Essays on the Economics of the Family

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Abstract

This dissertation contains three essays analyzing how families form and how family members interact. The first chapter studies and connects recent trends in age at marriage and divorce. The second chapter looks within marriages to analyze household bargaining. The final chapter examines the effects on cohort characteristics of the changes in fertility induced by the legalization of abortion.

In my first essay, I explore the extent to which the rise in age at marriage can explain the rapid decrease in divorce rates for cohorts marrying from 1980 to 2004. Three different empirical approaches all demonstrate that an increase in women's age at marriage can explain at least 60 percent of the decline in the hazard of divorce since 1980. I further develop and simulate an integrated model of the marriage market to demonstrate that monotone decreases in gains to marriage could lead to both the initial rise in divorce and its subsequent fall.

My second essays analyzes the impact of the early 1990s state waivers from welfare guidelines to understand how changes in options outside of marriage affect household expenditures. Welfare waivers decreased the public assistance available to impoverished divorced women and thereby reduced a woman's bargaining threat point in marriage. Using expenditure data and an empirical synthetic control approach, I find that decreases in potential welfare benefits altered the expenditure patterns of two-parent families containing less-educated or stay-at-home mothers. The changes in expenditure patterns suggest that reductions in a wife's outside options cause her utility within marriage to decline.

My third essay examines how cohorts whose mothers had legal and safe access to abortion differ from those whose mothers did not. Using both birth certificate and wage data, I demonstrate that granting women access to abortion led to changes in child characteristics, even among groups of children born within months or weeks of each other. Analysis further suggests that soon after legalization, women used abortion to better-time their births. Moreover, access to abortion increased the eventual wages of low-wage, black, and Hispanic workers but not the wages of whites or high-wage workers.

Contents

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Introduction

After rising for three decades, American divorce rates fell rapidly from 1980 to 2004. The mean age at marriage also substantially increased after 1970. In the late 1960s and early 1970s, young women gained access to technology allowing them to better control their fertility. Welfare policy shifted in the 1990s to encourage single mothers to take a more active role in the labor market. For these and many other reasons, the American family has vastly changed in recent decades. This dissertation aims to explore some of the causes and consequences of this change, as well as providing insight into how spouses interact.

My first essay aims to explain the over 30 percent drop in divorce rates that began in the late 1970s. Using data from the Survey of Income and Program Participation and the 1979 National Longitudinal Survey of Youth, I explore the extent to which the rise in age at marriage can explain the rapid decrease in divorce rates for cohorts marrying from 1980 to 2004. Three different empirical approaches all demonstrate that the increase in women's age at marriage can explain at least 60 percent of the decline in the hazard of divorce since 1980. Other (plausibly exogenous) factors, such as improvements in women's labor market opportunities and increased access to birth control, largely impacted divorce rates over this period by changing age at marriage.

I further develop an integrated model of the marriage market to demonstrate that monotone decreases in the gains to marriage (due to the aforementioned exogenous changes) can produce both the increase in age at marriage and the rise and fall of divorce rates observed in the U.S. since 1950. Finally, I show that the recent changes in age at marriage and divorce are associated with more egalitarian marriages and decreased marital conflict. But the new patterns of family formation also imply a polarization in the lives of children born to more and less educated women.

This first chapter explains a major transition in family formation and dissolution. My second chapter digs deeper into family life, to explore how spouses interact with one another during marriage. Most models of the family assume that couples bargain with reference to some option outside of marriage, usually the utility a husband and wife could enjoy upon divorce. I test this common assumption.

I analyze the impact of the early 1990s state waivers from Aid to Families with Dependent Children (AFDC) guidelines to understand how changes in options outside of marriage affect household expenditures. AFDC waivers decreased the public assistance available to impoverished divorced women and thereby reduced a woman's bargaining threat point in marriage. Using the Consumer Expenditure Survey (CEX) and an empirical synthetic control approach, I find that decreases in potential welfare benefits altered the expenditure patterns of two-parent families. Waivers were associated with increased expenditure on food at home relative to restaurant meals and decreased expenditure on child care and women's clothing, suggesting greater home production and decreased consumption by women. Such changes are evident only for households containing a woman with a reasonable probability of receiving welfare benefits if her marriage ended. The changes in expenditure patterns suggest that reductions in a wife's outside options cause her utility within marriage to decline.

My third chapter analyzes one important decision that households make: when to have children. In this essay, I consider how abortion legalization might impact the types of women who have children. I also explore the impact of these changes in fertility choices on cohort characteristics.

Three years before the Supreme Court ruled on the case of *Roe v. Wade*, New York became the first state to allow all women legal access to abortion on demand. This study analyzes the impact of the early reform on the wages of native New Yorkers 35 to 40 years after the legislation. I use reported age and quarter of birth in the American Community

Surveys to estimate the probability that a worker's mother completed her first trimester of pregnancy after the New York reform and thus had access to legal abortion. I then compare the wages of those reporting the same age (in whole years) but with different estimates of mother's abortion access. By allowing women to better-time their births, the legalization of abortion increased the average wages of black, Hispanic, and lower-wage workers. However, half of the estimated effect of abortion dissipates when one compares the wages of more broadly spaced cohorts, suggesting that a substantial portion of abortion's impact quickly faded. Additionally, I use birth certificate data from New York to examine the socioeconomic characteristics of infants conceived in the weeks before and after New York's reform. This analysis further suggests that soon after legalization, positive selection increased among mothers giving birth.

Taken together, these essays use economic methodology to help us better understand how the family has changed over time, the ramifications of these changes, and how modern spouses interact.

1 Why Have Divorce Rates Fallen? The Role of Women's Age at Marriage

1.1 Introduction

Between 1950 and 1979, divorce rates more than doubled in the United States. Only one-quarter of the marriages that started in the 1950s ended in divorce. But half of all unions beginning in the 1970s would eventually dissolve. Divorce rates, however, soon began to fall back to previous levels. American couples marrying in 2008 are projected to divorce about 40 percent less often than those who wed at the height of marital instability.

Many studies have tried to explain the initial rise in divorce rates.[1] Decreases in marital stability have been linked to decreases in occupational segregation by gender, the rise of the welfare state, household technological progress, and changing social attitudes toward divorce.[2] Several authors have also worked to uncover a connection between family law and divorce rates.[3] Others have focused on the relationship between divorce and female labor force participation or wages.[4]

Despite the substantial attention given to the rise in divorce, there is a dearth of information on why marriages subsequently became more stable. The leveling and decline of divorce has been documented and some authors have discussed potential reasons for the downward trend.[5] But the change remains unexplained.

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[1]See Stevenson and Wolfers (2007) for a survey.

[2]See McKinnish (2007), Moffitt (1997), Greenwood and Guner (2008), and Thornton (1989) respectively.

[3]See Friedberg (1998), Parkman (1992), Peters (1986), and Wolfers (2006).

[4]See Becker (1973, 1974, 1991), Johnson and Skinner (1986), Oppenheimer (1997), Ruggles (1997), and Weiss and Willis (1997).

[5]Goldstein (1999), Kreider and Ellis (2011), and Stevenson and Wolfers (2007, 2011) all document the trend in divorce rates. Isen and Stevenson (2010), Neeman, Newman, and Olivetti (2008), Rasul (2006), and Stevenson and Wolfers (2007) provide various hypotheses for the causes of the trend, although none of these have been formally tested. Goldin and Katz (2002) and Mechoulan (2006) also propose potential explanations

In this paper, I demonstrate that increases in age at marriage must be a key part of any explanation for the decrease in divorce rates after 1980. Indeed, holding a bride's age constant, marriages beginning from 1980 to 2004 are at equal risk of divorce. Thus, age at marriage can statistically explain the fall in divorce.

For age at marriage to actually explain this change in divorce rates, increases in brides' ages must cause decreases in divorce. Many reasons could justify such a relationship. Older brides have spent more time in the marriage market and thus are better informed about their options and optimal mate. Waiting to marry may also lessen a woman's incentive to search for a new partner during marriage, as a wife's outside options could deteriorate with age.

I use three different empirical strategies to explore the potential for a causal relationship between a bride's age and her risk of divorce. All estimates suggest that age at marriage and divorce are robustly correlated and, under certain conditions, can be said to be causally linked.

The first method controls for the driving forces behind family change at the state-year level to determine the extent to which omitted variables may lead to a spurious relationship between age at marriage and divorce.[6] My results suggest that this bias is limited. Moreover, further analysis shows that if observable variables are at least one-quarter as important as unobservable variables are in predicting age at marriage, then increases in age at marriage cause decreases in the probability of divorce.

A second analysis uses the marital histories of sisters to determine if differences in family background bias estimates of age at marriage's effect on divorce. If the family accounts for a large proportion of the variation in marital stability and a small proportion of the variation in age at marriage, these results suggest a causal relationship between age at

for the decline and demonstrate that access to birth control and divorce laws (respectively) may play a role in explaining the fall in divorce.

[6] See Stevenson and Wolfers (2007) for a summary of these factors.

marriage and divorce.

I also use state minimum age at marriage laws as instrumental variables (IVs) to more sharply pin down the causal effect of early teenage marriage on divorce. If these laws are binding for some teens but changes in the laws do not otherwise influence divorce rates, the IV procedure will estimate a local average treatment effect (LATE) of early marriage.

All of the above analyses point to the same conclusion: uncorrected estimates do not largely overstate the relationship between age at marriage and divorce. Thus, if estimates are mainly biased because of omitted (measurable) variables that cause changes in family formation or if a bride's family accounts for a large proportion of the variation in divorce risk but a small proportion of the variation in age at marriage or if the IV estimator is valid, then my results demonstrate that increases in age at marriage caused most of the fall in divorce from 1980 to 2004. All estimates suggest that age at marriage can explain 60 percent or more of the decline. That is, although factors such as female labor force participation (see Neeman, Newman, and Olivetti 2008) and access to reproductive technology (as in Akerlof, Yellen, and Katz 1996 or Goldin and Katz 2002) surely caused changes in the family, these and other driving forces largely impacted divorce rates from 1980 to 2004 by changing age at marriage.

Overall, these results suggest that decreases in the gains to marriage led to increases in age at marriage, which in turn drove down the divorce rate for cohorts marrying from 1980 to 2004. But this explanation may appear inconsistent with changes during the 1960s and 1970s, when the gains to marriage likely fell (e.g., because of increases in female labor force participation) but divorce rates rose. Without a framework for the relationship between the value of marriage and marital stability, these two periods seem to produce contradictory evidence on the correlation between divorce rates and the gains to marriage.

I build a simple search model of the marriage market to show that decreases in the gains to marriage from 1960 to 2004 can be consistent with both increases in divorce prior to 1980 and decreases in divorce afterward. In the model, decreases in the relative value of marriage lead to higher contemporaneous divorce rates. But the changes also induce unmarried women to be pickier about whom they marry and to marry at later ages. Both of these effects imply that a decrease in the value of marriage causes divorce rates to eventually decline. Therefore, the framework shows that reductions in the gains to marriage can lead to the initial rise and the subsequent fall of marital instability.

I conclude by discussing some descriptive evidence on the relationship between age at marriage and many other variables of interest. Results from several datasets suggest that increases in age at marriage are associated with large changes in the lives of men, women, and children. As a woman's age at marriage increases, she tends to argue with her husband less often, disagreements are reported to be more civil, and gender roles are less pronounced. Additionally, women who marry later spend less time both married and divorced. Moreover, older brides have fewer children but bear children before marriage more often. A polarization thus emerges in child living circumstances. As the age at marriage increases, children become more likely to live in a married household if their mother was married when they were born but less likely to live in a married household if their mother was single at the time of their birth.

1.2 Age at Marriage and Divorce

The mean age at first marriage began to increase rapidly and saliently in America around 1970, as depicted in Figure 1.1. The average age of first-time brides increased by almost five years from 1970 until the early 2000s; first-time grooms also married more than four years later in 2000 than they did in 1970.[7,8]

The women who wed as age at marriage began to rise experienced higher divorce rates than any other marriage cohort in the past 60 years. To examine both the rise and fall of marital instability, Figure 1.2 depicts the trend in divorce for marriages beginning from 1950 to 2004, derived from a Cox hazard regression of the form

$$\log h_i(t) = \log h(t) + \delta_{iy} + \beta X_i + \varepsilon_{it}. \tag{1.1}$$

Woman i's hazard of divorce after t years of marriage is $h_i(t)$, δ_{iy} is a vector of variables indicating the year (in five-year groups) that a woman first married, and X_i is a vector of other control variables. The specification allows for a fully flexible hazard rate across the duration of a marriage but requires that covariates have the same proportional effect on the hazard of divorce for all t. I focus my analysis on divorce by year of marriage using the retrospective accounts of women's first marriages commencing from 1950 to 2004, reported in the 2001, 2004, and 2008 panels of the Survey of Income and Program Participation

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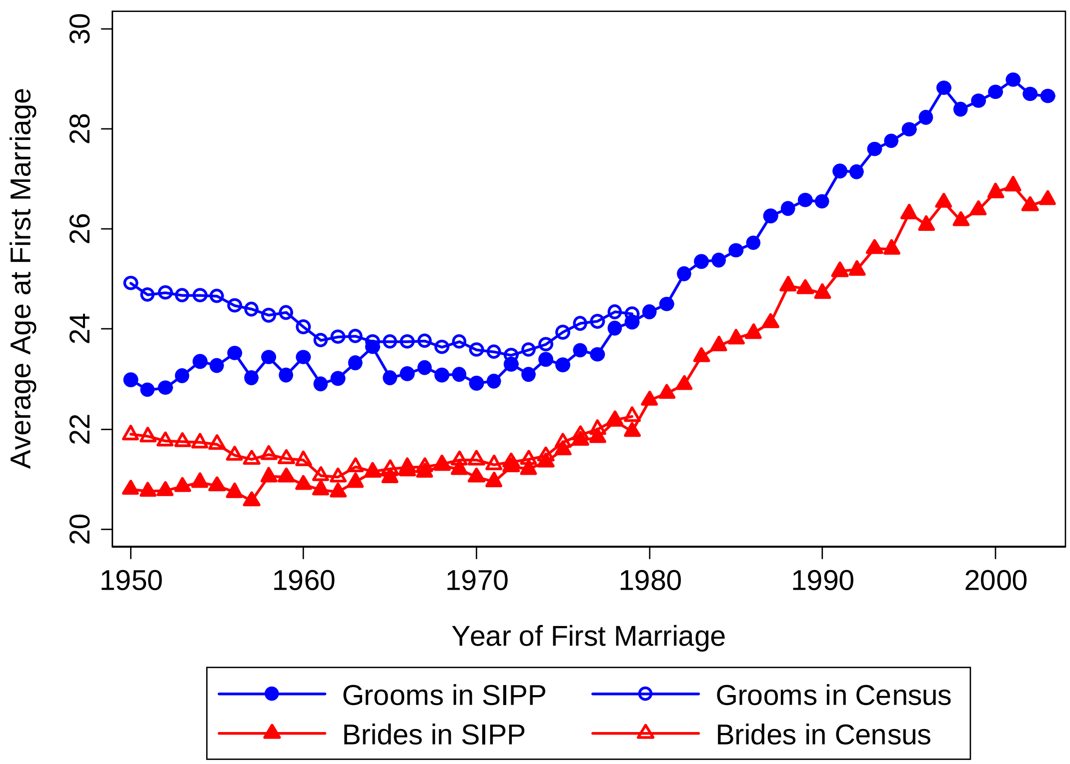
[7]The change in age may be slightly overstated in the SIPP before 1960 due to selective mortality; however, most of the change is concentrated after 1960, when selective mortality is relatively unimportant. Estimates of average age at marriage using data from the 1960-1980 Censuses demonstrate this effect (see Figure 1.1).

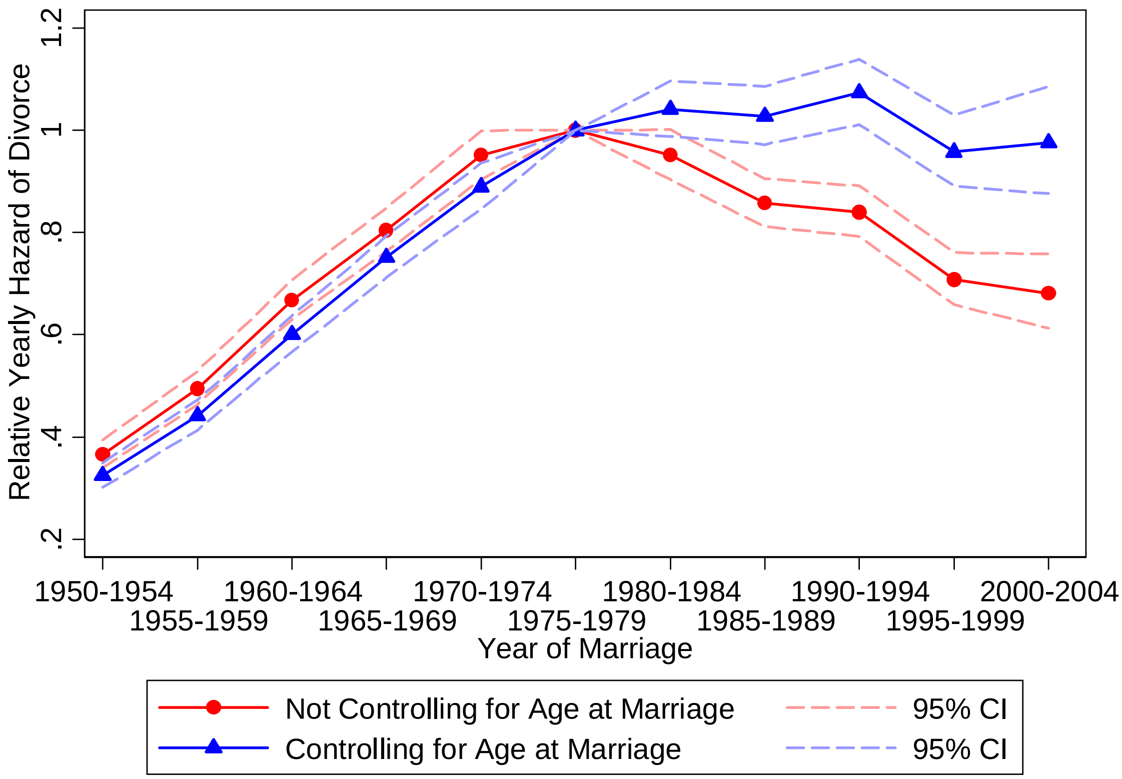
[8]Changes in age at marriage occurred across all age quantiles, causing a shift in the variable's distribution. See Stevenson and Wolfers (2007) for a discussion of the causes of these changes. Key papers providing explanations for trends in age at marriage using a myriad of factors include: Akerlof, Yellen, and Katz (1996), Bitler, et al. (2004), Becker (1973, 1974, 1991), Brien, Lillard, and Stern (2006), Ellwood and Bane (1985), Goldin and Katz (2002), Gould and Passerman (2003), Loughran (2002), and Mechoulan (2006).

Figure 1.1: Age at First Marriage: 1950 to 2003



Notes and sources: Average age of first-time brides and grooms by year of marriage. SIPP sample: Women (N=74,339) and men (N=65,200) from the 2001, 2004, and 2008 SIPP panels with complete information on first marriages, 1950-2003. Census sample: Women (N=1,064,745) and men (N=1,021,497) from the largest IPUMS Census sample directly following their first marriage, 1950-1979 (i.e., age at marriage in 1975 is calculated using the reported age at first marriage for those who married in 1975 in the 1980 Census 5 Percent Sample). Difference in SIPP and Census samples likely due to selective mortality.

Figure 1.2: Hazard Rates of Divorce Across Marriage Cohorts



Notes and sources: Women's first marriages from the 2001, 2004, and 2008 SIPP, 1950-2004 (N=74,339). See Appendix A.1.1 for details. Effects are from a Cox hazard regression setting the hazard of divorce for marriages occurring from 1975 to 1979 to one. Observations censored at time of interview or time of death of spouse. Age at marriage controls include indicators for marrying under 18, 18-19, 20-22, 23-26, 27-29, 30-34, 35-39, and 40+. Robust standard errors used to calculate 95 percent confidence intervals.

(SIPP).[9]

Figure 1.2 shows estimates of the relative hazard of divorce by marriage cohort, $h_y = \exp(\delta_y)$, from regressions controlling only for year of marriage ($h_y = 1$ for marriages beginning from 1975 to 1979). Couples marrying between 1970 and 1984 faced the highest divorce rates. Marriages beginning both before and after this period were more stable, with unions beginning in the late 1990s and early 1960s dissolving at similar rates.

Trends in the relative hazard of divorce are very different when one holds age at marriage constant, as shown by the second line in Figure 1.2. Given the large increase in the age at marriage (see Figure 1.1) and previous findings demonstrating a negative relationship between bride's age and divorce risk (e.g., Becker, Landes, and Michael 1977, Teachman 2002, and Lehrer 2008), it is not surprising that the age-adjusted and unadjusted trends diverge. Although divorce propensities still increase for marriages beginning from 1950 to 1979, there is no longer a subsequent decrease in stability once one controls for age effects. Thus, the increase in age at marriage statistically explains the decrease in divorce for women first marrying from 1980 to 2004.[10,11]

Although I examine only those living in the US, Americans are not alone in experiencing increasing age at marriage and marital stability after 1980. The United Nations (2009) provides measures of the singulate mean age at marriage and divorce rate around 1980 and 2004 for 28 OECD countries.[12] In each country, age at marriage rose over

[9] See Appendix A.1.1 for details. The following analyses focus on the age of *women* at marriage and the divorce rate for *first marriages*. The SIPP does not report age at marriage for both spouses unless a marriage remains intact. Using male age at first marriage yields largely similar results.

[10] Trends both with and without age controls are robust to different specifications of the hazard function. Figure A.3 demonstrates this by looking at the results from four different regressions predicting the probability of divorce before a couple's fifth, tenth, 15th, and 20th anniversaries, with and without controls for age at marriage.

[11] See Figures A.4 and A.5 for relative hazard rates by age at marriage over marriage cohorts and over birth cohorts.

[12] The singulate mean age at marriage is calculated using the marital status of a country's population by age to estimate the average number of years a member of the population was single.

the given period, by between 0.7 (Norway) and 7.4 years (Belgium). Divorce rates also decreased in many of these countries throughout this time.[13]

The rise and fall in divorce was widespread within the US and occurred across groups of women differing by race, education, and location, as shown in Figure 1.3.[14]

In all selected subgroups, controlling for age at marriage mitigates the decline in divorce from 1980 to 2004. For whites, non-college graduates, those living in urban areas, and those from both liberal and conservative states, the hazard rate of divorce conditional on age at marriage is roughly constant or increasing from 1980 to 2004. Age at marriage has a weaker effect on both non-whites and college graduates; the age-adjusted divorce rate in these groups decreased during the 1990s.[15]

A nonparametric function relating age at marriage to divorce demonstrates how powerful a bride's age is in predicting her marriage's stability.[16] Figure 1.4 shows the hazard of divorce by age at marriage (relative to age 22) in the SIPP, controlling for year of marriage fixed-effects and other observable characteristics.[17] The curve is both decreasing and convex, as initially suggested by Becker, Landes, and Michael (1977). Marriages beginning when a bride is 18 are twice as likely to end in divorce than those starting when a woman is 22. Brides in their mid-30's have marriages four times as stable as teenage

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[13]Specifically, divorce rates in Canada, Germany, Hungary, Iceland, the United Kingdom, and Sweden all fell from the early 1980s to the early 2000s. However, no statistically significant relationship exists between the change in age at marriage and the change in divorce rates cross-nationally.

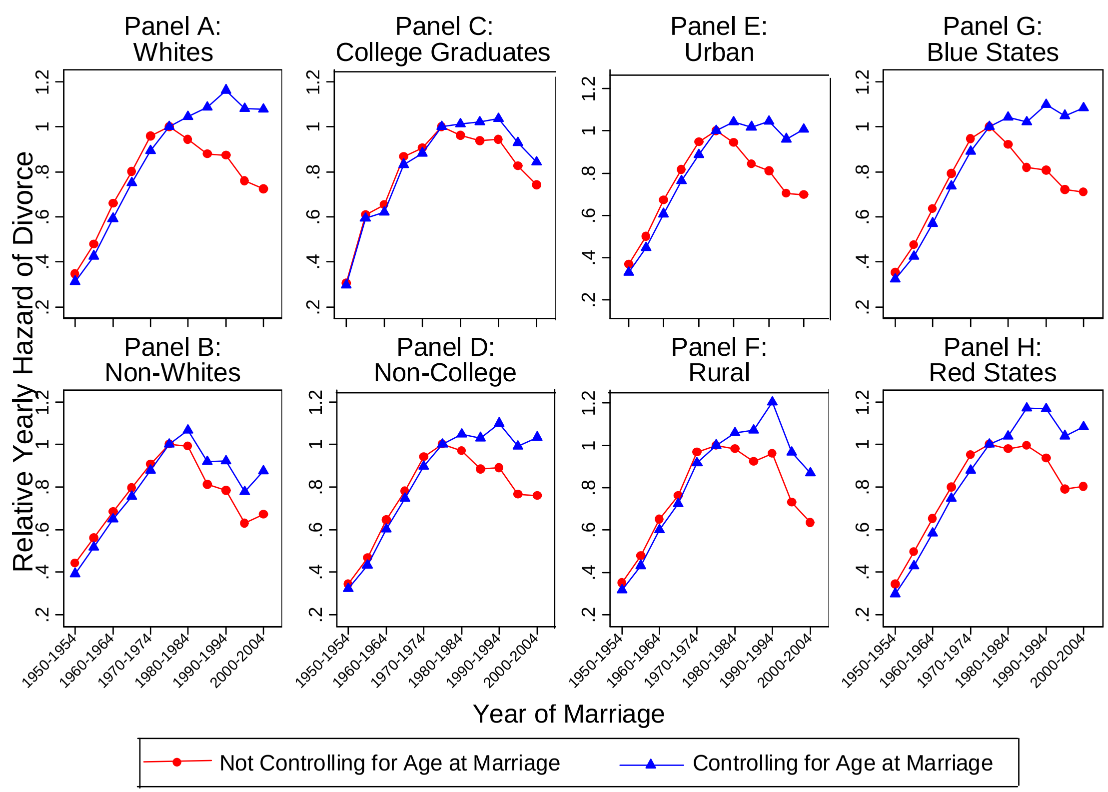
[14]Looking within smaller subgroups yields similar results. Of non-whites, the trend in divorce for blacks is most affected by age at marriage controls. Of non-college graduates, the trend in divorce for high school dropouts is least affected by age at marriage controls.

[15]Note that more educated women began the period of interest with relatively high ages at first marriage. The convexity of the age-divorce function (see Figure 1.4) then implies a smaller decrease in divorce for any increase in age.

[16]See Lehrer (2008) and Lehrer and Yu (2011) for more detailed discussions of the shape of the relationship between age at marriage and divorce.

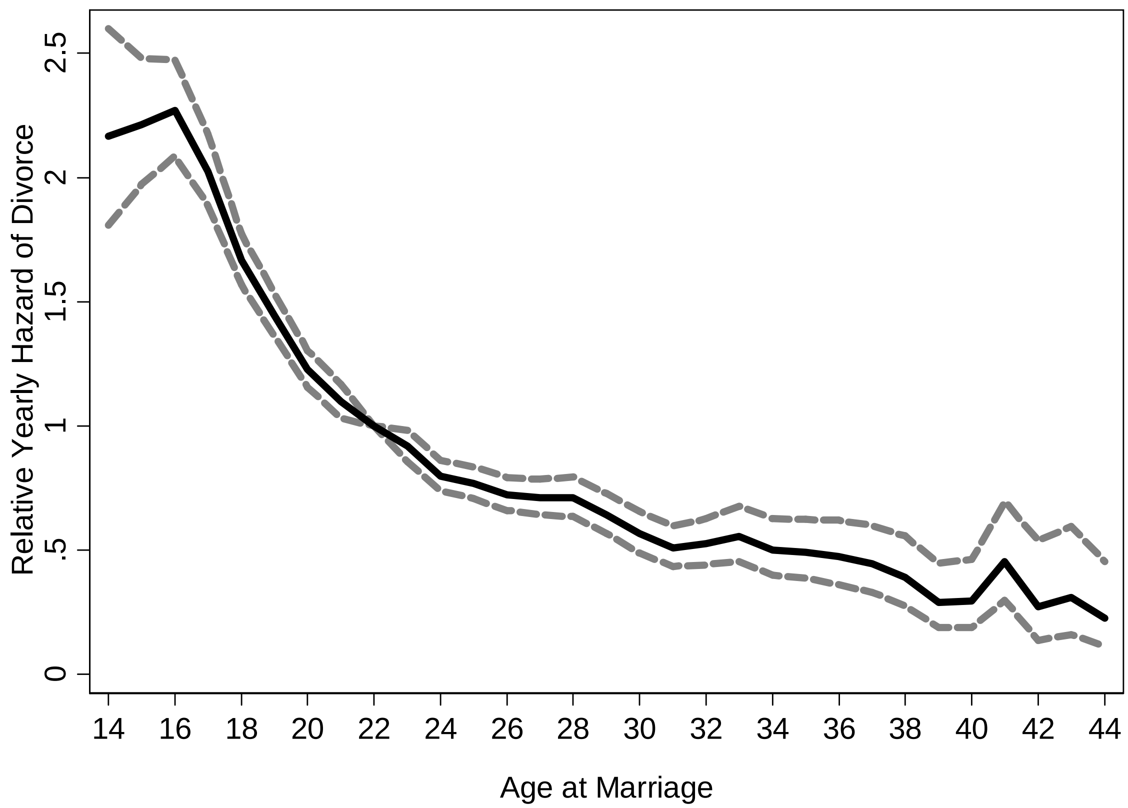
[17]Controls include indicators for urban location and census division at interview, education at marriage in four groups, being black, white, Hispanic, or another race, and having children prior to marriage.

Figure 1.3: Divorce Hazard Rates by Race, Education, and Location



Notes and sources: Women's first marriages from the 2001, 2004, and 2008 SIPP, 1950-2004. See Appendix A.1.1 for details. Effects are calculated by five-year group from a Cox hazard regression setting the hazard of divorce for marriages occurring from 1975 to 1979 to one. Observations censored at time of interview or time of death of spouse. Age at marriage controls include indicators for marrying under 18, 18-19, 20-22, 23-26, 27-29, 30-34, 35-39, and 40+. College/non-college status as of time of marriage, urban/rural status at interview. Red/blue states classified using state of birth and the mean margin of victory in the 1992-2008 presidential elections (red is Republican candidates favored).

Figure 1.4: The Relationship Between Age at Marriage and Divorce



Notes and sources: Women's first marriages from the 2001, 2004, and 2008 SIPP, 1950-2004 (N=73,338). Marriage at age 22 is the baseline category. See Appendix A.1.1 for details. Observations censored at time of interview or time of death of spouse. Coefficients from Cox hazard regression also controlling for year of marriage fixed-effects, premarital childbearing, education at marriage (four groups), race (black, Hispanic, white, and other), census division and urban location (both at interview). Robust standard errors used to calculate 95 percent confidence intervals.

brides.[18,19]

1.2.1 Decomposing the Determinants of Divorce

Combining the coefficients from estimates of eq. (1.1) with the change in the vector of independent variables over time provides additional information on the relative contribution of various factors to the decline in divorce rates (see Table 1.1). I use these estimates to decompose the actual change in divorce from 1980 to 2003 into components predicted by age at marriage, predicted by other observables, and not predicted by the included variables.[20,21]

On average, first marriages starting in 1980 have a divorce hazard rate about 37 log points higher than those that began in 2003. A decline in teenage marriage explains almost half of this fall in divorce. Teens comprised only 15 percent of first-time brides in 2003 but about 40 percent of brides in 1980. Because women who marry as teens divorce far more often than those who wait to marry, the change accounts for a substantial portion of the fall in the average hazard of divorce. Other increases in age at marriage imply further declines in divorce, with changes in age explaining 80 percent of the total change in the hazard rate.

Brides were more educated in 2003 than in 1980, but the increase in female education does not imply a large decline in divorce by itself. The doubling of the proportion of brides

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[18]The convexity of the relationship also suggests caution when interpreting a coefficient on a linear variable for age at marriage.

[19]Further analysis of the relationship between brides' ages and divorce rates indicates that age at marriage has a roughly constant effect across marriage cohorts when one holds constant the distribution of age at marriage (as in Dinardo, Fortin, and Lemieux 1996). Moreover, one can show that marriages beginning earlier in life have higher divorce rates early in marriage. But after a couple's tenth anniversary, age at marriage becomes less predictive of divorce.

[20]Age at marriage jumps from 2003 to 2004 in my SIPP panels, although it is not clear if this change is real or due to sampling error or changes in methodology. Thus, I consider the change from 1980-2003, instead of 1980-2004.

[21]Decompositions of the change in the divorce rate from 1980 to 1995 or 2000 yield largely similar results.

Table 1.1: Decomposing the Change in Divorce Hazards

	(1)	(2)	(3)	(4)	(5)	(6)
Indep. Vars.	Coefficient (Change in Ln Hazard)	Standard Error	1980 Indep. Var. Mean	2003 Indep. Var. Mean	Change from 1980 to 2003 (4)-(3)	Effect of Change on Hazard Rate (Ln Points) 100 (1) *(5)
Age at Marriage						
Under 18	0.945***	[0.0281]	0.0990	0.0272	-0.0718	-6.78
18-19	0.559***	[0.0210]	0.3061	0.1269	-0.1792	-10.02
20-22	0.255***	[0.0229]	0.1895	0.1350	-0.0545	-1.39
27-29	-0.165***	[0.0338]	0.0787	0.1414	0.0627	-1.03
30-34	-0.420***	[0.0419]	0.0496	0.1396	0.0900	-3.78
35-39	-0.622***	[0.0694]	0.0193	0.0696	0.0502	-3.13
40+	-1.21***	[0.106]	0.0169	0.0486	0.0316	-3.83
Total Change in Log Hazard Predicted by Change in Age						-29.96
Ed. at Marriage						
High School	0.0321	[0.0203]	0.3491	0.2222	-0.1270	-0.41
Some College	0.00946	[0.0225]	0.2803	0.3208	0.0405	0.00
College	-0.307***	[0.0308]	0.1604	0.3357	0.1753	-5.38
Black	0.0771***	[0.0228]	0.1037	0.0955	-0.0082	-0.063
Hispanic	-0.569***	[0.0346]	0.0690	0.1760	0.1071	-6.09
Other Race	-0.295***	[0.0323]	0.0682	0.0928	0.0246	-0.726
Premarital Childbearing	0.315***	[0.0206]	0.1 446	0.2862	0.1417	4.46
Urban (Interview)	0.0749***	[0.0162]	0.7825	0.8262	0.0437	0.328
Total Change in Log Hazard Predicted by the Above Factors						-40.70
Actual Change in Log Hazard from 1980 to 2003						-36.99
Change in Log Hazard Unexplained by Observables						-3.46

Notes and sources: Women's first marriages from the 2001, 2004, and 2008 SIPP, 1950-2004 (N=74,339). See Appendix A.1.1 for details. Coefficients from Cox hazard regression that also includes controls for year of marriage. Coefficients measure changes in log hazard rates. Observations censored at time of interview or time of death of spouse. Omitted categories are marriage between ages 23 and 26, white, and less than high school education. Robust standard errors in brackets. *** $p<0.01$.

with a college degree only yields a decrease in the hazard rate of 5.4 log points. The hazard rates associated with all other levels of education are approximately the same. Thus, the increased education of brides only accounts for about 15 percent of the change in divorce rates from 1980 to 2003.

The changing racial composition of married families also predicts a notable component of the change in the hazard rate. The proportion of Hispanic brides more than doubled from 1980 to 2003, implying a 6 log point decrease in overall divorce hazards. Additionally, women who enter a (first) marriage with a child have higher divorce rates than those who do not. Women with children increased from 14 to 28 percent of first-time brides and thus divorce hazard rates fell 4.5 log points less than they otherwise would have. Together, the observable variables predict a decrease in divorce from 1980 to 2003 approximately equal to the actual change.[22,23]

1.2.2 Younger Brides Differ from Older Brides

The decomposition in Table 1.1 may tempt one to conclude that changes in the age at marriage caused most of the decline in divorce. But the relationship between these variables cannot be taken as causal without further thought. Differences between women who wait to marry and those who do not could be driving the correlation. To see how younger and older brides differ, I estimate the relationship between age at marriage and several variables from the SIPP and National Survey of Family Growth (NSFG, see Table 1.2).[24]

The SIPP demonstrates that women who marry for the first time later in life are more educated; the rate of college graduation increases by 1.5 percentage points if one considers

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[22]Similar results hold if one instead considers the effect of a man's age at first marriage on the probability that his first marriage ends in divorce.

[23]The NLSY and National Survey of Family Growth (NSFG) yield coefficients similar to those in the first column of Table 1.1.

[24]See Appendix A.1.1 (A.1.3) for details on the SIPP (NSFG).

Table 1.2: Woman's Age at Marriage and Characteristics at Marriage

Dependent Variable	Mean	Coefficient on Wife's Age at Marriage	SE	Obs.
Women in SIPP				
Less than High School	0.208	-0.0126***	[0.000356]	74339
High School	0.336	-0.00350***	[0.000337]	74339
Some College	0.283	0.00131***	[0.000303]	74339
College or More	0.173	0.0148***	[0.000326]	74339
Had Children Before Marriage	0.162	0.00878***	[0.000307]	74339
White	0.760	-0.00335***	[0.000329]	74339
Black	0.095	0.00334***	[0.000228]	74339
Hispanic	0.083	-0.00196***	[0.000236]	74339
Women in NSFG				
Catholic	0.292	0.0103***	[0.000958]	34124
Protestant	0.579	-0.0130***	[0.00110]	34124
(Ex-) Husbands in NSFG				
Age	24.38	0.840***	[0.0106]	34124
Less than High School	0.202	-0.00179***	[0.0001]	27292
High School	0.388	-0.00171***	[0.0001]	27292
Some College	0.197	0.000348***	[0.0001]	27292
College	0.090	0.00157***	[0.0001]	27292
Previously Married	0.123	0.0206***	[0.00109]	16066
White	0.705	0.00770***	[0.00145]	12597
Black	0.095	0.00543***	[0.000889]	12597
Hispanic	0.151	-0.0135***	[0.00116]	12597
Catholic	0.307	0.00692***	[0.00160]	9146
Protestant	0.531	-0.0101***	[0.00173]	9146
Relationships in NSFG				
Cohabited Before Marriage	0.420	0.0139***	[0.00129]	16895
Mos. Cohabited (Given Any Time)	14.18	1.27***	[0.133]	7272
Shotgun Marriage	0.163	-0.0205***	[0.000819]	31195
Age Diff. (Male-Female, Mos.)	36.31	-1.87***	[0.126]	34124
Same Education Level	0.43	-0.0179***	[0.00109]	27292
Same Race	0.894	0.000205	[0.00120]	12597

Notes and sources: Ever married women in the 2001-2008 SIPP and the 1973-2008 NSFG. Women's first marriages beginning 1950-2004. See Appendices A.1.1 and A.1.3 for details. Coefficients from regressions also including controls for year of marriage in five-year groups. Shotgun marriage is defined as when a woman has her first child between zero and eight months after marriage. Same race is defined as husband and wife both being black, white, Hispanic, or another race. Same education level is defined as husband and wife both having less than high school, high school, some college, or college (or more) education. Robust standard errors in brackets. ***p<0.01.

a group of brides who are one year older. Later weddings are also more likely to involve blacks, Catholics, and women with children.[25] As Catholics and college-graduates have lower rates of divorce than others, these differences reinforce a positive association between age at marriage and marital stability, though high rates of divorce among black women and women with children before marriage might temper this relationship.

Waiting one additional year to marry is also associated with a 1.4 percentage point increase in the probability of premarital cohabitation and a 2 percentage point decrease in the probability of a shotgun wedding.[26] Moreover, as a woman's age at marriage increases, spouses' ages move closer together but fewer husbands and wives have the same educational attainment. Shotgun marriages and marriages among those far apart in age are associated with high rates of divorce, reinforcing a positive relationship between age at marriage and marital stability. If educational homogamy or premarital cohabitation relate directly to marital stability, these factors will also bias estimates of age at marriage's effect on divorce.[27]

1.3 The Direct Impact of Age at Marriage on Divorce

Consider the following hypothetical experiment, which would allow one to estimate the unbiased, causal relationship between a bride's age and her marriage's stability. Suppose that a woman selects her husband from a pool of men. If she chooses a spouse when she is older, she may make a better-informed decision. Alternatively, she may behave differently within her marriage if she waits to make her choice (i.e., older brides may have more limited outside options and thus be less tempted to leave their husbands). These effects should lead to greater stability in marriages beginning later in life.

[25] Similar traits are associated with the men who marry older women, as suggested by assortative mating.

[26] Defined as a couple marrying zero to eight months prior to a woman's first birth.

[27] The literature on cohabitation and marital stability remains inconclusive due to selection effects.

If one could manipulate when a woman selects her husband, one could then simply compare the stability of marriages randomly chosen to start at earlier or later ages. Such an experiment is, of course, infeasible. But comparing women who are very similar (and likely have the same requirements for a spouse) or those that marry at different ages for exogenous reasons will allow one to closely approximate the estimates from this ideal experiment (for some group of women).

Interpreting this experiment requires that no matter when a woman is given the chance to marry, she chooses a spouse from her set of potential mates. However, women selected to choose a husband at older ages may decide not to marry at all (and thus will never divorce). In a sense, this selection effect represents an alternative way for increases in age at marriage to imply decreases in the divorce rate.

If this channel was important in explaining the decrease in divorce, one would expect two things. First, the number of women who ever married would have rapidly declined from 1980 to 2004. Second, the change in the number of women ever marrying would explain the decrease in divorce at an aggregate level. Between 1980 and 2004, the change in the proportion of women age 40 to 49 who had ever married was small (only about 6 percent). Moreover, if I create a panel of marriage and divorce rates by state and year of birth, the change in this marriage rate predicts only a small decrease in divorce from 1980 to 2004. Therefore, the following analysis treats the direct relationship between a bride's age and marital stability as the key force behind the link between age at marriage and divorce.[28]

1.3.1 Controlling for Factors Influencing Family Structure

Extensive research has focused on measuring the impact of various forces on age at marriage. Increases in female labor force participation and relative wages, increases in

[28]Furthermore, using samples of women for whom this selection effect may be less important (e.g., whites) yields similar results in the following analyses. Additionally, my IV technique employs variation unrelated to eventual marriage rates and thus will not capture the selection effect.

access to reproductive technology, decreases in the costs of being or becoming single, and increases in inequality have all been proposed as major causes of the rise in brides' ages.[29]

In Section 1.4, I develop a search model where a host of factors influence both age at marriage and divorce. Women (both when single and when married) search for potential spouses distinguished by their wage rates. Those who marry later in life are less inclined to search for a new spouse while married, even conditional on spousal quality. Thus, increases in age at marriage cause a decrease in the divorce rate.

One would like to estimate the direct effect of age at marriage on divorce. But many of the variables that influence a bride's age could also affect marital stability. My model predicts that women will marry later and be less prone to divorce if they have a stronger attachment to the labor market, greater access to family planning tools, or lower costs of looking for a mate while single. Greater variance in male wages also raises the age at first marriage but has an ambiguous effect on divorce rates.

These correlations will bias estimates of the causal effect of age at marriage on divorce. Different models of the marriage market may imply different changes in divorce in response to changes in the determinants of family formation. But in most models of family structure, at least some of these forces have the power to influence both age at marriage and divorce, suggesting the potential for omitted variable bias.[30]

To reduce bias, I estimate regressions of the form

$$\log h_i(t) = \log h(t) + \beta X_i + \delta_{iy} + \theta_{is} + \alpha Age_i + \gamma C_{isy} + \varepsilon_{ist}$$

[29] For example, see Becker (1973, 1974, 1991) and Neeman, Newman, and Olivetti (2008) on female labor force participation; Akerlof, Yellen, and Katz (1996) or Goldin and Katz (2002) on reproductive access; Gould and Passerman (2003) or Loughran (2002) on income inequality; and Bitler, et al. (2004), Ellwood and Bane (1985), Hoynes (1996b), Mechoulan (2006), or Rasul (2006) on other costs of being/becoming single.

[30] See Becker (1991) or Stevenson and Wolfers (2007) for discussions of forces influencing marriage and divorce patterns.

where C_{isy} is a vector of the variables thought to influence family structure, measured at the state of birth (s) by year of marriage (y) level. The vector includes measures of access to abortion, access to oral contraceptives, rates of cohabitation, Comstock laws, female labor force participation, the gender gap in wages, occupational segregation by gender, unilateral divorce legislation, welfare generosity, and male wage inequality.[31]

I estimate the effect of age at marriage on the log hazard rate of divorce (α) separately using the entire SIPP sample and the limited number of state-year variables available from 1950 to 2004 (Table 1.3A) and the period in which all the C_{isy} variables can be matched to the SIPP (1968-2004, Table 1.3B). All specifications indicate that waiting one extra year to marry is associated with a 9 to 10 percent lower hazard rate of divorce.[32] Adding individual-level controls decreases the estimate of α (in absolute value) by about 1 percentage point. But the inclusion of the C_{isy} terms does not change the coefficient on age at marriage in a meaningful way. Similar results hold when one uses several dummy variables for age at marriage instead of a single, continuous variable.

Though the inclusion of C_{isy} does not affect α, these variables do predict both age at marriage and divorce. One can reject a hypothesis of $\gamma = 0$ at the 5 percent level.[33] The full set of state-year variables also predicts almost one-third of the 4.9 year change in women's age at marriage from 1968 to 2003. The more limited set of variables can explain 10 percent of the 5.7 year change in bridal age from 1950 to 2003.[34]

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[31]Appendix A.1.4 contains details on these variables and their measurement.

[32]The average yearly hazard rate of divorce is 2.0 percent at $t = 10$, 1.4 percent at $t = 20$, and 0.6 percent at $t = 30$.

[33]When individually included in the regression, most of the C_{isy} variables do not have coefficients significantly different from zero; however, all significant γ coefficients are consistent with the model presented in Section 1.4. Further, when individually added to the regression, none of the variables changes α.

[34]Estimates from regressions also controlling for state of birth and year of marriage effects.

Table 1.3: Divorce Risk Controlling for the Determinants of Family Structure

Dependent Variable: Log Yearly Hazard of Divorce				
Panel A: Year of Marriage: 1950-2004				
	(1)	(2)	(3)	(4)
Age at Marriage	-0.0956***	-0.0845***	-0.0845***	-0.0869***
	[0.00290]	[0.00305]	[0.00306]	[0.00309]
Observations	63,035	63,035	63,035	63,035
p-value on State-Year Variables			0.0255	0.000
Panel B: Year of Marriage: 1968-2004				
	(1)	(2)	(3)	(4)
Age at Marriage	-0.0937***	-0.0826***	-0.0827***	-0.0846***
	[0.00312]	[0.00325]	[0.00326]	[0.00330]
Observations	43,971	43,971	43,971	43,971
p-value on State-Year Variables			0.0421	0.000
Controls for Both Panels				
State of Birth FE	X	X	X	
Year of Marriage FE	X	X	X	
State of Birth Quadratic Trends		X	X	
Individual-Level Variables		X	X	X
State-Year Variables			X	
State-Year FE				X

Notes and sources: Women's first marriages from the 2001, 2004, and 2008 SIPP, 1950-2004. See Appendix A.1.1 for details. Coefficients measure changes in log hazard rates. Individual variables are for having children prior to marriage, urban location and census division (both at interview), education at marriage (four groups), and race (black, white, Hispanic, and other). State-year variables available for 1950-2004 are the abortion rate, the log average real monthly welfare benefit adjusted for family size, and indicators for unilateral divorce availability, a sales ban on contraceptives, and 18-year-old's access to birth control pills. Additional state-year variables available for 1968-2004 are the proportion of people likely cohabiting, female labor force participation (full time, full year and any hours), the log real gender gap in wages, the log real 90-50 and 50-10 wage differentials, an occupation-industry gender segregation index, the proportion of women working in traditionally male jobs and the proportion of women working in traditionally female jobs. See Appendix A.1.4 for details. Only the abortion rate is significant when included with no other state-year variables in regressions using marriages starting from 1950 to 2004. The proportion of women working any hours and the proportion of women working in traditionally male jobs are significant when included individually and with no other state-year variables in regressions using marriages starting from 1968 to 2004. Robust standard errors clustered by state of birth in brackets. *** $p<0.01$.

Three potential factors likely led the impact of age at marriage on divorce to remain constant across the specifications. First, omitting some of the C_{isy} variables likely biased α upward, while the omission of others biased α downward. Together, the effects cancelled each other out. Even if there was a net bias, both γ and the effect of C_{isy} on age at marriage are small and easily swamped by a large unbiased value of α. Finally, variables measured at the state-year level may simply not pick up much of the important variation in the factors influencing age at marriage and divorce. Even when one looks within state-year pairs, controlling for all factors varying at this level, α does not substantially change.

These estimates suggest that observable variables do not lead to much bias in estimating the effect of age at marriage on divorce. But there may be important unobservable variables. I thus also use the method proposed by Altonji, Elder, and Taber (2005) to determine how large the bias from omitting unobservables would have to be to explain the estimated coefficient on age at marriage.

I simplify the problem of omitted variable bias by focusing on individual estimates of the coefficients associated with indicators for marrying before a certain age (18, 22, or 28) in regressions predicting divorce before certain points in a couple's marriage (the fifth, tenth, 15th, or 20th anniversary). I estimate these effects using the 1968-2004 SIPP sample and either no other covariates or the full vector of observable controls. The coefficients vary somewhat with the inclusion of controls, giving one a sense that selection on observables, particularly year of marriage and bride's education, may be important to some extent (see Table 1.4). Given these estimates, one can then assume that the true effect of age at marriage on divorce is zero ($\alpha = 0$) and back out the implied extent of selection on unobservables (relative to observables).

Unobservables would have to strongly influence age at marriage for selection to explain the entire estimated value of α (see Table 1.4). To conclude that age has no causal effect on ten-year divorce rates, selection on unobservable characteristics would have to be

Table 1.4: The Relative Importance of Selection on Observables and Unobservables

Dependent Variable=1 if Marriage Ends in Divorce by Given Anniversary				
Panel A: Age Less Than 18				
Anniversary	5th	10th	15th	20th
ME of Bride's Age$<$18 from	0.145***	0.275***	0.242***	0.220***
Probit Regression without Controls	[0.010]	[0.018]	[0.018]	[0.019]
ME of Bride's Age$<$18 from	0.100***	0.195***	0.169***	0.153***
Probit Regression with Controls	[0.008]	[0.015]	[0.015]	[0.018]
Relative Selection on Unoberservables				
Required to Eliminate Effect	4.97	4.94	4.60	4.27
Brides Average Age Given 18 or Over	24.14	23.64	23.08	22.53
Brides Average Age Given Under 18	16.26	16.27	16.28	16.28
Panel B: Age Less Than 22				
Anniversary	5th	10th	15th	20th
ME of Bride's Age$<$22 from	0.079***	0.213***	0.208***	0.194***
Probit Regression without Controls	[0.004]	[0.006]	[0.006]	[0.008]
ME of Bride's Age$<$22 from	0.059***	0.188***	0.178***	0.164***
Probit Regression with Controls	[0.004]	[0.005]	[0.006]	[0.008]
Relative Selection on Unoberservables				
Required to Eliminate Effect	3.19	3.21	2.98	2.62
Brides Average Age Given 22 or Over	27.02	26.60	26.11	25.62
Brides Average Age Given Under 22	18.99	18.97	18.96	18.95
Panel C: Age Less Than 28				
Anniversary	5th	10th	15th	20th
ME of Bride's Age$<$28 from	0.056***	0.163***	0.202***	0.203***
Probit Regression without Controls	[0.004]	[0.008]	[0.010]	[0.012]
ME of Bride's Age$<$28 from	0.041***	0.137***	0.182***	0.184***
Probit Regression with Controls	[0.004]	[0.007]	[0.009]	[0.013]
Relative Selection on Unoberservables				
Required to Eliminate Effect	1.57	1.71	1.47	1.20
Bride's Average Age Given 28 or Over	33.08	32.75	32.49	32.20
Bride's Average Age Given Under 28	21.34	21.17	20.99	20.80
Proportion of All Couples Divorced	0.11	0.26	0.36	0.42

Notes and sources: Women's first marriages from the 2001, 2004, and 2008 SIPP, 1968-2004. See Appendix A.1.1 for details. Regressions without controls include only the specified age indicator. Regressions with controls include state of birth and year of marriage fixed-effects, individual controls for having children prior to marriage, urban location and census division (both at interview), education at marriage (four groups), and race (black, white, Hispanic, and other). All of the state-year variables from Table 1.3 are also included. See Appendix A.1.4 for details. Probit regressions estimated, marginal effects reported. Observations censored at date of interview or death of spouse. Relative selection on unobservables (compared to observables) calculated using Altonji, Elder, and Taber (2005). Robust standard errors clustered by state of birth in brackets. *** p$<$0.01.

about five times as strong as selection on observable characteristics. If one allows early marriage to have an effect on divorce, but imposes that there is no difference in divorce rates between those marrying before and after age 28, selection on unobservables would have to be more than 1.7 times as important as selection on observables. Because the observed variables I use include important determinants of both age at marriage and divorce (and indicators for year of marriage and state of birth), these levels of relative selection are unlikely. Therefore, age at marriage, and not some other combination of observable or unobservable factors, is the main proximate cause of the decline in divorce.

1.3.2 Controlling for Family Background

Family and personal background could also affect both age at marriage and marital stability.[35] Many religions, for example, advocate either early marriage, limited divorce, or both. Young women who grew up in intact families may view marriage and divorce differently from those who experienced their parents' separation. I therefore use the 1979 National Longitudinal Survey of Youth (NLSY) to control for factors such as religion and childhood family structure.[36] The survey tracks a single cohort of women (age 14 to 22 in 1979), who on average married in 1984 at age 23.

The most straightforward approach to using the data adds controls for family background to the hazard regression, as in

$$\log h_i(t) = \log h(t) + \beta X_i + \alpha Age_i + \gamma F_i + \varepsilon_{it} \tag{1.2}$$

where F_i is a vector of background variables (controls for religion, religious participation, family structure, media access, and mother's and father's labor force participation,

[35] See Gruber (2004) and Heaton (2002).

[36] These variables are omitted from the SIPP. See Appendix A.1.2 for details on the NLSY.

education, and occupation). The NLSY also includes a sample of almost 900 sisters that I use to estimate regressions with family fixed-effects, as in

$$\log h_{if}(t) = \log h_f(t) + \beta X_i + \alpha Age_i + \varepsilon_i. \tag{1.3}$$

Ideally, the inclusion of family effects or family background variables allows one to estimate the effect of age at marriage on divorce, holding some of the determinants of the gains to marriage constant. However, adding fixed-effects to eq. (1.3) may increase the bias in α. In particular, fixed-effects will only lessen the bias if the fraction of variability in divorce that the family explains exceeds the fraction of variability in age at marriage that the family explains.[37,38]

Similar to the results found when adding controls at the state-year level, including controls for factors other than education does not change the coefficient on age at marriage in a meaningful way, as demonstrated by the estimates in Table 1.5. In addition, including family effects does little to change the value of α (see Table 1.6) . Specifications that replace the linear age at marriage term in eqs. (1.2) and (1.3) with a set of dummy variables also produce qualitatively similar results. Overall, the estimates from the NLSY further suggest that increases in age at marriage, and not changes in family structure, religion, or other background variables, explain most of the drop in divorce rates from 1980 to 2004.

1.3.3 IV Using State Age Restrictions on Marriage

These initial analyses allow one to understand potential threats to a causal interpretation of the relationship between age at marriage and divorce, but they do not

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[37] Griliches (1979) shows that this is a necessary and sufficient condition for the addition of fixed-effects to reduce bias if there is no measurement error. Stronger assumptions may be needed if age at marriage is measured with error.

[38] One cannot test this assumption but the inherent randomness of the marriage market makes it more likely to hold.

Table 1.5: Divorce Risk, Age at Marriage, and Family Background

	Dependent Variable: Log Yearly Hazard of Divorce					
	(1)	(2)	(3)	(4)	(5)	(6)
Age at Marriage	-0.0766***	-0.0627***	-0.0635***	-0.0633***	-0.0575***	-0.0574***
	[0.0098]	[0.0111]	[0.0113]	[0.0112]	[0.0116]	[0.0135]
Race, Education, Location		X	X	X	X	X
Children Before Marriage			X	X		X
Religion, Religiosity			X	X		X
Family Structure				X		X
Media Access Controls				X		X
Parental Characteristics						X
Complete Parent Data					X	X
Observations	3,831	3,831	3,831	3,831	2,827	2,827

Notes and sources: First marriages of women in the NLSY. See Appendix A.1.2 for details. Coefficients measure changes in log hazard rates. Observations censored at time of interview or time of death of spouse. Race controls include indicators for being black, white, Hispanic, or of another race; education controls for education level at marriage in four groups; location controls are for urban status and census region at marriage. Religion includes indicators for attending services once a month or more and once a week or more, and indicators for being Catholic, Protestant, or another religion. Children before marriage is an indicator for a woman having a child prior to her first marriage. Family structure includes indicators for the presence of the biological mother, biological father, both biological parents, and any father figure, as well as the number of older and younger siblings. Media access controls include indicators for the presence of newspapers, magazines, and a library card in the home. Parental characteristics are mother's and father's LFP, Duncan SEI score (standardized), and years of education. The first two variables utilize the adult in the household acting as parent at age 14, as opposed to the actual parent. Robust standard errors clustered by family in brackets. *** $p<0.01$.

Table 1.6: Within-Family Estimates of the Effect of Age at Marriage on Divorce Risk

Dependent Variable: Log Yearly Hazard of Divorce				
	(1)	(2)	(3)	(4)
Age at Marriage	-0.0752***	-0.0726***	-0.0810***	-0.0906***
	[0.0197]	[0.0120]	[0.0231]	[0.0255]
Race, Education, Location	X	X	X	X
Children Before Marriage	X	X	X	X
Religion, Religiosity		X	X	X
Family Structure		X	X	
Media Access Controls		X	X	
Parental Characteristics			X	
Family Effects				X
Complete Parent Data			X	
Observations	894	894	627	894
Families	422	422	297	422

Notes and sources: First marriages of women in the NLSY. See Appendix A.1.2 for details. Coefficients measure changes in log hazard rates. Observations censored at time of interview or time of death of spouse. Race controls include indicators for being black, white, Hispanic, or of another race; education controls for education level at marriage in four groups; location controls are for urban status and census region at marriage. Religion includes indicators for attending services once a month or more and once a week or more, and indicators for being Catholic, Protestant, or another religion. Children before marriage is an indicator for a woman having a child prior to her first marriage. Family structure includes indicators for the presence of the biological mother, biological father, both biological parents, and any father figure, as well as the number of older and younger siblings. Media access controls include indicators for the presence of newspapers, magazines, and a library card in the home. Parental characteristics are mother's and father's LFP, Duncan SEI score (standardized), and years of education. The first two variables utilize the adult in the household acting as parent at age 14, as opposed to the actual parent. Average sibling age range is 29 months. Robust standard errors clustered by family in brackets. *** $p<0.01$.

provide point estimates of the causal impact of age at marriage. To do this, I use an IV procedure that exploits variation in marriage age from laws limiting the earliest age that a woman can marry, with and without parental consent.[39,40] Because the regressions use instruments defined at the birth cohort level, the analysis is conducted on a sample consisting of ever-married women in the SIPP born from 1920 to 1974, regardless of year of marriage.

In total, 39 (22) states changed the minimum age at marriage with (without) parental permission, on average 2.00 (1.95) times. These changes then identify systems of equations such as

$$Y_{it} = \beta_t X_i + \theta_{its} + \delta_{itc} + \alpha_t (Age_i < 18) + \varepsilon_{ist} \quad (1.4)$$

$$(Age_i < 18) = \widetilde{\beta} X_i + \widetilde{\theta}_{its} + \widetilde{\delta}_{itc} + \varphi A_{isc} + e_{ist} \quad (1.5)$$

where Y_{it} is a variable indicating if a couple divorces within t years of marriage, X and θ are defined as before, δ is a vector of birth cohort fixed-effects, $(Age_i < 18)$ is an indicator for a girl marrying prior to her 18th birthday, and A_{isc} is a vector of indicators for the legal status of marriage (with and without parental consent) for girls of different ages.

Minimum age at marriage laws force many teenagers to wait to marry even if they have found a desirable spouse. The laws, however, are not binding for all teens seeking to wed. Many young women go across state lines or misrepresent their age to obtain an illegal marriage license.[41] Moreover, an analysis of legal records indicates that an underage girl could sometimes receive judicial permission to marry if she could present good reason (e.g.,

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[39]Each girl is matched to a vector of indicators for the laws prevailing in her birth state when she is age 16.

[40]Dahl (2010) previously used this instrument to determine the relationship between early marriage and welfare receipt. See Appendix A.1.5 for details on these laws.

[41]See Blank, Charles, and Sallee (2009).

her pregnancy) to the court. My IV estimates of α are therefore LATEs specific to teens who do not circumvent these laws but would otherwise choose to marry.[42] Note, however, that changes in the proportion of brides under age 18 can explain about one-fifth of the fall in divorce from 1980 to 2003 (see Table 1.1). Understanding the effects of marriage on young teens who can be persuaded to wait to marry is therefore important for explaining divorce trends.

By discouraging early marriage, these laws could also discourage women from ever getting married. The pool of married women, and thus divorce rates, could change. However, I find no evidence that more restrictive age at marriage laws during a woman's teenage years decreased the probability that she appears in my sample of marriages. Further, more restrictive minimum age at marriage laws are not associated with higher rates of likely cohabitation in the CPS (both overall and for women under 25), suggesting these laws do not encourage non-marital unions.[43,44] Changes in the laws are also not preceded by trends in teenage marriage, young divorce, or single motherhood.[45]

The legal variables are relevant instruments, as demonstrated in Table 1.7 by the the first-stage of the IV procedure. Logically, states with more permissive laws have higher rates of early teenage marriage. Together, the variables have a joint F-statistic near 12 and a probit specification demonstrates that the strength of the instruments does not rely on the specific functional form chosen. Thus, weakness of these instruments is likely not a problem.[46]

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[42]This LATE could also reflect the effect of a culture that encourages such early marriages, or a combination of such culture and the act of marrying at a young age.

[43]See Appendix A.1.4 for details on the measure of likely cohabitation.

[44]One might also worry that limits on teenage marriage decreased age at marriage for those with ages just above a cutoff, violating the monotonicity assumption of instrumental variables. However, one can reject the hypothesis that raising the minimum age at marriage leads women above the new minimum to wed earlier.

[45]See Appendix A.1.5 and Figure A.2.

[46]Estimates using limited information maximum likelihood, which Stock and Yogo (2002) show to be

Table 1.7: First-Stage Estimates Using Minimum Age at Marriage Laws as Instruments

Dependent Variable=1 if Age at Marriage<18

Panel A: OLS

Minimum Age of Marriage with Parent's Permission			Minimum Age of Marriage without Parent's Permission		
Age	Coefficient	SE	Age	Coefficient	SE
12-13	0.0202***	[0.00580]	15	0.0574***	[0.01033]
14	0.0103	[0.00828]	16	-0.00618	[0.0193]
15	0.0274***	[0.00943]	18	0.0116***	[0.00454]
16	0.0244***	[0.00496]	19	0.0168***	[0.00714]
17	-0.00246	[0.00820]	20	-0.0189***	[0.00482]
		Observations		60,914	
		F-Statistic		11.71	
		Dependent Variable Mean		0.094	

Panel B: Probit (Marginal Effects)

Minimum Age of Marriage with Parent's Permission			Minimum Age of Marriage without Parent's Permission		
Age	Coefficient	SE	Age	Coefficient	SE
12-13	0.00805**	[0.00398]	15	-0.00558	[0.00436]
14	0.00584	[0.00407]	16	0.00551	[0.00874]
15	0.0118**	[0.00598]	18	0.00473***	[0.00174]
16	0.00810***	[0.00219]	19	0.0119***	[0.00317]
17	-0.00203	[0.00251]	20	-0.0168	[0.00777]
		Observations		60,914	
		χ^2-Statistic		107.39	
		Dependent Variable Mean		0.094	

Notes and sources: Ever married women in the 2001, 2004, and 2008 SIPP, born from 1920 to 1974. See Appendix A.1.1 for details. Regressions also include controls for year and state of birth fixed-effects, education at marriage (four groups), having children prior to marriage, race (black, Hispanic, white, and other), census division, and urban location (both at interview). Omitted categories are 18 for age with permission and 21 for the age permission (no state in this period has 17 as the minimum age of marriage without parental consent). Laws matched to year woman is 16. See Appendix A.1.5 for details. Robust standard errors clustered by state of birth in brackets. *** $p<0.01$, **$p<0.05$.

I calculate the marginal effect of early teenage marriage from eq. (1.4) using a bivariate probit model. The coefficients from the IV regressions are generally larger than the standard estimates, as depicted in Figure 1.5. But the two coefficient vectors are statistically indistinguishable. Non-IV probit regressions imply that marriage before age 18 is associated with a 12 percentage point or 50 percent increase in the probability of divorce before one's tenth anniversary (a 10 percentage point or 25 percent increase in the probability of divorce before the 20th anniversary). At the tenth anniversary, the IV and non-IV estimates are nearly identical; at the 20th anniversary, the IV estimates are about 50 percent larger than those produced by a standard probit model. The IV estimates significantly differ from zero at most anniversaries, despite their large standard errors. Additionally, these results are robust to analyzing only more recent cohorts of women, using only laws for age at marriage with parental permission as instruments, using minimum age at marriage laws for both men and women as instruments, or considering the effect of laws on subsets of women who marry before they are 20, 22, or 25 years old.[47]

The LATEs estimated using minimum age at marriage laws as instruments are relatively well-defined but somewhat limited due to an inability to extrapolate the results to non-compliers. The lack of generalizability makes it difficult to determine to what precise extent this analysis alone suggests that increases in age at marriage can explain the fall in divorce. It does, however, suggest that age at marriage can be a very important predictor of divorce, affects some group of women, and has the potential to explain at least a portion of the fall in the divorce rate for couples marrying between 1980 and 2004.

Moreover, similarity between the plausibly causal estimates produced using IV and

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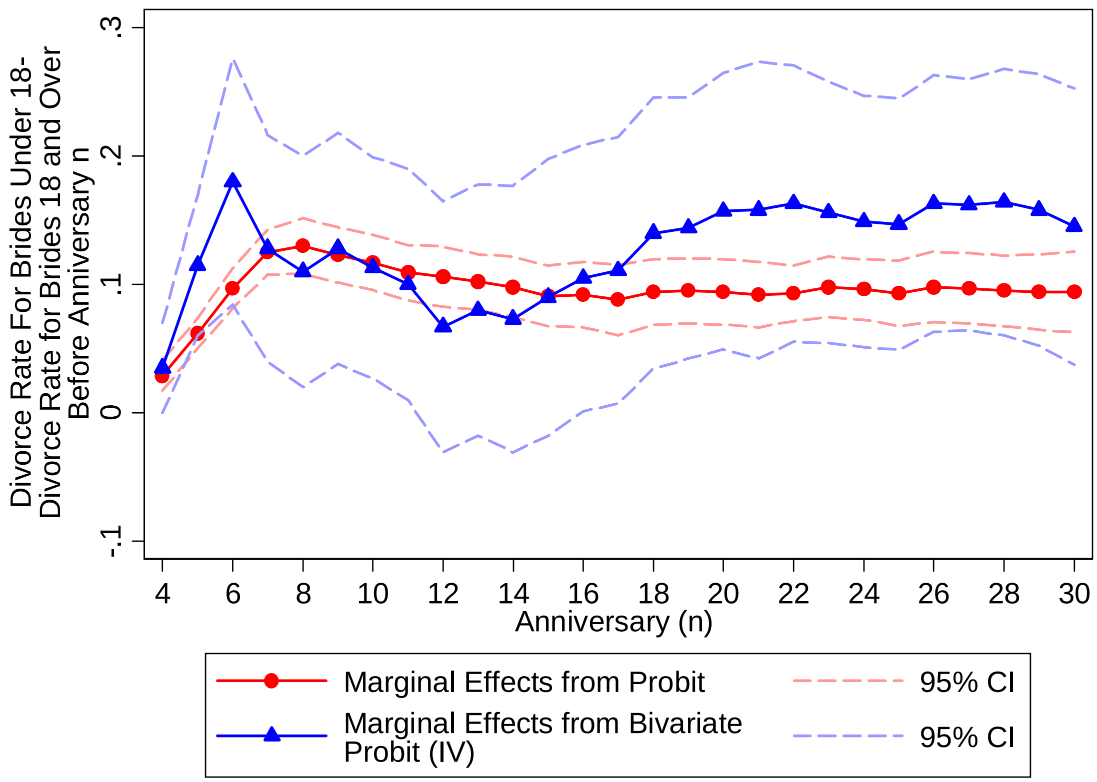
more robust to weak instruments, confirm my results.

[47]However, the estimates are somewhat sensitive to the specific form of the regressions used to calculate the effects in (1.5). One could instead model both stages of the regression as linear or use a linear first-stage and a hazard function for the second stage. Many combinations may be of interest. The specification shown has the most conservative point estimates of all attempted combinations, suggesting the true effect of age at marriage on divorce may be larger. All other specifications yielded less precise point estimates, though effects were often statistically different from zero.

Figure 1.5: IV Estimates of the Effect of Marriage Before Age 18 on Divorce



Notes and sources: Women's first marriages from the 2001, 2004, and 2008 SIPP (born 1920-1974, N=60,914). See Appendix A.1.1 for details. Observations included in estimating the effect of teenage marriage on the probability of divorce before the nth anniversary if the possible length of marriage (the difference between the date of spousal death or date of interview and date of marriage) is n or more years. Regressions also include year of marriage and state of birth fixed-effects and controls for having children prior to marriage, race (black, Hispanic, white, and other), census division and urban location (both at interview), and education (four categories). Robust standard errors clustered by state of birth used to create 95 percent confidence intervals. First stage using minimum age at marriage laws to predict age at marriage less than 18 in Table 1.7.

the estimates calculated using state-year fixed-effects (see Table 1.4A) suggests that estimates using the latter method are not highly biased measures of the effect of age at marriage on divorce.[48] Together, my three analyses indicate that an increase in age at marriage is the main proximate cause for the decrease in divorce rates from 1980 to 2004. Comparing the uncorrected and corrected coefficients on age at marriage further suggests that increases in age at marriage account for at least 60 percent, and potentially more, of the decline in divorce.

1.4 A Model of Marriage and Divorce

Past work on the family suggests that various factors relating to decreases in the gains to marriage (e.g., increased access to birth control or a women's growing role in the labor market) led to increases in age at marriage.[49] The empirical evidence in Section 1.3 further indicates that these increases in age drove down divorce rates. Within the literature, my results therefore imply that decreases in the gains to marriage lead to lower rates of divorce, via increases in age at marriage. However, this hypothesis is potentially at odds with trends earlier in the century. Most of the variables associated with the gains to marriage evolved monotonically from 1960 to 2004. But the earlier part of this period is characterized by increasing divorce rates. Similar changes in the gains to marriage thus appear to imply very different changes in the divorce rate before and after 1980.

To better understand how decreases in the relative value of marriage could lead to an inverted U-shaped trend in divorce, I consider a simple, one-sided search model. Assume

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[48]My IV estimates capture LATEs for a specific group of women. The women who are affected by minimum age at marriage laws likely marry earlier than average. Additionally, the first stage of the IV procedure is stronger when one considers less-educated women. Estimating eq. (1.4) using the subset of women who marry before age 22 and do not have a college degree yields coefficients on age at marriage smaller than those reported in Table 1.4A and closer to the IV estimates reported in Figure 1.5, further supporting the limited bias of the estimates produced in Section 1.3.1.

[49]For example, see Becker (1973, 1974, 1991) on female labor force participation and Goldin and Katz (2002) on the pill.

that women search for potential husbands, distinguished only by their wage rates. Search is costly, and once married, the cost of looking for a (different) husband increases. Women benefit from being married because of household income sharing and economies of scale. The workhorse search model is augmented with an initial period of endogenous educational attainment. The framework abstracts from the search behavior of men and several other complications but allows for a rich relationship between age at marriage and marital stability.[50]

In a world with gains from marriage but without any frictions, a woman will always wed the best man in the population willing to marry her. Requiring women to search for a husband adds frictions to the marriage market in a straightforward and tractable manner. These frictions then lead to marriage delay and, occasionally, divorce.[51]

The key distinguishing prediction of this particular search model is that changes in the gains to marriage asymmetrically impact current and future marital stability. A decrease in the relative value of marriage leads to a higher divorce rate among women married at the time of a change. But the same change induces single women to be pickier about whom they marry and to wait longer to marry. These effects imply that a decrease in the value of marriage first leads to higher divorce rates but eventually causes marriages to become more stable. The asymmetry allows this relatively simple model to predict that monotone changes in the gains to marriage can produce an inverted U-shaped trend in divorce rates

[50] For a review of modern search models, see Mortensen and Pissarides (1999). Two-sided models yield predictions similar to those of the one-sided model developed in this section under certain household sharing rules, wherein a change in women's circumstances elicits a larger reaction from women than men.

[51] Indeed, my model captures several important facts that simpler models of search cannot. In particular, models in which divorce occurs due to learning about match quality or shocks to match quality (e.g., Jovanovic 1979) cannot replicate the fact that divorce decreases in probability as age at marriage increases without implying that higher reservation values lead to higher divorce rates conditional on age. Further, more complex models of learning about one's own perfect mate can imply an untenable decrease in the importance of age at marriage over time or require a high degree of non-stationarity or implausibly large discount factors (c.f., Neeman, Newman, and Olivetti 2008). Finally, models that focus on temporal uncertainty about the common value of a partner (e.g., Bergstrom and Bagnoli 1993) speak to the relationship between quality and age at marriage but do not produce interesting dynamics in divorce unless one adds complex frictions or restrictions to the matching function.

over time.

Three features of my model lead to this key finding.[52] First, search for a higher-earning spouse while currently married provides the mechanism for divorce.[53] Second, the model applies a fixed sharing rule to the incomes of husband and wife, making utility nontransferable.[54] These two elements allow negative shocks to the value of marriage to imply contemporaneously higher divorce rates, while increasing the stability of future marriages. Third, I assume that women can only search for a spouse for a finite time. All three assumptions together lead marriages beginning later in life to be more stable, even conditional on the characteristics of both spouses.

1.4.1 Setup

Formally, the model begins as a woman exits high school with a known offer of marriage. Men are distinguished only by their (constant) wage rates drawn from the commonly known distribution $F(w)$. The first potential husband a woman meets after high school receives wage $w_h \geq 0$. A woman can accept this man's proposal, decline and enter the workforce, or decline and go to college. Each period a woman is not in school she can earn z, either in actual income or imputed from home production. Going to college

[52] See Burdett (1978) for the model on which this is based. This model is also similar to that of Neeman, Newman, and Olivetti (2008), who demonstrate that learning combined with nontransferable utility can imply that women who are more attached to the labor force will divorce less often conditional on age at marriage.

[53] Note this assumption implies that in steady state, remarriage directly follows divorce. In the SIPP, many women quickly remarry following divorce (about one-half of women who remarry do so within three years of divorce). However, one cannot distinguish divorces due to search behavior from those due to parameter shifts, making it difficult to address the precise relationship between search-based divorce and remarriage.

[54] See Legros and Newman (2007) for a discussion of the effects of nontransferable utility on union formation and matching.

increases this wage and the value of a woman's first offer of marriage after graduation.[55,56] After her schooling is complete, a woman chooses one of three states each period: single and searching for a spouse, married and searching for a better spouse, or married and not searching. Such decisions are relevant to all women until they reach a certain age T.[57]

Searching for a spouse costs $kc < z$ when single and c when married, where $k < 1$.[58] If search occurs, a woman receives a recallable offer of marriage with probability λ.[59] For simplicity, I assume no savings instrument and risk neutral agents. When single, a woman enjoys flow benefit z and when married to a man with wage $w > 0$, she gets flow benefit $(z + w)/2^{\phi}$, where $\phi \in (0, 1)$ represents the degree of scale economies a family can achieve.

The value of getting an offer of marriage from a man with wage w' at time t is thus $\psi_t(w') = \max\{V_{St}(w'), V_{Lt}(w'), V_{Mt}(w')\}$, where the values of being single (V_{St}), married and looking for a new spouse (V_{Lt}), and married and not searching (V_{Mt}) may be written

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[55] I assume that, with certainty, women meet a mate after college with a wage greater than w_h. One could alternatively assume that female college graduates draw potential mates from a distribution that first order stochastically dominates the distribution of the wages of the potential spouses of non-graduates.

[56] See Chiappori, Iyigun, and Weiss (2009) for a discussion of the returns to education in the marriage market.

[57] One can think of T as a woman's age at death but more nuanced interpretations for T as some date of exit from the marriage market are also possible. For example, if men only propose to fertile women, the relevant time frame for marriage could be far shorter than the lifetime.

[58] Women in college also pay kc to search but receive an offer of marriage with certainty.

[59] All offers of marriage are recallable, in that once a woman has found a man with a given wage rate willing to marry her, she can do so again costlessly. This allows for easier exposition, as it makes the expected gross gains from search while single and while married the same. If offers were not recallable, the relative gains to marriage would increase, as marriage with search would essentially allow a woman to hold a given offer while still looking for a better mate. Under certain conditions about the relative importance of this motivation to marry, the model without recall will produce similar comparative statics.

respectively as

$$
\begin{aligned}
V_{St}(w') &= z - kc + \beta\lambda(1 - F(w'))(E[\psi_{t+1}(w)|w > w'] - \psi_{t+1}(w')) \qquad (1.6)\\
&\quad +\beta\psi_{t+1}(w') \\
V_{Lt}(w') &= \frac{z + w'}{2^{\phi}} - c + \beta\lambda(1 - F(w'))(E[\psi_{t+1}(w)|w > w'] - \psi_{t+1}(w')) \qquad (1.7)\\
&\quad +\beta\psi_{t+1}(w') \\
V_{Mt}(w') &= \frac{z + w'}{2^{\phi}} + \beta\psi_{t+1}(w'). \qquad (1.8)
\end{aligned}
$$

Each value function combines current utility flows, the net expected value of any search, and the discounted (using factor β) future value of optimal decision making.[60]

Women with higher wages will prefer to remain single longer because they must give their spouse part of their earnings when they marry. The effect is tempered by higher economies of scale within marriage, as a woman will effectively lose less of her income to her spouse when this multiplier increases. A woman will choose to search for a different mate when she has higher expected net returns to doing so, because of low costs, a high arrival rate, low discount rate, or high expected benefit to finding a better spouse. Further, as a potential mate's wage increases, the value of being married and not searching increases at a faster rate than the value of bring married and continuing to search, which in turn rises quicker than the value of being single.

[60] Many of the model's assumptions can be relaxed to some degree. For example, one can add a match-specific component to marriage, so long as the common value of a husband (his wage) remains an important determinant of divorce. With this addition, factors other than rent-seeking can lead to divorce and some divorces will be welfare-improving for both ex-spouses. Women who do not search for a mate can receive unsolicited offers from potential spouses with some (small) probability. Further, children and other factors giving women a preference for marriage or stability may be incorporated. One can also add a degree of flexibility to the household income-sharing rule. I omit these extensions for easier exposition and to focus on the ability of this simple model to replicate the trend in divorce from 1950 to 2004 using only unidirectional shocks.

1.4.2 Marriage and Divorce in a Search Framework

Similar analysis then implies the following convenient property.

Proposition 1 *Each period, a woman's decision process exhibits the reservation wage property. That is, for any pair of options j and k (among remain single, marry but continue to look for a better mate, and marry and stop searching) at time t, there exists some unique w_{jkt} where a woman is indifferent between options j and k for $w' = w_{jkt}$, strictly prefers one option when $w' > w_{jkt}$, and strictly prefers the other alternative when $w' < w_{jkt}$.*[61]

Therefore, a woman's pairwise preferences can be determined by comparing her best offer of marriage to w_{LSt}, w_{MSt}, and w_{LMt}, the points of indifference between marriage with search and singlehood, marriage without search and singlehood, and the two different marriage options. Being married and searching is preferred to being single if and only if

$$w' \geq w_{LSt} = z(2^{\phi} - 1)/2^{\phi} + c(1 - k). \tag{1.9}$$

A woman is thus more likely to prefer marriage with search to singlehood if she has lower wages, if the costs of search while married and while single are similar, or if economies of scale are large.

Likewise, a woman will prefer marriage without search to singlehood if and only if

$$w' \geq w_{MSt} = z(2^{\phi} - 1)/2^{\phi} - kc + \beta\lambda \int_{w_{MSt}}^{\infty} (\psi_{t+1}(w) - \psi_{t+1}(w_{MSt}))f(w)dw. \tag{1.10}$$

Thus, a woman will be more likely to want to marry and stop searching, rather than stay single, as her wage falls, the cost of search increases, or the expected gain from finding a man with a higher wage decreases.

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[61] The proof for this and all other propositions can be found in the Appendix A.2.

Similarly, a married woman will prefer not to search for a better mate if and only if $w' \geq w_{LMt}$, implicitly defined by

$$c = \beta\lambda \int_{w_{LMt}}^{\infty} (\psi_{t+1}(w) - \psi_{t+1}(w_{LMt}))f(w)dw. \tag{1.11}$$

Put simply, a married woman (whose husband earns w') will search if the costs of search (c) are low or the expected gains from search ($\beta\lambda \int_{w'}^{\infty}(\psi_{t+1}(w) - \psi_{t+1}(w'))f(w)dw$) are high.

A woman's choice of marital status then rests on the ordering of these cutoff values, restricted by the following proposition in a convenient manner.

Proposition 2 $w_{LSt} > w_{MSt}$ *if and only if* $w_{MSt} > w_{LMt}$.

The proposition allows one to rule out certain counterintuitive preferences (e.g., a woman cannot choose marriage without search for some value of w but then prefer marriage with search for a higher value of w). Proposition 2 also indicates that the only possible orderings of $\{w_{MSt}, w_{LSt}, w_{LMt}\}$ are (i) $w_{LMt} > w_{MSt} > w_{LSt}$ and (ii) $w_{LSt} > w_{MSt} > w_{LMt}$. If ordering (i) occurs, a woman will stay single if $w' < w_{LSt}$, marry but continue to search if $w_{LSt} \leq w' < w_{LMt}$, and marry and stop searching if $w' \geq w_{LMt}$. Women with ordering (i) can be called "divorce-prone," as certain values of w' will induce them to search for a new spouse while married. If ordering (ii) holds, a woman optimally stays single if $w' < w_{MSt}$ or otherwise marries and does not search. Thus, women with ordering (ii) will not divorce in steady state. Women with higher wages, lower scale economies in marriage, lower relative costs of search when single, and higher costs of search during marriage (holding kc constant) are less likely to be categorized as divorce-prone.

In a fully stationary model, a woman's reservation values would not change over time and she would either be divorce-prone or not throughout her entire life. But since this model involves a marriage market of finite length, the triplet $\{w_{MSt}, w_{LSt}, w_{LMt}\}$ varies over time and women may switch classifications. In particular,

Proposition 3 *If $w_{LMt} > w_{MSt} > w_{LSt}$, then w_{LM} decreases with age. Otherwise, w_{MS} decreases. w_{LS} is constant across all ages in both cases.*

To see the implications of this statement, first consider a woman not prone to divorce at age t. Propositions 2 and 3 then imply that she will not choose to seek divorce in any subsequent period. That is, a woman who is not divorce-prone will not become divorce-prone.

The propositions are more interesting when applied to the initially divorce-prone woman. At age t, she accepts all offers of marriage from men with wages above w_{LSt} but continues to look for a new spouse when w is relatively close to this threshold. If she is still divorce-prone at age $t + 1$, she will still accept the same set of marriage proposals but will now search over a more limited range of w. In essence, there is some group of marriages that involve search when a woman is $t,$ but not at $t + 1,$ years of age. The restriction in search to a smaller measure of values then implies lower divorce rates. Alternatively, the woman might switch her ordering of cutoff values so that she in no longer divorce-prone at age $t + 1$, also decreasing the expected probability of divorce as her age at marriage increases.

Lemma 1 *Increases in age at marriage decrease divorce rates, conditional on a wife's education and a husband's wage rate.*

To close the model, note that more women seek education when the return in either the labor market or the marriage market increases. Additionally,

Proposition 4 *Women who go to college marry at later ages (for sufficiently large λ). Women who go to college are also less likely to divorce, both overall and holding spousal earnings and age at marriage constant.*

Increases in education cause women to marry later for two reasons. First, in my model (and in most data) women complete their education before marrying. Thus, college mechanically increases age at marriage. Second, an increase in education increases a woman's wages, making her more selective about whom she marries and leading her to search longer for a suitable husband. Furthermore, higher wages will cause college women to search less within marriage (see Proposition 5 for details). Thus, women with more education divorce less often, even conditional on a husband's wage and a wife's age at marriage.

1.4.3 Comparative Statics

Like the results for age at marriage and education, the key comparative statics of the model rely on three components of the framework: search during marriage, nontransferable utility, and a time limit on search. These assumptions together imply that shocks to many variables can lead to both higher current rates of divorce and lower future rates of divorce. For example, a decrease in the costs of search while single due to legalized abortion will lead to higher rates of divorce among those married when the law changes. But the women who marry after the reform will have lower divorce rates because they have higher standards for a spouse, marry later, and obtain more education. Given this, shocks to reproductive rights and other variables from the 1960s through the 1990s can imply both an increase in age at first marriage and an inverted U-shaped pattern in divorce.[62] Formally,

Proposition 5 *Consider (i) an increase in a woman's wages* (z), *(ii) a decrease in her cost*

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[62]In essence, this result captures the fact that marriages formed under one regime will not necessarily persist under alternatives. The matches made in the 1950s, 1960s, and 1970s were optimal given the conditions then; however, as society progressed, such marriages dissolved, raising the divorce rate. In my model, the divorce rate decreases once the pace of change slows and a new steady-state is reached. Additionally, women in the new regime wait longer to marry, further increasing the stability of their future marriages. Therefore, the key intuition of this model is very similar in spirit to less formal conceptualizations (c.f., Isen and Stevenson 2010, Stevenson and Wolfers 2007) that changes in one's optimal spouse can lead to both a surge and subsequent fall in divorce.

of search while single (k), (iii) an increase in her return to education, or (iv) a decrease in economies of scale (increase in ϕ). All of these changes lead to higher contemporaneous divorce rates but lower divorce rates for future marriages (both conditional and unconditional on age at marriage and education). (i)-(iv) also lead unmarried women to obtain more education and marry later.

To explore this result, consider an increase in women's wages (z) holding the distribution of male wages ($F(w)$) constant. As the gender gap in wages falls, the relative gains to being single increase, and a man must earn a higher wage for a woman to choose to marry him. This leads to higher contemporaneous divorce rates, as some women no longer find their husbands' wages adequate. Divorce-prone women who are not yet married will increase the minimum wage that they require from a potential mate but not the range of w over which they choose marriage without search. The probability that these women search given they marry, and thus their probability of eventual divorce, then declines. Furthermore, the change in cutoff values can lead a previously divorce-prone woman to become non-divorce-prone. This will also increase the eventual marital stability of those unmarried when z increased. Essentially, women married to the most marginal group of husbands will choose to become single after a decrease in the gender gap in wages, raising the divorce rate. But women who are single at the time of the change will never marry men from this marginal group, lowering their eventual rates of divorce.

Additionally, increases in a husband's minimum wage will lead women to search longer for a sufficiently high-earning spouse, increasing age at marriage. This increase then further decreases the likelihood that these women's eventual marriages will end in divorce. Finally, the increase in women's wages raises the absolute returns to attending college and increases enrollment. Proposition 4 then implies further increases in both age at marriage and marital stability. Therefore, as the gender gap in wages closes, some current marriages will end but the stability of new marriages will increase both conditional and unconditional on women's education and age at marriage.

Figure 1.6 shows a graphical representation of the effect of the increase in z (given $F(w)$) on divorce-prone women who remain divorce-prone after the change. Before the gender gap narrowed, married, divorce-prone women (whose choice sets are depicted in Panel A) could be married to any man with wages greater than w_{LS}. When these women's wages increase, w_{LS} increases to w'_{LS}. The women married to men with wages between w_{LS} and w'_{LS} were previously content to stay married, albeit while continuing to search for a better spouse. But after the shock to z, these women no longer find their husbands adequate and leave their marriages. Thus, a decrease in the gender gap in wages will increase divorce rates.

The changes in the reservation values associated with an increase in z are the same for single and married women; however, their interpretation is different. Before the shock, if a single, divorce-prone woman (whose choice sets are depicted in Figure 1.6B) met a man with wages between w_{LS} and w'_{LS}, she would have married him. Such marriages would have been likely to end in divorce, as women married to these men continue searching for new mates. After the change in the gender gap in wages, these unions never form. As the change in z does not influence the relative value of marriage with and without search (w_{LM} does not change$)$, search decreases within these women's eventual marriages. Moreover, as the group of men a woman is willing to marry shrinks, it takes her longer to find a suitable mate, increasing her age at marriage. Thus, the divorce rate will decrease for these women both directly because of the narrowing of the gender gap and because the change in wages leads these women to marry later, which in turn strengthens their eventual marriages.

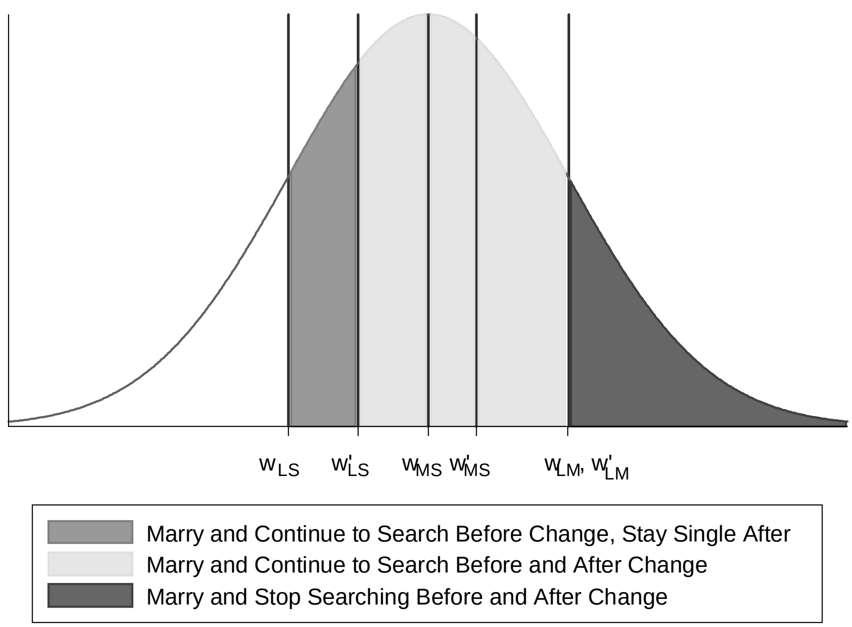
Though my empirical work focuses on the importance of this latter, indirect effect, the model allows either effect to be the dominant force behind the decline in divorce. Altogether, the model demonstrates that decreases in the gains to marriage will temporarily push the divorce rate up. Women who are single at the time of a shock will then marry later

Figure 1.6: Change in Divorce and Search Following an Increase in Female Wages

Holding the Distribution of Male Wages Constant

Panel A: Married, Divorce-Prone Woman

Panel B: Single, Divorce-Prone Woman



Notes: Case where divorce-prone woman does not become non-divorce-prone. Change in female wages (z) holding distribution of male wages ($(F(w))$) constant.

and become less likely to search during marriage, eventually bringing the divorce rate back down.

Many of the variables related to the gains to marriage (and listed in Proposition 5) changed in the 1970s and 1980s. For example, female real earning power (z) grew rapidly. Expanding access to birth control and abortion likely decreased the relative costs of marital search while single. Increases in female labor force participation and decreases in household specialization could have reduced household economies of scale (increased ϕ). Moreover, a woman's increasing role in the market implies greater returns to her education. Within the model, changes in all of these factors would imply higher rates of divorce for couples married before a change and lower rates of divorce for couples marrying afterward.

1.4.4 Simulation

The model's comparative statics show that after a drop in the value of marriage, divorce rates will increase for a time and then decrease. But the precise trend in divorce rates could have many different shapes. I thus calibrate and simulate the model to determine potential paths for the divorce rate in response to shocks to the gains to marriage.[63]

For the simulation, I ignore the initial period of educational attainment and assume all women are identical. Women can search for a husband for $T = 10$ periods, each of which may be thought of as two years.[64] I use a one-year discount rate of 0.90, reflecting the young age of women as they enter the marriage market. Potential husbands have wages drawn from a log-normal distribution with mean and variance matching the wage distribution for married men ages 18 to 35 in 1980, roughly the middle of my sample.[65] A

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[63] An analytical description for the path of divorce would depend on the model's parameters, their distributions in the population, and the entire series of innovations to the gains to marriage. Thus, the high dimensionality of the problem limits one's ability to describe the analytic path of the divorce rate in the general case.

[64] Assuming women enter the marriage market at 18, they are then age $2t + 18$ at the end of period t.

[65] See Appendix A.1.4 for details.

woman's first offer of marriage (received with certainty in period 1) is drawn from a similar distribution with mean and variance corresponding to the moments of the wage distribution for married men age 18 to 20 in 1980. I set z equal to 60 percent of the mean male wage rate in the first period, reflecting the gender gap that predominated in the 1950s, 1960s, and 1970s. Estimates from Browning, Chiappori, and Lewbel (2010) imply $2^{\phi} = 1.10$ for my initial cohort.[66]

Statistics from the Marital Instability over the Life Course Panel Study (MILC) lead me to choose $\lambda = \frac{2}{3}$, implying that marriage proposals arrive on average once every three years.[67] Little guidance is available on the costs of search while married but c must combine potential psychic costs due to guilt, as well as the expected cost of divorce and the actual cost of search. I therefore set c equal to the mean male wage, suggesting that if a woman married to an average man searches for a new husband in a given period, she forfeits that period's maximum gain from marriage.[68] Finally, I select the initial relative cost of search while single (k) by finding the value of k implying that the average woman in my initial cohort marries at age 21.7 (after 1.85 periods), the mean age at marriage in 1950.[69] The calibration yields $k = 0.17$.

I then consider how age at marriage and divorce evolve in response to the following shocks: (i) economies of scale decreasing from $2^{\phi} = 1.10$ at $t = 15$ to $2^{\phi} = 2$ at $t = 30$,

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[66] Browning, Chiappori, and Lewbel (2010) suggest that $2^{\phi} = 1.31$; however, the authors use data corresponding to a period of relatively high female labor force participation (1974-1992). I calculate the full-time, full-year labor force participation of young, married women during this period and associate with it a value of $2^{\phi} = 1.31$. I then assume that $2^{\phi} = 2$ if a woman works outside the home full-time throughout the year. Further assuming that the relationship between 2^{ϕ} and female labor force participation is linear, I then calculate $2^{\phi} = 1.10$ for my initial cohort, based on participation rates in 1950 (See Appendix A.1.4).

[67] The MILC suggests that, on average, men and women date for two years prior to marrying. I use $\lambda < 1$, as setting $\lambda = 1$ would suggest all men believe all women are acceptable wives.

[68] This cost must be relatively high to yield reasonable divorce rates. See Appendix A.3 for details.

[69] The 1960 IPUMS Census reports a mean age at marriage for women of 21.9, while the SIPP reports a lower age. The Census sample may be more reliable (due to fewer issues of selective mortality) but I intend to match estimates in the SIPP. I thus use an intermediate value.

corresponding to women entering the workforce at rates similar to prime-age men, (ii) the ratio of female to male wages increasing from 60 percent in the first 25 periods to 90 percent by period 30, due to z growing while $F(w)$ does not change (in real terms), and (iii) the costs of search while single decreasing to zero between $t = 20$ and $t = 22$, representing a large and rapid shock due to changes in sexual mores and access to legal abortion and the pill.[70] The timing of these changes matches the general sequence of events over the 20th century. Women began to increase their presence in the labor market relatively early on. As this presence grew, social norms changed and women gained greater control over their fertility in the late 1960s and early 1970s. Finally, after being stable for several decades, the gender gap in wages began to narrow during the 1980s.

I simulate the model and its response to each of the above shocks using cohorts born from $t = 0$ to $t = 34$.[71] Each of the changes individually leads age at marriage to increase and divorce rates to first rise and subsequently fall, as shown in Figure 1.7.

Combining the shocks leads the trends and levels in age at marriage to be close to those seen in Figure 1.1, using $age = 2t + 18$. The simulated divorce rate increases for cohorts beginning search between $t = 9$ and $t = 14$, stays roughly constant for cohorts entering the marriage market from $t = 15$ to $t = 22$, and then falls before leveling out for the 29th and later cohorts. This matches the basic pattern observed in the data and depicted in Figure 1.2.[72]

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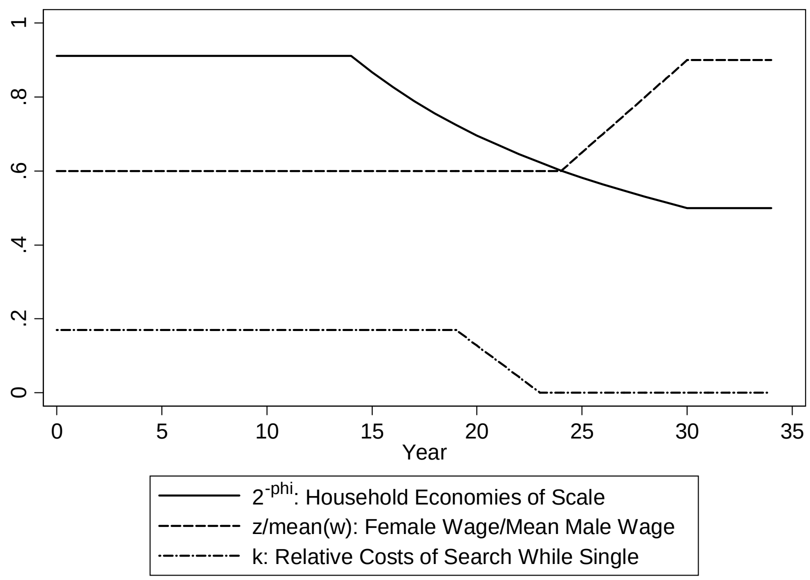
[70]Note that I assume that changes in female labor force participation, the gender gap in wages, and reproductive access occurred across time and do not allow for differential changes in these variables across cohorts. Assuming that changes in the value of marriage are somewhat larger across cohorts (relative to within cohorts) will lead to a smaller initial increase in divorce rates. However, as long as some non-trivial portion of the change occurs within cohorts, the model will produce the same general path for the divorce rate. Work by O'Neill and Polachek (1993) and Weinberger and Kuhn (2010) demonstrates that changes in the gender gap in wages occurred both within and across cohorts throughout the 1980s and 1990s. Goldin (2006) discusses within and across cohort trends in female labor force participation.

[71]See Appendix A.3 for sensitivity analysis and additional simulations.

[72]The model also suggests that the divorce rate should fall below initial levels in later periods. Although current levels of divorce still exceed rates during the 1950s, the simulation does not account for decreases in the costs of divorce over time. If added, this implies a less drastic downturn.

Figure 1.7: Simulation of Model: Implications of a Decrease in the Gains to Marriage

Panel A: Changes in Parameter Values



Panel B: Resulting Trends in Divorce Rate and Age at First Marriage

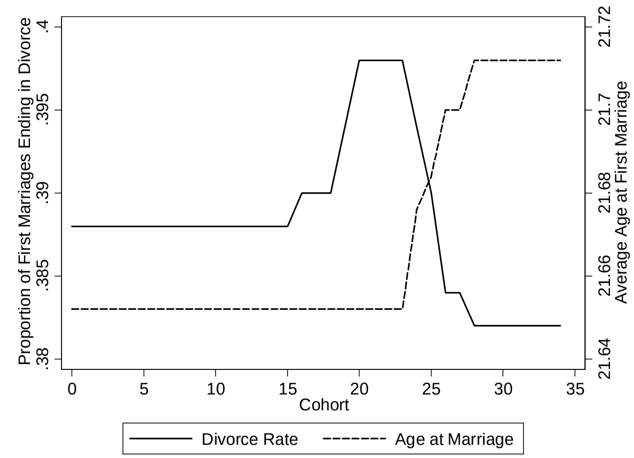
Continued on following page.

Figure 1.7, Continued: Simulation of Search Model: Implications of a Decrease in the Gains to Marriage

Panel C: Resulting Trends in Divorce Rate and Age at First Marriage, Changes in Individual Factors

C.1: Relative Costs of Search While Single

C.2: Female Wage/Mean Male Wage



C.3: Household Economies of Scale

Notes: See Section 1.4 for details on the model and its parameterization. Appendix A.3 contains a sensitivity analysis for this simulation.

Altogether, the hypothesis that decreases in the gains to marriage led to increases in age at marriage, which in turn decreased the divorce rate from 1980 to 2004, can be reconciled with decreases in both the gains to marriage and marital stability before 1980. My simulation demonstrates that decreases in the relative value of marriage, and the resultant increases in age at marriage, could have caused both the observed rise and fall in divorce rates.

1.5 The Implications of Later Marriage

The previous theoretical and empirical work suggests that increases in age at marriage caused large changes in the probability of divorce. Because the increase in brides' ages had such a powerful effect on marital stability, the change in age at marriage must have also impacted the living arrangements of adults and children and the characteristics of intact marriages.

1.5.1 Living Circumstances of Children and Adults

The broader effects of bride's age on marital status can be seen by considering the relationship between age at marriage and the number of years a woman spends married, shown in Table 1.8. Conditional on year of birth, women who marry one year later spend about 9 fewer months married and 3 fewer months previously married. One can further decompose this time into 24 fewer weeks spent in a first marriage, 16 fewer weeks spent remarried, 9 fewer weeks spent divorced, and 2 fewer weeks spent widowed.[73] Thus, although waiting to marry leads to lower divorce rates, remarriage narrows the difference in the time younger and older brides spend with any spouse after they first marry.

Increases in age at marriage are also associated with differences in fertility patterns, as shown by various indicators of total fertility in cols. (2)-(4) of Table 1.8. Women who

[73] Does not add to 52 due to rounding and a slight difference in mortality.

Table 1.8: Woman's Age at Marriage, Total Time Married, and Fertility

	(1)	(2)	(3)	(4)
Dependent Variable	Years Married	Any Children	Number of Children	Age at First Birth
Type of Regression	OLS	Probit (ME)	Poisson	OLS
Sample	Ever Married Women	Ever Married Women Over 40	Ever Married Women Over 40	Ever Married Mothers Over 40
Age at First Marriage	-0.771***	-0.00761***	-0.0165***	0.507***
	[0.00364]	[0.000237]	[0.000770]	[0.0100]
Survey Year Fixed Effects	X	X	X	X
Birth Cohort Fixed Effects	X			
Marriage Cohort Fixed Effects		X	X	X
Observations	73,631	53,068	53,068	37,127

Notes and sources: Ever-married women in the 2001, 2004, and 2008 SIPP, first married 1950-2004. See Appendix A.1.1 for details. Women excluded from col. (1) if married four or more times due to incomplete marriage record. Marginal effects reported in col. (2). Women excluded from col. (4) if first child born before 1960 due to missing data. Robust standard errors in brackets. ***$p<0.01$.

marry later are less likely to bear any children and have fewer children overall. But eventual mothers who marry later wait less time to have children once married and are more likely to have had children before marriage. A one-year increase in age at marriage is associated with an increase in age at first birth (conditional on any birth) of about six months.

The changes in age at marriage, divorce, and fertility combine to alter the living conditions of children, depicted in Figure 1.8.[74] The graph shows the change in the probability of a child's mother being married associated with the mother's age at marriage increasing by one year (given she ever marries). As mother's age at marriage increases, children born within married families are less likely to experience their parents' divorce. This reflects the positive relationship between age at marriage and marital stability. However, as a woman's age at marriage increases, so too does the probability that she has children before marriage. Therefore, a one-year increase in a woman's age at marriage is associated with an overall decrease in the probability that her child lives in a married family (shown by the results that do not condition on mother's marital status at birth).[75] As these results condition on a mother ever marrying and marriage rates decreased as age at marriage increased, the estimates understate the aggregate negative association between average age at marriage and the probability that a young child's mother is married.

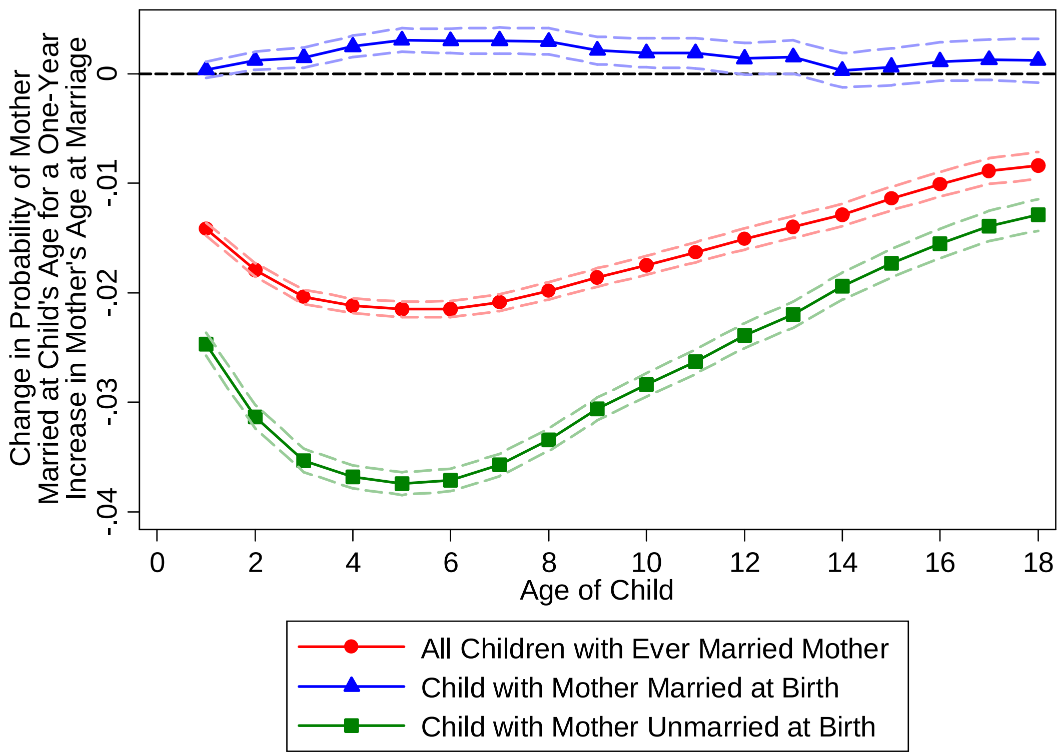
These results suggest potentially important distributional effects of the increase in age at marriage and corresponding decrease in divorce. Together, the trends create a split in society.[76] Instead of having a large number of children living some portion of their early years in married families, children are now more likely to live in the same type of household they were born in, be it married or single. If divorce decreases child well-being,

[74]The SIPP reports the dates of birth for first- and last-born children. I focus on the larger sample of first-born children.

[75]Note that the rise of cohabitation likely increases the probability that a child born outside of marriage lives with both of his or her parents, but such arrangements have been shown to be less stable than marriages (c.f., Gemici and Laufer 2011 or Kennedy and Bumpass 2008).

[76]See McLanahan (2004) or Ellwood and Jencks (2004).

Figure 1.8: Living Circumstances of First-Born Children



Notes and sources: First-born children of women in the 2001, 2004, and 2008 SIPP (N=46,660), born 1960-1998. Children excluded if mother marries four or more times due to incomplete maternal marriage record. Regressions also include child's year of birth fixed-effects and controls for mother's education at marriage (four groups), race (black, Hispanic, white, and other), census division and urban location (both at interview). Observations censored at time of interview or time of death of spouse. Robust standard errors used to calculate 95 percent confidence intervals.

the change may be beneficial because fewer children experience the dissolution of their parents' marriage. But if a father figure (and a father's income) is important for child development, this bifurcation in living circumstances could lead to greater inequality among children and greater eventual inequality among adults.[77]

1.5.2 How Are Early and Late Marriages Different?

To explore how increases in age at marriage lead to differences in marital harmony, I use data from both the National Survey of Families and Households (NSFH) and Marital Instability over the Life Course Panel Study (MILC).[78] The following analysis considers only women still married to their first husbands.

The marriages of those who wed earlier and later in life differ, as shown in Tables 1.9 and 1.10.[79,80] Results from the NSFH indicate that although increases in a woman's age at marriage do not increase her or her husband's reported happiness with marriage, marriages occurring later in life exhibit characteristics usually attributed to better marriages. Couples who marry later are more likely to spend quality time together (though a four year increase in age at marriage is associated with a couple having sex about one time less per month). Furthermore, spouses report arguing less as a woman's age at marriage increases. Men report significantly fewer arguments about household chores, money, in-laws, and kids; women report arguing less about money, spending time together, sex, and in-laws. Men

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[77] Kim (2011) demonstrates that both forces are likely important.

[78] See Appendix A.1.3 for details.

[79] Regressions control for year of marriage indicators in five-year groups and the gender of the respondent in the MILC. Questions in the NSFH are answered by the spouse they pertain to; a designated respondent answered all questions in the MILC.

[80] In the regressions analyzing characteristics of current marriages, one could control for duration of marriage (age at interview); however, the NSFH and MILC conducted all Wave I interviews at roughly the same time. Therefore, duration of marriage (current age) and year of marriage (given age at marriage) are collinear and one cannot control for both in the same regression. Using either alternative set of variables leaves the qualitative results mostly unchanged.

Table 1.9: Woman's Age at Marriage and Characteristics of Marriage from MILC

Dependent Variable	Mean	Coefficient on Wife's Age at Marriage	SE	Obs.
Knew Each Other at Age 12	0.056	-0.00574***	[0.00153]	1610
Months Dated Before Marriage	23.90	0.400**	[0.159]	1607
Woman Works or Wants to Work				
For Financial Reasons	0.796	-0.00870**	[0.00397]	1508
To Have Financial Independence	0.460	-0.00186	[0.00447]	1508
To Have a Career/Accomplishments	0.731	0.0174***	[0.00404]	1508
To Get Away from Home/Family	0.354	-0.00407	[0.00409]	1508
To Be Around People	0.821	0.0106***	[0.00340]	1508

Notes and sources: Intact first marriages (of women) from MILC Wave I, beginning 1950-1979. See Appendix A.1.3 for details. Coefficients from regressions also controlling for year of marriage in five-year groups and if husband or wife answered questions. Work variables are only for women who worked during the marriage or would like to return to work. Robust standard errors in brackets. ***p<0.01, **p<0.05.

Table 1.10: Woman's Age at Marriage and Spouses' Reports of Characteristics of Marriage

	Wife's Responses from the NSFH			
Dependent Variable	Mean	Coeff. on Wife's Age at Marriage	SE	Obs.
Happy with Marriage	0.881	-0.0003	[0.0012]	3465
Spend Time Alone Almost Every Day	0.466	0.00627***	[0.0022]	3396
Times per Month Have Sex	6.746	-0.269***	[0.0256]	2739
Times per Month Argue About				
Household Tasks	1.269	-0.00896	[0.0113]	3256
Money	1.508	-0.0347***	[0.0126]	3264
Spending Time Together	1.509	-0.0415***	[0.0144]	3249
Sex	0.971	-0.0175*	[0.0101]	3188
Having (more) Kids	0.167	-0.0045	[0.0055]	3205
In-Laws	0.591	-0.0200***	[0.0072]	3224
Kids (In General, if Applicable)	1.843	0.0124	[0.0199]	2841
Deal with Disagreements with Spouse by				
Keeping to Self	0.516	0.00115	[0.0022]	3246
Calmly Discussing	0.842	0.00211	[0.0015]	3249
Yelling	0.350	-0.00103	[0.0020]	3245
Throwing Things at or Hitting Him/Her	0.023	-0.00150**	[0.0006]	3242
Injured by Spouse Last Year	0.0724	-0.00269***	[0.0010]	3241
Own Weekly Hours of Housework	36.40	-0.478***	[0.101]	3465
Divorce Wrong Unless Extreme Circumstances	0.757	-0.00586***	[0.0018]	3333
Work in Market at 1st Anniversary	0.637	0.0273***	[0.0026]	1861
Work in Market at 5th Anniversary	0.579	0.0199***	[0.0027]	1586
Work in Market at 10th Anniversary	0.612	0.0132***	[0.0031]	1201

Continued on the next page.

Table 1.10, Continued: Woman's Age at Marriage and Spouses Reports of Characteristics of Marriage

	Husband's Responses from the NSFH			
Dependent Variable	Mean	Coeff. on Wife's Age at Marriage	SE	Obs.
Happy with Marriage	0.918	-0.0004	[0.0011]	3465
Spend Time Alone Almost Every Day	0.434	0.0118***	[0.0021]	2974
Times per Month Have Sex	6.573	-0.243***	[0.0277]	2435
Times per Month Argue About				
Household Tasks	1.243	-0.0248**	[0.0104]	2882
Money	1.456	-0.0537***	[0.0121]	2879
Spending Time Together	1.764	-0.0187	[0.0223]	2866
Sex	1.187	-0.0149	[0.0121]	2805
Having (more) Kids	0.241	-0.00387	[0.0051]	2826
In-Laws	0.585	-0.0156***	[0.0060]	2836
Kids (In General, if Applicable)	1.639	-0.0332*	[0.0181]	2495
Deal with Disagreements with Spouse by				
Keeping to Self	0.575	0.000218	[0.0022]	2871
Calmly Discussing	0.839	0.00394***	[0.0015]	2875
Yelling	0.298	-0.00355*	[0.0020]	2869
Throwing Things at or Hitting Him/Her	0.0178	0.0000771	[0.0009]	2858
Injured by Spouse Last Year	0.024	-0.000948*	[0.0005]	3064
Own Weekly Hours of Housework	13.91	0.0363	[0.072]	3465
Divorce Wrong Unless Extreme Circumstances	0.804	-0.00436**	[0.0018]	2899

Notes and sources: Intact first marriages (of women) from NSFH Wave I, beginning 1950-1984. See Appendix A.1.3 for details. Coefficients from regressions also including controls for year of marriage in five-year groups. Monthly values are calculated by taking less than once per month=.5, once per month=1, several times per month=3, once per week=4, several times per week=10, and almost everyday=20. Disagreement variables are indicators for if this is a way one deals with disagreements "sometimes" or more often. Hours of housework reflect sum of usual weekly hours in nine activities. Work at given anniversary calculated using work history. Robust standard errors in brackets. ***p<0.01, **p<0.05, *p<0.1.

who marry older women are also significantly more likely to report dealing with disagreements by calmly discussing issues and report yelling during arguments less frequently. Moreover, women who marry at later ages report that they are less likely to throw things at or hit their husband when upset and both spouses are less likely to report getting physically injured during an argument.

Both the MILC and NSFH also indicate that marriages occurring when a woman is older are characterized by less traditional values; women work more in the market, spend less time doing housework, and both spouses are less likely to report a moral issue with divorce as a bride's age rises. Increases in a woman's age at marriage are also associated with different reasons for her working or wanting to work. In 1980 (when the MILC began), women who married later in life were more likely to want to work to be around people, have a career, or feel like they had accomplished something. Moreover, they were less likely to work for purely financial reasons, suggesting that couples were either more financially sound, less likely to view their jobs as simple means to an end, or both.[81]

Although none of these relationships is necessarily causal, these results provide a rationale for the plausibly causal relationship between age at marriage and marital stability shown in the previous sections. They also imply that waiting to marry may produce gains within marriage and not simply affect relationships on the cusp of divorce.

1.6 Conclusion

During the past several decades, women moved into the labor force, experienced wage gains, and gained greater control over their fertility. As these changes occurred, divorce rates first rapidly rose but then began to fall. Although much is known about the initial rise in divorce, little had been previously said about its subsequent strong and sustained decline.

This paper demonstrates that once one controls for bride's age, cohorts marrying from

[81] These results are robust to controlling for the difference in spouses' ages.

1980 to 2004 have similar risks of divorce. To determine if age at marriage is the proximate cause of the decline in divorce, I use three different techniques that mitigate bias in estimates of the effect of bride's age on marital stability. Controlling for the major causes of family change (e.g., female labor force participation, access to birth control, and divorce laws), controlling for family background (including family fixed-effects), and instrumenting for early teenage marriage using state laws governing the minimum age at marriage, I provide evidence suggesting that the true, causal relationship between a woman's age at marriage and her future probability of divorce cannot be substantially weaker than suggested by uncorrected estimates. All of the estimates suggest that the hazard of divorce falls by at least 6 percent when a bride waits one additional year to marry, implying that age at marriage can explain at least 60 percent of the fall in the divorce rate for cohorts marrying from 1980 to 2004.

Within the literature on the family, the results indicate that decreases in the relative value of marriage caused an increase in age at marriage, which in turn caused the divorce rate to decrease from 1980 to 2004. But the gains to marriage changed in a similar manner before and after 1980. Thus, I also address the consistency of my findings with respect to trends in earlier decades, when the gains to marriage decreased but divorce rates increased. Using a search model of the marriage market, I demonstrate that shocks to the gains to marriage can differentially impact the eventual divorce rates of currently married and single women. The asymmetry allows monotonic decreases in the gains to marriage (e.g., because of increases in female labor force participation and abortion access), and the resultant increases in age at marriage, to cause both the increase in divorce for cohorts marrying from 1950 to 1979 and the subsequent decline in marital instability.

Analysis of many different outcomes also suggests that the increase in age at marriage and the resulting decrease in divorce may have further implications for the lives of men, women, and children. Descriptive regressions demonstrate that increases in age at marriage may improve marital quality, even for inframarginal unions. Furthermore, increases in age at marriage may result in a polarization in the lives of the nation's children. As age at marriage increases, children born within marriage are less likely to see their parents divorce, but a higher average age at marriage in general means that more children will never live within a married family. These results point to many avenues for future research on the effect of age at marriage on child welfare and outcomes. Given past analysis of marriage and the family (e.g., Johnson and Skinner 1986, Stevenson 2007, and Willis 1999), my results also suggest the potential for future work on the effect of age at marriage on investments in children, marriage-specific capital, and human capital.

2 Do Outside Options Matter Inside Marriage? Evidence from State Welfare Reforms

2.1 Divorce Threat and Marital Bargaining

Economists often assume that spouses face a potential threat of divorce when bargaining with each other. Thus, economic models of marriage imply that outside options can influence consumption and leisure within marriage. However, evidence on the validity of this assumption is limited. To measure the impact of a woman's outside options on the choices of married families, I analyze changes in household expenditure in response to state-level welfare reforms. These plausibly exogenous changes in social insurance decreased the expected utility of divorced women and thus the household bargaining power of married women, if couples indeed bargain with reference to the threat of divorce.

The reforms (termed "waivers") were granted to states from 1992 to 1997 and preceded the federal replacement of AFDC (Aid to Families with Dependent Children, the social insurance program for impoverished families established in 1935) with TANF (Temporary Assistance for Needy Families, initiated by congress's Personal Responsibility and Work Opportunity Act and phased-in between 1996 and 1997). Waivers typically decreased the net expected welfare benefits available to unmarried mothers by increasing the costs of receiving welfare. States that were granted waivers could require that single mothers on welfare spend more time in work or training programs (and receive no additional income). Waivers could also allow states to tighten welfare eligibility requirements. Although the policies were multifaceted and varied much across states, back of the envelope calculations suggest that a waiver was equivalent to an approximately 25 percent decrease in the social insurance benefit available to indigent mothers.

But how relevant could the changes in these laws have been for mothers who were currently married? Likely very relevant. Though married women are only eligible for welfare in rare circumstances, many divorced women rely on this safety net. For example, almost one-quarter of all divorced women with children received AFDC in 1990.[82] The change in the law therefore worsened the expected outside options of at least this fraction of married mothers and could have affected far more women. Furthermore, a substantial economic literature exists on the effect of welfare waivers on potential recipients (see Blank 2002 and Grogger, Karoly, and Klerman 2002 for surveys). As previously married women accounted for a substantial fraction of AFDC caseloads, these copious studies suggest that welfare reform should also alter a wife's threat point within marriage.[83,84]

I use the 1984 to 1997 Consumer Expenditure Survey (CEX) to analyze the effect of welfare waivers on expenditures on goods that require a high (woman's) time commitment (e.g., unprepared food items) versus those that do not (e.g., prepared foods; child care services), as well as sex-specific goods (e.g., women's versus men's clothing). I focus my study on married mothers who have a higher ex-ante probability of ever utilizing AFDC benefits because of their education level or labor force participation.

This paper uses standard and modified difference-in-differences (DD) techniques to estimate the effects of welfare waivers on married families. States passed and implemented welfare reforms at different times in the early and mid-1990s. If the precise timing of these waivers does not otherwise relate to changes in household bargaining power or expenditure

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[82]In the 1990 Census, 22 percent of divorced women with children reported receiving public assistance in the past year. In the 1979 National Longitudinal Survey of Youth, 24 percent of women who had children and subsequently divorced (between 1979 and 2003) received welfare payments within five years of leaving their spouse; more than half of these women received benefits for 24 months or more over these five years. Furthermore, 71 percent of married women in the National Survey of Families and Households (Wave I, conducted in 1988) note that they would be financially worse off if their marriage ended. Only 4 percent of wives believe their financial situation would improve with divorce.

[83]See US Census (1995) for details on the characteristics of welfare recipients.

[84]Schoeni and Blank (2000) show that waivers affected the AFDC participation, earnings, poverty rates, labor force participation, and fertility of single women. Also see Grogger (2003) and Moffitt (1999) on labor supply and earnings and Bitler, Gelbach, and Hoynes (2006); Kaushal and Kaestner (2001); Kearney (2004); and Levine (2002) on fertility.

more generally (conditional on measurable household characteristics), the standard DD procedure will yield unbiased estimates of the effect of welfare reform. The modified DD procedure goes one step further to ensure accurate results, estimating a DD effect using an optimal synthetic control group (see Abadie, Diamond, and Hainmueller 2010).

Either technique reveals that welfare waivers are associated with changes in the budgets of married families suggesting decreases in female bargaining power. Waivers increased the share of a household's budget allocated to food purchased at grocery and similar stores (by 2-3 percentage points or 10-15 percent of food expenditures) and decreased the shares spent on women's clothing (by 0.4 percentage points or about 8 percent of the total amount spent on all clothing) and child care (by about 0.4 percent or 7 percent of child care expenditures for those who purchase any care).

My findings are robust to several different specifications. Furthermore, placebo tests and analyses of other variables suggest that welfare waivers are responsible for the estimated expenditure changes. Overall, the results indicate that a wife's divorce-based outside options shape the decisions of married families, a common - but largely untested - assumption.

2.2 Divorce and AFDC Waivers

Models analyzing married households typically incorporate (explicitly or implicitly) an outside option related to divorce.[85] A cooperative Nash bargaining game demonstrates one way a model could link utility within and outside of marriage. Suppose that utility is fully transferable between household members, marriage yields total utility of one, and a husband and wife would receive d_h and d_w respectively if they divorced, where

[85] Examples of theoretical papers considering divorce-threat include Manser and Brown (1980) and McElroy and Horney (1981), or more recently, Chiappori, Fortin, and Lacroix (2002) and Chiappori, Iyigun, and Weiss (2007). Notable exceptions include Lundberg and Pollak (1993, 1994), who develop alternate models of spousal bargaining related to threat points within marriage.

$d_h + d_w < 1$. Then, the only possible division of utility satisfying invariance to affine transformation, Pareto optimality, independence of irrelevant alternatives, and symmetry is that a wife receives utility of $u_w = d_w + \frac{1}{2}(1 - d_h - d_w)$, while her husband enjoys $u_h = d_h + \frac{1}{2}(1 - d_h - d_w)$.[86] The very simple framework captures the dependence between utility within marriage and expected utility upon divorce.

State-level waivers from AFDC guidelines provide a natural experiment to test if d_w influences the determinants of u_h and u_w. That is, welfare waivers lessened the insurance available to women in the case of divorce and material hardship, thus decreasing the expected value of d_w. The above model then predicts that after a welfare waiver, a wife's utility should decrease and her husband's utility should increase. McElroy (1990) first proposed a similar test of bargaining within married households. Lundberg and Pollak also note that if couples bargain with reference to a divorce-related threat point "changes in the welfare payments available to divorced mothers...should affect distribution between men and women" (Lundberg and Pollak 1996, p. 147).

In order for welfare waivers to impact the expected value of a married woman's outside option (and thus a woman's utility within marriage), three conditions must hold.[87] First, the waivers had to cause a sufficient change in the social insurance available to single mothers. This study considers only major waivers, most of which limited the length of

[86]One could also derive the same result in expectation if one spouse was randomly selected to offer a division of the gains from marriage, the other spouse could accept or reject this division (and instead seek divorce), and each spouse has an equal probability of playing each role.

[87]I focus my study on AFDC waivers (instead of other welfare reforms) because states gradually phased-in waivers, allowing for comparisons between states and an evaluation of the role of expectations of future waivers. Further, although people potentially anticipated future waivers (Peterson 1995), anecdotal evidence suggests that the TANF reform was largely unexpected and likely would not influence the implicit state-level experiment I construct (Weaver 2000).

benefit receipt and/or exemptions from training programs.[88,89] Though each resulted in different rules, a typical waiver might require a woman to work an additional 60 hours per month (for the same transfer) if she spent more than two years on welfare. If a woman did not participate in this work program, half of her AFDC benefit could be revoked (an average value of about $2,000). Alternatively, valuing this time at the 1990 federal minimum wage implies that such a requirement led to a $2,700 per year decrease in "full income" (Becker 1965). Comparing these valuations to the incomes of women receiving welfare in the 1990 Census (less than $10,000 on average, including in-kind transfers) suggests that a welfare waiver may be thought of as similar to a 25 percent reduction in income. Furthermore, Blank (2001) calculates that the waivers led to an 8 percent decrease in AFDC caseloads.[90] Thus, welfare waivers largely restricted recipient households' opportunity sets.

Second, a non-trivial number of married mothers must (eventually) rely on the insurance provided by the welfare system. Although many women may be financially secure within marriage, poverty is common among divorced mothers. Almost 40 percent of women with children in the 1979 National Longitudinal Survey of Youth reported incomes below the poverty line within five years of their divorce. More than half of these women also reported receiving welfare during that period. Moreover, the literature on poverty suggests that divorce and indigence are intimately linked.[91]

Finally, to have an effect on married parents, the change in policy must be salient to

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[88]Formally, major waivers limited (i) how long one could stay on welfare, (ii) how long one could stay on welfare without working, (iii) the receipt of additional benefits after having additional children while receiving aid, and/or (iv) who was exempt from required training programs. They also may have increased (v) the amount of money one could earn without facing a loss in benefits, and/or (vi) punishments for failure to attend training programs. All of these reduced d_w except (v). However, the level of income that was ignored in the calculation of benefits was small (in some cases as low as $30), and thus reasonably ignored.

[89]Although states could ask for AFDC waivers as early as 1962, they did not apply for major exceptions until the early 1990's. Prior to that time, the process of approval was far more onerous (CEA 1997).

[90]See Blank (2002) for a review of estimates of the effect of welfare waivers on caseloads.

[91]For instance, Bane and Ellwood (1986) report that many women fall below the poverty line because of divorce.

those currently ineligible for welfare benefits. Although the evidence is somewhat mixed, the literature indicates that welfare waivers increased (decreased) the labor force participation and income (poverty and fertility) of unmarried women in a potentially noticeable manner.[92] Moreover, Ellwood and Bane (1985) demonstrated that benefit levels can have non-trivial impacts on divorce rates and household composition.[93] If women in married families responded to waivers by divorcing their husbands less often, they were likely aware of the changes in the welfare system. Further, George H.W. Bush repeatedly mentioned the waivers in the 1992 Presidential Debates, increasing public awareness of these policies (Weaver 2000).

Overall, the evidence indicates that changes in welfare benefits have powerful effects on divorced mothers. Therefore, welfare reform should also affect mothers who remain married if outside options affect decision making within marriage.

2.3 The Consumer Expenditure Survey

Data from the CEX allows one to consider changes in expenditure indicative of shifts in the leisure, home production, and consumption of different family members, and thus infer changes in the relative utility of husband and wife. The survey contains two parts: an interview survey (CEXI) for expenditures over the course of three months and a diary survey (CEXD) in which households record everyday purchases for one or two weeks (in particular, groceries at a disaggregated level). From each of these surveys, I select married couples interviewed between 1984 and 1997, with complete data on income and

[92] Schoeni and Blank (2000) show that waivers affected the AFDC participation, earnings, poverty rates, labor force participation, and fertility of single women. Also see Grogger (2003) and Moffitt (1999) on labor supply and earnings and Bitler, Gelbach, and Hoynes (2006); Kaushal and Kaestner (2001); Kearney (2004); and Levine (2002) on fertility, or Blank (2002) and Grogger, Karoly, and Klerman (2002) for surveys.

[93] Bitler, et al. (2004) and Schoeni and Blank (2000) later confirmed these results.

expenditure, where both spouses are non-disabled and of ages 18 to 64.[94] Families in my sample also include a male head of household working at least 30 hours in a typical week (full-time) and children under 18 that are biologically related to both spouses. These restrictions isolate intact families currently ineligible for AFDC that contain a wife who could potentially receive funding if divorced.

From this set of households, I select two subgroups: families with a mother without any college education and families with a mother who reports typically working only part-time or not at all. Any mother can imagine the possibility of needing monetary assistance to care for her children but these groups contain those more likely than average to request public aid. I base my analysis on such women hailing from several states that implemented a welfare waiver (CA, GA, IL, MI, and NJ) and many control states.[95]

I examine multiple expenditure categories to determine how a waiver altered household expenditure. First, "food for home consumption" (as opposed to "food away from home") represents a key measure of inputs into the home production process. If families purchase more food for consumption at home, home production activities are probably greater. Although one cannot strictly say that a larger grocery bill implies less female leisure (and thus less female utility), it suggests an increase in the consumption of a commodity with female-labor-intensive production (Becker 1965, 1991). To further analyze home production, I also use the CEXD to obtain detailed data on grocery purchases that I categorize by the amount of time (high, intermediate, or low) likely combined with the intermediate good when creating a meal.[96] Similarly, higher expenditures on child care and food away from home represent substitutions away from female-labor-intensive home production. I further use the change in "home maintenance services" to determine what

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[94]See Appendix B.1.1 for details.

[95]Ibid.

[96]Note that though the CEXI and CEXD are classified together as the CEX, different families are used in each survey and the studies should be thought of as giving alternative measures of expenditure.

types of households drive the implied change in home production. This category contains only housekeeping and gardening services; one would expect only families with high permanent income to adjust their expenditure on these luxuries.

Other categories more directly relate to bargaining power, such as men's and women's clothing. These goods represent the clearest indicators of gender-specific consumption in the CEX but they are also relatively durable. Therefore, changes in the stock of clothing may misrepresent changes in utility flows from clothing. Children's clothing, educational expenditures, and alcohol and tobacco expenditures further round out my analysis, as expenditure on these goods has been shown to respond to the relative income of spouses.[97] Finally, I consider the remainder of expenditures not detailed above and the remainder of income not accounted for by CEX expenditures.[98]

Taken together, these categories will detail the changes in expenditure associated with a decrease in wives' divorce-based outside options.

2.4 Difference-in-Differences Estimates

Standard difference-in-differences (DD) regressions provide initial evidence on how welfare waivers influenced married families. In particular, I estimate

$$Y_{jit} = \delta_{jt} + \mu_{js} + \eta_{js} t_{js} + \alpha_j Treat_i * Post_t + \theta_j Z_i + \varepsilon_{jit} \tag{2.1}$$

where Y_{jit} is the expenditure share that family i (living in state s) spends on good j in period t, δ_{jt} and μ_{js} are time- and state-specific fixed-effects, t_{js} is a state-specific time trend, $Treat_i$ indicates if the family resides in a state that implemented a welfare waiver,

[97] For example, see Boserup's (1970) classic work, or more recently Rangel (2006).

[98] Note that one should not think of this residual income measure as savings, due to issues of under-reporting of total expenditures in the CEX (c.f., Attanasio, Battistin, and Ichimura 2004 and McCarthy, et al. 2002).

$Post_t$ indicates that a family was observed after a welfare waiver went into effect, and Z_i contains various family-level controls.[99,100] These definitions imply that α_j measures the impact of welfare reform on good j's expenditure share.

Basic DD estimates of α_j controlling for state and year fixed-effects, but no other variables, can be found in col. (1) of Tables 2.1A and 2.1B. Col. (2) of each panel contains estimates of treatment effects from eq. (2.1) controlling for all available covariates and using only the subsamples of households reporting positive expenditure within category j to estimate the effect of a waiver on expenditure share j.[101]

The DD estimator provides prima facie evidence that welfare waivers affected the expenditure decisions of married families. In the post-waiver period, both families with a mother who does not work full-time and families with a less-educated mother significantly increased expenditure on food for home consumption. The CEXD data indicates that such changes likely come from increases in the amount of high time-input groceries purchased. The group of families with less-educated wives also significantly decreased expenditures on women's clothing. If one conditions on household characteristics, both groups of families significantly increased expenditure on men's clothing. These results suggest that welfare waivers increased expenditures on intermediate goods used in home production and decreased expenditure on female-specific goods in favor of goods purchased for men, children, and the entire household.

Despite the clear results, standard DD estimates of eq. (2.1) may be biased. Many families report zero expenditure within some categories, making it difficult to interpret

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[99] See Appendix Table B.1 for a list of treated and control states and the dates of waiver implementation.

[100] Z includes controls for family size (dummy variables for three, four, five, or six or more members), mother's labor force participation (full-time, part-time, or not working), education (five categories for each spouse), income (female earned, male earned, and unearned), race (a dummy variable for both spouses being white), urban location, and having any children aged zero to six, seven to 12, and 13 to 17.

[101] The coefficients estimated in Tables 2.1A and 2.1B may thus be interpreted as applying only to the potentially changing population of families who report purchases within a category.

Table 2.1A: DD Estimates of the Effect of AFDC Waivers on Expenditure Shares (x100)

	Families with Mother with ≤ HS Ed			
	(1)		(2)	
	Effect	SE	Effect	SE
Food for Home Consumption	1.476***	[0.539]	1.631***	[0.475]
High Time Input Groceries/All Groceries	2.236***	[0.845]	1.677	[2.055]
Intermed. Time Input Groceries/All Groceries	-1.347**	[0.556]	0.169	[1.151]
Low Time Input Groceries/All Groceries	-0.889	[1.045]	-1.948	[1.596]
Food Away from Home or Prepared by Others	0.037	[0.109]	-0.095	[0.188]
Child Care	-0.220	[0.249]	-1.08*	[0.643]
Home Maintenance Services	0.000	[0.025]	-0.55*	[0.310]
Men's Clothing, etc.	-0.023	[0.073]	0.326***	[0.126]
Women's Clothing, etc.	-0.205**	[0.101]	-0.449***	[0.155]
Children's Clothing and Accessories	0.063	[0.092]	-0.202	[0.168]
Education	-0.124	[0.222]	-1.28	[0.818]
Alcohol and Tobacco	-0.021	[0.176]	-0.697*	[0.362]
Residual Expenditure	-0.981**	[0.386]	0.535	[0.888]
Residual Income	6.81*	[4.024]	4.815	[5.909]
Conditional on Expenditure>0 in Category?	No		Yes	
Additional Control Variables?	No		Yes	
Total Families in CEXI	15,839			
Total Families in CEXD	4,272			

Notes and sources: See Appendix Tables B.2A and B.2B for proportion with expenditure greater than zero in each category. The denominator of the share is the quarterly, BLS-reported total expenditure for all goods, except the grocery variables. These shares are weekly shares of all grocery expenditure. Data comes from families with positive expenditure in selected treated and control states in CEXI and CEXD, 1984-TANF implementation. See Appendix B.1.1 for details. All specifications include state and year effects, additional controls are: state-specific time trends, controls for family size (indicators for three, four, five, or six or more members), mother's labor force participation (full-time, part-time, or not working), education (five categories for each spouse),age of both spouses, income (female earned, male earned, and unearned), race (an indicator for both spouses being white), urban location, and having children aged zero to six, seven to 12, and 13 to 17. Standard errors clustered by state are reported in brackets. Estimates calculated using full sample weights. For a two-tailed test at significance level .05, the conservative Bonferroni-corrected size of the test (to account for Type I error and the large number of expenditure categories) is 0.004. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.1B: DD Estimates of the Effect of AFDC Waivers on Expenditure Shares (x100)

	Families with a Mother Not Working FT (30+ hrs/wk)			
	(1)		(2)	
	Effect	SE	Effect	SE
Food for Home Consumption	1.748***	[0.598]	3.706**	[1.460]
High Time Input Groceries/All Groceries	2.860***	[0.882]	2.288	[1.652]
Intermed. Time Input Groceries/All Groceries	-1.518***	[0.423]	0.846	[0.981]
Low Time Input Groceries/All Groceries	-1.342**	[0.684]	-3.038**	[1.339]
Food Away from Home or Prepared by Others	0.039	[0.132]	-0.177	[0.333]
Child Care	-0.392***	[0.130]	-0.798	[1.026]
Home Maintenance Services	-0.105*	[0.063]	0.664	[0.511]
Men's Clothing, etc.	-0.046	[0.081]	0.762***	[0.188]
Women's Clothing, etc.	-0.144	[0.127]	-0.163	[0.236]
Children's Clothing and Accessories	0.071	[0.106]	-0.092	[0.343]
Education	-0.136	[0.297]	-0.95	[0.801]
Alcohol and Tobacco	-0.161*	[0.093]	-1.263***	[0.352]
Residual Expenditure	-0.875	[0.783]	-2.479	[1.540]
Residual Income	0.825	[4.426]	7.688*	[3.948]
Conditional on Expenditure>0 in Category?	No		Yes	
Additional Control Variables?	No		Yes	
Total Families in CEXI	14,140			
Total Families in CEXD	4,057			

Notes and sources: See Appendix Tables B.2A and B.2B for proportion with expenditure greater than zero in each category. The denominator of the share is the quarterly, BLS-reported total expenditure for all goods, except the grocery variables. These shares are weekly shares of all grocery expenditure. Data comes from families with positive expenditure in selected treated and control states in CEXI and CEXD, 1984-TANF implementation. See Appendix B.1.1 for details. All specifications include state and year effects, additional controls are: state-specific time trends, controls for family size (indicators for three, four, five, or six or more members), mother's labor force participation (full-time, part-time, or not working), education (five categories for each spouse),age of both spouses, income (female earned, male earned, and unearned), race (an indicator for both spouses being white), urban location, and having children aged zero to six, seven to 12, and 13 to 17. Standard errors clustered by state are reported in brackets. Estimates calculated using full sample weights. For a two-tailed test at significance level .05, the conservative Bonferroni-corrected size of the test (to account for Type I error and the large number of expenditure categories) is 0.004. *** p<0.01, ** p<0.05, * p<0.1.

regressions both conditional and unconditional upon $Y_{jit} > 0$. Further, reported expenditure in the CEX is unrealistically low, potentially due to incorrect reports of $Y_{jit} = 0$.[102,103] Though classical measurement error in the dependent variable will only decrease my estimates' precision, the error in Y_{jit} cannot be classified as white noise due to the lower bound on Y_{jit}. One also may be concerned that incorrect reports of $Y_{jit} = 0$ are non-random. Thus, this non-classical measurement error could lead to biased estimates of α_j.

The procedure has further hurdles to surmount. Deviations from the state-specific trends in (2.1) are not random, implying pre-existing trends could bias my estimates. To formally test the parallel-trends assumption of DD, I use only data from 1984 to 1992:3 and test for a placebo "treatment effect" at various points in this entirely pre-treatment period. That is, I set $Post_t = 1$ after some hypothetical date of intervention and estimate α_j using only pre-waiver data. If these placebo tests yield significant α_j coefficients, the DD estimates will be biased. In fact, 30 (21 percent) of these estimates are significant at the 5 percent level, out of 140 tests run (14 expenditure categories, two samples, and five "treatment" years).[104] Moreover, these violations come from a variety of different expenditure categories. Thus, the DD estimates cannot be more than suggestive on their own.

2.5 Synthetic Control Estimates

To provide further evidence on the effect of welfare waivers on married families, I use an alternative estimator robust to many of the issues with DD. Proposed by Abadie,

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[102] See Attanasio, Battistin, and Ichimura (2004) and McCarthy, et al. (2002).

[103] See Appendix Tables B.2A and B.2B for the proportion of families reporting non-zero expenditure by category. One could aggregate data into larger categories to mitigate the issues of reporting $Y_{jit} = 0$ but much valuable detail would then be lost. Additionally, some categories with the largest potential non-reporting issues (e.g., child care) represent unique expenditures not easily combined with other categories.

[104] Significance calculated using standard errors clustered by state. Using higher-order time trends does not improve results.

Diamond, and Hainmueller (2010, hereafter ADH), the synthetic control estimator is a cousin to both DD and propensity score matching. Essentially, ADH's approach accounts for the fact that all control units are not equally comparable to all treated units. Therefore, instead of weighting each control equally, ADH use a data-driven procedure to select an ideal control group. One first takes the set of all potential control observations and determines which characteristics are important in predicting the outcome variable (potentially including lagged values of this outcome). Next, the researcher searches for the weighted average of these control groups (the "synthetic control") that minimizes the difference between the characteristics of the treated and control observations. One then uses the synthetic control to calculate a treatment effect using DD. Unlike propensity score matching, this technique uses aggregate data on treated and control units (in my case, states instead of families), making it more robust to the issues of measurement error and non-reporting previously discussed.

Functionally, ADH's synthetic control approach assumes that instead of eq. (2.1) the regression of interest is

$$Y_{jit} = \delta_{jt} + \boldsymbol{\lambda}_t \mu_{js} + \theta_{jt} Z_i + \alpha_{js} Treat_s * Post_t + \varepsilon_{jit}, \tag{2.2}$$

allowing the effect of unobservables to vary over time. The specification embodies the assumption that state-specific unobservables affect outcomes in all states in the same way at any point in time (though the effect of these unobservables can vary nonparametrically over time), whereas (2.1) and similar regressions assume unobservables impact a state in a parametric manner over time.

2.5.1 Estimating Treatment Effects with a Synthetic Control

I use the synthetic control approach to select a control group for each treated state. Matches are based on both demographic characteristics and consumption shares for eight years prior to the date of a waiver's implementation.[105]

Overall, the procedure chooses relatively intuitive control groups. Moreover, the treated and synthetic control groups are demographically similar. Table 2.2 shows the absolute and normalized differences between treated and control observations. Balance in the treated and control samples is reasonable and in no case does the normalized difference exceed 0.25 standard deviations in absolute value.[106]

The trends in pre-treatment expenditure also demonstrate that this procedure has a major advantage over a standard DD; Figure 2.1 shows no strong, pre-existing trends in differences between treated and control states. Furthermore, the weak trends that do exist would bias the estimates against confirming the DD findings. Therefore, if the results using this technique match those in Section 2.4, one can be reasonably certain of the overall credibility of my qualitative conclusions.

2.5.2 The Impact of Welfare Waivers on Expenditure

To condense the effects calculated for five treated states and two treated periods, I first find the mean treatment effect over time by state and then compute a population-weighted average effect across states. Table 2.3 reports the treatment effects for my two subsamples of interest (the first column of each panel) and the associated standard errors calculated using a Monte Carlo procedure (the second columns).[107]

In both main samples, the synthetic control procedure indicates that expenditure on

[105] A formal description of the method is detailed in Appendix B.2.

[106] The one potential concern lies in the high value of the normalized difference for male earned income; however, using only pre-treatment expenditure on clothing (which may be more sensitive to relative spousal income, c.f., Lundberg, Pollak, and Wales 1997) to determine the control group leads to a better match for this variable and similar estimates of the effect of a welfare waiver on these categories.

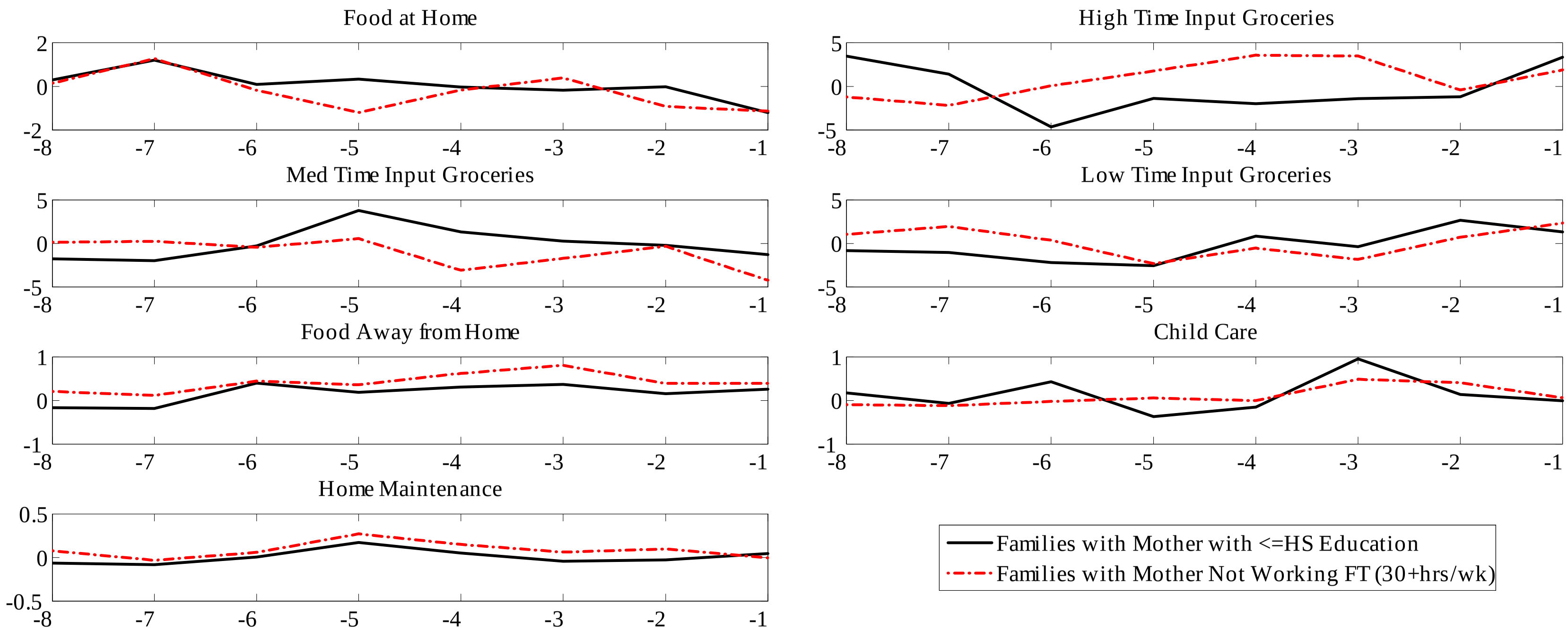
[107] Each of the five treated states has a donor pool of potential control states that did not implement a welfare waiver in the given period. Selecting randomly from each of the five donor pools then gives a draw from the distribution of null treatment effects. For each subsample and specification of interest, I use 10,000 randomly selected draws from this distribution to calculate standard errors.

Table 2.2: Differences in Treatment and Synthetic Control Prior to AFDC Reform

	Families with Mother with ≤ HS Ed		Families with a Mother Not Working FT (30+ hrs/wk)	
Variable	Diff.	Diff./SD	Diff.	Diff./SD
Proportion with Father's Education				
...HS Grad	-0.01	-0.02	-0.03	-0.06
...Some College	0.030	0.07	0.05	0.13
...College Graduate	0.02	0.08	0.01	0.02
...Graduate School	0.01	0.02	0.01	0.01
Proportion with Mother's Education				
...HS Grad	-0.01	-0.02	-0.03	-0.06
...Some College			0.04	0.09
...College Graduate			-0.00	-0.01
...Graduate School			0.00	0.01
Proportion with Family Size=				
4	0.01	0.01	0.03	0.06
5	0.01	0.03	0.01	0.02
6+	-0.00	-0.01	-0.03	-0.09
Proportion with any children 0-6	0.05	0.06	0.00	0.00
Proportion with any children 7-12	0.01	0.02	-0.01	-0.02
Proportion with any children 13-17	-0.01	-0.01	0.01	0.01
Proportion with both spouses white	-0.03	-0.11	0.01	0.04
Proportion urban	-0.02	-0.10	-0.01	-0.07
Female Age	-0.13	-0.02	0.07	0.01
Male Age	-0.20	-0.02	-0.34	-0.04
Proportion with wife not working	0.00	0.01	0.03	0.05
Proportion with wife working PT	0.02	0.05	-0.03	-0.05
Mean Male Earned Income	6062	0.24	7475	0.17
Mean Female Earned Income	239	0.02	325	0.04
Mean Unearned Income	-122	-0.01	-582	-0.04

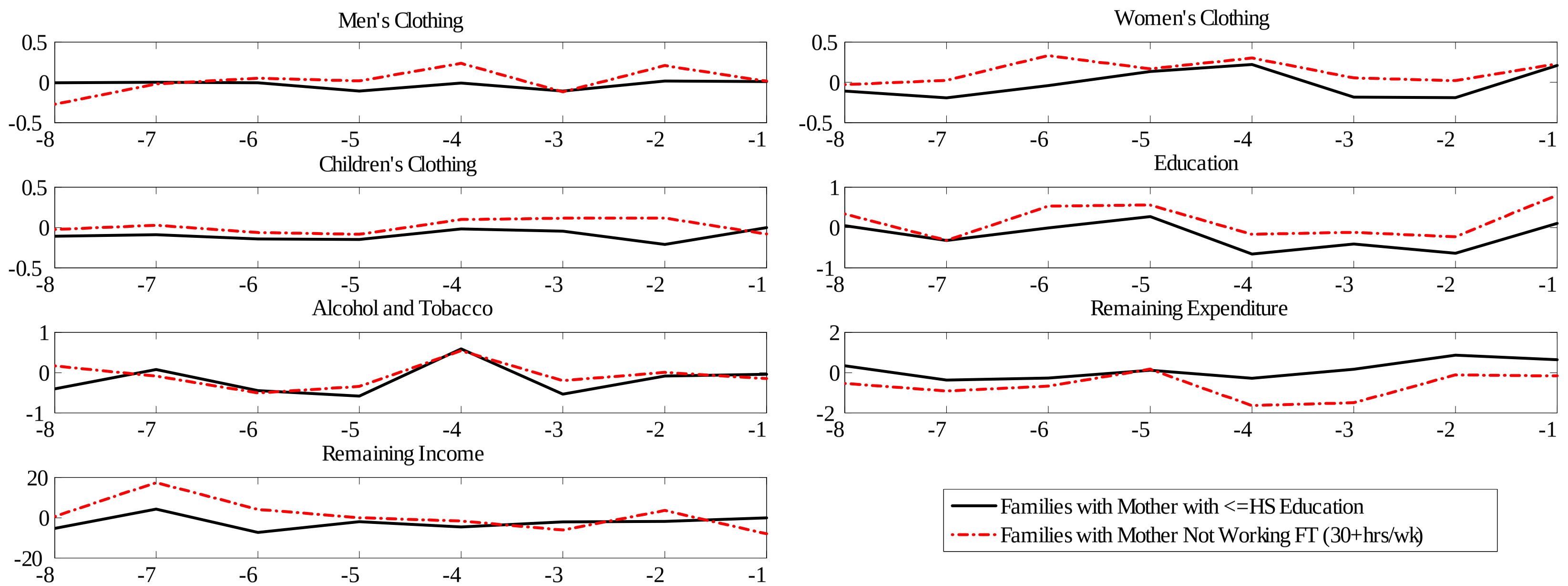
Notes and sources: Variables from selected samples of the CEXI, 1984-1992:3 (prior to the first waiver implementation). Income measured in 2000 dollars. See Appendix B.1.1 for details. Differences are treated value-control value.

Figure 2.1: Difference between Treated and Synthetic Control Expenditure Shares (x100) in the Pre-Treatment Period



Continued on the next page.

Figure 2.1, Continued: Difference between Treated and Synthetic Control Expenditure Shares (x100) in the Pre-Treatment Period



Notes and sources: The denominator of the share is the quarterly, BLS-reported total expenditure for all goods, except the grocery variables. These shares are weekly shares of all grocery expenditure. Data comes from families in selected treated and control states in CEXI and CEXD, 1984-TANF implementation. See Appendix B.1.1 for details. The synthetic control group for each treated state was chosen based on pre-treatment expenditure, family size (three, four, five, or six or more members), mother's labor force participation (FT, PT, NW), education (five categories for each spouse), age of mother and father, income (female earned, male earned, and unearned), race (a dummy variable for both spouses being white), urban location, and having children aged zero to six, seven to 12, and 13 to 17, using Abadie, Diamond, and Hainmueller's (2010) method. Estimates calculated using full sample weights.

Table 2.3: Synthetic Control Estimates of the Effect of AFDC Waivers on Expenditure Shares (x100)

	Families with Mother with $\leq$ HS Ed		Families with Mother Not Working FT (30+ hrs/wk)	
Category	Effect	SE	Effect	SE
Food for Home Consumption	3.36***	1.27	1.61**	0.66
High Time Input Groceries/All Groceries	1.46	1.87	0.09	1.74
Intermediate Time Input Groceries/All Groceries	-0.28	1.42	1.35	1.19
Low Time Input Groceries/All Groceries	-1.17	1.43	-1.45	2.20
Food Away from Home or Prepared by Others	-0.19	0.34	-0.11	0.27
Child Care	-0.26	0.34	-0.40**	0.18
Home Maintenance Services	-0.03	0.06	-0.21	0.13
Men's Clothing, Accessories, and Personal Care	0.21	0.23	0.07	0.17
Women's Clothing, Accessories, and Personal Care	-0.40**	0.20	-0.34*	0.18
Children's Clothing and Accessories	0.20	0.30	-0.11	0.26
Education	-0.35	0.38	-0.85**	0.42
Alcohol and Tobacco	-0.51	0.44	-0.40	0.41
Residual Expenditure	-2.02	1.24	0.74	1.09
Residual Income	-0.89	14.65	-0.40	7.24

Notes and sources: The denominator of the share is the quarterly, BLS-reported total expenditure for all goods, except the grocery variables. These shares are weekly shares of all grocery expenditure. Data comes from selected treated and control states in CEXI and CEXD, 1984-TANF implementation. See Appendix B.2 for details. A synthetic control group for each treated state was chosen based on pre-treatment expenditure, family size (three, four, five, or six or more members), mother's labor force participation (FT, PT, NW), education (five categories for each spouse), income (female earned, male earned, and unearned), age of both spouses, race (a dummy variable for both spouses being white), urban location, and having children aged zero to six, seven to 12, and 13 to 17, using Abadie, Diamond, and Hainmueller's (2010) method. Estimates calculated using full sample weights. For a two-tailed test at significance-level .05, the conservative Bonferroni-corrected size of the test (to account for Type I error and the large number of expenditure categories) is 0.004. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

food for home consumption (and thus home production) significantly increases in response to a welfare waiver, by 1.6 to 3.4 percent of quarterly expenditure on all goods and services. Although small in terms of the family's total budget, the effect implies more than a 10 percent increase in expenditure on all food. Due to the CEXD's small sample size, one cannot determine how household members spend this additional money within the grocery store. But because expenditure on food away from home does not significantly change, the amount of time used to create meals likely increased after a waiver's implementation.

Changes in expenditure on child care also suggest that households increase home production in response to a welfare waiver. In my sample of mothers working less than full-time, a waiver resulted in the child care expenditure share dropping by 0.4 percentage points (or about 7 percent of child care expenditures for those purchasing these services). The results for food for home consumption and child care strongly contrast with the insignificant and smaller results for home maintenance services, a luxury not often purchased by the lower-income families one would expect to be influenced by welfare policy.

Additionally, families with less-educated mothers react to a welfare waiver by spending significantly less on women's clothing, accessories, and personal care. The effect is slightly smaller, though still significant at the 10 percent level, for mothers not working full-time. There is no significant change in children's or men's clothing purchases, suggesting a transfer of resources from women to other household members. A null effect on total reported expenditure (not shown in Table 2.3) also indicates that the effects of a waiver on food at home, child care, and women's clothing do not simply reflect a correlation between waiver implementation and a sluggish state economy.

One can alternatively interpret these results using the average economic loss associated with a welfare waiver. As previously discussed, an average waiver had an effect approximately equivalent to a 25 percent decrease in the value of social insurance for indigent mothers. Using this metric, one can say that a 1 percent decrease in a woman's insurance against divorce and poverty led to a 0.4 to 0.8 percent increase in expenditure on food for home consumption and a 0.2 to 0.5 percent decrease in expenditure on child care (for those who purchase the service) within marriage. Further, some families respond to the percentage change by decreasing expenditure on female clothing by 0.9 percent. Altogether, a 1 percent decrease in the welfare benefits of poor, divorced mothers is associated with a reallocation of 0.1 to 0.2 percent of a two-parent family's budget.

Though one cannot determine the precise way in which home production or utility changed, the evidence indicates that welfare waivers caused the consumption patterns of married families to shift. Interpreted through the lens of the framework in Section 2.2, the data support the importance of outside options in the allocation of utility among family members

2.5.3 Robustness of Estimates

To verify these findings, I conduct robustness analyses using both subsamples of families. Additionally, I produced results for alternate, at-risk populations of women (low predicted wages, low assets, etc.) and all families with biological children. These samples yielded similar results.

Although little work analyzes how welfare specifically impacts married women, studies suggest that welfare generosity affected population-wide fertility and female headship rates.[108] Such effects could cause expenditure to shift without a change in bargaining power.[109] Thus, Tables 2.4A and 2.4B contain treatment effects estimated using (i) expenditure shares adjusted by family size and (ii) a synthetic control method which uses the proportion of women who are married as a covariate. Neither of these specifications

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[108]See Blank (2002).

[109]Similarly, the changes in expenditure could be the by-product of changing female labor force

Table 2.4A: Synthetic Control Estimates of the Effect of AFDC Waivers on Expenditure Shares (x100)

Robustness to Family Formation Assumptions and Choice of Control Group
Families with Mothers with a High School Education or Less

	Expenditure Shares Adjusted for Family Size		Match Based on Proportion of Women Married		Control Implemented Waiver Before TANF		Control Does Not Implement Waiver Before TANF	
Category	Effect	SE	Effect	SE	Effect	SE	Effect	SE
Food for Home Consumption	1.83**	0.77	2.07***	0.78	1.89**	0.83	2.62***	0.96
High Time Input Groceries/All Groceries	1.11	1.79	1.05	1.73	1.34	1.73	-1.54	2.08
Intermediate Time Input Groceries/All Groceries	0.19	1.57	0.14	1.67	-0.10	1.96	0.97	1.04
Low Time Input Groceries/All Groceries	-1.29	1.56	-1.19	1.51	-1.24	1.12	0.57	2.64
Food Away from Home or Prepared by Others	-0.03	0.20	-0.04	0.21	-0.04	0.22	0.23	0.22
Child Care	-0.53	0.52	-0.47	0.49	-0.66	0.43	-0.20	0.42
Home Maintenance Services	0.00	0.04	-0.01	0.04	-0.03	0.05	-0.05	0.05
Men’s Clothing, Accessories, and Personal Care	0.14	0.15	0.10	0.16	0.20	0.13	0.13	0.08
Women’s Clothing, Accessories, and Personal Care	-0.19	0.16	-0.25	0.17	-0.33	0.19	-0.17*	0.10
Children’s Clothing and Accessories	0.03	0.16	0.03	0.15	-0.06	0.11	-0.01	0.20
Education	0.07	0.39	0.00	0.38	-0.10	0.38	0.08	0.28
Alcohol and Tobacco	-0.08	0.17	-0.06	0.16	0.03	0.16	-0.09	0.18
Residual Expenditure	-1.23	0.75	-1.39*	0.75	-0.90*	0.59	-2.55**	0.92
Residual Income	-1.62	7.59	-1.37	8.41	1.67	7.36	-1.20	6.53

Notes and sources: The denominator of the share is the quarterly, BLS-reported total expenditure for all goods, except the grocery variables. These shares are weekly shares of all grocery expenditure. Data come from families with biological children, 1984-TANF implementation. See Appendix B.1.1 for details on data selection. A synthetic control group for each treated state was chosen based on pre-treatment expenditure, family size (three, four, five, or six or more members), mother’s labor force participation (FT, PT, NW), education (five categories for each spouse), income (female earned, male earned, and unearned), race (a dummy variable for both spouses being white), urban location, and having children aged zero to six, seven to 12, and 13 to 17, using Abadie, Diamond, and Hainmueller’s (2010) method. Estimates calculated using full sample weights. For a two-tailed test at significance-level .05, the conservative Bonferroni-corrected size of the test (to account for Type I error and the large number of expenditure categories) is 0.004. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.4B: Synthetic Control Estimates of the Effect of AFDC Waivers on Expenditure Shares (x100)

Robustness to Family Formation Assumptions and Choice of Control Group
Families with Mothers Working Less than 30 Hours per Week

	Expenditure Shares Adjusted for Family Size		Match Based on Proportion of Women Married		Control Implemented Waiver Before TANF		Control Does Not Implement Waiver Before TANF	
Category	Effect	SE	Effect	SE	Effect	SE	Effect	SE
Food for Home Consumption	1.64**	0.65	1.56**	0.70	1.85**	0.82	0.75	0.52
High Time Input Groceries/All Groceries	0.26	2.13	-0.40	1.81	-0.99	2.33	0.48	1.47
Intermediate Time Input Groceries/All Groceries	2.12*	1.21	1.32	1.19	0.63	1.17	2.97*	1.69
Low Time Input Groceries/All Groceries	-2.37	2.62	-0.92	2.33	0.36	2.01	-3.45	2.57
Food Away from Home or Prepared by Others	-0.16	0.27	-0.09	0.28	-0.21	0.33	0.10	0.13
Child Care	-0.51**	0.26	-0.34	0.28	-0.31	0.25	-0.64*	0.38
Home Maintenance Services	-0.13	0.12	-0.18	0.13	-0.19	0.17	-0.11	0.07
Men's Clothing, Accessories, and Personal Care	0.03	0.15	0.08	0.18	0.11	0.20	0.00	0.13
Women's Clothing, Accessories, and Personal Care	-0.36**	0.17	-0.37*	0.19	-0.32*	0.20	-0.37*	0.23
Children's Clothing and Accessories	-0.15	0.27	-0.12	0.25	-0.12	0.32	-0.29	0.25
Education	-0.94**	0.38	-0.76	0.42	-0.90**	0.36	-0.32	0.38
Alcohol and Tobacco	-0.29	0.38	-0.42	0.40	-0.38	0.44	-0.48**	0.23
Residual Expenditure	0.87	1.04	0.65	1.12	0.47	0.69	1.35	1.25
Residual Income	2.61	7.56	-1.94	7.36	-3.04	6.72	5.07	8.57

Notes and sources: The denominator of the share is the quarterly, BLS-reported total expenditure for all goods, except the grocery variables. These shares are weekly shares of all grocery expenditure. Data come from families with biological children, 1984-TANF implementation. See Appendix B.1.1 for details on data selection. A synthetic control group for each treated state was chosen based on pre-treatment expenditure, family size (three, four, five, or six or more members), mother's labor force participation (FT, PT, NW), education (five categories for each spouse), age of both spouses, income (female earned, male earned, and unearned), race (a dummy variable for both spouses being white), urban location, and having children aged zero to six, seven to 12, and 13 to 17, using Abadie, Diamond, and Hainmueller's (2010) method. Estimates calculated using full sample weights. For a two-tailed test at significance-level .05, the conservative Bonferroni-corrected size of the test (to account for Type I error and the large number of expenditure categories) is 0.004. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

changes the previous qualitative conclusions.

Further, one could be concerned about more general changes in family composition after a waiver. Indeed, one major drawback of ADH's approach is that the characteristics of families are controlled for only prior to policy implementation. Therefore, the effects estimated in the previous section reflect both changes in expenditure directly due to AFDC waivers and changes in expenditure occurring because the policy changed the composition of my sample. I thus attempted to combine the synthetic control approach with OLS regression by using residuals from a regression of Y_{jit} on the Z_i controls as the dependent variable in the synthetic control procedure.

The adjustment attenuates the sizes of the effects of a waiver on food for home consumption, child care, and women's clothing, though coefficients remain of the same sign and similar magnitude. But using errors from regressions of expenditure shares that involve many $Y_{jit} = 0$ values makes interpretation of the results difficult and standard errors are not well-defined for this procedure. Additionally, I used the synthetic control groups calculated in Section 2.5.2 to determine if AFDC waivers had an effect on the demographic composition of families. That is, I use the same synthetic control groups that I used to estimate the effects in Table 2.3 but calculate the effect of a waiver on Z_i. Overall, the effect of a waiver on these variables is small and few differences are significant, implying little change in household composition in response to a welfare waiver. Thus, it is unlikely that demographic change drives my findings.

Alternatively, if the bargaining power of wives in my control states changed because of expectations of a future waiver, a comparison of treated and control groups will underestimate the impact of the policy. Tables 2.4A and 2.4B therefore also contain treatment effects calculated using only eventually treated states or only never treated states as controls. The effects are roughly similar using either control group and neither group

participation. I consider this possibility in Section 2.6.

exhibits effects consistently larger in absolute value, opposing a hypothesis of future reform influencing current bargaining power.

To further analyze the impact of expectations, one can look at how treatment effects evolved over time in the states that first implemented AFDC waivers. California, Michigan, and New Jersey all passed waivers in late 1992, and one can thus examine the treatment effects in these states over four years.[110] If as more states passed waivers, people in control states began to expect a waiver in their own state, treatment effects should fall in absolute value over time. Figure 2.2 shows that such a trend may exist.

The effect of a waiver on food for home consumption, child care, and women's clothing is larger (in absolute value) in the first year of a policy than in subsequent years. Expectations therefore may have attenuated the waiver's effect on these goods; however, none of the other expenditure categories exhibit this type of shape. Thus, the expectation of future reform in control states did not lead to falsely negative results.

Additionally, one might be concerned that the few married households who receive AFDC benefits drove the estimated changes in expenditure.[111] Although my sample selection procedure alleviates this issue, to more fully address the problem, I restricted my sample to those families containing a male in the top 90 percent of the income distribution by year and state. The effect of a waiver does not change in a meaningful way in either subsample, further supporting the irrelevance of the small portion of AFDC benefits received by married families.

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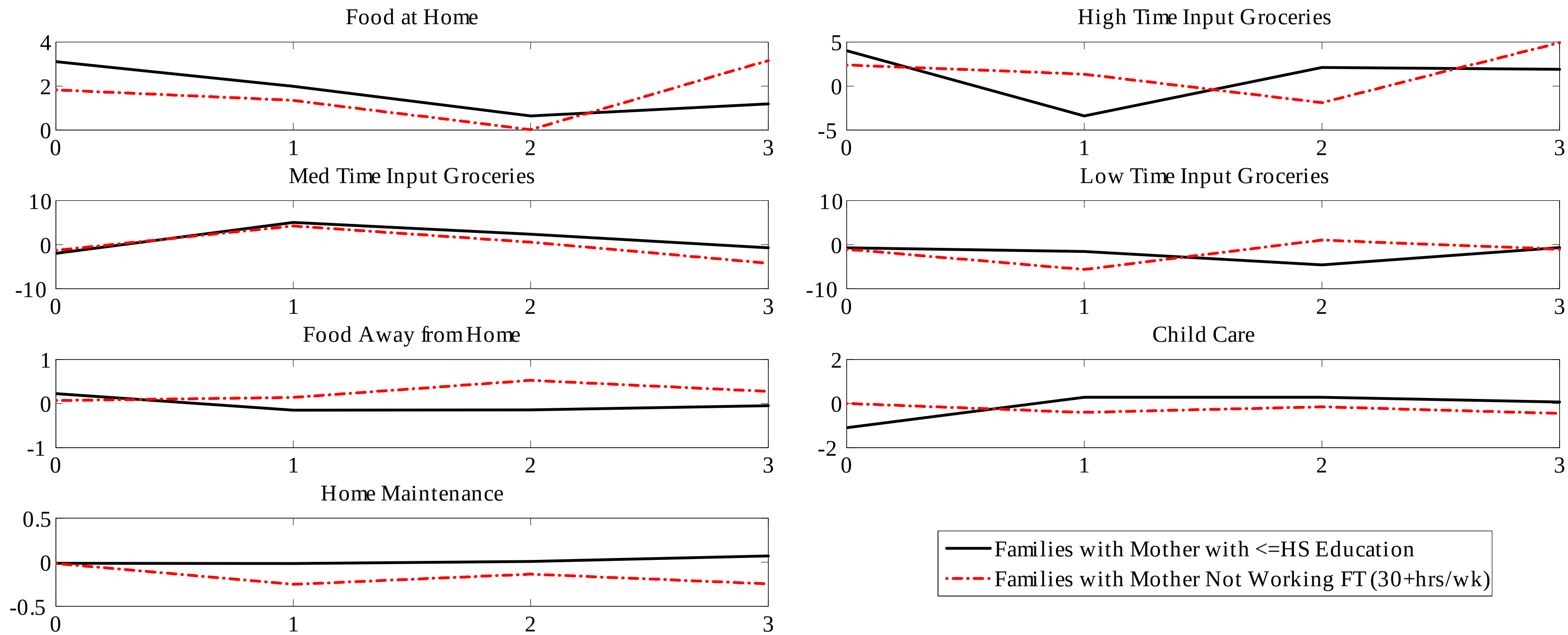
[110] The control states available for this analysis are FL, MN, NY, PA, and TX.

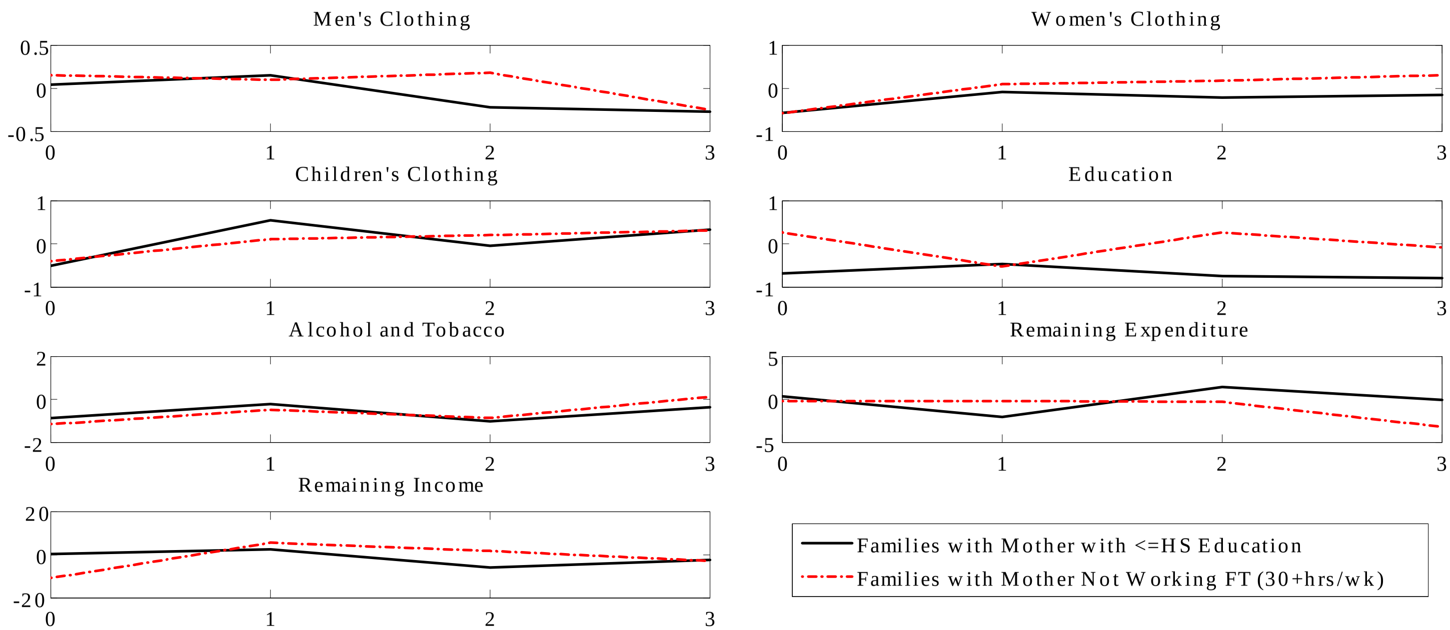
[111] A small fraction of AFDC benefits went to married parents through the AFDC-UP program. To qualify for receipt, families may not contain an adult working more than 100 hours per month. See Hoynes (1996a) for further details. I have restricted my sample to families with a male usually working 30 hours per week or more to avoid potential participation in this program.

Figure 2.2: Treatment Effect of AFDC Waiver on Expenditure Shares (x100) by Number of Years Post-Reform



Continued on the next page.

Figure 2.2, Continued: Treatment Effect of AFDC Waiver on Expenditure Shares (x100) by Number of Years Post-Reform



Notes and sources: Expenditure shares from all families from the earliest AFDC waiver implementers (CA, MI, and NJ) with biological children in the CEXI and CEXD samples. See Appendix B.1.1 for details. The denominator of the share is the quarterly, BLS-reported total expenditure for all goods, except the grocery variables. These shares are weekly shares of all grocery expenditure. A synthetic control group for each treated state and expenditure category was chosen based on pre-treatment expenditure, family size (three, four, five, or six or more members), mother's labor force participation (FT, PT, NW), education (five categories for each spouse), income (female earned, male earned, and unearned), race (a dummy variable for both spouses being white), urban location, and having children aged zero to six, seven to 12, and 13 to 17, using Abadie, Diamond, and Hainmueller's (2010) method. The following control states were available for this extended-time analysis: FL, MN, NY, PA, and TX. Estimates calculated using full sample weights.

Finally, the procedure in Section 2.5.2 focused on using standard predictors of household expenditure to choose a synthetic control for each state. Although such matching may be sensible, the procedure could select treated and control states that have very different social insurance systems. Therefore, Table 2.5 contains estimates of the effect of a waiver also using the characteristics of a state's (pre-waiver) welfare program as predictors of household expenditure.[112]

The results again show strong evidence for an increase in expenditure on food for home consumption in response to a welfare waiver (about 2 percent of the family's budget) and weaker responses for women's clothing and child care (marginally significant decreases of 0.3 and 0.4 percentage points respectively for mothers not working full-time in the market). One sees no change in the home production placebo category, men's clothing, or children's clothing, though expenditure not accounted for by my categorization does decrease for less-educated women.[113]

Overall, using either set of variables to find the best synthetic control leaves one with a very similar conclusion: welfare waivers impacted some of the decisions that married families make. The Nash-bargaining framework of Section 2.2 then implies that threat-points related to divorce have an effect on the division of household utility.

[112]The predictors are family-size adjusted average AFDC benefits (US Census 1984-1996), maximum EITC for a mother of two (US House Ways and Means Committee 1984-1996, Tax Policy Center 2010), the proportion of unmarried women with children receiving AFDC, the proportion of unmarried women with children living below the poverty line, and the proportion of unmarried women with children living below the poverty line who receive AFDC (1990 Census). The final three measures are included both for all unmarried mothers and for those with a high school education or less.

[113]Similarly, one can vary the vector of controls used to find the synthetic control group, using either fewer demographic variables or higher moments of the demographic variables. The results remain the same in both cases. Thus, the estimates are not sensitive to the precise choice of the matching vector used in the synthetic control procedure.

Table 2.5: Synthetic Control Estimates of the Effect of AFDC Waivers

Robustness to Matching on the Characteristics of the State Welfare System

Category	Families with Mother with ≤ HS Ed Effect	SE	Families with a Mother Not Working FT (30+hrs/wk) Effect	SE
Food for Home Consumption	2.03**	0.98	1.74***	0.63
High Time Input Groceries/All Groceries	1.10	1.87	0.27	1.76
Intermediate Time Input Groceries/All Groceries	-0.69	1.77	1.05	1.24
Low Time Input Groceries/All Groceries	-0.41	1.44	-1.32	1.76
Food Away from Home or Prepared by Others	0.05	0.19	-0.13	0.32
Child Care	-0.49	0.50	-0.42*	0.23
Home Maintenance Services	0.00	0.04	-0.08	0.14
Men's Clothing, Accessories, and Personal Care	0.24	0.15	0.00	0.18
Women's Clothing, Accessories, and Personal Care	-0.25	0.16	-0.29*	0.15
Children's Clothing and Accessories	0.05	0.14	-0.10	0.26
Education	-0.02	0.41	-0.39	0.37
Alcohol and Tobacco	-0.10	0.17	-0.21	0.32
Residual Expenditure	-1.50**	0.75	-0.13	0.93
Residual Income	10.16	7.93	7.63	12.35

Notes and sources: The denominator of the share is the quarterly, BLS-reported total expenditure for all goods, except the grocery variables. These shares are weekly shares of all grocery expenditure. Data comes from selected treated and control states in CEXI and CEXD, 1984-TANF implementation. See Appendix B.1.1 for details. A synthetic control group for each treated state was chosen using Abadie, Diamond, and Hainmueller's (2010) method and pre-treatment expenditure shares, family size (three, four, five, or six or more members), mother's labor force participation (FT, PT, NW), education (five categories for each spouse), income (female earned, male earned, and unearned), age of both spouses, race (a dummy variable for both spouses being white), urban location, having children aged zero to six, seven to 12, and 13 to 17, and characteristics of the state social insurance programs: average AFDC benefits (Census 1984-1996) adjusted for the number of children reported by single mothers and year effects (1980 and 1990 Censuses), maximum EITC for a mother of two (US House Ways and Means Committee 1984-1996, Tax Policy Center 2010), the proportion of unmarried women with children receiving AFDC, the proportion of unmarried women with children living below the poverty line, and the proportion of unmarried women with children living below the poverty line who receive AFDC (1990 Census). The final three measures are included both for all divorced mothers and for those with a high school education or less. Estimates calculated using full sample weights. For a two-tailed test at significance-level.05, the conservative Bonferroni-corrected size of the test (to account for Type I error and the large number of expenditure categories) is 0.004. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

2.5.4 Placebo Tests

In addition to the above analysis, three placebo tests indicate that my results are not spurious. First, I considered the impact of welfare waivers on couples without children. As the receipt of AFDC both before and after the policy change was largely contingent on parenthood, families without children should be less sensitive to welfare policy. I also estimate the impact of welfare waivers on mothers with a college degree. Such women could potentially become eligible for welfare but likely would not need the subsidy due to high levels of human capital. As both groups of women should not expect to receive welfare, changes in welfare policy should affect neither their outside options nor their expenditure decisions within marriage. I also utilize the more standard placebo test of dating a waiver prior to its actual occurrence.[114] Table 2.6 contains the "treatment effects" for these analyses.

The estimates are very different from those found using the actual treated populations and periods. Most are both small and insignificant but three exceptions emerge: married women without children substitute away from groceries requiring a lot of time to produce a final meal and women with more education spend less on child care and men's clothing after a waiver. If anything, two of these effects suggest that female bargaining power may have been increasing in the states that implemented welfare waivers; however, the negative coefficient on child care does call the results for this category into some question. Together, these tests generally suggest that the changes discussed in Section 2.5.2 are the result of welfare reform and not some other shift in supply or demand.

[114] I focus on dating the reform two periods prior to its occurrence both because the treatment effects previously calculated use an average of effects taken over two periods and because the synthetic control estimator preforms better with more pre-treatment data available. Alternate choices of a placebo period yield similar, though noisier, results. I report effects for all married families with biological children and a full-time-working father; my smaller subsamples yield largely similar effects.

Table 2.6: Synthetic Control Estimates of the Placebo AFDC Waivers on Expenditure Shares (x100)

	Married Women without Children		All Families with Biological Children, Mother with College Education or More		All Families with Biological Children, Dare Waiver Two Years Prior to Occurrence	
Category	Effect	SE	Effect	SE	Effect	SE
Food for Home Consumption	0.47	0.41	0.25	0.44	0.32	0.50
High Time Input Groceries/All Groceries	-4.22**	1.80	-2.18	3.03	-0.05	0.95
Intermediate Time Input Groceries/All Groceries	2.49**	1.26	2.95	1.96	0.39	0.87
Low Time Input Groceries/All Groceries	1.73	1.82	-0.77	3.37	0.02	0.52
Food Away from Home or Prepared by Others	-0.39	0.30	-0.59	0.40	-0.06	0.90
Child Care	0.01	0.01	-0.73**	0.35	-0.38	0.52
Home Maintenance Services	-0.02	0.08	-0.06	0.12	-0.43	0.42
Men's Clothing, Accessories, and Personal Care	-0.24	0.12	-0.37**	0.17	0.18	0.29
Women's Clothing, Accessories, and Personal Care	-0.17	0.24	-0.23	0.21	-0.16	0.17
Children's Clothing and Accessories	0.06	0.05	-0.02	0.16	10.2	8.04
Education	-0.20	0.55	-0.01	0.39	1.67	5.46
Alcohol and Tobacco	-0.10	0.21	0.10	0.18	-6.16	5.16
Residual Expenditure	0.56	0.64	1.68*	0.88	-5.18	12.1
Residual Income	2.42	3.25	9.40	3.59	-5.45	6.11

Notes and sources: The denominator of the share is the quarterly, BLS-reported total expenditure for all goods, except the grocery variables. These shares are weekly shares of all grocery expenditure. Data come from families with biological children, 1984-TANF implementation. See Appendix B.1.1 for details on data selection. A synthetic control group for each treated state was chosen based on pre-treatment expenditure, family size (three, four, five, or six or more members), mother's labor force participation (FT, PT, NW), education (five categories for each spouse), income (female earned, male earned, and unearned), age of both spouses, race (a dummy variable for both spouses being white), urban location, and having children aged zero to six, seven to 12, and 13 to 17, using Abadie, Diamond, and Hainmueller's (2010) method. Estimates calculated using full sample weights. If For a two-tailed test at significance-level .05, the conservative Bonferroni-corrected size of the test (to account for Type I error and the large number of expenditure categories) is 0.004. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

2.6 Alternative Explanations

Although small in absolute terms, the effects estimated in Section 2.5 are large when compared to the proportion of expenditure usually allocated to most categories. I consider some alternative explanations for the shifts in expenditure to allay any doubts about the channel through which the effect operates.

If welfare waivers caused women to change their labor force participation, one cannot attribute the changes in expenditure to a reduction in women's outside options.[115] In particular, one would expect decreases in expenditure on clothing and increases in home production if waivers led currently married women to work less in the market.[116] Further, policies impacting the labor supply of one group can influence others through competition in the labor market.[117] If married women are better substitutes for single women than are single or married men, increases in the labor force participation of single women after welfare reform could lower wives' relative wages, leading women to have less bargaining power through a different channel.

Table 2.7 thus contains estimates of the impact of AFDC reform on the labor force participation, log yearly hours, and log hourly wages of a sample of married women with children from the March Current Population Survey (CPS, see Appendix B.1.3 for details), calculated using both the synthetic control method and the traditional DD approach.[118] The

[115]Note that one expects a woman to take less leisure once her bargaining power decreases; however, without knowing family members' preferences for home and market goods and the relative disutility of home and market work, it is unclear whether women will increase or decrease their market hours of work in response to a change in bargaining power. Despite this issue, if waivers changed the number of hours married mothers work in the market, this too would provide evidence that outside options have an effect on intact marriages.

[116]See Hurst (2008).

[117]See Rothstein (2008).

[118]The control variables in both procedures include a quadratic in potential experience interacted with six categorical variables for education, and indicators for living in a rural area, being black, being Hispanic, and having two, three, or four or more children. I also include year fixed-effects, state fixed-effects, and state-specific time trends in the DD regression. DD regressions of indicator variables are estimated using a

Table 2.7: DD and Synthetic Control Estimates of Treatment Effect of AFDC Waiver on Other Variables

	Families with Mother with $\leq$ HS Ed				Families with Mother Not Work FT (30+hrs/wk)			
	DD		Synthetic Control		DD		Synthetic Control	
Dependent Variable	Effect	SE	Effect	SE	Effect	SE	Effect	SE
Work Full-Time, Full-Year*	0.027	0.048	0.024	0.097				
Work Any Hours in Year*	0.053	0.034	0.041	0.036	0.024	0.045	0.038	0.024
Log Annual Hours	-0.015	0.018	0.018	0.131	0.086	0.062	0.109	0.076
Log Hourly Wage	0.062	0.077	-0.048	0.046	0.030	0.065	-0.043	0.168
Husband's Log Hourly Wage	-0.004	0.050	-0.060	0.110	0.004	0.040	-0.006	0.049
Any Food Stamps Last Year*	0.015	0.020			0.002	0.005		
Log Food Stamps Received	0.131	0.166			-0.271	0.187		

Notes and sources: Data come from married families with children in the March CPS, 1984-TANF implementation. See Appendix B.1.3 for details on data selection. A synthetic control group for each treated state and variable was chosen based on pre-treatment variable values, and controls for number of children (two, three, or four or more), education (six categories), a quadratic in potential experience interacted with education categories, age, race (dummies for black and Hispanic), and urban location, using Abadie, Diamond, and Hainmueller's (2010) method. DD regressions include these controls (except pre-treatment values), as well as time fixed-effects and state-specific quadratic time trends. Food stamp estimates include person-specific variables for both spouses. Synthetic cohort estimates are omitted due to small sample size. Estimates calculated using full sample weights by person (and by family for food stamp estimates).

*DD effects are marginal effects from a probit specification for binary variables. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

results suggest that changes in wages or labor force participation cannot explain the findings in Section 2.5. If anything, currently married women may work slightly more after a welfare waiver. Such an effect should lead to a decrease in home production, whereas the expenditure patterns in the CEX suggest an increase. Additionally, the estimates do not indicate any change in married mothers' wage rates and thus do not suggest other changes in bargaining power within the family.

As the strongest and most robust results from Section 2.5 involve food expenditures, one might also be concerned that food-related policies, instead of welfare waivers, drove the estimated changes. In particular, a change in the provision of food stamps to married families could cause the reported changes. But only 4 percent of the families in my CEX samples receive these benefits, limiting the potential importance of this mechanism. Further, Table 2.7 also contains estimates of the change in food stamp receipt (at any point in the year) and log food stamp benefits associated with a welfare waiver.[119] The results are noisy but provide little evidence that food-related policies could have driven the changes estimated in Section 2.5.

2.7 Welfare and the Married Household

The results of this analysis indicate that changes in welfare generosity affected the consumption patterns of married households. In response to a welfare waiver, selected subgroups of married families devoted about 2 percent more of their budget to food purchased for consumption at home and approximately 0.4 percent less of their budget to

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probit model.

[119]The control variables include indicators for having two, three, or four or more children, living in an urban area and, for each spouse, indicators for being black and being Hispanic and a quadratic in potential experience interacted with six categorical variables for education. I also include year fixed-effects, state fixed-effects, and state-specific time trends. DD regressions of indicator variables are estimated using a probit model. One cannot exclude families with part-time- or non-working husbands in these regressions because very few of the remaining families in my sample received food stamps. Further, the lack of families receiving food stamps prohibits estimation using the synthetic control procedure.

both child care and women's clothing. Back of the envelope calculations further suggest that these two-parent families reallocate 0.1 to 0.2 percent of expenditure in response to a 1 percent decrease in the average payout a wife would receive if divorced and permanently poor. The types of expenditure sensitive to welfare reform also suggest that this redistribution decreases the relative utility of wives.

Both standard DD estimation and a synthetic control approach designed to select the most suitable control group yield similar changes in expenditure patterns. Estimates are robust to a wide variety of specifications. Additionally, changes in female labor force participation, women's wages, family food stamp receipt, and the composition of married families cannot explain the effect that welfare policy has on expenditure. Together, the evidence strongly suggests that welfare waivers caused changes in the choices made within intact marriages. That is, outside options matter inside marriage.

In addition to evaluating the impact of AFDC on a group of people previously assumed unaffected by the program, this paper more broadly contains a test of unitary and collective models of the family. It produces results that refute the classic unitary model, showing that multiple preference relations must be considered in analyzing the household. Although many studies have shown this result using exogenous income shocks to either married partner *within* marriage (e.g., Bobonis 2009; Lundberg, Pollak, and Wales 1997; Phipps and Burton 1998; and Ward-Batts 2008) this analysis shows that a shock to potential utility *outside* of marriage impacts expenditure within married families. Thus, I am able to conclude that couples bargain with reference to divorce, a common – but rarely tested – assumption.

3 The Impact of Legal Abortion on the Wage Distribution: Evidence from the 1970 New York Abortion Reform

3.1 Introduction

A woman's right to terminate a pregnancy is a contentious and politically charged subject. Most argue about abortion rights based on moral or emotional grounds. Despite the plethora of arguments for and against abortion, we know little about how abortion's legalization changed the characteristics of cohorts born in the United States. When women can legally and safely terminate pregnancies, the group of mothers giving birth may change, leading to changes in cohort attributes.

On July 1, 1970, New York became the first state in the US to allow women access to abortion on demand until the 24th week of pregnancy. In this paper, I examine the extent to which this major change in reproductive rights impacted the characteristics of mothers giving birth in New York, their newborns, and these children's future wages. If mothers used abortion to more optimally time their births, the group of women delivering babies after abortion became legal will be positively selected. This selection could then lead to improved welfare among cohorts in adulthood.

To demonstrate that abortion access led to a sharp change in the characteristics of pregnancies carried to term, I first analyze natality data from the National Center for Health Statistics. This dataset contains information on date of birth and gestational duration, allowing me to determine the end of a woman's first-trimester of pregnancy. If a woman ended her first trimester after New York's reform, I assume she had access to legal abortion. The results indicate that access to abortion led to smaller cohorts characterized by proportionately fewer low-weight and African American births, mothers with more education, and increased data on paternal characteristics (a proxy for father's involvement). These changes suggest that mother's access to abortion may have impacted childhood living conditions (and thus long-run, adult outcomes).

To examine if changes in the characteristics of mothers and births yielded changes in child outcomes, I use wage data from native-born New Yorkers in the 2005 to 2010 American Community Surveys (ACS). Although this data does not allow one to calculate precise dates of conception or birth, I can use age (in whole years), survey year, and quarter of birth to estimate mother's abortion access. I thus compare the wages of individuals reporting the same age in the same survey year, but with different quarters of birth, implying that their mothers were at different stages in pregnancy when abortion was legalized in New York. Assuming that quarter of birth influences wages in the same way across age-survey year pairs, this procedure estimates the effect of abortion's legalization on the eventual wages of children carried to term.[120]

Using the constructed likelihood of mother's abortion access, I find that the wages of native-born New Yorkers whose mothers had access to legal abortion are higher than the wages of those in utero when abortion was illegal. In particular, mother's abortion access is associated with higher wages for black and Hispanic workers. Further, mother's access implies increases in the bottom quantiles of the wage distribution. Access does not imply large or significant changes in the upper-half of the wage distribution or the wages of whites. Together with the results from the natality data, these findings imply that abortion access quickly changed the selection of women into motherhood and thus the characteristics of birth cohorts.

Although a comparison of adults born within a few months of each other implies substantial wage gains associated with abortion access, comparing more broadly spaced cohorts yields smaller, but still significant, effects of abortion. This suggests potentially important medium- and long-term effects of abortion's legality. Given time, women could

[120] See Buckles and Hungerman (2010) on the relationship between birth quarter and socioeconomic characteristics.

react to abortion's legalization by altering their contraceptive behavior. Social norms may also slowly change in response to abortion reform (c.f., Akerlof, Yellen, and Katz 1996).

Beginning with Donohue and Levitt (2001), economists have debated the relationship between abortion and crime.[121] Additional work has focused on the impact of abortion on child and young-adult outcomes.[122] This work adds to the literature on abortion's effect on cohort-level characteristics in several ways. First, past work does not consider how abortion reform shaped cohorts at later ages.[123] By analyzing cohorts in adulthood, I can determine how abortion's legalization affected cohorts in the long run.

Additionally, most past analyses of the impact of abortion legalization compare children born in states that first legalized abortion to those born in states where the Supreme Court's ruling on *Roe v. Wade* later gave women access (c.f., Gruber, Levine, and Staiger 1999). But the states that legalized abortion before *Roe v. Wade* are non-random and non-representative of America.[124,125] The comparison might also misrepresent the effect of abortion by assuming that women had access only if they lived in a state where abortion was legal. Indeed, many women from other states gained access to abortion by traveling to New York.[126] By using only native New Yorkers in my analysis, I can be agnostic about geographic variation in access to abortion.

Finally, the literature on abortion generally focuses on the average marginal effect of

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[121]For details on this debate, see Charles and Stephens (2006), Donohue and Levitt (2001, 2004a, 2004b, 2008), Dills and Miron (2006), Dills, Miron, and Summers (2008), Foote and Goetz (2008), and Joyce (2004a, 2004b, 2009, 2010).

[122]See Ananat, et al. (2010), Gruber, Levine, and Staiger (1999), and Pop-Eleches (2006).

[123]Of course, cohorts exposed to legal abortion in the United States are only now of an age where wages represent a reasonable proxy for permanent income.

[124]Alaska, California, Hawaii, New York, and Washington legalized abortion on demand in 1970; all other states legalized abortion in 1973.

[125]Other analyses instead compare the outcomes of those born in states with higher or lower take-up of abortion, potentially leading to endogeneity and omitted variable biases (c.f., Donohue and Levitt 2001).

[126]See Joyce, Tang, and Zhang (2011) and Levine, et al. (1999).

abortion access; however, evidence on the illegal and legal use of abortion suggests that the effect of reform may vary with race and socioeconomic status. To more completely consider the relationship between abortion and child outcomes, I use quantile regression to analyze changes throughout the wage distribution. I also examine abortion's effect within racial groups.

I begin my analysis by reviewing the history of abortion in New York and explaining why this state provides an ideal natural experiment to study the effects of abortion legalization. Next, I briefly outline why one might expect a relationship between abortion access and cohort characteristics. Analysis of both birth certificate and wage data then suggests that the composition of expectant mothers, and thus the characteristics of children carried to term, changed when abortion became legal. Finally, I demonstrate that abortion reform in New York also caused increases in the wages of those born elsewhere in the United States.

3.2 History of Abortion in New York

Throughout history, many different laws and norms allowed a woman to limit her fertility after conception. In early America, English common law permitted abortion prior to a fetus's "quickening" (approximately the end of a woman's first trimester).[127] Most scholars indicate that midwives used many techniques to terminate pregnancies.[128,129] Despite this tradition, New York became the first state to outlaw abortion in 1826 (David, et al. 1988). By 1900, the rest of America had followed.

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[127] Even if abortion was illegal, no conclusive test for pregnancy before quickening existed until 1926, making many abortifacients *de facto* legal. For details, see Dellapenna (2006).

[128] Common techniques include the administration of abortifacients and injury of the mother (Dellapenna 2006).

[129] See Garrow (1998) for details on the majority opinion on *Roe v. Wade* and its historical discussion of abortion prior to quickening. Dellapenna (2006) provides an alternate historical analysis.

Limited evidence exists on the nature or number of illegal abortions during the period of restriction. Lee (1969) provides a rare glimpse into illegal abortion in an interview study of 114 women who purposefully terminated a pregnancy in the 1950s or 1960s, before any state legalized abortion on demand. The women in the sample received abortions from a variety of different practitioners, including licensed doctors in the US and abroad, doctors whose licenses had been revoked, dentists, nurses, and a chiropractor. The study indicates that although illegal abortions were obtainable (particularly for better-educated and higher-income women), the procedures were both dangerous and costly. On average, the women in Lee's study paid $488 for an abortion and over one-third of them had to travel over 250 miles. Almost 20 percent of the women in the study experienced a medical crisis after their abortion and 28 percent reported great pain during or after the procedure. Popular publications further indicate that some physicians were known to have robbed or sexually abused women seeking illegal abortions (c.f., Reuben 1969).

The nature of abortion in America changed on April 11, 1970, when New York became the first state to allow women legal access to abortion on demand (up to the 24th week of pregnancy, see Garrow 1998).[130] Infrastructure in public and private hospitals and clinics quickly increased to meet the demand for abortion. On July 1, 1970, doctors performed the first legal procedures.

Both national and local media coverage ensured that women all over the United States knew they could come to New York to have an abortion. The reform received much attention in the *New York Times* and in nationally circulated women's magazines (e.g.,

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[130] Formally, the law stated that "An abortional act is justifiable when committed upon a female with her consent by a duly licensed physician acting (a) under reasonable belief that such is necessary to preserve her life, or (b) within 24 weeks of the commencement of her pregnancy" (Planned Parenthood–World Population 1972, p. 1). Although the gestational limitation of abortion was extended to 24 weeks to make the bill less appealing to swing voters, the New York legislature passed the bill by a single vote when Rep. Michaels declared: "I realize, Mr. Speaker, that I am terminating my political career, but I cannot in good conscience sit here and allow my vote to be the one that defeats this bill. I ask that my vote be changed from 'no' to 'yes'" (Garrow 1998, p. 420).

Cosmopolitan). To inform women of their new options, organizations handed out pamphlets with titles such as "If You Want an Abortion..." both in New York and out-of-state (Brody 1970, *New York Times* 1970). Advertisements for abortion referral services and packages ranged from informative ads in national magazines warning women of profit-seeking doctors to a "banner drawn by a blimp flying over a beach in Miami" (Edmiston 1971, p. 42).[131]

The total cost of an abortion declined rapidly after legalization, particularly for poorer and less-educated women. The cost of abortion during one's first trimester ranged from zero to 200 dollars in most New York hospitals and clinics, with fee reductions based on income.[132] Moreover, early legal abortions were exceptionally safe. By the second half of 1971, the complication rate had dropped to 0.57 percent, from an initial 1.27 percent in 1970 (Planned Parenthood–World Population 1972). Maternal mortality in New York also decreased by 40 percent from 1970 to 1971 (Pakter and Nelson 1971).

Figure 3.1 shows the number of abortions performed in New York City over time. The trend suggests that the news coverage, easy access, and low costs of abortion culminated in a surge in the number of procedures.[133] By 1971, over 200,000 abortions were performed in New York City alone, implying that there were 447.7 abortions in New York for every

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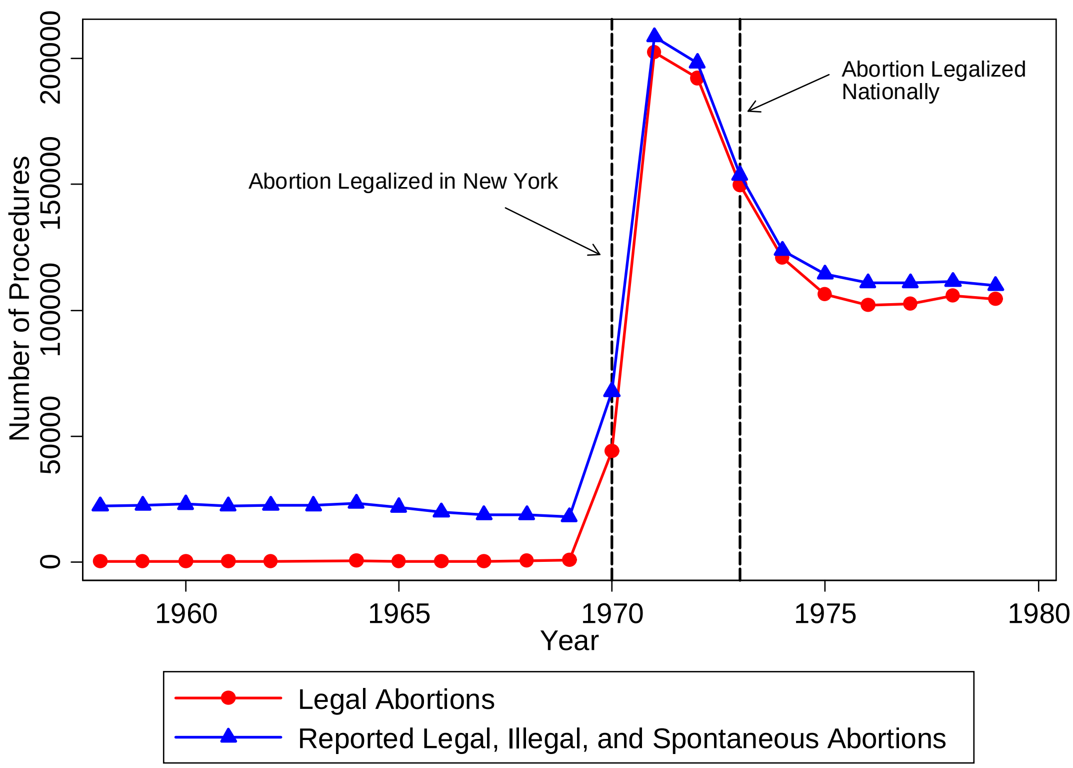
[131] An example of the former appears in *Ms. Magazine*'s November 1972 issue. It states: "The main difference between a $150 abortion and a $1000 abortion is that the doctor makes an extra $850," and further lists contact information for a free referral service.

[132] Medicaid covered 84% of first-trimester abortions in New York City's municipal hospitals from July 1, 1970 to March 31, 1971. Second-trimester abortions were less common and more costly, ranging from $350 to $450 (Pakter and Nelson 1971).

[133] This figure also contains the total number of legal, illegal, and spontaneous abortions by year in New York City. Spontaneous abortions (miscarriages) make up the bulk of such procedures prior to 1970 and are included in the figure if reported to the Department of Health by a hospital. If a woman miscarried at home, the lost pregnancy would not be reflected in this figure. Although detailed records are not available, work by Lee (1969) suggests that many recorded miscarriages were actually illegal abortions. Many abortionists practiced catheterization, a procedure that began with the abortionist but was completed in a hospital and looked like a miscarriage. Additionally, many women had to be hospitalized after having an illegal abortion. If the abortion was incomplete at the time of hospitalization, but it was not completely evident that the woman had purposely induced the abortion, such an event would be counted as a spontaneous (and not illegal) abortion.

Figure 3.1: Number of Abortions Preformed in New York City



Source and Notes: NYC Department of Health and Mental Hygiene, *Summary of Vital Statistics, 1961-1980*. Spontaneous abortions include reported and medically documented miscarriages.

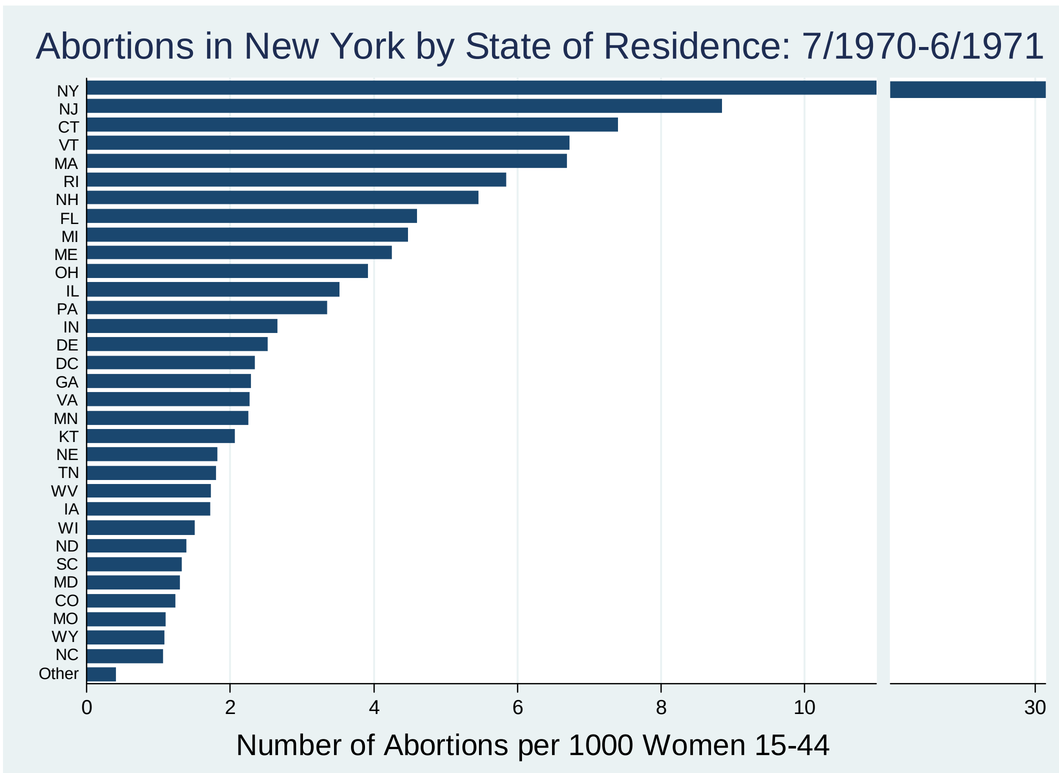
1,000 live births (Pakter and Nelson 1971). Work by Levine, et al. (1999) suggests that these abortions implied relatively small, though non-trivial, changes in fertility.

The characteristics of women receiving abortions in the year following New York's legalization suggest that women used abortion to better-time their pregnancies and prevent pregnancies later in life. Pregnant women over 35 and under 20 utilized abortion most heavily (Pakter and Nelson 1971), comprising 10 and 25 percent of the women receiving abortions respectively (Planned Parenthood–World Population 1972). Women who sought abortions were also disproportionately African American. Although black women gave birth to 31.2 percent of children in New York City in 1971, they had 42.8 percent of all abortions in the first year of legality (amounting to 563 abortions per thousand live births). Hispanic women had far fewer abortions in the same period (only 250 per 1,000 live births), likely because of their Catholic roots. White women had a rate in between (Pakter, et al. 1973).[134]

During the first year of legalization, most women seeking abortions in New York City were not city residents. Women from other states received 44.5 percent of the 215,000 abortions performed in New York State during this first year (see Pakter, Harris, and Nelson 1971, Planned Parenthood–World Population 1972). The strong out-of-state demand can be seen in various different ways. As depicted in Figure 3.1, the number of abortions in New York declined in 1973, when the Supreme Court's ruling on *Roe v. Wade* allowed women to obtain procedures closer to home. Figure 3.2 demonstrates the rate at which women in other states received abortions in New York. Although New Yorkers certainly had the most abortions in New York, more than 0.5 percent of women age 15 to 44 living in New Jersey, Connecticut, Vermont, Massachusetts, Rhode Island, and New Hampshire traveled to New York from July 1, 1970 to June 30, 1971 to receive a legal abortion. Finally, Joyce, Tang, and Zhang (2011) demonstrate that New York's legalization impacted fertility

[134]According to Jones and Kooistra (2011), similar abortion rates prevailed in New York during the 2000s.

Figure 3.2: New York Abortions in the First Year of Legalization



Source: Number of abortions from Planned Parenthood–World Population (1972). Population data from 1970 IPUMS Census sample.

rates in many communities throughout the country. These facts suggest that reform in New York led to major changes in access to abortion for women across the United States.

3.3 Abortion, Selection, and Cohort Characteristics

I consider how an increase in abortion access could lead to changes in cohort characteristics using a framework similar to those of Ananat, et al. (2009) and Kane and Staiger (1996). In this simple model, a woman faces two sequential decisions. She first chooses her contraceptive behavior, implying the probability that she becomes pregnant. If conception occurs, a woman can choose to abort or carry her pregnancy to term.

As the cost of abortion decreases, more pregnant women will have abortions. In particular, abortion rates will increase among mothers who became pregnant but would have chosen to avoid conception if free and highly effective birth control was available. That is, when the cost of abortion decreases, a woman's average expected utility from having a child increases, suggesting more positive selection of mothers.

However, this prediction is complicated by other mechanisms. Women will also decrease contraceptive use in response to a decrease in the cost of abortion, resulting in more pregnancies. The additional women who choose to become pregnant when the cost of abortion falls will have lower expected gains from having children, leading the group of pregnant women to become more negatively selected. The data suggests the importance of this channel. Though over 25 percent of New York pregnancies ended in abortion in the year after legalization, Levine, et al. (1999) find a much smaller drop in cohort size associated with reform.

The change in abortion law in New York led to increases in access to abortion but differential changes in costs for women of different means. Expanding the model to allow women of different backgrounds to experience different changes in the total cost of abortion further obscures the implications of reform for the characteristics of mothers carrying their pregnancies to term.

Altogether, the model suggests that when the cost of abortion drops, pregnant women become more negatively selected but more positive selection occurs between pregnancy and childbirth. Therefore, abortion reform could lead to overall positive or negative selection of mothers. If mothers become more positively selected with respect to income, maturity, or time available to devote to childrearing, one would expect increases in the wages of children carried to term.

Although changes in selection into pregnancy and motherhood are likely the main mechanisms behind abortion's effect on eventual wages, other forces could also lead abortion legalization to impact wages. Donohue and Levitt (2001) suggest that changes in cohort size could imply changes in average cohort outcomes. Additionally, Akerlof, Yellen, and Katz (1996) suggest that abortion access may have led to important changes in social norms or marriage markets.[135] This too could lead mothers' abortion access to influence the wages of their grown children. The data needed to address if such changes occurred quickly enough to be relevant for this study does not exist; however, these mechanisms are probably not highly relevant to identification strategies, like mine, that compare workers born within months of each other.

3.4 Immediate Effects of Abortion Legalization

This study first uses the National Center for Health Statistics Vital Statistics Natality Birth Data (NCHS) to determine how mother's abortion access changed the size of cohorts and the attributes of mothers and their newborns.[136] The NCHS contains both date of birth and weeks of gestation, allowing one to closely estimate the week a woman became

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[135] Also, see Goldin and Katz (2002) on how giving better reproductive control to a limited group of women may have important spillover effects.

[136] See Appendix C.2 for details.

pregnant, and thus the final week of her first trimester. If the mother of a baby completed her first-trimester after July 1, 1970 (when the New York law went into affect), I consider her to have had access to legal abortion ($abort\_access$). I similarly determine mother's access to oral contraceptives at conception ($pill\_access$). I select all births from this dataset where gestational age implies a woman became pregnant between April 1968 and March 1972.

First, I use a differences-in-differences approach by year and month to determine the effect of mother's access to abortion. I estimate regressions such as

$$Y_{iym} = \alpha_y + \gamma_m + \beta_B(abort\_access_{iym}) + \phi_B(pill\_access_{iym}) + \varepsilon_{iatq}, \tag{3.1}$$

where Y is a given outcome at birth and α and γ are year and month of conception fixed-effects.

Alternatively, one can use even finer variation in date of conception with a regression discontinuity (RD) approach. Because this technique uses very closely spaced births, it has the added advantage of estimating a relatively pure selection effect of abortion.[137]

My RD design compares the outcomes of eventual births to mothers in their 10th week of pregnancy when abortion became legal to those in their 14th week of pregnancy at the time of legalization.[138] Although both groups of women could have received a legal abortion, the cost to the woman only slightly later in pregnancy was far higher. Until a woman's twelfth week of pregnancy, doctors could use dilation and curettage (D and C) or suction to terminate a pregnancy. Gutcheon (1973) compares the trauma of these out-patient procedures to having one's wisdom teeth removed. If a woman was more than 12 weeks pregnant, physicians could not use these simpler procedures. Instead, doctors

[137]Cohort size should not influence characteristics at birth. Moreover, social norms and contraceptive use likely did not change over the extremely short horizon used in this analysis.

[138]I omit women in weeks 11 through 13 of pregnancy due to potential measurement issues or errors in calculating pregnancy length.

would generally make a women wait until the fetus was slightly more developed, so they could induce a still-birth using an in-patient procedure known as "salting out" or saline induction. Such procedures were far more financially, physically, and mentally costly.[139]

My RD estimator calculates the effect of abortion legalization on the characteristics of births in the NCHS using

$$\beta_{RD} = \lim_{x \downarrow 7/1/1970} E[Y_i | X = x] - \lim_{x \uparrow 7/1/1970} E[Y | X = x] \qquad (3.2)$$

where X is the (medically-determined) final date at which a doctor could perform an early-pregnancy abortion. I estimate the two limit functions using both split-side polynomial regression and local linear regression. Because my wage analysis will compare adults by quarter of birth, I use a 12-week bandwidth. The RD results are generally robust to using multiples of this distance or the Imbens and Kalyanaraman (2009) optimal bandwidth.

Table 3.1 contains the estimated effects of abortion access and Figure 3.3 graphs the split-side polynomial RD estimates. The results suggest that children whose mothers had access to first-trimester abortion were born into different circumstances than those whose mothers did not have access. The cohort of babies conceived less than 12 weeks before New York's abortion reform is about 10 percent smaller than the cohort conceived only slightly earlier. Further, all estimates suggest that abortion access leads the proportion of a cohort that is black to decrease by between 4.6 and 9.3 percent.[140] The RD estimates also suggest that mothers with access to abortion have significantly heavier babies and are about 7 to 8 percent less likely to have a low-weight birth. Moreover, these children are born into smaller families that may have greater per capita resources.

Additionally, the socioeconomic composition of mothers giving birth changed with

[139] For details, see Gutcheon (1973).

[140] Information on Hispanic ethnicity is not available in the NCHS.

Table 3.1: Estimates of the Effect of Abortion Access from Birth Certificate Data

Panel A: All Births

Method	(1) Black	(2) Father Ed Missing	(3) Low Birth Weight	(4) Birth Weight (g)
Diff.-in-Diff.	-0.00810***	-0.0186***	-0.00157	3.450
	[0.00273]	[0.00221]	[0.00147]	[3.673]
RD: Split Polynom.	-0.0165***	-0.00887**	-0.00733**	27.14***
	[0.00447]	[0.00353]	[0.00324]	[6.825]
RD: Local Linear	-0.0162***	-0.00907***	-0.00595*	16.32*
(12 Weeks)	[0.00476]	[0.00297]	[0.00310]	[8.77]
RD: Local Linear	-0.0163***	-0.0137***	-0.00655***	19.93***
(24 Weeks)	[0.00418]	[0.00252]	[0.00208]	[5.38]
Mean	0.177	0.10	0.0849	3,235
Observations	571,406	571,406	567,464	567,464

Panel B: Black Births

Diff.-in-Diff.		-0.0342***	0.00001	-5.72
		[0.00626]	[0.00399]	[8.305]
RD: Split Polynom.		0.00438	-0.0248***	52.49***
		[0.0125]	[0.00949]	[17.30]
RD: Local Linear		0.00457	-0.0165*	30.46*
(12 Weeks)		[0.0104]	[0.00995]	[18.13]
RD: Local Linear		-0.0158**	-0.0133**	23.84**
(24 Weeks)		[0.00737]	[0.00638]	[11.85]
Mean		0.294	0.143	3,059
Observations		100,960	100,305	100,305

Continued on the next page.

Table 3.1, Continued: Estimates of the Effect of Abortion Access from Birth Certificate Data

Panel A: All Births

Method	(5) Mother's Tot. Births	(6) Mother's Yrs of Ed	(7) Mother HS Dropout	(8) Weekly Births
Diff.-in-Diff.	-0.0251**	0.0410**	-0.00826***	-326.16
	[0.0115]	[0.0163]	[0.00249]	[1,083]
RD: Split Polynom.	-0.0798***	0.0348	-0.0112**	-293.5***
	[0.0218]	[0.0283]	[0.00543]	[102.2]
RD: Local Linear	-0.0554***	0.0297	-0.00720	-108.54
(12 Weeks)	[0.0194]	[0.0305]	[0.00547]	[80.11]
RD: Local Linear	-0.0563***	0.0568**	-0.0133***	-222.40***
(24 Weeks)	[0.0147]	[0.0241]	[0.00449]	[63.01]
Mean	2.54	11.82	0.292	2,774
Observations	536,421	555,842	555,842	206

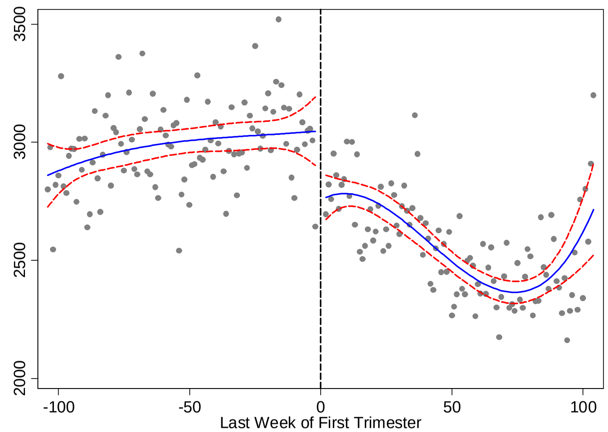
Panel B: Black Births

Method	(5) Mother's Tot. Births	(6) Mother's Yrs of Ed	(7) Mother HS Dropout	(8) Weekly Births
Diff.-in-Diff.	-0.0610*	0.0951***	-0.00855	-153.40
	[0.0316]	[0.0241]	[0.00572]	[200.5]
RD: Split Polynom.	-0.143**	0.00974	0.000434	-101.1***
	[0.0605]	[0.0556]	[0.0139]	[27.62]
RD: Local Linear	-0.0828	0.0406	0.00131	-66.03***
(12 Weeks)	[0.0646]	[0.0412]	[0.0102]	[20.31]
RD: Local Linear	-0.0955**	0.0466	0.000872	-86.40***
(24 Weeks)	[0.0422}	[0.0302]	[0.00773]	[18.23]
Mean	2.80	11.12	0.459	499.61
Observations	91,472	96,087	96,087	206

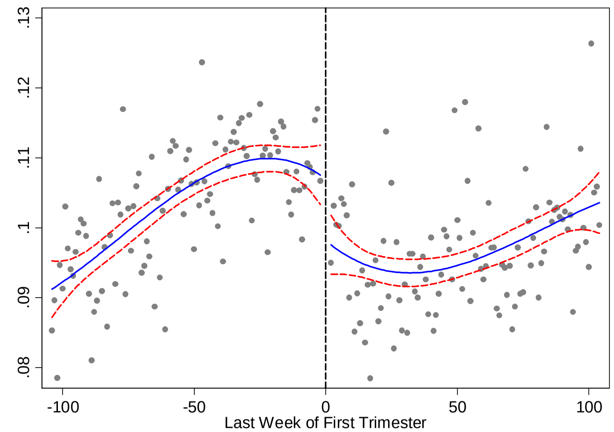
Notes and Sources: Data from NCHS Vital Statistics Natality Birth Data for mothers who are residents of New York and conceived April 1968-March 1972. Abortion access defined as a woman still being in her first trimester or not-yet-pregnant as of July 1, 1970. See Appendix C.2 for details. Mother's total births are capped at 20 or more. Difference-in-differences estimates include month of conception and year of conception fixed-effects and mother's access to the pill at conception. Standard errors clustered by month of conception. Split-side polynomial regression uses a third-order polynomial, split by treatment. Local linear regression estimated using Gaussian kernel, standard errors estimated using 2000 bootstrap replications. Robust standard errors in brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figure 3.3: The Effect of Abortion Access on Birth Outcomes

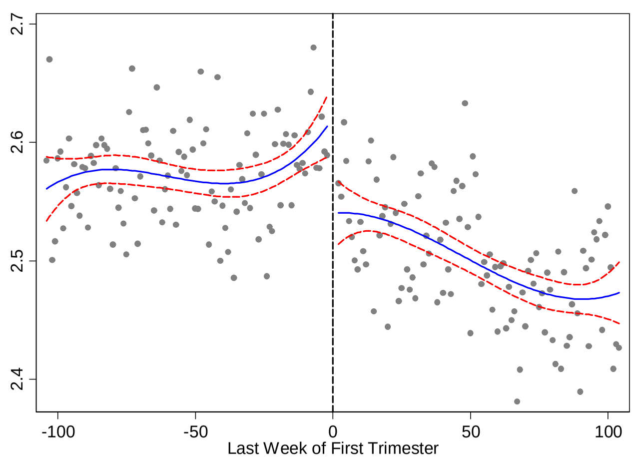
Panel A: Number of Births



Panel B: Proportion Missing Father's Education



Panel C: Number of Mother's Total Births



Panel D: Proportion Black

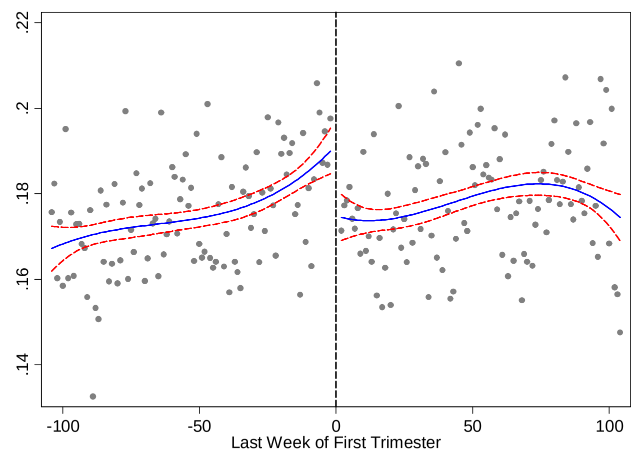


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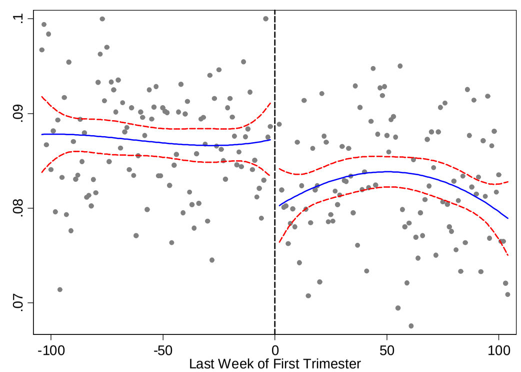
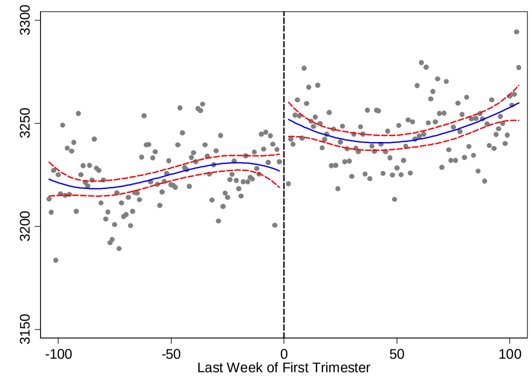
Figure 3.3, Continued: The Effect of Abortion Access on Birth Outcomes

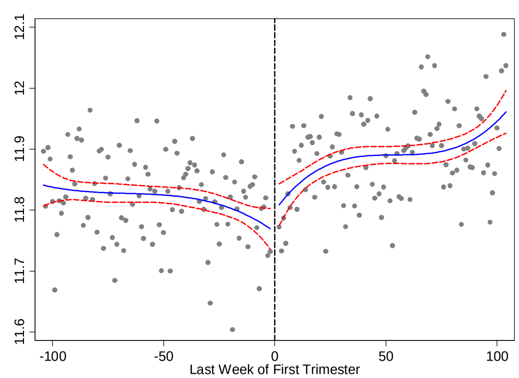
Panel E: Proportion Low Birth Weight (<2,500 g)



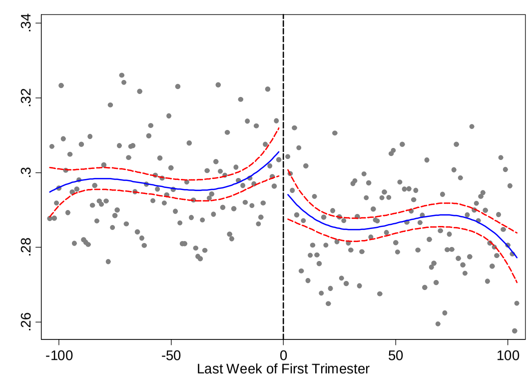
Panel F: Weight at Birth (g)



Panel G: Mother's Years of Education



Panel H: Proportion of Mother's Without High School Degree



Notes and Sources: Data from NCHS Vital Statistics Natality Birth Data for mothers who were residents of New York and conceived April 1968-March 1972. Abortion access defined as a woman still being in her first trimester or not-yet-pregnant as of July 1, 1970. See Appendix C.2 for details. Split-side polynomial regression using a third-order polynomial, split by treatment, graphed. Robust standard errors used to create 95 percent confidence intervals.

abortion reform. Women who had children after abortion became legal in New York State were more educated (significantly so in some specifications). Although New York did not collect information on mother's marital status, one can use the availability of information on a child's father to proxy for single motherhood. About 10 percent of birth records indicate valid data on a mother's education but no information for father's education. This rate of non-reporting is 0.9 to 1.9 percentage points lower in the group of mothers who had access to legal abortion, suggesting an increase in the prevalence of births within two-parent families. Despite these changes, mothers age at birth (excluded from the tables and graphs) does not change after the reform.[141] Furthermore, if one focuses on births to women living in New York City, where articles in the *New York Times* suggest abortion access increased most rapidly, similar results hold.

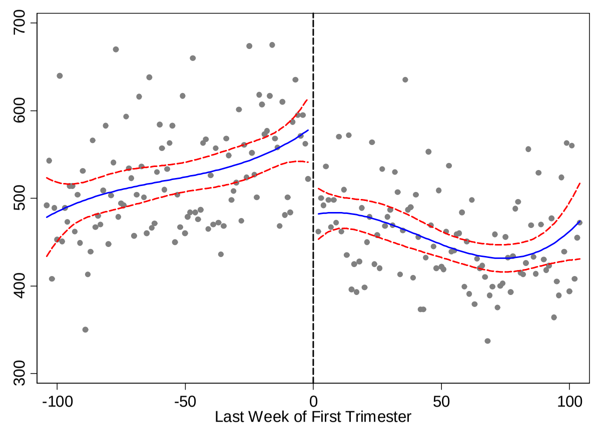
Panel B of Table 3.1 and Figure 3.4 contain the corresponding estimates within the sample of African American newborns (the NCHS does not identify Hispanic ethnicity). Cohorts of African American births shrank by as much as 20 percent after abortion's legalization. Additionally, black infants born after reform are less likely to have low weight at birth, are heavier on average, and are born into smaller families. Although these changes are large and robust to various RD specifications, changes in other variables are less clear. The average age and education level of black mothers do not change after abortion's legalization. Nonetheless, the change in birthweight suggests important unobservable variables could shift in the black families having children.

This analysis suggests a real and perceptible change in the number and characteristics of births after abortion's legalization. The birth certificate data indicate that when women gain better and safer access to abortion, the children they carry to term are better-off upon

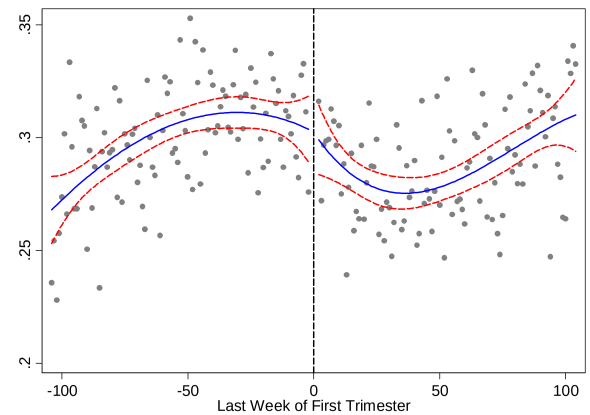
[141] Note that although pregnant teens had the highest proportion of abortions, abortion was also utilized heavily by women over 30. The proportion of babies born to mothers between 18 and 29 significantly increases with abortion reform. Additionally, the interquartile range of mother's age at birth significantly decreases.

Figure 3.4: The Effect of Abortion Access on Birth Outcomes for Blacks

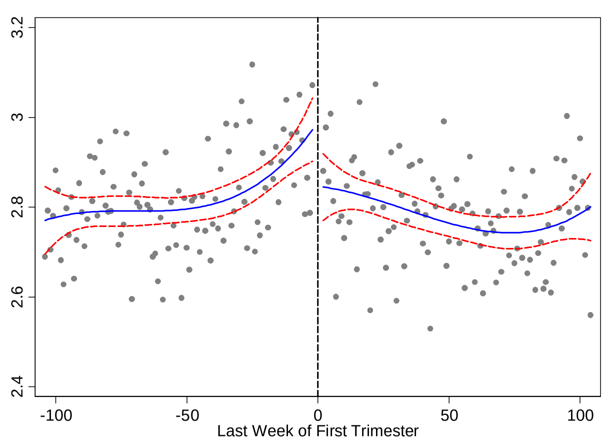
Panel A: Number of Births



Panel B: Proportion Missing Father's Education



Panel C: Number of Mother's Total Births



Panel D: Proportion Black (N/A)

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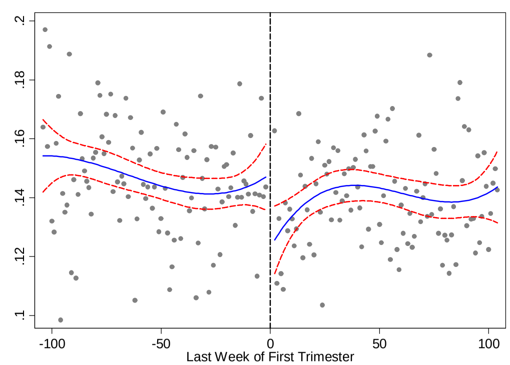
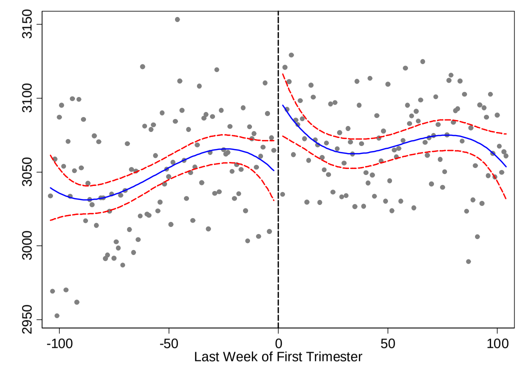
Figure 3.4, Continued: The Effect of Abortion Access on Birth Outcomes for Blacks

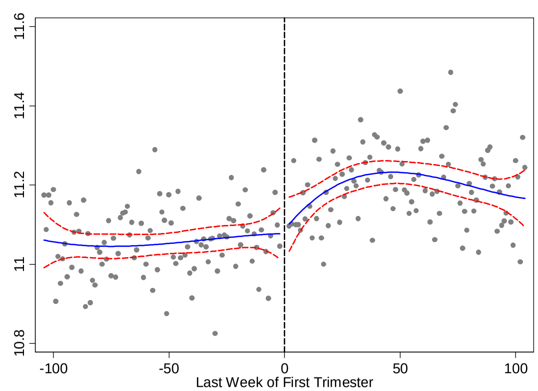
Panel E: Proportion Low Birth Weight (<2,500 g)



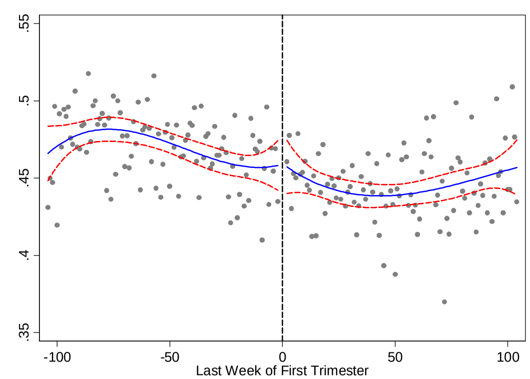
Panel F: Weight at Birth (g)



Panel G: Mother's Years of Education



Panel H: Proportion of Mother's Without High School Degree



Notes and Sources: Data from NCHS Vital Statistics Natality Birth Data for mothers who were residents of New York and conceived April 1968-March 1972. Abortion access defined as a woman still being in her first trimester or not-yet-pregnant as of July 1, 1970. See Appendix C.2 for details. Split-side polynomial regression using a third-order polynomial, split by treatment, graphed. Robust standard errors used to create 95 percent confidence intervals.

arrival. These advantages could easily persist through adulthood.

3.5 Identification in the ACS

To determine the effects of legal abortion access on cohort outcomes in adulthood, I use data from the 2005 to 2010 American Community Surveys (ACS). Focusing on legalization in New York, I select a sample of black, white, and Hispanic men and women born in-state between 1964 and 1975.[142] I consider both the total earned income and hourly wages of workers in my analysis. The sample (N=95,314) is selected in a standard manner and excludes the self-employed, active-duty military, and those with allocated data.[143] I draw similar samples from the rest of the United States.

3.5.1 Estimating Abortion Access in the ACS

Unlike in the NCHS, one cannot exactly determine mother's access to abortion in the ACS. But one can use quarter of birth to predict the probability that a person's mother had legal access to abortion. A person of age a, interviewed in year t, was either born in year $t - a$ if they had celebrated their birthday that survey year or year $t - a - 1$ if they had not done so already. Further, because ACS interviews are conducted uniformly across the year, one can conclude that those with birthdays earlier in the year are more likely to have celebrated a birthday already when interviewed.[144] Thus, an a-year-old interviewed at t and born in quarter 1 is more likely to have been born in year $t - a$ than an a-year-old interviewed at t and born in quarter 2.

Assuming all pregnancies last exactly nine months, a person's mother would have access to (first-trimester) abortion if he or she was born after January 1, 1971, as this

[142] Formally, $1965 \leq survey\ year - age \leq 1975$.

[143] The selection procedure closely follows Autor, Katz, and Kearney (2008). See Appendix C.3 for details.

[144] See Appendix C.4 for details on the uniformity of interviews across the year.

implies New York legalized abortion prior to the end of the mother's first trimester (six months prior to the birth). Therefore, people with $t - a = 1971$ but who were born in different quarters have different probabilities of being born after January 1, 1971. These individuals then have mothers with different predicted access to legal abortion.

I compare two people interviewed in 2005 at age 34 ($t - a = 1971$) who are of the same expected age but have mothers with different expected access to legal abortion.[145] First, consider Amy, born in quarter 1 (Table 3.2, Panel A). Assuming, on average, that people like Amy were born in mid-February, if Amy is interviewed in the second half of quarter 1 or anytime in quarters 2 through 4, she has already had her birthday. Thus, Amy was born in 1971. If Amy was interviewed in the first half of quarter 1 and reports being 34, she was born in 1970. Therefore, assuming that interviews and birthdays occur uniformly across and within quarters, the probability that Amy was born before January 1, 1971 is $7/8$ and $abort\_access = 7/8$.

Alternatively, consider Bob, who was born in quarter 2 (see Table 3.2, Panel B). If Bob was interviewed between mid-April and the end of December, one can infer that he was born in 1971. Otherwise, his responses suggest he was born in 1970. Therefore, the probability that Bob was born before January 1, 1971 is $5/8$ and $abort\_access = 5/8$. Similar calculations yield mother's pill access at conception.[146]

Even though Amy and Bob have mothers with different expected access to abortion, they are the same expected age (again, see Table 3.2). If Amy was interviewed in quarter 2, 3, or 4, she would be 34 and 3, 6, or 9 months old. If she was interviewed in quarter 1, she either just turned 34 or will be 35 very soon (on average, she is 34.5). Given that interviews occurred uniformly across quarters, Amy's expected age is the average of these values:

---

[145] Note that calculations are based on the null hypothesis that abortion did not affect the composition or number of births. If abortion decreased the birth rate (see Section 3.4), then more births occurred in 1970 than 1971. Thus, my measure of abortion access may be overstated, implying I understate the effect of abortion access on wages.

[146] See Appendix C.4 for a validation of these calculations using birth certificate data.

Table 3.2: Identification of the Probability a Person's Mother Had Access to Legal Abortion

Panel A: Amy, Age 34 in 2005, Born in Quarter 1, Pr(Mother Had Legal Access)=0.875

	Quarter of Year Interviewed				
	1	2	3	4	Overall
Probability Interviewed During Quarter	0.25	0.25	0.25	0.25	
If interviewed this quarter. . .					
. . . probability already had birthday	0.50	1.00	1.00	1.00	0.875
. . . expected age	34.50	34.25	34.50	34.75	34.50
. . . probability born in 1971	0.50	1.00	1.00	1.00	0.875
. . . probability born in 1970	0.50	0.00	0.00	0.00	0.125

Panel B: Bob, Age 34 in 2005, Born in Quarter 2, Pr(Mother Had Legal Access)=0.625

	Quarter of Year Interviewed				
	1	2	3	4	Overall
Probability Interviewed During Quarter	0.25	0.25	0.25	0.25	
If interviewed this quarter. . .					
. . . probability already had birthday	0.00	0.50	1.00	1.00	0.625
. . . expected age	34.75	34.50	34.25	34.50	34.50
. . . probability born in 1971	0.00	0.50	1.00	1.00	0.625
. . . probability born in 1970	1.00	0.50	0.00	0.00	0.375

Notes: Author's calculations based on the assumptions that birthdays and interviews are evenly distributed across and within quarters and that gestation lasts exactly nine months.

34.5. Likewise, Bob would be three months away from his 35th birthday if interviewed in quarter 1, just 34 or almost 35 if interviewed in quarter 2, 34.25 if interviewed quarter 3, and 34.5 if interviewed in quarter 4. Thus, he is also predicted to be 34.5. My identification strategy essentially compares the average Amy with the average Bob to determine the impact of abortion.

3.5.2 Identification Strategy

To formally compare those of the same expected age, interviewed in the same survey year, but with mothers who had different access to abortion, I use the specification

$$Y_{iatq} = \alpha_{at} + \gamma_q + \beta(abort\_access_{atq}) + \phi(pill\_access_{atq}) + \varepsilon_{iatq}, \tag{3.3}$$

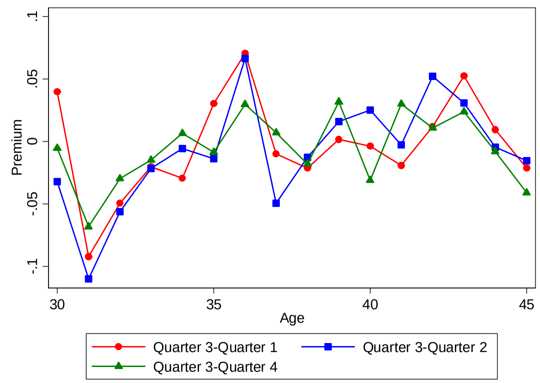
where Y is a given outcome of interest (e.g., wages, earnings), α is a vector of age-survey year fixed-effects, and γ is a vector of quarter of birth fixed-effects. I cluster standard errors by cohort, defined as the expected year and quarter of birth. If quarter of birth does not differentially influence earnings across age-year pairs (see Buckles and Hungerman 2010), this regression will estimate the effect of abortion access on cohort characteristics.[147]

To assess this assumption, Figures 3.5A-D contain the quarter of birth premium for both wages and earnings across ages and survey years in a sample of workers whose mothers' access to abortion does not depend on quarter of birth. Deviations appear random in all plots, lending credibility to my identification strategy. Similarly random, though noisier, trends exist for the race-specific wage premia. I thus use specification (3.3) to estimate both the mean change in characteristics associated with abortion's legalization and

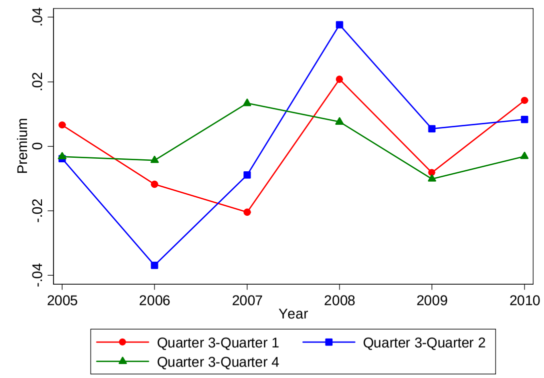
[147]This assumes that abortion legalization was the only reform impacting pregnant women in the third quarter of 1970. If other important changes occurred in these three months, my estimator will yield the combined effect of these policies. To mitigate this concern, I control for access to birth control and look at historical records of changes in New York's policies on prenatal care, public health, and other potentially important factors in pregnancy. No major changes occur in New York during the period of interest.

Figure 3.5: Trends in the Quarter of Birth Premium

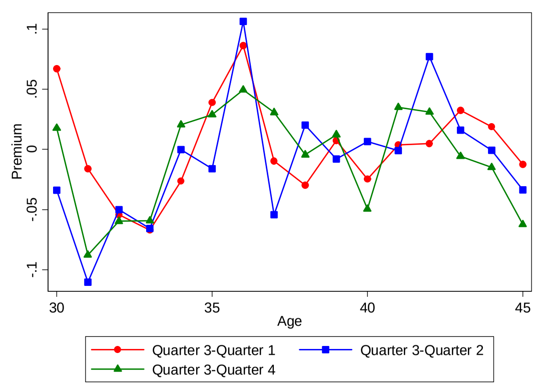
Panel A: Birth Quarter Premium Over Ages
Log(Hourly Wage)



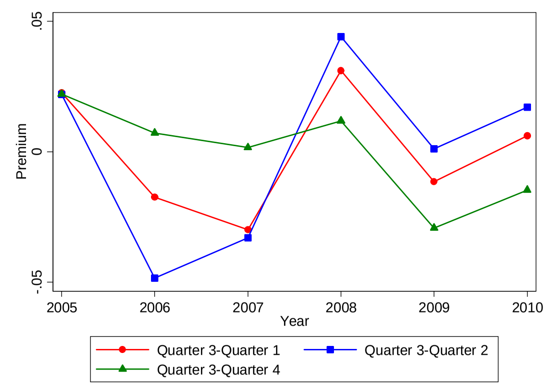
Panel B: Birth Quarter Premium Over Survey Years
Log(Hourly Wage)



Panel C: Birth Quarter Premium Over Ages
Log(Annual Earnings)



Panel D: Birth Quarter Premium Over Survey Years
Log(Annual Earnings)



Notes and Sources: Wages from whites, blacks, and Hispanics in the ACS 2005-2010, born 1964-1975. Sample excludes survey year-age pairs in which abortion access depends on quarter of birth. See Appendix C.3 for details. Estimates calculated using ACS sample weights. Residuals from regressions of wages or earnings on age-survey year fixed-effects, birth quarter fixed-effects, and mother's pill access.

quantile effects. As abortion access is determined probabilistically, one can think of this paper's main specification as the reduced-form of an underlying instrumental variable approach.

Additionally, one must ask if the estimates from eq. (3.3) represent a temporary or more permanent change. Right after legalization, women may use abortion differently than in the long-run. Furthermore, if changes in abortion access influence women's contraceptive choices, social norms, or the marriage market, one might expect the relationship between access and cohort outcomes to fade over successive cohorts. To determine how abortion access might impact more broadly-spaced cohorts, I estimate regressions of the form

$$Y_{iatq} = \alpha_{1a} + \alpha_{2t} + \beta_{LR}(abort\_access_{atq}) + \phi_{LR}(pill\_access_{atq}) + \varepsilon_{iatq}, \tag{3.4}$$

dropping cohorts in which abortion access depends on quarter of birth (and not simply age and survey year). This specification yields a longer-run effect of abortion on child outcomes, by comparing those of the same age in different survey years.

3.6 The Effect of Abortion Legalization on New Yorkers

3.6.1 Overall Effects

To determine the effect of abortion reform on average earnings, I estimate eq. (3.3) using the entire sample of individuals born in New York State. Table 3.3 contains the coefficients from regressions of log earnings and hourly wages. Neither outcome significantly changes in response to abortion access in specifications excluding access to the pill. When one includes this control, mother's access to abortion is associated with average hourly wages rising by (a marginally significant) 4.3 percent. The effect on log earnings is small and positive, though noisily estimated.

Table 3.3: The Effect of Legal Access to Abortion on Earnings and Wages

Panel A: Log(Earnings)

	(1)	(2)	(3)	(4)
Mother Had Access to Abortion	0.00583	0.00575	-0.029	0.0317
	[0.0465]	[0.0470]	[0.0397]	[0.0626]
Mother Had Access to Abortion*Black			0.147**	
			[0.0646]	
Mother Had Access to Abortion*Hispanic			0.0492	
			[0.121]	
Mother Had Access to Abortion*Female				-0.0313
				[0.0533]

Panel B: Log(Wage Rate)

	(1)	(2)	(3)	(4)
Mother Had Access to Abortion	0.0394	0.0429*	-0.0023	0.00668
	[0.0241]	[0.0255]	[0.0223]	[0.0377]
Mother Had Access to Abortion*Black			0.168***	
			[0.0336]	
Mother Had Access to Abortion*Hispanic			0.120***	
			[0.0424]	
Mother Had Access to Abortion*Female				0.0879**
				[0.0333]
Pill Access Control		Y	Y	Y
Fixed-effects by Race			Y	
Fixed-effects by Gender				Y
Observations	95,314	95,314	95,314	95,314

Notes and Sources: Data from whites, blacks, and Hispanics in the ACS 2005-2010, born 1964-1975. See Appendix C.3 for details. All specifications include age-survey year and birth quarter fixed-effects. Estimates calculated using ACS sample weights. Robust standard errors clustered by birth cohort (expected year and quarter of birth) in brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Focusing on the average impact of abortion legalization obscures potentially important heterogeneity. Although one cannot link adults to their parents in Census data, biological children generally inherit their mother's race.[148] As abortion records included information on patient's race, one can then see if differences in mothers' use of abortion imply differences in children's outcomes.

Column (3) of Table 3.3 therefore contains coefficients from the interaction of mother's abortion access and indicator variables for being black or Hispanic. Although there were no significant average earnings gains, African Americans whose mothers had access to abortion realized a significant 15 percent increase in earnings. Considering hourly wages instead, both African Americans and Hispanics (but not whites) have higher wages if their mother had access to legal abortion. The effect for blacks may be expected, as black women utilized abortion most heavily in New York from 1970 to 1972. The result for Hispanics is more puzzling, as Hispanic mothers had far fewer abortions per birth than whites. But if Hispanic women were more judicious about when to have abortions, one could see stronger positive selection of mothers (and thus larger wage changes) in this subgroup after legalization.[149]

I also consider how abortion access differentially impacted male and female babies carried to term. Although sex-selective abortion was not relevant in the 1970s due to technological constraints, mother's abortion access may impact women and men differently. The specifications in column (4) of Table 3.3 contain the interaction of an indicator for being female and mother's abortion access. Though earnings regressions are too noisy to permit conclusions, wage regressions suggest that women's wages significantly increase

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[148] Though interracial childbearing could lead parents to identify as a different race than their children, this rarely occurred in the period I consider. In the 1970 Census, over 99% of children in New York reported the same race as their mother and less than 1% of married couples were interracial.

[149] The assumption that Hispanics used abortion more judiciously is consistent with differences in beliefs about the morality of abortion at this time. However, data on abortions is limited and one cannot directly assess this assumption.

with mother's access to abortion. The average effect for males alone is small and insignificant.

Changes in parental selection likely drive the observed wage changes but other forces could also be important. If abortion access influenced social norms or the marriage market, such changes likely occurred slowly. Though one cannot rule out these forces, the very short time-horizon used in this analysis suggests only a minor potential role for these effects.

Differently, cohort size (shown in the previous section to fall after abortion's legalization) could affect earnings in two ways. First, if different cohorts have limited substitutability in production, the wages of smaller cohorts should exceed those of larger, but equally productive, cohorts. If one compared people in widely-spaced cohorts, cohort size differences could result in significant wage differences; however, my analysis compares people born over the course of a short period, who are likely good substitutes. Thus, cohort size should not act through this particular channel.

Alternatively, resources (e.g., classroom space) could be scarcer in childhood for those born into larger cohorts. Again, my analysis uses the wages of closely-spaced cohorts to infer the effect of abortion. Thus, changes in resources across schools or neighborhoods will not be reflected in my estimates. But if abortion reform led to a change in class size, one cannot rule out effects through this channel.[150] School-starting-age rules in New York indicate that those whose mothers had abortion access were generally in different grades than those whose mothers did not have access (see Barua and Lang 2009). Therefore, a drop in the birthrate will imply a rather similar change in classroom size. Using the large estimates of Fredriksson, Ockert, and Oosterbeek (2011) and assuming a two-person decrease in class size implies that at most one-third of the overall estimated effect of abortion can be accounted for by class-size changes. Therefore, the change in parental selection is likely the driving force behind the changes in wages estimated in Table 3.3.

[150] See Chetty, et al. (2010) and Fredriksson, Ockert, and Oosterbeek (2011).

To get a sense of how the labor force must have changed to produce these effects, I ask: if abortion simply led to a truncation of the wage distribution, at what point would this truncation have to occur to yield the estimated effects? By examining the wages of those in my sample with $abort\_access = 0$, I find that removing the bottom 2 percent of the wage distribution would imply the estimated 4.3 percent rise in overall wages. That is, if mothers' abortion decisions were strictly based on the future wages of their children, an abortion rate of only 2 percent could yield the estimated change in wages. For African Americans, a 17 percent average wage increase is equivalent to dropping the bottom 12 percent of the race-specific wage distribution. For Hispanics, the 12 percent increase in wages could be achieved by removing workers below the eighth percentile of the wage distribution.

Differently, one can use the estimated change in the birth rate to set the proportion of workers "missing" and calculate the subset of the wage distribution they would have to be drawn from to produce the estimated results. Births to New Yorkers fell by about 10 percent after legalization (20 percent for blacks). If one randomly omitted 10 percent of earners from the bottom 65 percentiles of the wage distribution, wages would increase by about 4.3 percent, the estimated effect of abortion reform. If one omitted 20 percent of African Americans from the bottom four deciles of the race-specific wage distribution, the average wage of black workers would rise by about 17 percent.[151]

Of course, these estimates represent extremes derived from ignoring the additional pregnancies occurring after legalization. Although the birth rate dropped by only 10 percent after legalization, over 25 percent of New York pregnancies ended in abortion during the year following New York's reform. By using the birth rate, and not the abortion rate, I overstate the degree of selection needed to rationalize the point estimates. Differently, if all women in the absolute worst circumstances have abortions but would otherwise birth children who do not participate in the labor force, one would need the "missing" workers to

[151] One cannot calculate the change in the Hispanic birth rate using the NCHS.

be drawn from further into the bottom tail of the observed wage distribution.

The coefficients estimated in this section are robust to a wide variety of clustering procedures. I also find similar results when controlling for educational attainment and potential experience. Despite this robustness, comparing the magnitude of the estimated effects with the quarter of birth premium across ages (see Figure 3.5) raises some questions. Although my estimates of β come from a combination of the quarter of birth premia in Figure 3.5, the large range of these premia suggests that I may underestimate the standard error of β.

A placebo test allows me to assess the size of β in relation to the variation in the quarter of birth premium. I consider the set of coefficients derived from placebo reforms, defined as the "effect" of being born after January 1 of several different years before abortion's legalization. Placebo estimates of reform from 1960 to 1969 yield estimates suggesting that the overall effect of reform may not be statistically distinguishable from zero; however, these tests yield tighter predictions for the interactions between abortion access and race indicators. Using the small number of placebo estimates to calculate a standard error, both interaction terms remain significant at the 5 percent level. Thus, one should be careful in concluding that abortion had an effect on overall wages or earnings. But estimates for minority groups likely represent real changes.

3.6.2 Effects Over the Wage Distribution

In addition to race, parents also pass on much of their socioeconomic status to their children.[152] Anecdotal evidence suggests that wealthier women had greater access to abortion prior to its legalization and historical prices indicate that poorer women had easier access to abortion after the New York reform. Therefore, one might expect that abortion's

[152] See Solon (1999) and Black and Devereux (2012).

legalization led to larger changes in selection into motherhood for lower-income women.[153] As children from poorer families tend to have lower wages themselves, estimates of eq. (3.3) may vary across the quantiles of the wage distribution.

Figures 3.6 and 3.7 thus contain the estimates from quantile regressions of log hourly wages and total annual earnings. Figure 3.6 suggests that abortion legalization caused large wages gains among lower-wage workers. Effects are around 10 log points and statistically significant from roughly the 10th to the 30th quantile. From the 60th wage quantile and up, all estimates are statistically indistinguishable from zero. Further, after the 70th quantile, point estimates are small. The gap between the 10th and the 50th quantiles of the wage distribution is largely unaffected by abortion's legalization but the gap between the 10th and the 90th quantiles significantly narrows.[154] This is consistent with historical accounts suggesting that New York's reform led to much larger changes in the cost of abortion for lower-income New Yorkers.

Focusing on earnings, the results are far noisier. Although point estimates suggest that earnings increased by more in the bottom half of the distribution than at the top, the estimates' imprecision do not allow for precise inference.

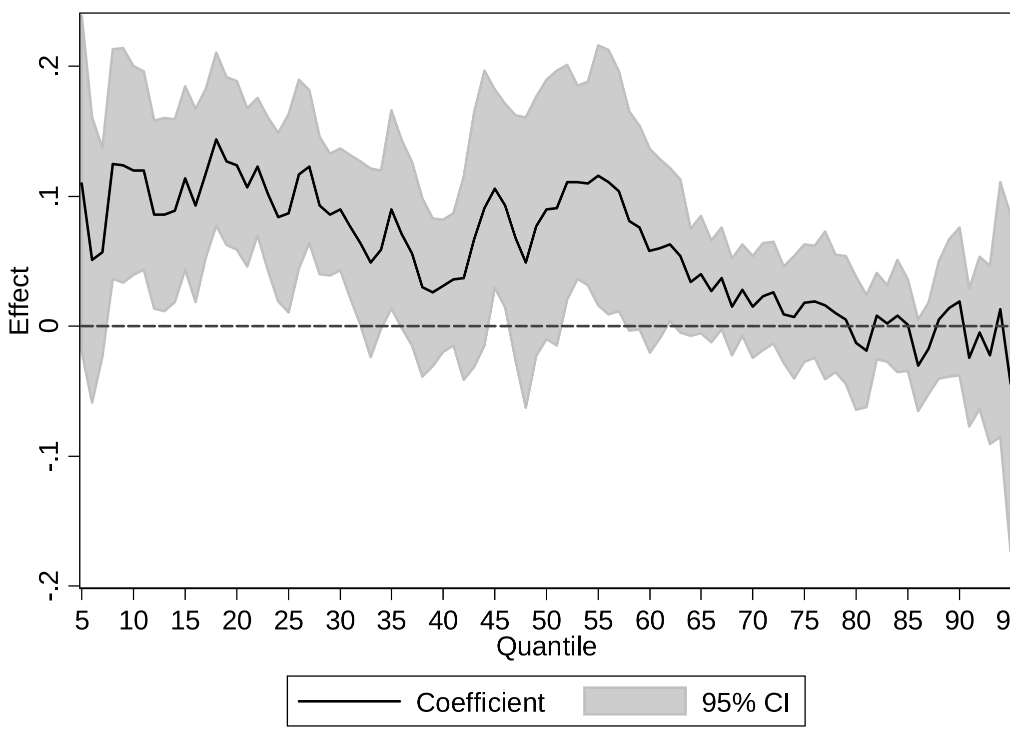
One can also estimate eq. (3.3) using quantile regression within racial groups. Figure 3.8 contains the estimated effect of abortion reform on the hourly wage distributions of whites, blacks, and Hispanics.[155] Each racial group exhibits a different trend. For whites, most quantile effects are statistically indistinguishable from zero but the effect of abortion on lower quantiles is larger than that for higher quantiles. Blacks gain throughout the wage distribution; most estimates are both large (over 15 percent) and significant. The wages of Hispanics in the bottom half of the race-specific wage distribution increase, while the top

[153]This also may explain the differential effects by race discussed in the previous section.

[154]The 90-50 gap narrows by 7 percent with a t-statistic of 1.61.

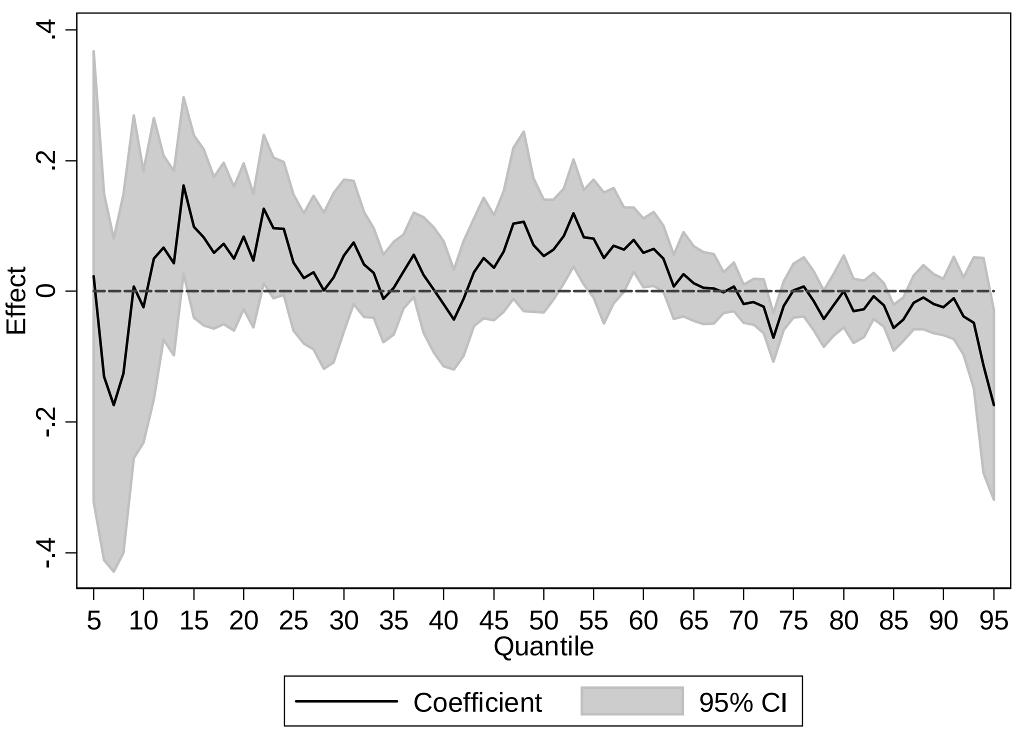
[155]Earnings regressions yielded very noisy estimates.

Figure 3.6: The Effect of Abortion Access Throughout the Wage Distribution



Notes and Sources: Data from whites, blacks, and Hispanics in the ACS 2005-2010, born 1964-1975. See Appendix C.3 for details. Effects calculated using data collapsed to age-year-birth quarter cells (N=252). Effects from regression including controls for mother's birth control pill access, age-survey year fixed-effects, and birth quarter fixed-effects. Estimates calculated using ACS sample weights. Robust standard errors clustered by birth cohort (expected year and quarter of birth) used to create confidence intervals.

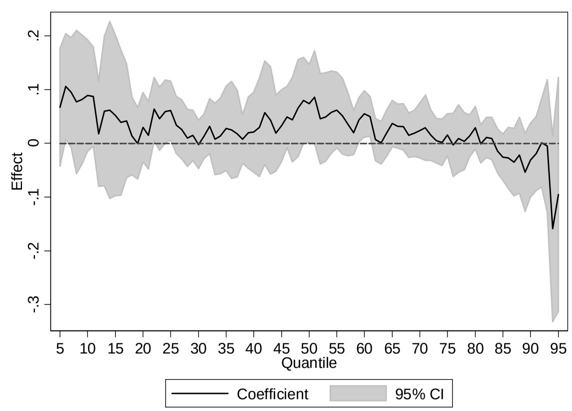
Figure 3.7: The Effect of Abortion Access Throughout the Earnings Distribution



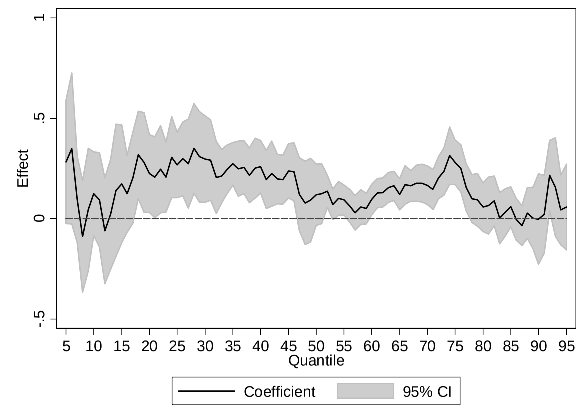
Notes and Sources: Data from whites, blacks, and Hispanics in the ACS 2005-2010, born 1964-1975. See Appendix C.3 for details. Effects calculated using data collapsed to age-year-birth quarter cells (N=252). Effects from regression including controls for mother's birth control pill access, age-survey year fixed-effects, and birth quarter fixed-effects. Estimates calculated using ACS sample weights. Robust standard errors clustered by birth cohort (expected year and quarter of birth) used to create confidence intervals.

Figure 3.8: The Effect of Abortion Access Throughout the Wage Distribution, By Race

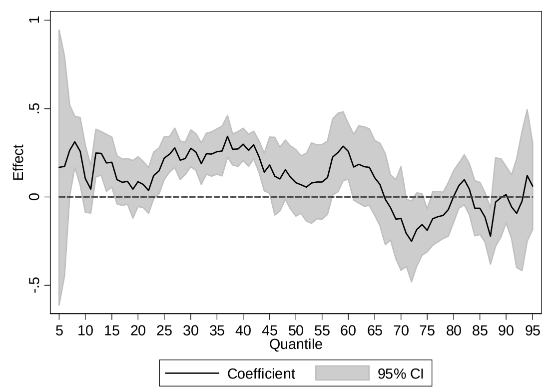
Panel A: Whites



Panel B: Blacks



Panel C: Hispanics



Notes and Sources: Data from whites, blacks, and Hispanics in the ACS 2005-2010, born 1964-1975. See Appendix C.3 for details. Effects calculated using data collapsed to race-age-year-birth quarter cells (N=756). Effects from regression including controls for mother's birth control pill access, age-survey year fixed-effects, and birth quarter fixed-effects. Estimates calculated using ACS sample weights. Robust standard errors clustered by birth cohort (expected year and quarter of birth) used to create confidence intervals.

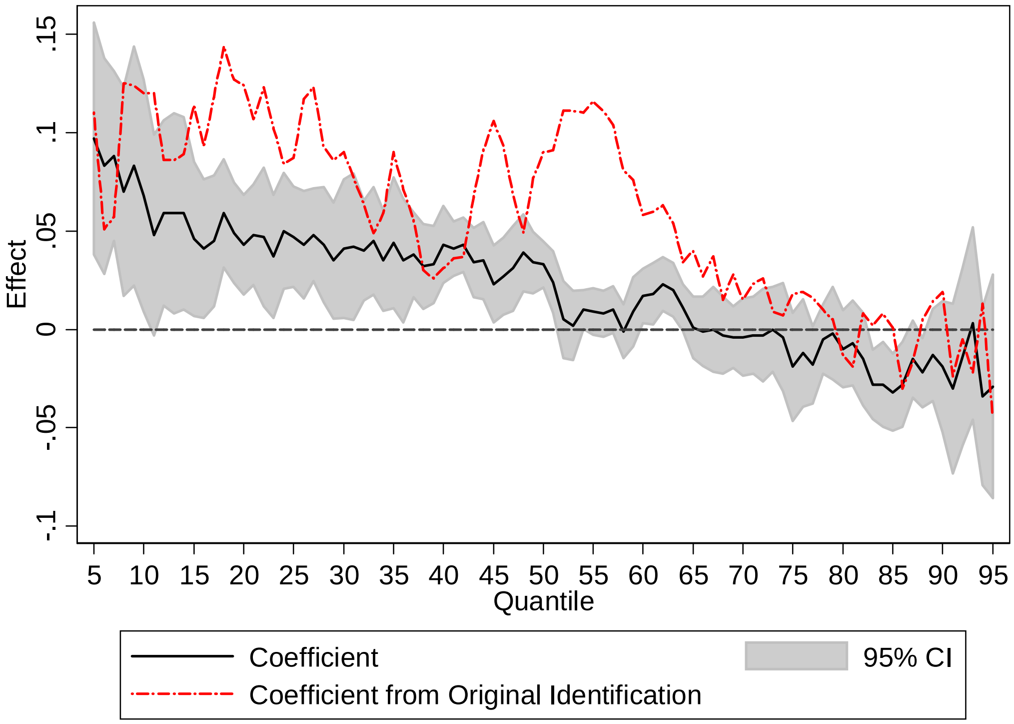
quartile of the wage distribution exhibits marginally significant losses. Altogether, the results suggest that abortion access led to moderate, short-run decreases in overall and racial inequality. They also suggest that abortion's legalization may have differently affected the fertility choices of women of different backgrounds.

The coefficients in Table 3.3 and Figures 3.6-3.8 represent very short-term effects of abortion, estimated using variation derived from conceptions within a few months of abortion's legalization. Alternatively, one can use the multiple cross-sections of the ACS for identification, as suggested by eq. (3.4). Instead of making age-birth quarter comparisons, this strategy compares individuals of the same age across surveys, essentially assuming that the relationship between age and hourly wages is stable from 2005 to 2010. If women responded to legal abortion by decreasing contraceptive use, one would expect these estimates to be smaller than those from eq. (3.3). Additionally, if abortion legalization changes social norms about marriage, divorce, or childbearing, the partial equilibrium and general equilibrium effects of abortion may differ. Furthermore, the women having abortions immediately after legalization may be different in some way than those aborting in the years following reform.[156]

Figure 3.9 contains the quantile effects estimated using eq. (3.4). The longer time-horizon used in these regressions yields smaller effects, though wages still significantly increase by about 5 percent in the bottom half of the distribution. These estimates suggest that abortion legalization may have had a longer-run effect. That is, the estimates comparing children conceived within a few months of legalization do not simply reflect a one-time change in the wage distribution that was subsequently undone by changes in women's contraceptive use or social norms.

[156]For example, they may be more informed about the increase in access or concentrated in geographic areas where abortion clinics more quickly opened.

Figure 3.9: Longer-Run Effects of Abortion Access Throughout the Wage Distribution



Notes and Sources: Data from whites, blacks, and Hispanics in the ACS 2005-2010, born 1964-1975. See Appendix C.3 for details. Sample excludes survey year-age pairs in which abortion access depends on quarter of birth. Effects calculated using data collapsed to age-year-birth quarter cells (N=60). Effects from regression including controls for mother's birth control pill access and age and survey year fixed-effects. Estimates calculated using ACS sample weights. Robust standard errors clustered by birth cohort (expected year and quarter of birth) used to create confidence intervals. Estimates from original identification strategy from Figure 3.6.

3.6.3 Estimates of Other Variables

Overall, the previous analysis suggests that once women gain access to abortion, mothers become more positively selected. This in turn leads children to have higher wages. To consider more proximate causes for increased wages among grown children, I analyze the effect of abortion reform on a variety of other adult characteristics. Table 3.4 contains the estimates of abortion's effect calculated using specification (3.3).

Cohorts whose mothers had access to legal abortion contain more whites and fewer minorities, with the proportion of workers that are black decreasing by about 15 percent. Mother's access to abortion is associated with a worker having an additional quarter-year of education. Further, the proportion of a cohort without a high school degree drops sharply with mother's access. Children whose mothers had legal access to abortion are also less likely to be single parents themselves. All effects are significant at the 5 percent level and cannot be explained by racial composition. Additionally, estimates for the whole population imply that abortion reform may have led to decreased dependence on welfare and disability income.

Furthermore, there may be a small increase in the proportion of a cohort that is working when mothers have access to abortion. For African American men, the result is far stronger. Idleness of black men falls by 14 percentage points within cohorts whose mothers had access to legal abortion. This change implies that abortion's effect on the wages of African Americans may underestimate abortion's overall effect on income or welfare within this subgroup. Indeed, abortion access caused the average African American to live in a family with 28 percent higher total income. However, using a Heckman-style correction to account for changes in selection into the labor force implies wage gains similar to those suggested by OLS estimates.[157]

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[157] See Heckman (1979). Lacking a variable which influences participation but not wages, I identify the Heckman-correction using the functional form of the first-stage regression.

Table 3.4: Mother's Abortion Access and Other Outcomes

	All Individuals			Wage Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Var.	Effect	Effect	Mean	Effect	Effect	Mean
Black	-0.00135		0.141	-0.0205**		0.121
	[0.00925]			[0.00843]		
White	0.0497***		0.751	0.0568***		0.774
	[0.00936]			[0.0174]		
Years of Ed	0.174**	0.120*	14.21	0.268***	0.221***	14.52
	[0.0644]	[0.0595]		[0.0887]	[0.0763]	
High School	-0.0281***	-0.0238***	0.0629	-0.0260***	-0.0234***	0.0374
Dropout	[0.00820]	[0.00766]		[0.00478]	[0.00444]	
Single Parent	-0.0209**	-0.0151*	0.127	-0.0506***	-0.0426***	0.124
	[0.00882]	[0.00894]		[0.00973]	[0.00868]	
Poor	0.00173	0.00625	0.0976	-0.0320***	-0.0295***	0.0431
	[0.00591]	[0.00544]		[0.00714]	[0.00735]	
Receives SSI	-0.0108*	-0.00880	0.0356			
or TANF	[0.00645]	[0.00639]				
Unemployed	-0.00383	-0.00254	0.0486			
	[0.0168]	[0.0168]				
Working	0.0207*	0.0177	0.787			
	[0.0111]	[0.0110]				
Log(Yearly Hrs)	0.0221	0.0222	7.459	-0.0341	-0.0351	7.492
	[0.0333]	[0.0333]		[0.0260]	[0.0259]	
	All Black Males					
Working	0.138***		0.695			
	[0.0301]					
Institutionalized	0.0142		0.0822			
	[0.0168]					
Race Controls		Y			Y	

Notes and Sources: Data from whites, blacks, and Hispanics in the ACS 2005-2010, born 1964-1975. See Appendix C.3 for details. All specifications include age-survey year and birth quarter fixed-effects and a variable indicating mother's pill access at conception. Estimates calculated using ACS sample weights. Robust standard errors clustered by birth cohort (expected year and quarter of birth) in brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Finally, in light of Donohue and Levitt (2001) and related work, I use my identification strategy to examine the impact of New York's abortion reform on institutionalization rates. The ACS does not identify the type of institution for those in group quarters but one can examine institutionalization among black men, as this variable serves as a good proxy for incarceration within this subpopulation.[158] Although I find significant effects of abortion access on many other variables of interest, the results for institutionalization are insignificant.

Despite this null result, these estimates suggest that changes in selection into motherhood after abortion reform yielded cohorts that are different in a variety of ways.

3.7 The Effect of New York's Legalization on Non-Residents

By focusing on outcomes for native-born New Yorkers, I am able to find support for previous work suggesting that abortion led to positive selection. New York's legalization can also be used to demonstrate why previous experiments comparing early abortion-legalizers (such as New York) to the rest of the country might misrepresent the total effect of abortion reform.[159]

To see how the change in New York law could have impacted selection in other states, I use eq. (3.3) to estimate the effect of the New York abortion reform on the log hourly wages of those born in states where many women traveled to New York to receive abortions, in states where few women traveled to New York for abortions, and in states not bordering New York. The results (see Table 3.5) suggest that legalization in New York led

[158]The ACS only lists whether a person was institutionalized, not incarcerated. The 1980 Census was the last to indicate the type of institution. In that survey, most people between 30 and 45 in an institution were either in a mental institution, nursing home, or other similar facility. Moreover, fewer than two-thirds of the institutionalized younger, New York-born, African American men in the 1980 Census were actually in a correctional facility. Although this was before the period of mass incarceration, the data suggests one should be careful in using ACS institutionalization data to infer incarceration rates.

[159]Joyce, Tang, and Zhang (2011) and Levine, et al. (1999) also provide such evidence by demonstrating that the legalization of abortion prior to *Roe v. Wade* affected birth rates throughout the country.

Table 3.5: The Effect of New York's Abortion Reform in Other States

	New York		Top Ten Sending States	
	Below Median	Above Median	Below Median	Above Median
Mother Had Access to Abortion	0.0980***	0.0177	0.0463***	0.0278***
	[0.0139]	[0.0123]	[0.0129]	[0.00336]
Observations	39,788	55,525	121,165	134,620

	Non-Border States		Bottom Ten Sending States	
	Below Median	Above Median	Below Median	Above Median
Mother Had Access to Abortion	0.0269***	0.00286	-0.00384	0.0288
	[0.00885]	[0.00472]	[0.0209]	[0.0306]
Observations	483,218	438,329	42,312	35,959

Notes and Sources: Data from whites, blacks, and Hispanics in the ACS 2005-2010, born 1964-1975. See Appendix C.3 for details. Regressions include age-survey year and birth quarter fixed-effects. Robust standard errors clustered by birth cohort (expected year and quarter of birth) and state (when using multiple states) in brackets. Two-way clustering calculated as in Cameron, Gelbach, and Miller (2006). *** $p<0.01$, ** $p<0.05$, * $p<0.1$.
Top 10 Sending States: CT, FL, MA, ME, MI, NH, NJ, OH, RI, VT.
Bottom Ten Sending States: AR, AZ, ID, KS, MT, NM, NV, OR, SD, UT. States that legalized abortion before 1973 excluded.

to changes in wages for children born in other states.[160]

Someone haling from a state where many women traveled to New York to receive abortions would have higher wages if their mother conceived less than three months before the change in New York law. Unlike the effects for New Yorkers, workers gain throughout the wage distribution, though the average effect is lower within these states than within New York. Those below (above) the median tend to have 5 (3) percent higher wages if their mother could have had access to abortion in New York. Both high transportation costs and lack of Medicaid coverage suggest that New York's reform did not differentially lower the costs of abortion for out-of-state women of different means (unlike the change in costs for New York residents). The more uniform change in the wage distribution matches the more uniform change in costs for women living outside of New York.

If one further considers the impact of New York's reform on all states that do not border New York, the effect of legalization is smaller but still significant and around 3 percent for lower-earning workers. Finally, focusing on the ten states that sent the fewest women (per capita) to New York for legal abortions yields small and insignificant estimates of abortion's effect. Thus, abortion legalization in New York is associated with gains both locally and nationally; however, null results in states where few women obtained New York abortions suggest that the results for New Yorkers are not simply spurious.

3.8 Conclusion

Three years before the Supreme Court's decision on *Roe v. Wade*, New York became the first state to allow women legal and easy access to abortion. By lowering the total cost of terminating a pregnancy, this reform allowed women to more precisely time their births. This paper suggests that women took the opportunity, and the resulting cohorts were better-off. Birth certificate data implies that children were born into smaller families with

[160]See Figure 3.2 for a list of abortion rates by state of residence.

more educated mothers and more present fathers. Furthermore, children were less likely to be of dangerously low weight at birth.

Moreover, wage data from several decades later suggests that these, and other, improvements in child living conditions resulted in higher wages. On average, the wages of African Americans and Hispanics born in New York increased by 17 and 12 percent. Wages for all workers in the bottom deciles of the wage distribution also increased by about 10 percent. My analysis further suggests that even those born outside of New York gained from New York's reform. Wages increased in states that sent many women to New York to receive legal abortions. But there is no effect of New York's law on cohorts born in states where few women traveled to New York for this purpose.

As these results compare cohorts spaced very closely together, the estimated effects likely reflect changes in the choices of women to carry their pregnancies to term. That is, children's wages rise because legal abortion allows women to give birth during more secure times when they can better care for their babies. Some portion of the estimated effects may also reflect changes in contraception, cohort size, or social norms. But these forces are secondary in short-run comparisons. Using more broadly spaced cohorts implies a smaller effect of mother's abortion access on wages. This suggests that changes in social norms, the marriage market, and contraceptive behavior may counteract the effect of increases in the abortion rate in the long-run.

This paper validates prior work demonstrating that abortion legalization had a positive effect on children carried to term. By showing that wages increased for low-wage workers, I demonstrate that the costs of crime likely increased. This supports the negative relationship between abortion and crime described by Donohue and Levitt (2001). Furthermore, by using a new identification strategy, my analysis reinforces the findings of other studies demonstrating that abortion legalization led to increased average welfare among children carried to term.

Despite the congruence of these conclusions with many other studies in the literature, one must be careful in extrapolating these results. I demonstrate that increased abortion access led to changes in cohorts born very close together. Even my longest-run identification strategy compares adults born over the course of only five years. Therefore, this analysis cannot address the effect of permanently legalizing abortion. Moreover, my analysis focuses on a large change in access to abortion. Women went from having to pay high fees, travel long distances, and face much risk to terminate a pregnancy, to having easy access to a safe abortion near home. Thus, one should be careful in applying these results to more modest changes in the costs of abortion.[161] Finally, my results stem from abortion reform in New York, a relatively liberal state. One might expect the residents of different states to react differently to changes in the costs of abortion.

Altogether, this paper suggests that abortion reform had a real effect on cohorts born just after legalization. Access to abortion allowed women to have children at more desirable and secure times. The result is a cohort typified by higher wages and, very likely, improved welfare.

[161] In particular, see Kane and Staiger (1996).

Appendices

A Appendices to Accompany Chapter 1

A.1 Data Sources

A.1.1 Survey of Income and Program Participation

The bulk of my analysis utilizes the Survey of Income and Program Participation (SIPP) panels beginning in 2001, 2004, and 2008. These datasets provide retrospective information about a respondent's first marriage, including the year of marriage and the date and way a marriage ended, if applicable.[162] I focus my analysis on the 74,339 women in these SIPP waves with complete marital records who began their first marriages between 1950 and 2004.[163] I can also match 62,572 of these women to their state of birth, which I use to incorporate additional data.

A.1.2 National Longitudinal Survey of Youth (1979)

Because of many omitted variables in the SIPP, I also utilize the 1979 National Longitudinal Survey of Youth (NLSY), which follows young men and women as they marry and divorce.[164] The NLSY is much smaller than the SIPP (containing 3,831 women with adequate data) and may not be used to study trends over time, as the dataset includes a

[162] All of my analysis treats length of marriage as a censored variable if a given marriage has not ended due to divorce.

[163] Average age of marriage jumps in 2004, although such a change may be the result of differences in coding across SIPP panels or noise. Except when looking directly at trends in age at marriage, I will include 2004 in my analysis. In addition, all results are robust to the inclusion or exclusion of this year.

[164] Marriages occurring before a young adult enters the panel are also retrospectively recorded.

single cohort of individuals (age 14 to 22 in 1979).[165] On average, women in the sample marry at age 23 in 1984. This is slightly younger than the average age at marriage reported by the same cohort in the SIPP. The difference is likely due to attrition from the NLSY over time.

This data includes valuable family background variables, such as religion and the presence of a father figure within the home, making it relevant to my study. 2,827 women in this sample also report detailed parental characteristics (mother's and father's LFP, standardized Duncan SEI score, and years of education).[166] Finally, the NLSY contains 894 sisters (from 422 families), whom I use to estimate within-family regressions.

A.1.3 Other Data on Marital History and Marital Quality

Although the NLSY and SIPP provide comprehensive information on marital histories, neither dataset was designed to track and evaluate marriages. I therefore turn to several other surveys more explicitly created for this purpose. The broadest of these datasets is the National Survey of Family Growth (NSFG), which has surveyed married women every few years since 1973. The NSFG contains a large sample of women ages 15 to 44 and rich data on fertility; some waves also contain information about a woman's first spouse. I select all women from the NSFG with complete marital histories for first marriages beginning from 1950 to 2004, producing a sample of 34,124 women surveyed in 1973, 1976, 1982, 1988, 1995, 2002, or 2006-2008. All variables in this sample are reported by these women and thus male characteristics are reported by a man's wife or ex-wife.

I also utilize data from the initial waves of the National Survey of Families and Households (NSFH) and the Marital Instability Over the Life Course Panel Study (MILC), which provide personal details about the marriages of younger and older brides. I select

[165] Other NLS cohorts exist but cannot be used due to limited variation in age at marriage (the 1968 young women's cohort) or an insufficient time horizon (the 1997 cohort).

[166] I assign a Duncan score to those who do not work, as detailed in Dworkin (1981).

marriages beginning from 1950 to 1984 in the NSFH and those beginning from 1950 to 1979 in the MILC (the former survey was conducted in 1988 and the latter in 1980), yielding a total of 3,465 marriages in the NSFH and 1,610 marriages in the MILC. As these surveys contain information about current marriages, I use only observations for families with a woman married to her first spouse at the date of the interview. Data from the NSFH comes directly from the person that a question pertains to; data from the MILC comes from the designated respondent, regardless of gender.

A.1.4 State-Level Data

This section more carefully describes the state-year variables that I use in the analysis of Section 1.3.1.[167] All variables are measured using the average value for the five years prior to a woman's marriage and matched to individuals using state of birth and year of marriage.[168]

1. Abortion Access: the number of abortions per woman age 15-44. Numbers for 1970-1972 come from the Center for Disease Control (1971, 1972, 1974) and those for subsequent years are from the Guttmacher Institute (Jones and Kooistra 2011). I use information on abortion by state of occurrence to create the longest sample possible and assume a rate of zero prior to 1970 (when abortion was first legalized on demand in five states).

2. Cohabitation: I use the March Current Population Survey (CPS) for 1963-2004 to identify those households involving "likely cohabitation" using Manning's (1995) definition: two unmarried, unrelated, opposite-sex adults over age 15 living together

[167] Both Stevenson and Wolfers (2007) and Greenwood and Guner (2008) also discuss the possibility that household technological progress influenced trends in marriage and divorce. Although these factors are likely important, one cannot measure them at the state-year level and thus I must omit them from my analysis.

[168] Other choices for the form of C_{isy} yield similar results. Sophisticated methods of lag selection are computationally infeasible due to the large number of possible combinations of my 14 variables of interest.

with no other people above age 15. Manning shows that this definition gives aggregate estimates of cohabitation close to actual rates.

3. Comstock Laws: an indicator for a state sales ban on contraceptives, using Bailey's (2010) classification.

4. Reproductive Technology: an indicator for an unmarried 18-year-old being able to purchase the oral contraceptive pill, from Goldin and Katz (2002).

5. Unilateral Divorce Law: Wolfers's (2006) preferred classification of state unilateral divorce laws and reforms.

6. Welfare Benefits: The measures available for welfare vary over time. Benefit levels for early years are from the Statistical Abstracts of the United States (1941-1995). For 1940-1944 and 1946-1952, I use total benefits paid out/total families receiving benefits in June; the numbers for 1945 are from December. Benefits for 1951 and 1952 include reimbursement for medical services. For 1953-1965, the data sources provide average payments per family in December including medical benefits and such numbers without medical benefits during 1966-1973. For 1974-1995, average monthly benefits per family are taken over all months, excluding Medicaid payments. Starting in 1995, intermittent reports from the Administration for Children and Families (1995, 1997, 1999, 2001-2004) became available, allowing one to calculate the average monthly benefit. I log-linearly interpolate the missing years. Finally, the IPUMS Censuses provide estimates of the proportion of unmarried mothers who have one, two, three, or four or more children, at the state-by-year level. I create my final measure of log welfare benefits using the errors from a regression of the log benefit level on these proportions and year fixed-effects.

Many of the variables come from samples of workers in the 1964-2005 CPS. I drop anyone living in group quarters or with incomplete demographic information (on education, marital status, and state of residence). The sample includes only those age 18-35, as my analysis largely deals with people earlier in life.

7. Female Labor Force Participation: the proportion of married women working any hours for some number of weeks last year and the proportion of women who worked 30 or more hours in the past week and 50 or more weeks in the past year (full-time, full-year).[169]

8. Segregation by Gender at Work: I first use CPS data on those working any hours to compute the segregation index by occupation-industry cells. Both occupations and industries are classified into 16 groups (available upon request). I then calculate the segregation index for a given state at a given time as

$$Seg_i = \frac{1}{2}\sum_{i,o}\left|\frac{N_{iof}}{N_f} - \frac{N_{iom}}{N_m}\right|$$

   where i indexes industry, o indexes occupation, N_f (N_m) is the total number of working females (males) in the state at a given time, and N_{iof} (N_{iom}) is the number of females (males) primarily working in industry i and occupation o. I also create variables indicating the proportion of women working in traditionally male and traditionally female jobs, where traditionally male (female) jobs are defined as containing more than 95 percent male (75 percent female) workers in the 1950 Census.

Additionally, I use wage data from the CPS to control for both male wage inequality and the gender gap in wages. Using the sample of full-time, full-year workers from the CPS, I drop any workers in the armed forces, agricultural sector, or private household sector from the sample. I also omit any observations with allocated or missing wage and salary

[169] These are the only continuously available measures of work hours and weeks in the CPS from 1963-2005.

income and multiply any top-coded income variables by 1.5. The hourly wage is calculated by taking yearly wage income and dividing it by 50 times the number of hours worked last week. Any observations with nominal hourly wages below the minimum wage or a wage rate that would be top-coded if a person worked 30 hours per week for 52 weeks of the year are also removed from the sample.

9. Gender Gap in Wages: the difference in the log median wages of men and women, calculated using the above sample.

10. Wage Inequality: the difference in the 90th and 50th, as well as 50th and 10th, percentiles of the log wage distribution in the above sample.

A.1.5 State Age at Marriage Laws

The vast majority of data on minimum age at marriage laws comes from the 1933-2001 editions of the World Almanac and Book of Facts. The almanac stopped reporting these laws in 2001, and thus I use the database of the Cornell Legal Information Institute for the 2001-2004 laws. When the two sources for these laws do not match, information from state legislative archives resolves the conflict. If a law allows for marriage before the age of majority, but no age limit is specified, I set the age of marriage with consent to 12, the common law minimum age for girls.[170]

Many states changed their laws throughout time but some changes reported in the Almanac may be erroneous.[171] If a law changes for one or two periods only, then switches back, I remove the change. If a law changes for one period and then changes again, and the changes do not move in the same direction, the first change is set to the original value.

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[170]The age at marriage without parental consent was recorded by the Almanac to be the age of majority if no law provided for early marriage.

[171]See Dahl (2010). Although Dahl's work uses laws for a somewhat different period (1935-1969 versus 1936-1990 in my main analysis), I use the same data sources and process the raw data in a similar manner.

Massachusetts and Montana are omitted from the analysis, as there are many changes in the recorded laws that may or may not coincide with actual legislation. Figure A.1 shows the evolution of minimum age at marriage laws over time for women, with (Panel A) and without (Panel B) parental permission. The figures show a clear increase in the allowed age at marriage with parental consent but a decrease in the age without such consent.

To determine whether law changes were driven by rates of young marriage, childbearing, or divorce, I also looked at trends in these variables leading up to (first) changes in laws (shown in Figure A.2). Although much variation in these variables exists prior to a law change, one sees no clear trend in teenage marriage; the proportion of children under 11 living with young, unmarried mothers; or divorce among those under 25 before an increase or decrease in the minimum age, making these laws plausible instruments.

A.2 Technical Appendix

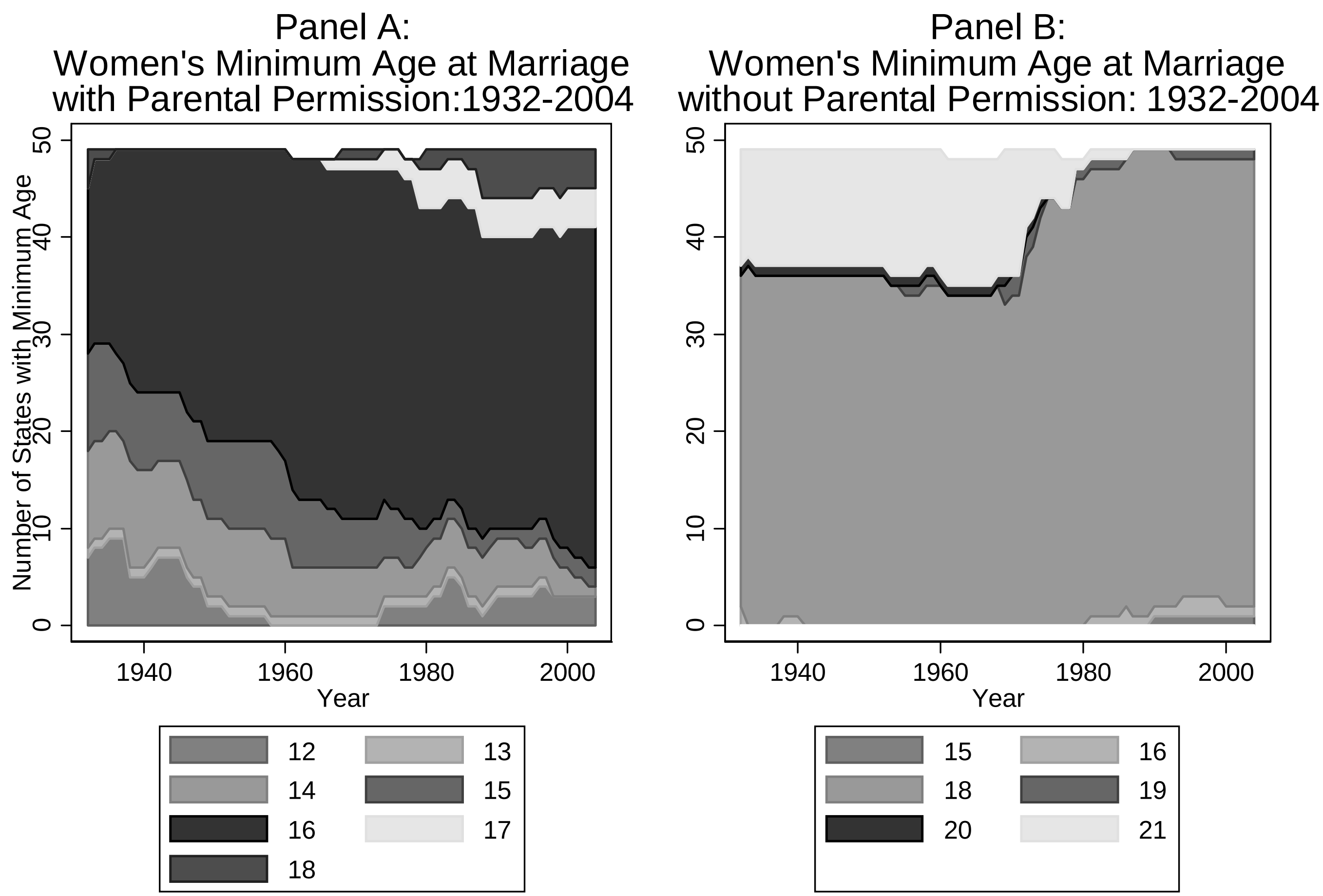
Proof. (Proposition 1) Taking the derivative of the definitions in (1.6)-(1.8) with respect to w' leads to the conclusion that

$$\frac{\partial V_{Mt}}{\partial w'} > \frac{\partial V_{Lt}}{\partial w'} > \frac{\partial V_{St}}{\partial w'}.$$

Increasing w' then increases the value of marriage faster than the value of marriage and search, which in turn increases faster than the value of being single. Thus, if marriage without search (marriage with search) is weakly preferred to either option (singlehood) at w', it will continue to be preferred for $w > w'$. Conversely, if singlehood (marriage with search) is preferred to either option (marriage without search) at w', it will continue to be preferred for $w < w'$. ■

Proof. (Proposition 2) Suppose $w_{LSt} > w_{MSt}$ and $w_{MSt} < w_{LMt}$. Then by the definitions of the cutoff points $V_{St}(w_{MSt}) > V_{Lt}(w_{MSt})$ from the first statement and

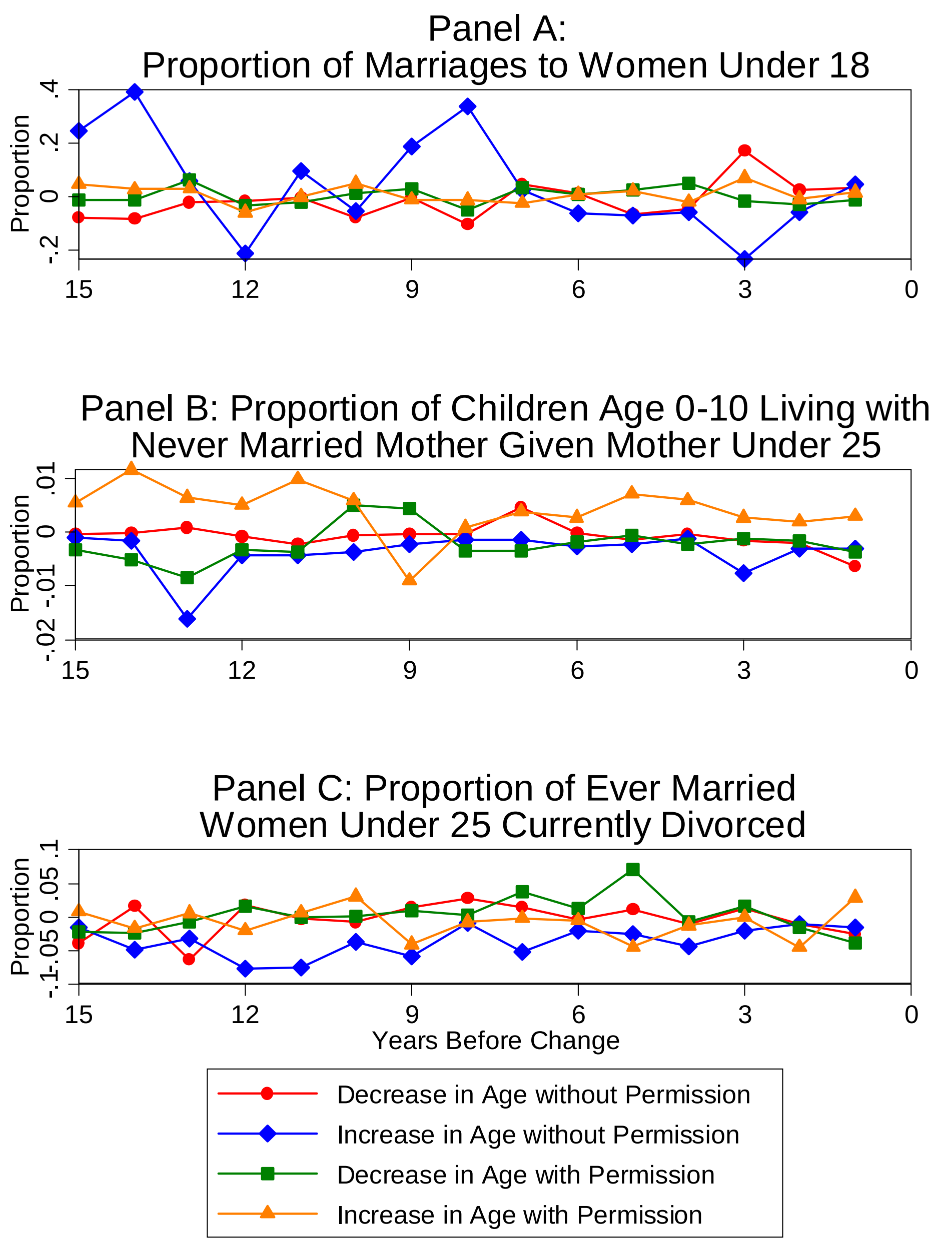
Figure A.1: Minimum Age at Marriage Laws Over Time



Notes: See Appendix A.1.5 for details.

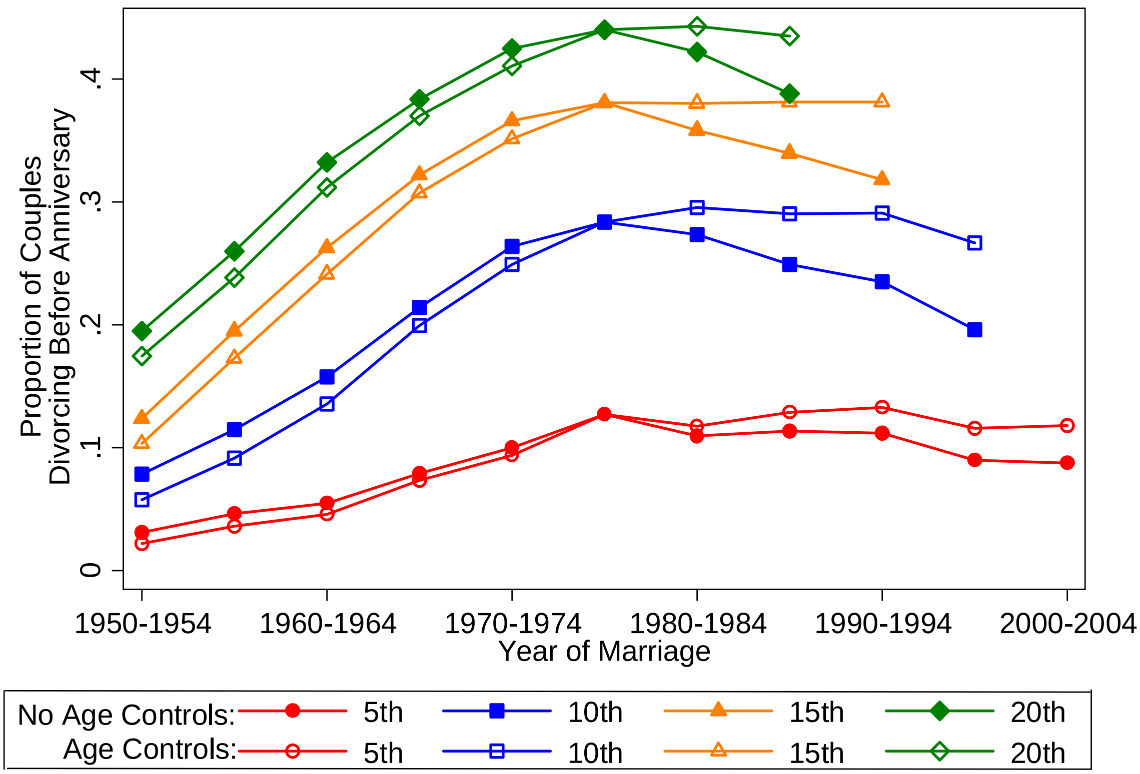
Figure A.2: Young Marriage, Single Motherhood, and Divorce

Before Changes in Minimum Age at Marriage Laws



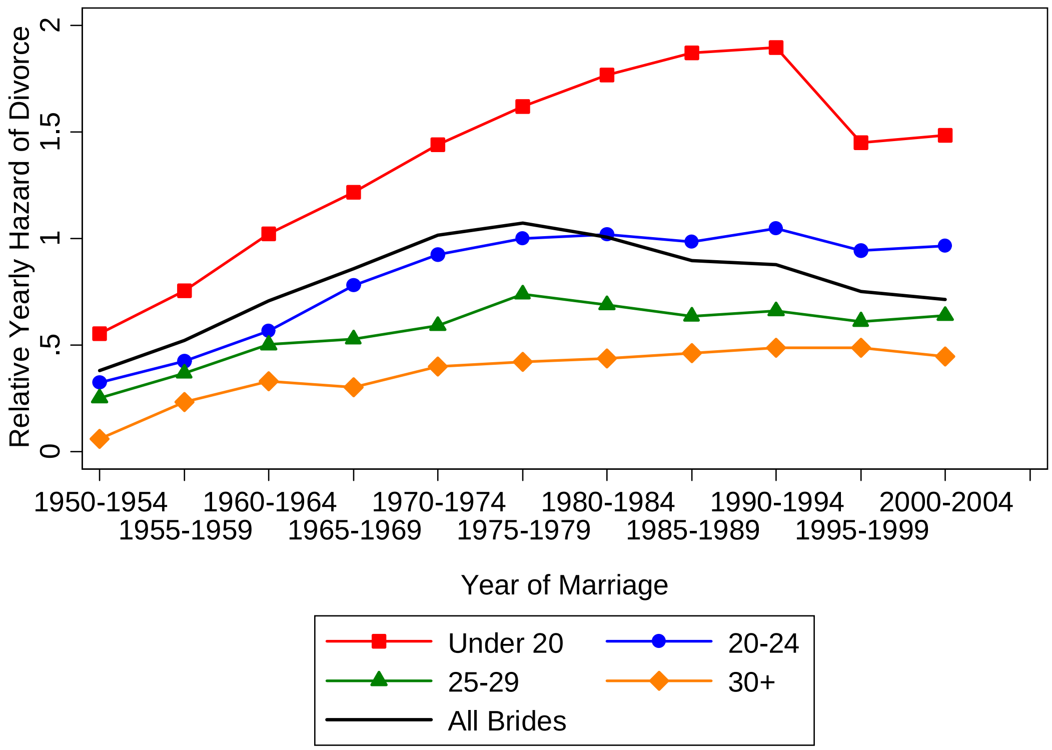
Notes and sources: See Appendix A.1.5 for details on law changes. Divorce and teenage marriage rates from CPS 1963-2004. Child living conditions from largest IPUMS Censuses (log-linearly interpolated for intercensal years). Residuals from regressions of given values on state and year fixed-effects plotted.

Figure A.3: Probability of Divorce by Selected Anniversaries Across Time



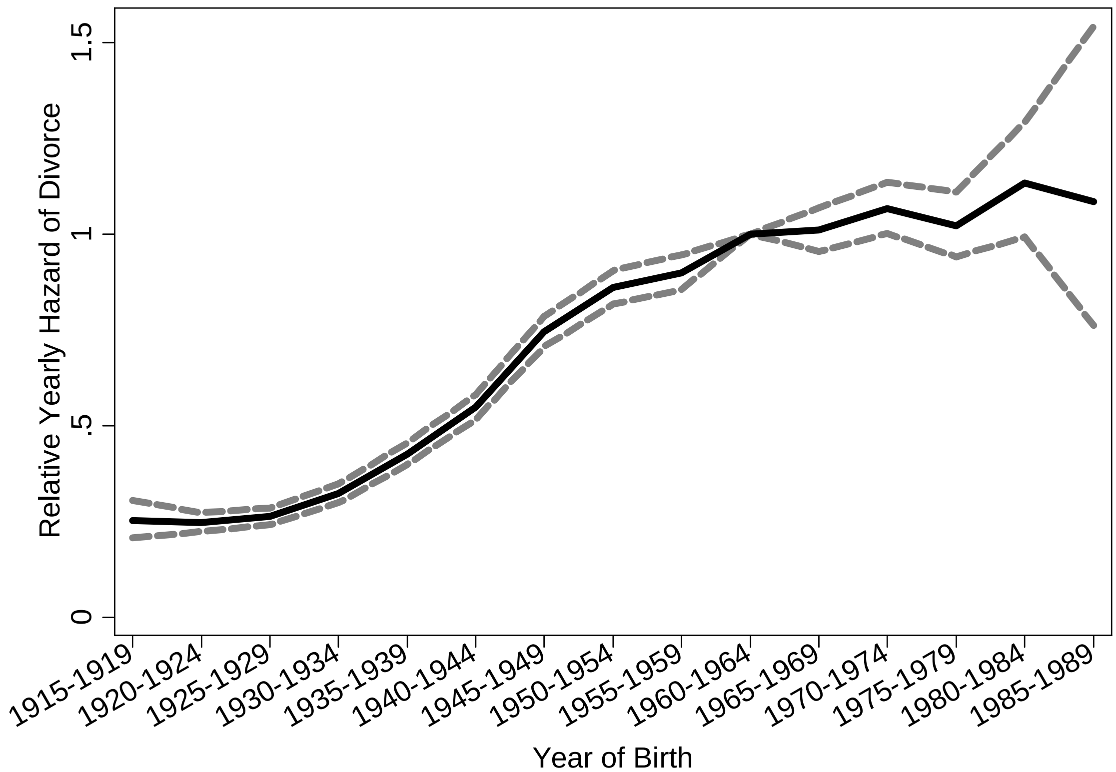
Notes and sources: Women's first marriages from the 2001, 2004, and 2008 SIPP, 1950-2004 (N=74,339). See Appendix A.1.1 for details. Effects are from probit regressions normalized so that conditional and unconditional regressions predict the same probability of divorce for marriages beginning from 1975 to 1979. Observations included in estimating the probability of divorce before the nth anniversary if the possible length of marriage (the difference between the date of spousal death or date of interview and date of marriage) is n or more years. Age of marriage controls include indicators for marrying under 18, 18-19, 20-22, 23-26, 27-29, 30-34, 35-39, and 40+.

Figure A.4: Hazard of Divorce Across Time by Woman's Age at Marriage



Notes and sources: Women's first marriages from the 2001, 2004, and 2008 SIPP, 1950-2004 (N=74,339). See Appendix A.1.1 for details. Observations censored at time of interview or time of death of spouse. Predictions from Cox hazard regression controlling for age of marriage (indicators for marrying under 20, 20-24, 25-29, and over 30) interacted with year of marriage in five-year groups, which sets the hazard rate of marriage for those beginning marriage from 1975 to 1979 at ages 20 to 24 to one.

Figure A.5: Hazard of Divorce Across Birth Cohorts



Notes and sources: First marriages of women born 1915-1989 in the 2001, 2004, and 2008 SIPP. Effects are calculated by five-year group from a Cox hazard regression setting the hazard of divorce for women born 1960-1964 to one and controlling for age at marriage using indicators for marrying under 18, 18-19, 20-22, 23-26, 27-29, 30-34, 35-39, and 40+. Observations censored at time of interview or time of death of spouse. Robust standard errors used to calculate 95 percent confidence intervals.

$V_{Lt}(w_{MSt}) > V_{Mt}(w_{MSt})$ from the second, implying $V_{St}(w_{MSt}) > V_{Mt}(w_{MSt})$, a contradiction of (1.10). Therefore $w_{LSt} > w_{MSt}$ implies $w_{MSt} > w_{LMt}$. Likewise, suppose that $w_{MSt} > w_{LMt}$ and $w_{MSt} > w_{LSt}$. Then $V_{Lt}(w_{MSt}) > V_{St}(w_{MSt}) > V_{St}(w_{MSt})$ which also contradicts (1.10). Therefore, $w_{LSt} > w_{MSt}$ if and only if $w_{MSt} > w_{LMt}$. ■

Proof. (Proposition 3)

Part 1: If $w_{LMt} > w_{MSt} > w_{LSt}$, then w_{LM} decreases over time.

Suppose $w_{LMt} > w_{LMt-1}$. Then if a woman met a man with wage w_{LMt} at $t-1$, she would strictly prefer to marry him and stop searching. Eq. (1.11) then implies

$$\beta\lambda \int_{w_{LMt}}^{\infty} (\psi_{t+1}(w) - \psi_{t+1}(w_{LMt}))f(w)dw > \beta\lambda \int_{w_{LMt}}^{\infty} (\psi_t(w) - \psi_t(w_{LMt}))f(w)dw.$$

Using $\psi_t(w) = V_{Mt}(w)$ for $w \geq w_{LMt}$ and rearranging the above then yields

$$\int_{w_{LMt}}^{\infty} (\psi_{t+1}(w) - \psi_{t+1}(w_{LMt}))f(w)dw > \frac{1}{1-\beta} \int_{w_{LMt}}^{\infty} \frac{w - w_{LMt}}{2^{\phi}} f(w)dw.$$

This can only hold if $T \to \infty$. Thus, in a model with a finite time horizon, $w_{LMt-1} > w_{LMt}$.

Part 2: Otherwise, w_{MS} decreases over time.

Similarly, suppose $w_{MSt} > w_{MSt-1}$. Then if a woman met a man with wage w_{MSt} at $t-1$, she would strictly prefer to marry him and stop searching. Eq. (1.10) then implies

$$\beta\lambda \int_{w_{MSt}}^{\infty} (\psi_{t+1}(w) - \psi_{t+1}(w_{MSt}))f(w)dw > \beta\lambda \int_{w_{MSt}}^{\infty} (\psi_t(w) - \psi_t(w_{MSt}))f(w)dw.$$

Using $\psi_t(w) = V_{Mt}(w)$ for $w \geq w_{MSt}$ and rearranging the above then yields

$$\int_{w_{MSt}}^{\infty} (\psi_{t+1}(w) - \psi_{t+1}(w_{MSt}))f(w)dw > \frac{1}{1-\beta} \int_{w_{MSt}}^{\infty} \frac{w - w_{MSt}}{2^{\phi}} f(w)dw.$$

As before, this can only hold if $T \to \infty$. Thus, in a model with a finite time horizon,

$w_{MSt-1} > w_{MSt}$.

Part 3: w_{LSt} does not change over time.

This follows directly from inspection of eq. (1.9). ■

Proof. (Lemma 1) Proposition 3 shows that over time women are less likely to search for a new mate while married. Thus, older women will be less likely to get divorced, regardless of their current marital status and conditional on both education and (future) spouse's wage. ■

Proof. (Proposition 4) To show that age at marriage increases with education, one must consider those who would marry after high school, those who would marry after college, and those who do neither. First take those who would marry their high school sweetheart. Going to college necessarily delays marriage and thus can only increase age at marriage. Additionally, those that marry neither their college nor their high school sweethearts marry later when they go to college. To see this, one can differentiate the value functions after education to find that

$$\frac{\partial V_{St}}{\partial z} \geq \frac{\partial V_{Lt}}{\partial z} = \frac{\partial V_{Mt}}{\partial z} > 0$$

implying that an increase in z (which occurs in college) increases w_{LSt} and w_{MSt}, leaving w_{LMt} unchanged. Therefore, the effect of college on earnings implies later marriage. Further, entering the marriage market later will mechanically lead to later marriage.

Finally, suppose that if a woman goes to college she will marry directly afterwards but she would not marry directly after high school. One can show that if the probability a woman will meet a suitable spouse between high school and college is greater than one half, these women will marry later if they go to college than if they do not. So long as the arrival rate is substantially large, this condition will hold.

One can finally see that marital stability increases with education by differentiating a woman's value functions with respect to z, as above, and noting the relative changes in cutoff values. ■

Proof. (Proposition 5)

Part 1: One can differentiate the various value functions to find that

$$\frac{\partial V_{St}}{\partial z} \geq \frac{\partial V_{Lt}}{\partial z} = \frac{\partial V_{Mt}}{\partial z} > 0$$

implying that an increase in z increases w_{LSt} and w_{MSt}, leaving w_{LMt} unchanged. All women thus have higher marriage standards and it takes them longer to marry if currently single. This also causes some already married women to choose to become single. The increase in w_{MSt} over w_{LMt} further leads some previously divorce-prone women to no longer be divorce-prone; however, the previously married women who make this change will either divorce in favor of singlehood or were not previously looking for a spouse (increasing divorce rates for the set of switchers who were married before the change). To see this last conclusion, denote the cutoff values after an increase in z with a $''$ symbol. Then, initially $w_{LM} > w_{MS} > w_{LS}$. If this change occurs $w''_{LS} > w''_{MS} > w''_{LM} = w_{LM}$. If those who switch orderings previously looked for a new partner, they will divorce as $w''_{LS} > w_{LM} > w$.

Finally, to see that education increases, note that the increase in z leads to an increase in the absolute return to education. Thus, holding search behavior constant, women will seek more education. Moreover, an increase in z leads women to be single for a longer period, further increasing the returns to education and supporting the result which holds search constant. Therefore, education must increase in response to an increase in z.

Parts 2 and 3: One can again differentiate to find that

$$\begin{aligned} 0 &= \frac{\partial V_{Lt}}{\partial k} = \frac{\partial V_{Mt}}{\partial k} > \frac{\partial V_{St}}{\partial k}. \\ 0 &> \frac{\partial V_{St}}{\partial \phi} > \frac{\partial V_{Lt}}{\partial \phi} \\ 0 &> \frac{\partial V_{St}}{\partial \phi} > \frac{\partial V_{Mt}}{\partial \phi} \end{aligned}$$

Steps similar to those above then give the conclusions for divorce and age at marriage.

For education, slightly different logic is required. In these cases, the relative value of being married decreases compared to that for being single. But as my model restricts marriage to those finished with their education, this will lead to an increase in the value of schooling versus search in the marriage market, thus increasing female college attendance.

Part 4: With a higher return to education, single women will obtain more schooling (increasing age at marriage and the eventual stability of marriage, see Proposition 4) and the wage a woman can earn will increase. Part 1 then implies that an increase in women's returns to education increases age at marriage, educational attainment, and marital stability for singles and increases divorce for married women. ■

Analysis of additional comparative statics for the model (with respect to λ and c) available upon request.

A.3 Simulation Appendix

A.3.1 Sensitivity Analysis

Chapter 1 of this dissertation develops and simulates a model of the marriage market. Recall that the model and the accompanying simulation demonstrated that monotone decreases in the gains to marriage can cause age at marriage to increase and divorce rates to first rise and then fall. The former trend occurs because as the gains to marriage decrease, women search longer for a spouse. The latter trend occurs because a decrease in the gains to marriage asymmetrically impacts married and single women. Decreases in the relative value of marriage lead to higher divorce rates among women married at the time of a change. However, the same change also induces single women to be pickier about whom they marry and to wait longer to marry. These effects imply that a decrease in the value of marriage first leads to higher divorce rates but eventually causes marriages to become more stable.

To situate this sensitivity analysis within the context of that paper, Table A.1 summarizes the parameters chosen for the initial model. Recall that the simulated model ignores women's choice of education and allows women to search for $T = 10$ periods of two years each. The original series of shocks to the gains to marriage and the resultant changes in age at marriage and divorce are shown in Figure 1.7A. The model responds similarly to changes in each individual variable, as shown by Figure 1.7B.

I consider the following variation in the model's parameters to demonstrate that this framework produces similar results even when these values vary substantially.

1. The arrival rate of offers of marriage is set to one-half or 1.5 times its initial value ($\lambda = 1/3$ or $\lambda = 1$).

2. The initial relative cost of search while single is set to one-half or double its initial value ($k = 0.085$ or $k = 0.34$). This parameter still decreases in a linear manner to $k = 0$ from $t = 20$ to $t = 22$.

3. The cost of search while married is set to one-half or double its initial value ($c = .5E[w]$ or $c = 2E[w]$).

4. The standard deviation of the normal distribution underlying the distribution of male wages is set to one-half or double its initial value ($w \sim \log N(1.92, 0.207)$ and $w_h \sim \log N(1.54, 0.140)$ or $w \sim \log N(1.92, 0.826)$ and $w_h \sim \log N(1.54, 0.560)$).

Figures A.6-A.13 contain the impulse response functions associated with allowing for one of the above changes. Figures A.14-A.37 show the impulse response functions associated with simultaneously allowing for two of the above differences. The only parameterizations that yield responses vastly different from the initial results are those where the costs of search during marriage are low. In this case, the divorce rate begins at such a high level that it cannot increase (women already choose to search during almost all marriages). Such parameterizations are unlikely to be relevant, as they yield baseline levels of divorce in excess of 80 percent.

Table A.1: Baseline Parameterization of Model for Initial Cohort

Parameter	Baseline Value
β	0.90^2
λ	$2/3$
2^ϕ	1.10
k	0.17
w	$\log N(1.92, 0.413)$
w_h	$\log N(1.54, 0.280)$
c	$E[w]$
z	$.6E[w]$

Figure A.6: Low λ

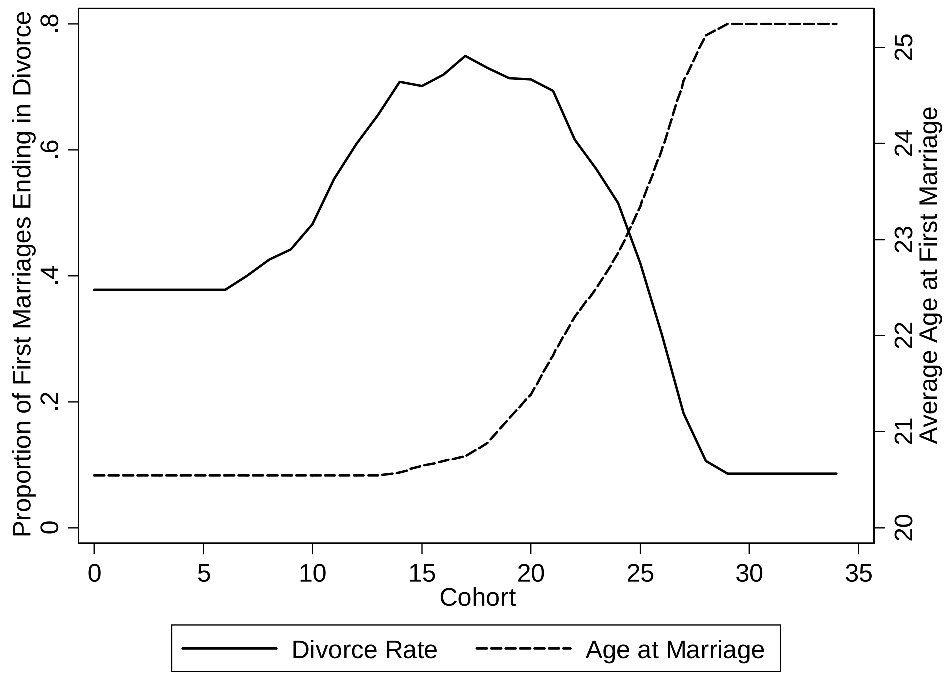


Figure A.7: High λ

Figure A.8: Low k

Figure A.9: High k

Figure A.10: Low c

Figure A.11: High c

Figure A.12: Low Male Variance

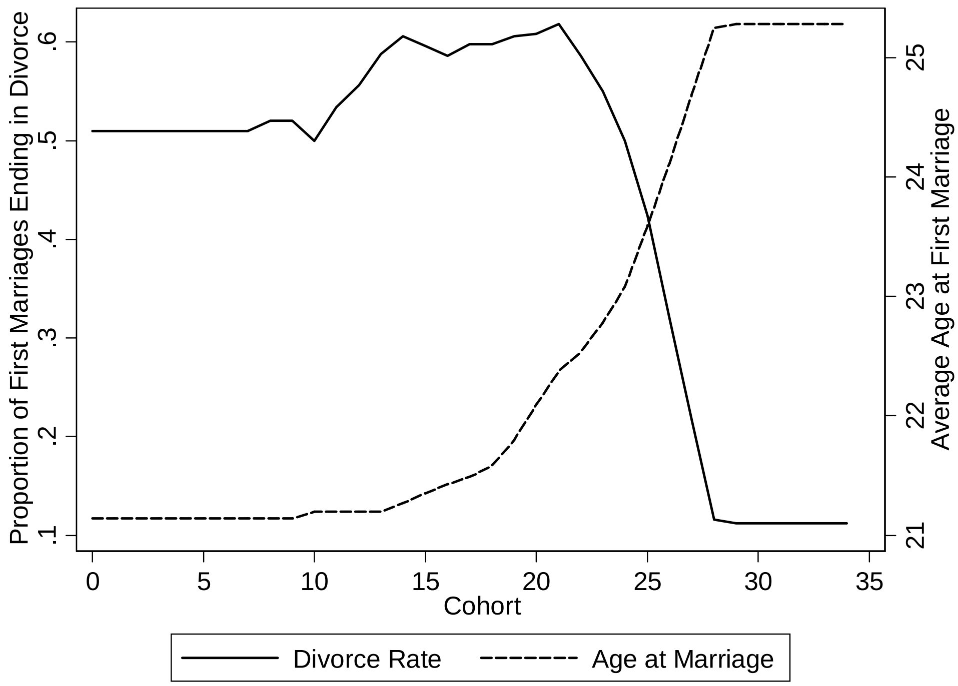


Figure A.13: High Male Variance

Figure A.14: Low Male Variance, Low λ

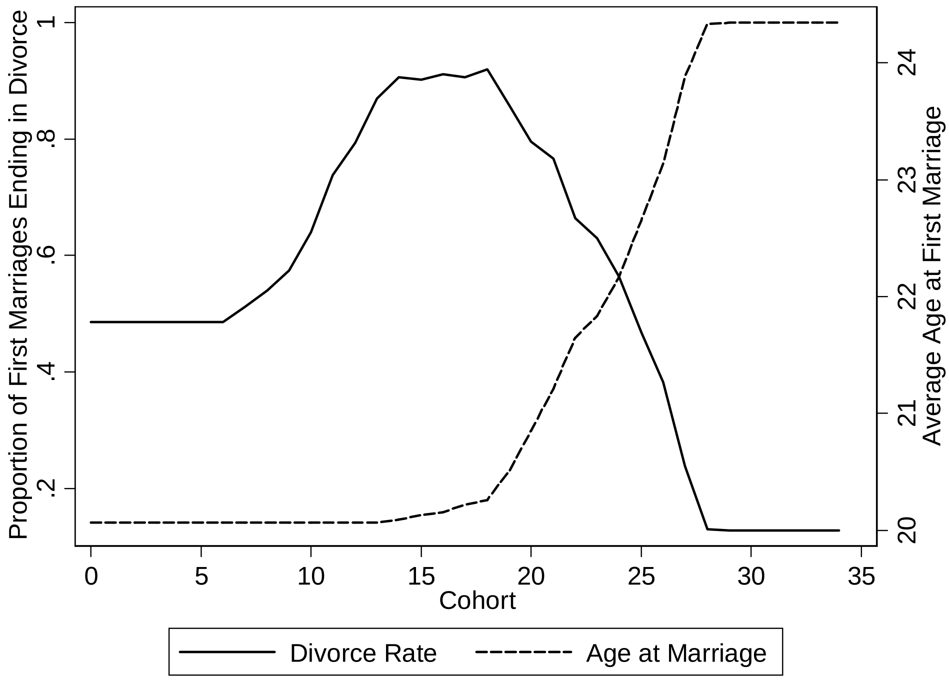


Figure A.15: Low Male Variance, High λ

Figure A.16: Low Male Variance, Low k

Figure A.17: Low Male Variance, High k

Figure A.18: Low Male Variance, Low c

Figure A.19: Low Male Variance, High c

Figure A.20: High Male Variance, Low λ

Figure A.21: High Male Variance, High λ

Figure A.22: High Male Variance, Low k

Figure A.23: High Male Variance, High k

Figure A.24: High Male Variance, Low c

Figure A.25: High Male Variance, High c

Figure A.26: Low c, Low λ

Figure A.27: Low c, High λ

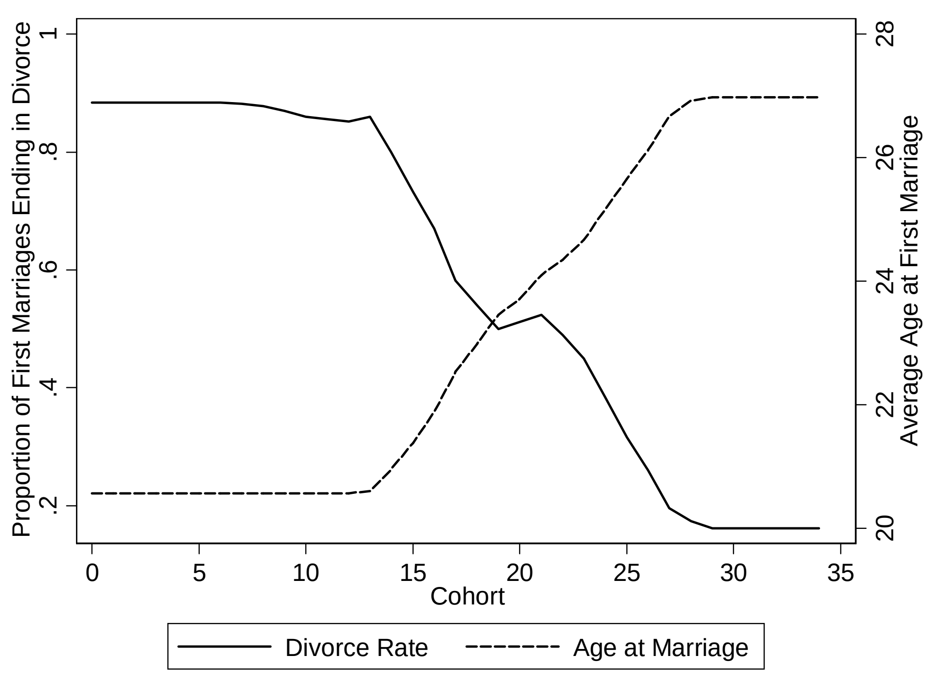


Figure A.28: Low c, Low k

Figure A.29: Low c, High k

Figure A.30: High c, Low λ

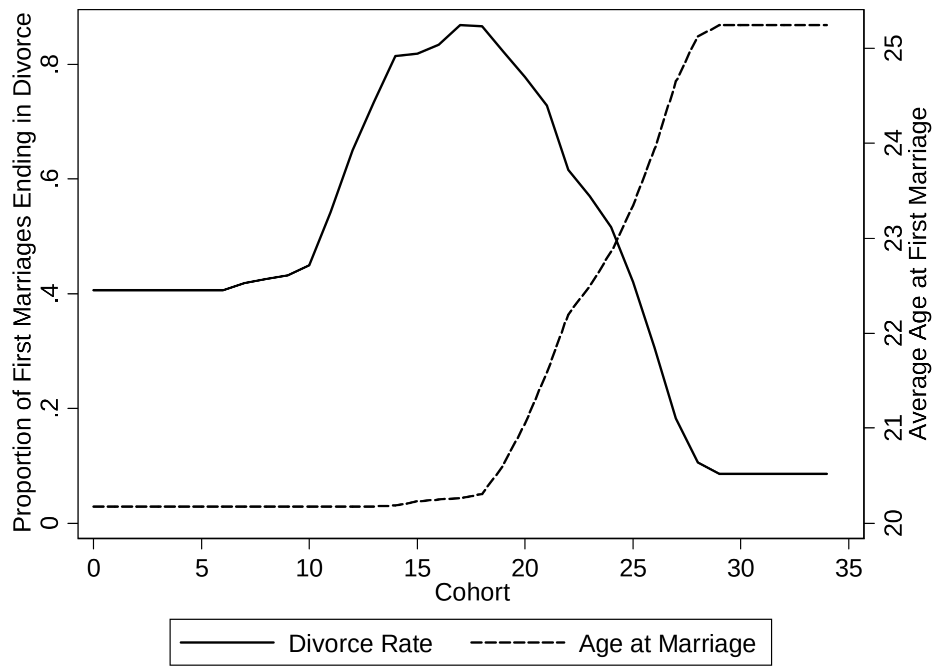


Figure A.31: High c, High λ

Figure A.32: High c, Low k

Figure A.33: High c, High k

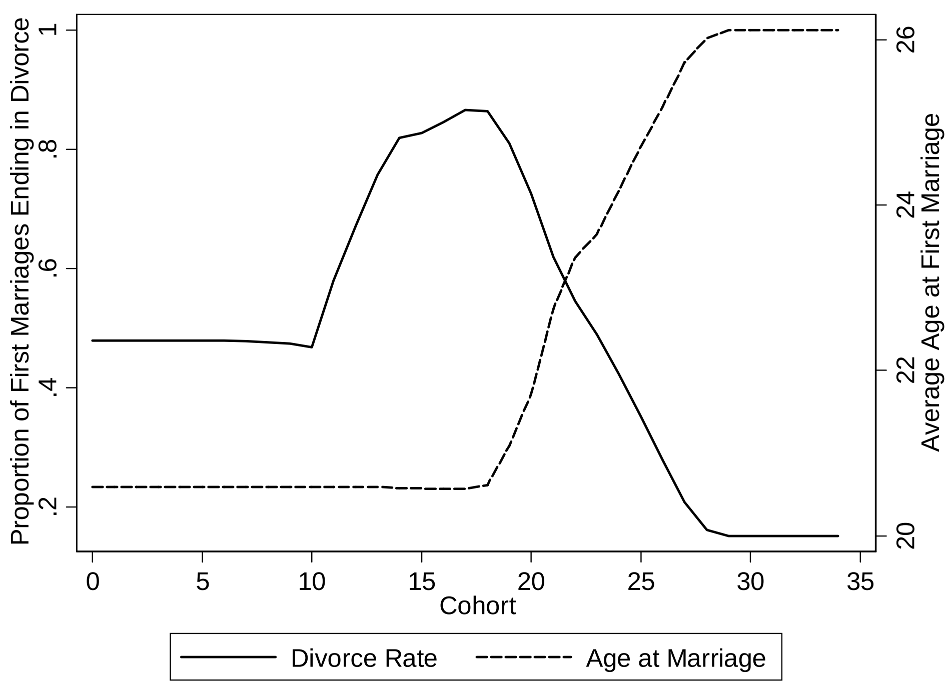


Figure A.34: Low λ, Low k

Figure A.35: Low λ, High k

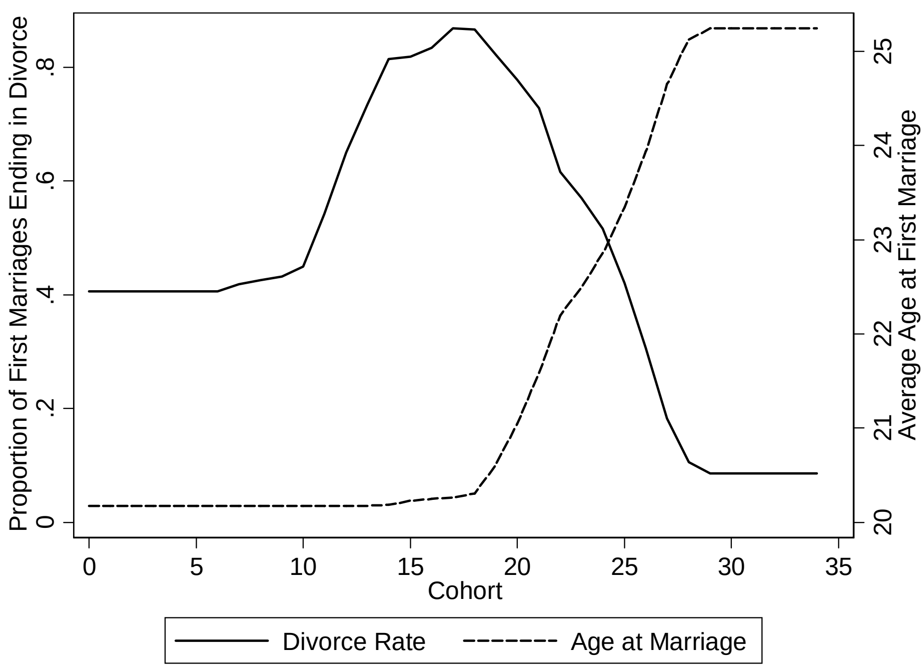


Figure A.36: High λ, Low k

Figure A.37: High λ, High k

A.3.2 Further Simulations

Changes in the Costs of Divorce My main simulation and the sensitivity analyses suggest that divorce rates are higher in the initial steady state (circa 1950) than in the final steady state (circa 2000). In reality, divorce rates remain higher today than in the 1950s, though lower than they were at their peak. This auxiliary simulation addresses the potential issue. I suppose that as the gains to marriage decreased, the cost of divorce also decreased for successive cohorts, starting at $t = 15$ (see Figure A.38A). Decreases in the gains to marriage need not be associated with a new steady state that has lower divorce rates, as shown in Figure A.38B by the simulation that changes c along with ϕ, z, and k. The changes in c also imply an initial dip in the average age at first marriage. But after falling briefly, brides' ages begin to rise again and the average bride from my final cohort marries just after her 25th birthday.

Changes in Male Wages Although this study focuses on trends in the divorce rate from 1950 to 2004, other important variation in divorce can be explored through my model. In particular, the changes in family structure associated with the Great Depression may be of interest. During the early 1930s, male unemployment increased greatly. At this time, many women did not work in the market and a woman's contribution to the household was largely linked to the value of her home production.

Thus, I analyze the effect of a decrease and subsequent rebounding of male wages (holding z constant) on age at marriage and divorce.[172] The divorce rate rose as the market tumbled during the Great Depression but it fell back to a level near its initial value as the market recovered.[173] Age at first marriage also increased during the depression and

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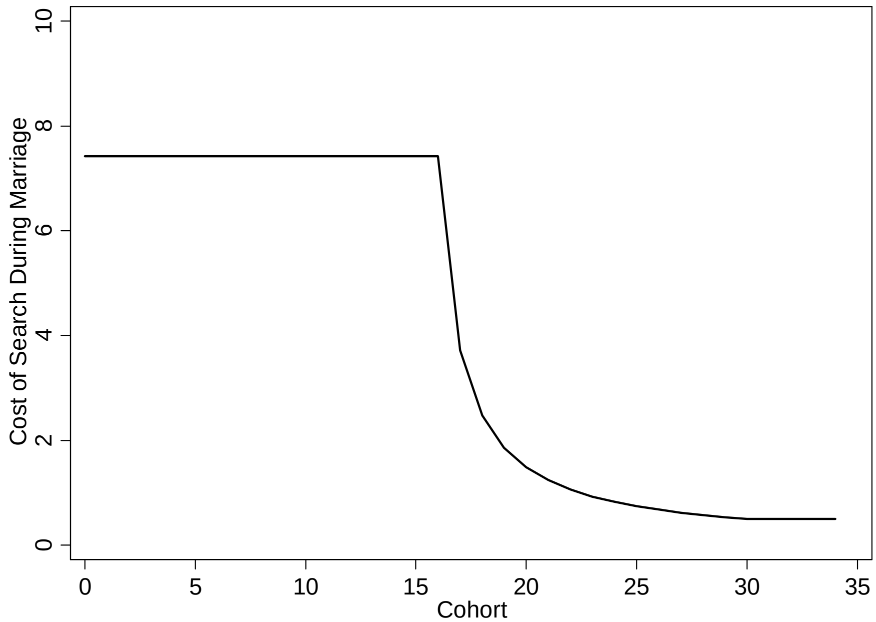
[172] Though the real wages of the employed did not change very much in the Great Depression (due to wage stickiness and falling prices), expected wages (accounting for the decreased probability of finding a job) certainly did.

[173] See Carter, et al. (2006).

Figure A.38: Simulation of Search Model Including Declining Costs of Divorce

Panel A: Changes in Costs of Search During Marriage



Panel B: Resulting Trends in Divorce Rate and Age at First Marriage

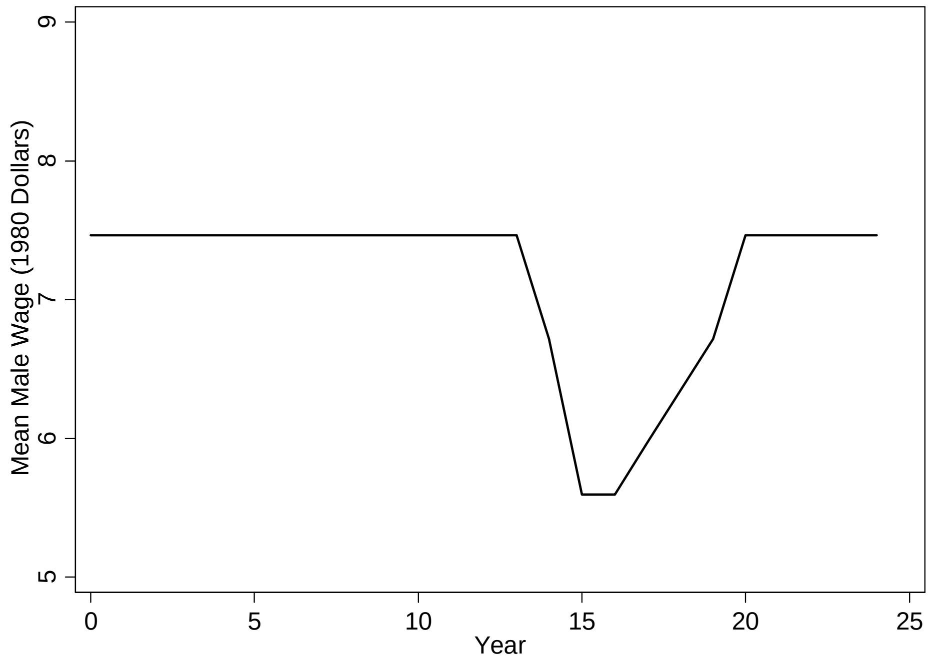
declined afterward.[174]

My model produces relatively similar changes in response to a drop and recovery of male wages, as shown in Figure A.39. The divorce rate and the average age at marriage spike for cohorts experiencing a decline in men's real wages. When the wage rate recovers, the age at marriage converges to initial levels. In response to the subsequent rebound of wages, the divorce rate first drops below its initial level but then converges back to its steady-state value.

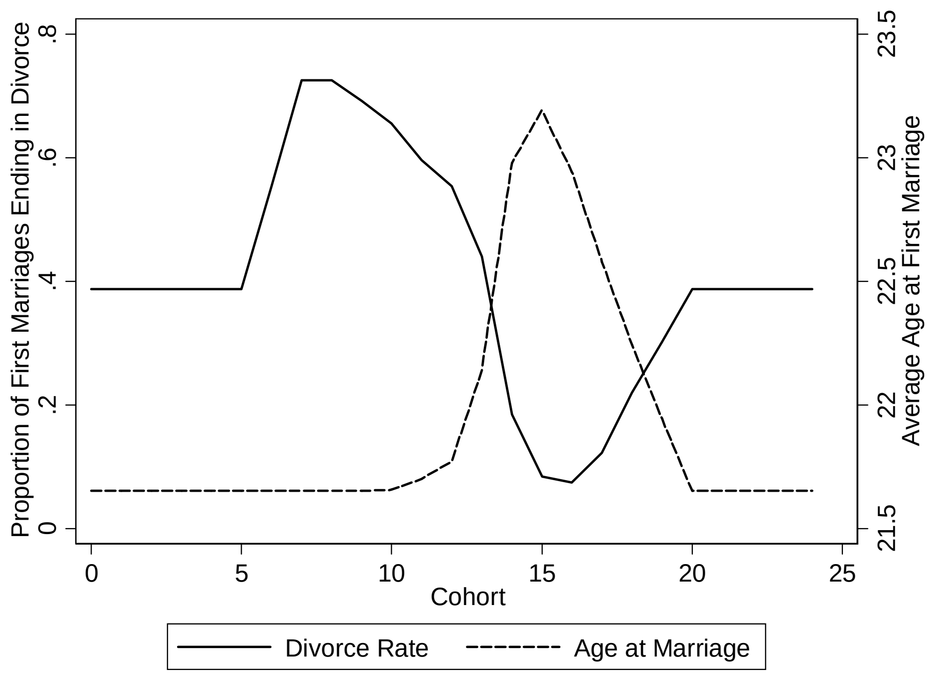
[174] See Rodgers and Thornton (1985).

Figure A.39: Simulation of Search Model: The Great Depression

Panel A: Changes in Male Wages (Variance Held Constant)



Panel B: Resulting Trends in Divorce Rate and Age at First Marriage



B Appendices to Accompany Chapter 2

B.1 Data Sources

B.1.1 Consumer Expenditure Survey

This paper uses data on a variety of different expenditures from the Consumer Expenditure Survey (CEX). The dataset contains two parts: an interview survey (CEXI) for expenditures over the course of the past three months and a diary survey (CEXD) in which households record smaller, everyday purchases for one or two weeks (in particular, groceries at a very disaggregated level). The former survey has a short panel component that I ignore. From both of these surveys, I select married couples with quality data on both spouse's incomes and household-level expenditure.[175] Further, each spouse is not disabled and ages 18 to 64 within the selected sample. The study only uses those households with expenditure data corresponding to the period between 1984 (early years of the CEX had different survey methodology) and the implementation of TANF (1996 or 1997, depending on the state). I also require that families in my sample include a male head of household usually working at least 30 hours per week and children under 18 that are biologically related to both spouses. These restrictions isolate the intact families ineligible for AFDC that contain a wife who could potentially receive funding if divorced.

Both the CEXD and CEXI contain data on state of residence for a limited number of consumer units. Five states are selected that include CEXI observations for at least 50 families with children for eight years before and two years after the government implemented a welfare waiver. See Appendix Table B.1.

The CEXD has far fewer observations but each of these states has at least 20 observations per each of the above years. Table B.1 also contains the precise number of

[175] I use the Gourinchas and Parker (2002) income imputation where possible.

Table B.1: Dates of AFDC Waiver Implementation and Sample Size

State	Date	Treated?	Control?	Observations in CEXI	Observations in CEXD
Alabama					
Alaska					
Arizona	Nov-95				
Arkansas	Jul-94				
California	Dec-92	✓		4596	1350
Colorado			✓	1110	360
Connecticut	Jan-96				
D.C.	Oct-95				
Delaware					
Florida			✓	2051	619
Georgia	Jan-94	✓		1205	315
Hawaii	Feb-97				
Idaho	Nov-93				
Illinois	May-95	✓		1777	547
Indiana	Oct-93		✓*	845	247
Iowa					
Kansas					
Kentucky					
Louisiana					
Maine	Nov-95				
Maryland	Mar-96		✓	917	345
Massachusetts					
Michigan	Oct-92	✓		844	220
Minnesota			✓*	1130	315
Mississippi	Jun-95				
Missouri	Oct-95		✓	1346	381
Montana	Feb-96				
Nebraska					
Nevada	Jul-96				
New Hampshire	Oct-92				

Continued on the following page.

Table B.1, Continued: Dates of AFDC Waiver Implementation and Sample Size

State	Date	Treated?	Control?	Observations in CEXI	Observations in CEXD
New Jersey		✓		1148	368
New Mexico					
New York			✓	2549	712
North Carolina	Oct-95				
North Dakota					
Ohio	Jul-96		✓	1883	550
Oklahoma					
Oregon	Feb-93				
Pennsylvania			✓	2894	854
Rhode Island					
South Carolina					
South Dakota	Jun-94				
Tennessee	Sep-96				
Texas	Jun-96		✓	2691	832
Utah	Jan-93				
Vermont	Jul-95				
Virginia	Jul-94		✓**	827	228
Washington	Jan-96		✓	1058	327
West Virginia	Jan-96				
Wisconsin			✓**	1015	269
Wyoming					

Notes and sources: Treated and control states selected as described in Appendix B.1.2.
Sample size listed is for all pre-TANF years.
Waiver dates taken from Schoeni and Blank (2000).
*Not used as control in GA. **Not used as control in GA or IL.

observations from both surveys. The control states listed in the table must have the same data available with respect to the date that a waiver was implemented in one of the treated states. These states include those that implemented a reform but did so late enough so that I can extract two years of data in which the treated state had a waiver in place but the control state did not.

Using the data selected by this procedure, I chose 12 expenditure categories encompassing 149 goods listed in the CEXI and all grocery listings (111 items) from the CEXD, as well as two residual categories accounting for the balance of income and expenditure:

- Food for Home Consumption: The CEXI measure of food expenditure includes all food and non-alcoholic beverages purchased at grocery stores, convenience stores, and specialty stores. The CEXD contains more detailed data, and I delineate three categories based on how much time is likely needed to produce a final meal using the good as an input: high (flour), intermediate (cake mix), and low (cake). Instead of a quarterly expenditure share, for the CEXD I calculate weekly expenditures as a proportion of all spending on groceries.

- Food Away from Home or Prepared by Others: expenditure on any food or non-alcoholic beverages that are, for certain, made by someone outside of the consumer unit.

- Child Care: day-care, care in one's home, and care in a preschool.

- (Luxury) Home Maintenance Services: housekeeping and gardening services.

- Men's (Women's) Clothing, Accessories, and Personal Care: all expenditures that could be associated with dress or appearance and are explicitly consumed by males (females).

- Children's Clothing and Accessories: These expenditures are analogous to those above but exclude personal care due to lack of identification in the CEXI. Goods within this category are explicitly purchased for girls, boys, or infants.
- Education: books, supplies, and tuition for kindergarten and higher grades.
- Alcohol and Tobacco: products consumed both inside and outside the home.
- Residual Expenditure: all remaining goods in the CEXI.
- Residual Income: the difference between one fourth of annual income and total reported quarterly expenditure.

A full characterization of the contents of each category is available upon request. Further, I list the categories in Appendix Tables B.2A and B.2B along with each one's expenditure share (as a fraction of BLS-measured total expenditure for the CEXI and total expenditure on groceries for the CEXD) before the first waiver was implemented (1992:4) for treated states and control states. The table also contains the fraction of families in these two types of states making any purchases within each category.

B.1.2 Waiver Dates

I utilize data from Schoeni and Blank (2000) on dates of waiver approval (see Table B.1) and implementation, basing the analysis on the latter. Any data coming from the period between when a waiver was approved and when it was implemented is discarded; however, all of the results stand if one instead uses the date of a waiver's approval to define the treatment indicator.

B.1.3 Current Population Survey

Similarly, I extract married women with children from the IPUMS version of the March CPS for 1984 through the implementation of TANF. Women must have a complete,

Table B.2A: Expenditure Shares and Proportions of Families Purchasing Goods Prior to Reform

Families with Mothers with a High School Education or Less

Category	Control Mean Share x100	Treated Mean Share x100	Control Proportion Purchasing	Treated Proportion Purchasing
Food for Home Consumption	15.18	15.90	100%**	100%
High Time Input Groceries/All Groceries	47.84	47.71	99%	99%
Intermediate Time Input Groceries/All Groceries	23.64	23.05	99%	99%
Low Time Input Groceries/All Groceries	27.94	28.14	98%	97%
Food Away From Home or Prepared by Others	2.66	2.73	97%	97%
Child Care	1.39	1.54	26%	28%
Home Maintenance Services	0.15	0.13	10%	9%
Men's Clothing, Accessories, and Personal Care	1.46	1.45	87%	87%
Women's Clothing, Accessories, and Personal Care	1.90	1.84	87%	86%
Children's Clothing and Accessories	1.92	1.97	80%	80%
Education	1.54	1.19	53%	46%
Alcohol and Tobacco	2.44	2.19	75%	73%
Residual Expenditure	71.35	71.07	100%	100%
Residual Income	34.19	37.01	100%	100%

Notes and sources: ** Significantly higher at the 5% level given standard errors accounting for state-level clustering. The denominator of the share is the quarterly, BLS-reported total expenditure for all goods, except the grocery variables. These shares are weekly shares of all grocery expenditure. Data comes from selected treated and control states in CEXI and CEXD, 1984-1992:3 (the first waiver implementation). See Appendix B.1.1 for more details on sample selection. Unconditional mean expenditure shares reported. Estimates calculated using full sample weights.

Table B.2B: Expenditure Shares and Proportions of Families Purchasing Goods Prior to Reform

Families with Mothers Not Working FT (30+hrs/wk)

Category	Control Mean Share x100	Treated Mean Share X100	Control Proportion Purchasing	Treated Proportion Purchasing
Food for Home Consumption	15.29	15.10	100%	100%
High Time Input Groceries/All Groceries	46.87	46.52	99%	99%
Intermediate Time Input Groceries/All Groceries	24.42	23.68	99%	99%
Low Time Input Groceries/All Groceries	28.11	29.21	98%	98%
Food Away From Home or Prepared by Others	2.50	2.72**	98%	98%
Child Care	0.72	0.91	27%	31%
Home Maintenance Services	0.23	0.28	15%	17%
Men's Clothing, Accessories, and Personal Care	1.52	1.52	89%	89%
Women's Clothing, Accessories, and Personal Care	1.88	1.94	87%	88%
Children's Clothing and Accessories	2.05	1.99	83%	85%
Education	1.64	1.66	52%	46%
Alcohol and Tobacco	1.96	1.75	71%	70%
Residual Expenditure	72.22	72.13	100%	100%
Residual Income	29.17	27.83	100%	100%

Notes and sources: ** Significantly higher at the 5% level given standard errors accounting for state-level clustering. The denominator of the share is the quarterly, BLS-reported total expenditure for all goods, except the grocery variables. These shares are weekly shares of all grocery expenditure. Data comes from selected treated and control states in CEXI and CEXD, 1984-1992:3 (the first waiver implementation). See Appendix B.1.1 for more details on sample selection. Unconditional mean expenditure shares reported. Estimates calculated using full sample weights.

non-allocated record for demographic and hours of work variables. Additionally, any observations for those who are disabled or living in group quarters are discarded. The CPS does not allow one to condition on the biological relationship between parents and children, so both parents and step-parents are included in the sample. To improve comparability with the CEX data, the CPS sample contains only families from the states with sufficient data in the CEX (see Table B.1). Regressions for female labor force participation and wages also condition on the male head of household working 30 or more hours per week in the market, as in the CEX (N=103,271, 72,457 of which worked any number of hours in the past year). A paucity of data does not allow one to make this restriction in the analysis of food stamp receipt (5,751 families received such aid).

The sample of female wage earners is further restricted to avoid many common issues with wage data. I drop those in the armed forces or agricultural sectors, as well as those with allocated wages. Women with wages under the first percentile or over the 98th percentile of men's wages are also excluded. This leaves a total of 56,520 female wage earners.

B.2 The Synthetic Control Estimator

Suppose for a given treated state s, there are S potential control states with adequate data, expenditure is observed for T_0 periods ($t = -8, ..., -1$) prior to treatment, and $t = 0, 1$ are the treated periods of interest. Let W_s be a vector of weights $W_s = (w_{1s}, w_{2s}, ...w_{Ss}) \geq 0$, such that $\sum_{n=1}^{S} w_{ns} = 1$. Denote $Y_{st}(Treat_s)$ as the vector of mean expenditure shares in state s at time t given treatment status $Treat_s$ and suppose $Y_{st}(1)$ =$Y_{st}(0)$ for all $t < 0$.

ADH show that the ideal synthetic control group has weighting vector W_s^* so that $\sum_{n=1}^{S} w_{ns}^* Y_{nt} = Y_{st}\ \forall t < 0$ and $\sum_{n=1}^{S} w_{ns}^* Z_n = Z_s$. This synthetic control is ideal in the sense that $\sum_{n=1}^{S} w_{ns}^* Y_{nt} = Y_{st}(0)$, leading to a straightforward calculation of the treatment effect for good j in state s at time t of $\widehat{\alpha_{stj}} = \widehat{Y_{stj}(1)} - \widehat{Y_{stj}(0)}$. Moreover, even if a match is not ideal, one can use the synthetic control group to calculate treatment effects using the standard DD equation or

$$\widehat{\alpha_{s\tau j}} = [\widehat{Y_{s\tau j}} - \sum_{n=1}^{S} w_{ns} \widehat{Y_{n\tau j}}] - \sum_{t<0} [\widehat{Y_{stj}} - \sum_{n=1}^{S} w_{ns} \widehat{Y_{ntj}}]/T_0. \tag{3.5}$$

A perfect match sets the second term in brackets to zero. Otherwise, including this term allows one to correct for an imperfectly matched control group in the standard way. One can see that the procedure amounts to calculating a basic DD estimate using the best possible control group given the available data.

Intuitively, one would like to choose W_s to minimize differences between the treated group and the synthetic control group. I iterate ADH's prescribed method to select weights minimizing the Mahanalobis distance between vectors of the form $(Z_i, Y_{i,-8}...Y_{i,-1})$. That is, the synthetic control group is chosen using both demographic predictors of expenditure and prior expenditure shares. The procedure yields a set of weights for each treated state, listed in Appendix Table B.3.

Table B.3: Weights Calculated by Treated State using the Synthetic Control Method

	Families with Mothers with ≤ HS Ed					Families with Mothers not Working FT (30+hrs/wk)				
	Treated States					Treated States				
Control States	CA	GA	IL	MI	NJ	CA	GA	IL	MI	NJ
CO	0.00	0.00	0.00	0.00	0.17	0.00	0.00	0.00	0.00	0.12
FL	0.00	0.00		0.00	0.00	0.23	0.00		0.00	0.00
IN	0.00		0.00	0.00	0.00	0.00		0.29	0.00	0.00
MD	0.00	0.53	0.00	0.00	0.38	0.00	1.00	0.00	0.00	0.58
MN	0.00	0.00	0.21	0.00	0.00	0.10	0.00	0.00	0.10	0.00
MO	0.00		0.09	0.00	0.00	0.01		0.23	0.23	0.00
NY	0.10	0.00	0.05	0.00	0.00	0.00	0.00	0.00	0.00	0.00
OH	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.13	0.00	0.00
PA	0.00	0.00	0.29	1.00	0.00	0.00	0.00	0.00	0.53	0.00
TX	0.76	0.47	0.35	0.00	0.00	0.50	0.00	0.23	0.00	0.00
VA	0.14		0.01	0.00	0.27	0.00		0.00	0.00	0.12
WA	0.00	0.00	0.00	0.00	0.00	0.16	0.00	0.12	0.15	0.02
WI	0.00			0.00	0.17	0.00			0.00	0.16

Notes and sources: Data from selected treated and control states in CEXI and CEXD, 1984-TANF implementation. See Appendix B.1.1 for details on data selection. Optimal weights were chosen based on pre-treatment expenditure, and family size (three, four, five, or six or more members), mother's labor force participation (FT, PT, NW), education (five categories for each spouse), income (female earned, male earned, and unearned), race (a dummy variable for both spouses being white), urban location, and having children aged zero to six, seven to 12, and 13 to 17, using Abadie, Diamond, and Hainmueller's (2010) method.

C Appendices to Accompany Chapter 3

C.1 Access to Abortion and the Pill

A mother is defined as having access to abortion ($abort\_access$) if she completed her first trimester of pregnancy after abortion's legalization in New York (July 1, 1970). In the NCHS, the last week of the first trimester is determined using gestational length and date of birth. In the ACS, I use the day six months prior to birth. Though New York allowed legal abortion until the end of a woman's second trimester, such procedures were rarer, more costly, and more dangerous (see Gutcheon 1973).

The variable $pill\_access$ indicates if a mother had legal access to the pill if she was unmarried and between 16 and 20 years of age at the date of conception, calculated in a fashion parallel to the determination of the end of her first trimester. Pill access for married women, women over 20, or women under 16 did not vary in New York over the time period of interest. See Bailey (2010) and Goldin and Katz (2002) for details.

C.2 Natality Birth Data

I use the National Center for Health Statistics Vital Statistics Natality Birth Data (NCHS) to demonstrate that abortion legalization led to sharp changes in the size and nature of birth cohorts. This 50 percent sample of births is available starting in 1968 and contains information on month and day of birth and gestational age in weeks. In some cases, day of birth is omitted (in particular, early in the sample). For these observations, I set a child's birthday to the median date of birth for those born in the same month between 1968 and 1975. I drop all observations reporting a gestational age at birth of 45 weeks or more. Those with missing gestational age data are assigned to have a pregnancy lasting the median number of weeks in the sample (40). I then use birthday and gestational age to estimate the week of conception, which implies the final week of a woman's first trimester.

The mothers in my selected sample all reside in New York. I select all births from this dataset where gestational age implies a woman became pregnant between April 1968 and March 1972, or ended her first trimester within two years of New York's legalization of abortion. To avoid potential issues from the estimation of gestational age and birth date, I omit any observations reflecting that a woman would have ended her first trimester of pregnancy within one week of the law change. The resulting sample contains 571,406 births.

C.3 The American Community Surveys

The bulk of my analysis uses the 2005 to 2010 American Community Surveys (ACS).[176] From the surveys, I select a sample of people born in New York who identify as white, black, or Hispanic. All observations in the sample have a value of survey year-age between 1965 and 1975. Thus, the workers were born in or after 1964 but no later than 1975. I omit any observation with allocated age, race, or ethnicity data. To avoid any concerns about second-trimester abortion, I also remove any observations in which expected access to first and second trimester abortion differ.

From the larger group, I select a sample of workers similar to that used by Autor, Katz, and Kearney (2008). These individuals report non-zero usual hours of work and weeks of work in the past year and identify as civilian, non-self-employed workers. I also omit any observations with allocated earnings data. I calculate the hourly wage as yearly wage and salary income divided by the product of usual weekly hours and weeks worked last year. In cases where weeks worked is only available as an interval variable, I use the mean weeks worked for observations within this interval.

The ACS topcodes income data above the 99.5th percentile by state, and replaces

[176]The ACS began recording quarter of birth in 2005. My identification strategy utilizes this variable in an important way, leading me to exclude pre-2005 data from my analysis.

such values with the mean income above this quantile. I omit any observation with an hourly wage that would imply topcoding if the worker reported working more than 1,750 hours per year (but is not topcoded due to limited hours of work). I further omit observations for hourly wages in the first percentile of the wage distribution. The resultant sample contains 95,314 observations. I draw similar samples from the rest of the United States, yielding 1,231,007 observations in total.

C.4 Validation of Estimates of Abortion Access in the ACS

My identification procedure hinges on two key assumptions: ACS interviews are conducted uniformly across the year and births occur uniformly across time.

Although detailed data on date of interview is unavailable, the sampling methodology used by the Census Bureau when conducting interviews for the ACS suggests the validity of the first assumption. Each year of the ACS actually consists of 12 independent monthly surveys, implying equal distribution of interviews over the months of the year. Each month, surveys are mailed out to households, who return them at their leisure. If a household does not return a survey by mail by the end of the month, they are contacted via phone the following month. Finally, some households not available by phone are interviewed in person. Staffing concerns suggest that phone and in-person interviews (about half of responses) occur over the course of the month. One might also assume that mail-in questionnaires are filled out throughout the month. Finally, the monthly nature of the survey and structure of interviews by mail, by phone, and in person implies that time of year is uncorrelated with interview type. Thus, one can reasonably assume that interviews occur roughly uniformly across the year.[177]

To assess the validity of my second assumption (uniformity of births), I suppose that each of the infants recorded in the NCHS survived into adulthood and was interviewed by

[177] For more detail, see United States Census Bureau (2009).

the ACS in 2005. For each observation, I randomly select a date of interview for the later survey and calculate the observation's corresponding age. I can then compare the actual probability of abortion access to the value estimated using only age, survey year, and birth quarter. Table C.1 contains a comparison of the methods.

Small differences emerge and my method may systematically underestimate mother's access to abortion. This is likely because abortion access decreased the number of pregnancies carried to term (see Table 3.1). That is, as fewer births occurred after abortion was legalized, a lower proportion of the population should have a mother who had access to abortion.

Despite this discrepancy, I continue to use the assignment rule depicted in Table 3.2. This rule is based on the null hypothesis that abortion had no effect on the number or type of children born. The actual assignment rule would imply some effect of abortion on births. A test of $\beta = 0$ based on actual access would thus not be interpretable as testing the null hypothesis of interest in its entirety. Moreover, because the probabilistic assignment generally underestimates abortion access, it will only bias estimates of the effect of abortion legalization downward. Therefore, one can view the estimates in Sections 3.6 and 3.7 as lower bounds.

Using the actual abortion access calculated from birth certificate data yields results comparable to those calculated using predicted access. The coefficients from the OLS regressions increase by about 1 log point. Blacks whose mothers had access to legal abortion have wages 18 percent higher than those whose mothers did not. Likewise, the wages of Hispanics increase by 13 percent on average when mothers gain access to legal and safe abortion. Using this alternative specification, the wages of whites still do not significantly change. Furthermore, Figure C.1 demonstrates that using actual and predicted abortion access yield similar results across the quantiles of the wage distribution.

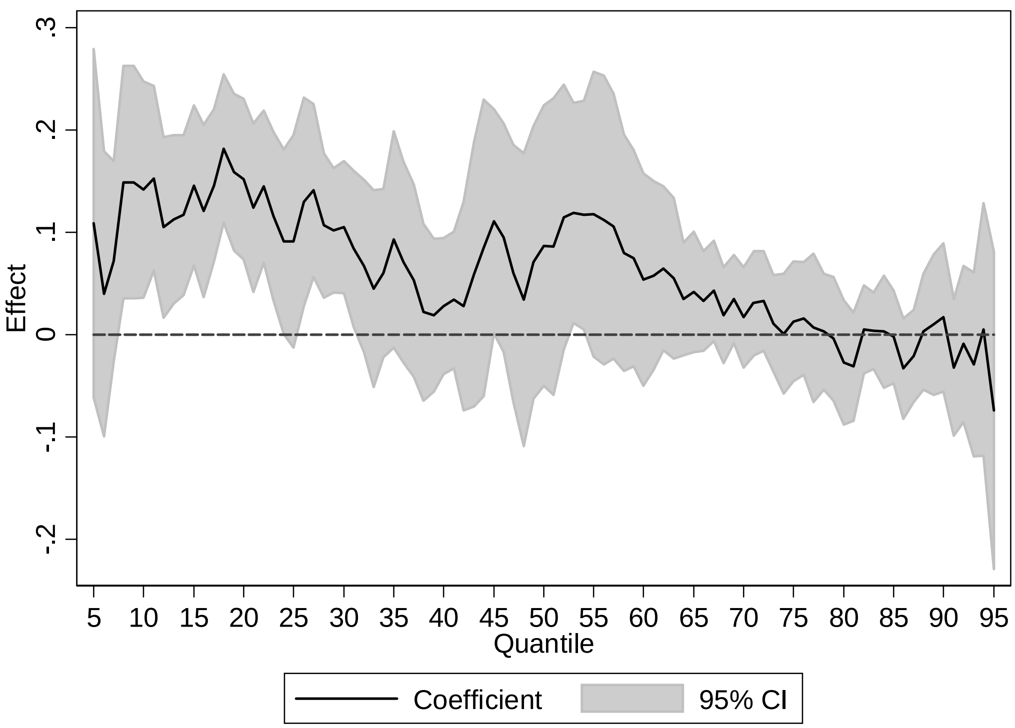
Table C.1: Mother's Actual and Predicted Abortion Access

		Proportion with 1st Trimester Access	
Age	Quarter of Birth	Actual	Predicted
. . .			
32	3	1	1
32	4	1	1
33	1	0.996	1
33	2	1	
33	3	1	1
33	4	1	1
34	1	0.750	0.875
34	2	0.609	0.625
34	3	0.349	0.375
34	4	0.145	0.125
35	1	0	0
35	2	0	0
35	3	0.0001	0
35	4	0.002	0
36	1	0	0
			. . .

Notes and sources: Actual probability of access from the NCHS Vital Statistics Natality Birth Data for mothers who were residents of New York and gave birth from 1968 to 1975. See Appendices A.2 and A.4 for details. Abortion access defined as a woman still being in her first trimester or not-yet-pregnant as of July 1, 1970. Predicted probability from author's calculations based on the assumptions that birthdays are evenly distributed across and within quarters and that gestation lasts exactly nine months.

Figure C.1: The Effect of Abortion Access Throughout the Wage Distribution

Calculated Using Actual Abortion Access from Birth Certificate Data



Notes and Sources: Data from whites, blacks, and Hispanics in the ACS 2005-2010, born 1964-1975. Abortion access calculated from birth certificate data. See Appendices C.3-C.4 and Table C.1 for details. Effects calculated using data collapsed to age-year-birth quarter cells (N=252). Effects from regression including controls for mother's birth control pill access, age-survey year fixed-effects, and birth quarter fixed-effects. Estimates calculated using ACS sample weights. Robust standard errors clustered by birth cohort (expected year and quarter of birth) used to create confidence intervals.

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