ_u^2. These random effects have age-specific coefficients, λ^{r}. Thus, although the same unmeasured family characteristics are assumed to influence progression at all at each age, their effects may differ across ages. Additionally, the model also includes residuals $e_{ij}^{(r)}$ that are specific to a particular respondent and age and are assumed to follow independent standard normal distributions.

Together, Eqs. (3) and (4) define a multilevel multiprocess model. The equations are connected in two ways. First, the teen birth indicator z_{ij} in (4) is a prior outcome of the teen fertility process in (3). Second, we allow for the possibility of a nonzero correlation between the unmeasured family-specific components u_j and v_j. In particular, u_j and v_j are assumed to follow a bivariate normal distribution with correlation ρ_{uv}. A value of ρ_{uv} that is significantly different from zero would suggest that the teen birth indicator is endogenous with respect to the adult outcome of interest.

The presence of u_j in all three labor equations in (4) and the correlation between u_j and v_j, means that Eqs. (3) and (4) must be estimated simultaneously. The software we used for this analysis is aML (See Lillard and Panis, 2000).

Rows 4 and 8 of Tables 3.1, 3.2, and 3.3 contain the teen birth estimates from the simultaneous equation models. Once again, the results suggest that the impact of teen childbearing on the probability of employment and earnings are largest for women in the earlier cohort. For example, teen childbearing reduces the probability of employment by 20.4 and 1.5 percentage points at age 20 for women in the NLSY79 and NLSY97 cohorts respectively. By age 25, the analogous results are 12.8 and .3 percentage points. Interestingly, the effect of teen childbearing on probability of being impoverished is largest at ages 23 and 25 for women in later cohorts than women in earlier cohorts. The results from the simultaneous equation model also suggest that teen childbearing increases the likelihood of participating in the SNAP program at all ages for women in both cohorts.

For men, the results in row 8 in Tables 3.1, 3.2, and 3.3 suggest that teen childbearing has a modest effect, at best, on the socioeconomic outcomes studied. For men in both cohorts, teen childbearing is associated with higher earnings at ages 20 and 23 (although only significantly different from zero in one case) and a statistically insignificant reduction in earnings at age 25. There is also some evidence that, at age 20, teen childbearing increases the probability of SNAP participation for men in the NLSY97.

Estimates of the parameters associated with the family-specific random effects in the simultaneous equation model are listed in Table 3.4. These results show that there is residual correlation at the family level between a respondent's socioeconomic status and a risk of teen childbearing, which is reflected in the estimate of a significant and positive correlation between the random effects for SNAP participation and teen childbearing equations, ρ_{uv}: the individuals with an above-average risk of dissolution ($u_j > 0$) tend to have above-average chances of participating in the SNAP program ($v_j < 0$).

VII. Conclusion

Using data from two cohorts of youths drawn from the NLSY79 and the NLSY97, we estimate the relationship between teen parenthood and various socioeconomic outcomes. Across a wide set of identification strategies designed to control for unmeasured family- and individual-level heterogeneity—including family fixed effects and simultaneous equation modeling—we find that teen motherhood is negatively related to employment and positively related to poverty and SNAP participation. Moreover, we find that negative effect of teen childbearing on employment is larger for women in the NLSY79 than women in the NLSY97, but that the effect of teen childbearing on poverty and SNAP participation is similar for women in both cohorts. This suggests that changes in social programs since the 1990s may have been successful at encouraging single mothers to work, but that wages and public assistance benefits may have lagged behind inflation.

In regards to men, our results show that the association between teen fertility and the socioeconomic outcomes we investigate is smaller for men than for women, very often close to zero and statistically insignificant. Estimates from methods that account for unobserved heterogeneity are almost all insignificant, suggesting that the correlation between teen childbearing and socioeconomic outcomes studied appear to be due to unobserved factors that affect both the teen childbearing decision and decisions related to one’s socioeconomic status.

Thus, while the teen childbearing penalty to teen fathers is small, at most, and has remained fairly stable across cohorts, the adverse effects of teen motherhood on poverty appear to be larger at later ages in more recent cohorts. These results are consistent with a number of possible explanations, including the general decline across cohorts in real value of public assistance benefits and the minimum wage.

Table 3.1 Effects of Teen Parenthood on Young Adult Outcomes- Age 20

			Employed		Log Earnings		Poverty	
			NLSY79	**NLSY97**	**NLSY79**	**NLSY97**	**NLSY79**	**NLSY97**
Women	**OLS**	(1)	-0.196***	-0.0773***	-0.387***	-0.0466	0.241***	0.144***
		SE	(0.0233)	(0.0204)	(0.0929)	(0.0542)	(0.0246)	(0.0211)
		N	3,714	3,949	2,883	3,105	3,389	3,402
	OLS w/ FE Sample	(2)	-0.165***	-0.0415	-0.199	0.0201	0.239***	0.191***
		SE	(0.0335)	(0.0312)	(0.1240)	(0.0785)	(0.0351)	(0.0328)
		N	1,906	1,639	1,509	1,292	1,754	1,436
	Family Fixed Effects	(3)	-0.158**	0.0585	-0.466	0.0445	0.209**	0.0816
		SE	(0.0793)	(0.0839)	(0.3590)	(0.2520)	(0.0860)	(0.0773)
		N	1,906	1,639	1,509	1,292	1,754	1,436
	aML[1]	(4)	-0.204***	-0.015	-0.438***	-0.025	0.182***	0.125***
		N	1,906	1,639	1,509	1,292	1,754	1,436
Men	**OLS**	(5)	-0.00707	-0.0262	0.000543	0.121*	0.0555*	0.0510**
		SE	(0.0310)	(0.0253)	(0.0916)	(0.0664)	(0.0318)	(0.0255)
		N	3,752	4,094	3,146	3,289	3,470	3,362
	OLS w/ FE Sample	(6)	-0.00345	-0.058	0.0127	0.0544	0.0507	0.0504
		SE	(0.0416)	(0.0373)	(0.1150)	(0.0981)	(0.0430)	(0.0374)
		N	2,077	1,748	1,719	1,389	1,910	1,430
	Family Fixed Effects	(7)	0.00963	-0.00696	0.391	0.308	-0.0849	0.0548
		SE	(0.0876)	(0.1060)	(0.3350)	(0.2880)	(0.1110)	(0.1070)
		N	2,077	1,748	1,719	1,389	1,910	1,430
	aML[1]	(8)	0.032	0.008	0.121	0.191***	0.01	0.046
		N	2,077	1,748	1,719	1,389	1,910	1,430

Notes: Each cell represents a separate regression. Controls: mother's education, birth order, test scores, family structure, race, urban status, dummies for missing variables

$* p < .1, ** p < .05, *** p < .01.$

[1]Marginal effects from the aML models are calculated using the parameter estimates from the outcome equation. Specifically, we draw 100 random effect values from a normal distribution with mean zero and standard deviation of the random effect estimated from the model to calculate predicted outcomes. We then average the result. Significance levels are based on the original parameter estimates.

Table 3.2 Effects of Teen Parenthood on Young Adult Outcomes- Age 23

			Employed		Log Earnings		Poverty	
			NLSY79	**NLSY97**	**NLSY79**	**NLSY97**	**NLSY79**	**NLSY97**
Women	**OLS**	(1)	-0.132***	-0.115***	-0.275***	-0.195***	0.191***	0.147***
		SE	(0.0239)	(0.0200)	(0.0810)	(0.0489)	(0.0255)	(0.0202)
		N	3,612	3,835	2,798	3,132	3,088	3,325
	OLS w/ FE Sample	(2)	-0.111***	-0.0962***	-0.268**	-0.149**	0.203***	0.136***
		SE	(0.0342)	(0.0313)	(0.1150)	(0.0711)	(0.0369)	(0.0305)
		N	1,852	1,614	1,460	1,304	1,582	1,409
	Family Fixed Effects	(3)	-0.137*	0.0236	-0.385	0.188	0.155	0.0319
		SE	(0.0794)	(0.0872)	(0.3620)	(0.2540)	(0.0969)	(0.0944)
		N	1,852	1,614	1,460	1,304	1,582	1,409
	aML[1]	(4)	-0.091***	-0.015***	-0.339***	-0.131***	0.053***	0.104***
		N	1,852	1,614	1,460	1,304	1,582	1,409
Men	**OLS**	(5)	-0.042	-0.0365	-0.0901	0.00856	0.0328	0.0783***
		SE	(0.0288)	(0.0234)	(0.0899)	(0.0561)	(0.0308)	(0.0262)
		N	3,639	3,961	3,129	3,366	3,072	3,325
	OLS w/ FE Sample	(6)	-0.0513	-0.0785**	-0.00813	-0.0131	0.056	0.0476
		SE	(0.0380)	(0.0355)	(0.1140)	(0.0832)	(0.0427)	(0.0371)
		N	2,016	1,696	1,715	1,417	1,689	1,406
	Family Fixed Effects	(7)	0.0239	0.082	0.0391	0.191	0.0359	-0.0355
		SE	(0.0865)	(0.0991)	(0.2420)	(0.3160)	(0.1270)	(0.1180)
		N	2,016	1,696	1,715	1,417	1,689	1,406
	aML[1]	(8)	0.011	0.004	0.02	0.068	-0.011	0.057*
		N	2,016	1,696	1,715	1,417	1,689	1,406

Notes: Each cell represents a separate regression. Controls: mother's education, birth order, test scores, family structure, race, urban status, dummies for missing variables

$* p < .1, ** p < .05, *** p < .01.$

[1]Marginal effects from the aML models are calculated using the parameter estimates from the outcome equation. Specifically, we draw 100 random effect values from a normal distribution with mean zero and standard deviation of the random effect estimated from the model to calculate predicted outcomes. We then average the result. Significance levels are based on the original parameter estimates.

Table 3.3 Effects of Teen Parenthood on Young Adult Outcomes- Age 25

			Employed		Log Earnings		Poverty	
			NLSY79	NLSY97	NLSY79	NLSY97	NLSY79	NLSY97
Women	**OLS**	(1)	-0.128***	-0.116***	-0.275***	-0.213***	0.200***	0.149***
		SE	(0.0238)	(0.0196)	(0.0738)	(0.0520)	(0.0258)	(0.0225)
		N	3,584	3,872	2,806	3,109	2,999	2,617
	OLS w/ FE Sample	(2)	-0.128***	-0.154***	-0.144	-0.301***	0.186***	0.147***
		SE	(0.0335)	(0.0307)	(0.0915)	(0.0844)	(0.0368)	(0.0341)
		N	1,854	1,618	1,456	1,303	1,534	1,110
	Family Fixed Effects	(3)	-0.135	0.008	-0.123	-0.136	0.14	0.0635
		SE	(0.0833)	(0.0817)	(0.2610)	(0.2460)	(0.1000)	(0.1310)
		N	1,854	1,618	1,456	1,303	1,534	1,110
	aML[1]	(4)	-0.128***	-0.003	-0.339***	-0.156***	0.082***	0.102***
		N	1,854	1,618	1,456	1,303	1,534	1,110
Men	**OLS**	(5)	-0.0643**	-0.0702***	-0.107	-0.142**	0.0402	0.0668**
		SE	(0.0284)	(0.0238)	(0.0937)	(0.0602)	(0.0317)	(0.0276)
		N	3,575	3,999	3,160	3,372	2,831	2,632
	OLS w/ FE Sample	(6)	-0.0568	-0.0674*	-0.102	-0.274***	0.0585	0.0953**
		SE	(0.0382)	(0.0350)	(0.1160)	(0.0972)	(0.0433)	(0.0427)
		N	1,987	1,711	1,736	1,416	1,558	1,125
	Family Fixed Effects	(7)	-0.004	-0.0377	0.212	0.326	-0.0972	-0.158
		SE	(0.0966)	(0.0986)	(0.2830)	(0.3250)	(0.1420)	(0.1540)
		N	1,987	1,711	1,736	1,416	1,558	1,125
	aML[1]	(8)	-0.021	-0.03	-0.011	-0.071	0.005	0.038
		N	1,987	1,711	1,736	1,416	1,558	1,125

Notes: Each cell represents a separate regression. Controls: mother's education, birth order, test scores, family structure, race, urban status, dummies for missing variables

$* p < .1, ** p < .05, *** p < .01.$

[1]Marginal effects from the aML models are calculated using the parameter estimates from the outcome equation. Specifically, we draw 100 random effect values from a normal distribution with mean zero and standard deviation of the random effect estimated from the model to calculate predicted outcomes. We then average the result. Significance levels are based on the original parameter estimates.

Table 3.4 aML Estimates of the Relationship between All Teenage Parenthood and Educational Attainment

	Women		Men	
	79	97	79	97
Employed (ρ)	0.139	-0.503 ***	-0.230	-0.149
	(0.181)	(0.132)	(0.167)	(0.171)
Poverty (ρ)	0.301	0.070	0.192	0.035
	(0.193)	(0.164)	(0.185)	(0.217)
SNAP (ρ)	0.543 ***	0.514 ***	0.460	0.524 ***
	(0.160)	(0.119)	(0.421)	(0.174)
Log Earnings (ρ)[33]	0.042	-0.002	-0.026	-0.036
	(0.255)	(37837.227)	(0.000)	(894228.617)

ρ is the correlation between the random effect in the specific outcome equation and the random effect in the fertility equation. * significant at 10%; ** significant at 5%; *** significant at 1%

[33] The log-likelihood for the earnings model may not reflect the global maximum.

APPENDIX

Appendix Table 3.3.1: Means for Dependent Variables

Dependent Variables	**NLSY79**		**NLSY97**		
	Women	**Men**	**Women**		**Men**
	Full	**Full**	**Full**	**Sibling**	**Full**
Employed 20	0.610	0.728	0.687	0.683	0.685
Employed 23	0.697	0.845	0.753	0.743	0.806
Employed 25	0.721	0.885	0.760	0.761	0.821
Earnings 20	$7,194.77	$11,091.69	$7,448.21	$7,498.64	$10,662.23
(Standard Deviation)	$7,367.27	$11,379.92	$7,783.25	$7,407.49	$10,572.34
Earnings 23	$12,307.32	$18,117.20	$13,826.81	$13,801.44	$18,353.16
(Standard Deviation)	$11,400.64	$15,657.40	$12,852.00	$12,586.52	$15,437.63
Earnings 25	$15,471.05	$24,698.97	$16,766.49	$16,844.58	$23,112.27
(Standard Deviation)	$13,853.84	$18,313.92	$15,424.08	$15,296.83	$19,064.31
Poverty 20	0.196	0.153	0.226	0.236	0.173
Poverty 23	0.165	0.120	0.177	0.173	0.139
Poverty 25	0.157	0.091	0.163	0.159	0.119
SNAP 20	0.075	0.008	0.094	0.099	0.025
SNAP 23	0.081	0.014	0.149	0.151	0.053
SNAP 25	0.082	0.014	0.168	0.168	0.062

Appendix Table 3.2 Means of Dependent Variables

	NLSY79			
	Female		Male	
Teen Birth	No	Yes	No	Yes
Employed 20	0.640	0.356	0.731	0.661
Employed 23	0.722	0.478	0.848	0.774
Employed 25	0.747	0.502	0.890	0.760
Earnings 20	7,657.458	3,282.046	11,160.830	9,404.987
	(7,391.179)	(5,845.735)	(11,426.940)	(10,019.250)
Earnings 23	13,099.910	5,574.998	18,265.340	14,527.200
	(11,496.700)	(7,786.652)	(15,546.850)	(17,757.510)
Earnings 25	16,340.900	8,031.597	25,013.750	16,853.230
	(13,936.170)	(10,514.020)	(18,300.310)	(16,846.110)
Poverty 20	0.159	0.517	0.146	0.330
Poverty 23	0.131	0.466	0.114	0.264
Poverty 25	0.123	0.471	0.087	0.200
SNAP 20	0.033	0.430	0.007	0.033
SNAP 23	0.051	0.341	0.011	0.079
SNAP 25	0.054	0.317	0.012	0.045
	NLSY97			
	Female		Male	
Teen Birth	No	Yes	No	Yes
Employed 20	0.707	0.584	0.686	0.670
Employed 23	0.787	0.594	0.812	0.742
Employed 25	0.791	0.613	0.831	0.711
Earnings 20	7,630.510	6,560.994	10,502.580	12,466.680
	(7,503.928)	(8,967.225)	(10,329.030)	(12,872.870)
Earnings 23	14,812.060	9,208.953	18,579.420	15847.500
	(12,523.850)	(13,353.980)	(15,474.690)	(14791.570)
Earnings 25	18,080.520	10,684.460	23,599.720	17,767.780
	(15,320.530)	(14,410.260)	(19,146.710)	(17,255.640)
Poverty 20	0.200	0.359	0.166	0.251
Poverty 23	0.139	0.358	0.130	0.237
Poverty 25	0.124	0.336	0.109	0.222
SNAP 20	0.037	0.371	0.016	0.132
SNAP 23	0.086	0.446	0.042	0.177
SNAP 25	0.111	0.422	0.054	0.152

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