The Economic Consequences of Unilateral Divorce for Children

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ABSTRACT

This paper provides new evidence on the economic consequences of unilateral divorce laws on the future labor market outcomes of children. Using a cohort of young adults from the 1990 census, we examine the effect of living in a unilateral divorce state as a child on education, earnings, and marital status. Women with many years of childhood exposure to unilateral divorce laws have lower wages and have completed less schooling. However, there is no statistically significant effect of unilateral divorce exposure on men's wages. Both women and men are more likely to marry and less likely to get divorced with more years of exposure to unilateral divorce as a child. We also explore alternative mechanisms through which unilateral divorce laws can affect children's outcomes. The evidence suggests that while divorce rates did increase significantly as a result of the laws, bargaining power within the household is also an important factor driving our results.

Introduction

Starting in the late 1960s, a fundamental shift in divorce laws in the United States took place with the advent of unilateral divorce. Unilateral divorce made it possible for one spouse to obtain a divorce without the consent of the other.[1] In this paper, we provide new evidence on the consequences of unilateral divorce on children’s later-in-life outcomes, including educational attainment, childbearing, marital status, and earnings. Our estimates suggest that women with many years of childhood exposure to unilateral divorce laws have lower wages and have completed less schooling. Women with 9 or more years of childhood unilateral divorce exposure had weekly wages 4.2% below comparable women with no childhood unilateral divorce exposure. They completed .12 fewer years of schooling. However, men with more years of childhood exposure to unilateral divorce laws have wages that are not statistically different from the wages of men without childhood exposure to unilateral divorce.

We then explore what mechanisms could be driving the impacts of unilateral divorce on families. We argue there are two primary channels through which unilateral divorce can affect children. First, we show a direct relationship between unilateral divorce laws and parent's divorce probability. Each additional year an individual lived in a unilateral divorce state increased the probability that a child's parents were divorced by about .5 percentage points (5% of the baseline divorce probability). We present evidence from a two-sample instrumental variable approach (2SIV) that emphasizes that divorce cannot be the sole mechanism that drives the negative impacts of unilateral divorce laws on children.

Second, we discuss the impact of unilateral divorce on bargaining power within families. Unilateral divorce makes marriage contracts unenforceable, and as a result, may shift the bargaining power within the household. To test this hypothesis, we provide evidence from the annual demographic files of the Current Population Survey from 1964-1985. Using a difference-

[1] See Buehler, 1995; Marvell, 1989; Freed and Foster, various years for discussions of the legal trend toward unilateral and no-fault divorce.

in-differences technique, we show that the labor force participation of married women increases in the years immediately following enactment of unilateral divorce laws. Further, the labor force participation of unmarried women falls over the same time period, implying that the estimated effects for married women are likely attributable to shifts in bargaining power within the household. This change in intra-marital bargaining due to unilateral divorce laws could make children worse off but has no relationship to divorce. We also discuss the recent literature that attributes changes in domestic violence to shifts in unilateral divorce laws. We conclude that any understanding of the impacts of unilateral divorce must explicitly account for both the direct effects on divorce rates and the potential changes in family behavior as a result of shifts in bargaining power.

Background and Motivation

Prior to the enactment of unilateral divorce, courts required that an individual show "fault" in order to be granted a divorce. The desire of a spouse to end a marriage was not in itself a valid legal reason for divorce. A spouse petitioning for divorce had to prove that the other spouse was responsible for the marriage's failure. Even if this was proven, the court could refuse to grant a divorce, if the petitioning spouse was him or herself partly at fault for the marital breakdown or had forgiven the marital transgression. Starting in the 1970's, states began to enact several legal reforms that lowered the legal hurdles to obtaining a divorce. No-fault divorce laws allowed divorce to be granted upon mutual consent of the parties involved. A movement towards unilateral divorce statutes soon followed this trend, which allowed divorces to be granted upon the petition of only one spouse.

Much of the recent political backlash against unilateral divorce laws appears to be motivated by the perception that children are harmed by parental divorce. Many politicians claim that the passage of unilateral divorce laws was responsible for the dramatic increases in divorce rates in the 1970s. At the same time, states are now considering either repealing or amending unilateral divorce laws. For example, Louisiana and Arizona now allow covenant marriages.

Under these new marriage arrangements, couples that opt for a covenant marriage can only divorce if the spouse has committed adultery, a felony, abandonment, sexual abuse, or by mutual consent. (Arizona Supreme Court, 2000). Many other states have implemented legal changes with regard to divorce laws; in Florida, for example, courts can require divorcing couples to take a 4 hour parenting course "designed to educate, train, and assist divorcing parents in regard to the consequences of divorce on parents and children" (Florida Statute §61.21).

Economists' interest in unilateral divorce has focused on two specific issues: the impact of these laws on divorce rates, and the implications of divorce laws on bargaining power within the household. The earliest study by Peters (1986) tested competing theoretical models of information within marriages, and concluded that unilateral divorce laws had little effect directly on divorce rates, but did significantly effect property settlements. Allen (1992) and Gray (1998) both used cross-sectional variation in unilateral divorce laws, but came to different conclusions about the impact of unilateral divorce on divorce rates. Allen finds that the divorce probability increases 1.4 percent by living in a no-fault state, where as Gray finds no significant impact of divorce laws on divorce rates. The coding of the state laws, the time periods studied, and the inclusion of various covariates account for at least part of the disparity in the results. Parkman (1992) argues that increases in labor force participation by married women as a result of unilateral divorce are driven by lack of investment in human capital. Ellman and Lohr (1998) and Brinig and Buckley (1998) use time series methods and also come to different conclusions about the causal impact of unilateral divorce laws on divorce rates. Friedberg (1998) used panel data on every divorce in the United States over a twenty-year period, and found that the divorce probability increased by .004 per person in states enacting unilateral divorce. More recent studies have focused on the impacts of unilateral divorce on the well being of spouses. Stevenson and Wolfers (2000) and Dee (1999) found sizeable and statistically significant impacts of unilateral divorce implementation on spousal murders, self-reported domestic violence, and suicide.

Estimation

We construct reduced form estimates of the impact of unilateral divorce on children's adult labor market outcomes. Our approach uses state-time variation in the implementation of the laws. States varied substantially in the year that they enacted unilateral divorce. In Figure 2, we depict the evolution of unilateral divorce laws by state and year of implementation. We also provide a list of the years each state implemented their unilateral divorce law in Appendix A.[2] Individuals born within the same year but in different states have different years of exposure to unilateral divorce based on the differential timing of states enacting unilateral divorce. So first we are comparing individuals born in the same year in states that had enacted unilateral divorce with those born in states that had not enacted unilateral divorce. Second, we are comparing individuals born in later years to those born in earlier years within the same state. In the 1990 Census, which is a sample entirely consisting of adults, *YearsUN* measures the individual's exposure to unilateral divorce laws, prior to turning 18 years old.[3] Assuming no migration[4], for individual j born in state k:

$$YearsUN_{jk} = \text{Year of Birth}_j + 19 - \text{Year Unilateral Divorce Enacted}_k \qquad (1)$$

A simple example illustrates our point. Alabama passed a unilateral divorce law in 1971. An individual born in Alabama in 1960 would have eight years of exposure to unilateral divorce by construction of *YearsUN* variable. Someone born in Alabama in 1964 has 12 years of exposure to unilateral divorce.

We likewise estimate reduced-form equations with two specifications. For individual j born in state k in year t, and outcome of interest y_{jkt}:

[2] There is some debate among lawyers about how to classify a state's divorce laws, as well as the timing of the laws. We mainly employ the law coding used by Brinig and Buckley (1998) (also used by Friedberg), but also test our results with law coding from Ellman and Lohr (1998). Our results are insensitive to the legal classification we use.

[3] We add 19 to the year of birth (not 18) because our law data generally indicates the first full year of effectiveness of each state's unilateral divorce law.

[4] The issue of selective migration is relaxed and discussed below.

$$y_j = \sum \boldsymbol{d}_k + \sum \boldsymbol{g}_t + \boldsymbol{p} YearsUN_{kt} \tag{2}$$

$$y_j = \sum \boldsymbol{d}_k + \sum \boldsymbol{g}_t + \sum \boldsymbol{p}_i \mathrm{I}(i-1 \leq YearsUN_{kt} < i) \tag{3}$$

where the $\boldsymbol{d}_k$ are state of birth main effects, the $\boldsymbol{g}_t$ are year of birth main effects.

In equations (2), *YearsUN* is entered linearly, constraining the model to estimate a constant change in y_j for a unit increase in *YearsUN*. In equations (3), a series of dummy variables are entered indicating varying levels of *YearsUN* – we have dummies for one to four years of exposure, five to eight years of exposure, and nine to twelve years of exposure. Equation (3) allows for non-monotonic or generally non-linear effects of unilateral divorce law.

This combination of state and time variation in unilateral divorce allows us to control fully for any year of birth and state of birth effects on later outcomes. Any differences between state environments that are fixed over time will be controlled for in our approach, as would any changing environmental influences that are not changing differentially across states. We discuss other possible caveats with interpreting the estimates later in the paper.

Effects on Educational Attainment

We begin with a sample of U.S. born individuals between ages 25-34 taken from the 1990 Census IPUMS 5% sample to study children's adult outcomes. Since the majority of unilateral divorce laws were implemented between 1969 and 1977, individuals in this sample (born between 1955 and 1964) were children at the time of the passage of the laws. Less than three percent of our sample were born with unilateral divorce already in effect in their state of birth.[5] The lower age cutoff of 25 allows us to observe individuals after completing school and when their labor force attachment should be strong. Our full sample has 1,791,630 observations.[6] Table 1 presents descriptive statistics for our full sample and split by treatment

[5] This fact may have important implications for our identification strategy, which we discuss in the section below on caveats.

[6] We excluded those observations that had greater than 12 years of exposure to unilateral divorce because these observations were drawn exclusively from Oklahoma and Alaska. In fact, including these

status. Table 1 provides a bivariate representation of our reduced form results by showing the mean values for each treatment dummy and the differences across groups of our outcomes: years of schooling completed, earnings, and marital status. The bivariate evidence provides an intriguing first glance at our reduced-form findings. Longer exposure to unilateral divorce laws is associated with fewer years of completed schooling; in particular, long exposure to unilateral divorce laws seems to reduce the probability of graduating from high school and of graduating from college.

Table 2a formalizes the descriptive evidence on education from Table 1. We regress years of education completed by April 1, 1990 on exposure to unilateral divorce laws as a child. For the full sample, living an additional year in a unilateral divorce state as a child reduces the completed education by .003 years. When we split the sample by gender, we find that the results are larger and more precisely measured for women; an additional year of exposure reduces education completed by .005 years. The results in Panel B from Table 2a are largely consistent with the results from the linear treatment effects. Allowing the effects to vary non-linearly does not dramatically change the estimates. The effects are also more or less monotonic across treatment groups, with higher levels of exposure leading to lower educational attainment. Women exposed to unilateral divorce for 9-12 years-completed .12 fewer years of schooling. We graph this relationship in Figure 2 based on the relationship in Table 2a.

The schooling reduction for women exposed to unilateral divorce is in contrast to the results for men, where there appear to be only a statistically significant effects of unilateral divorce laws on completing high school for men. The coefficient estimates are generally negative for men, but few coefficients are larger than their accompanying standard errors. Notice that the standard errors on the men's estimates are similar to the standard errors on the women's estimates. Our estimates would be able to detect effects for men, if they were similar in

observations increased the magnitudes of our coefficients. As shown in Appendix B, the other levels of exposure to unilateral divorce represent a variety of states, and so selection is less likely to be a problem.

magnitude to the effects for women. The effects are simply smaller for men than for women for all levels of higher education, and for high school drop outs as well.

In Table 2b, we show the probability of completing a given level of education as a function of exposure to unilateral divorce laws. This discrete parameterization provides more insight into the way that unilateral divorce reduces educational attainment -- for example, does it induce students to drop out of high school, or does it deter students from attending college? Consistent with Table 2a, the results in Table 2b vary by gender.

For women, exposure to unilateral divorce laws has statistically significant, negative effects on the probability of completing all levels of education. Unilateral divorce has the biggest effect on the probability of completing 12th grade (or higher), an associate's degree, and graduating from college. For example, nine to twelve years of exposure reduces the probability of a woman finishing high school by 1.4 percentage points (approximately 2% of the dependent variable mean). It also reduces the probability of completing a bachelor's degree by 2.3 percentage points (10% of the dependent variable mean).

For men, the effects are far less uniform. Exposure to unilateral divorce marginally reduces the probability of completing high school, but has little significant impact on other years of education. In fact, for each interval of exposure in the non-linear specification, the probability increases monotonically. Nine to twelve years of exposure to unilateral reduces the probability of a man completing high school by 1.9 percentage points (3% of the dependent variable mean). There appears to be virtually no effect on the completion of other levels of education, however.

Effects on Marital Status and Childbearing

In Table 3, we look for effects of unilateral divorce on marital status of adults in the 1990 Census, and in Table 4, we present results of the childbearing behavior for women. These regressions should be interpreted as the direct effect of unilateral divorce laws on the probability of a woman having a child, and on the number of children a woman has had. The first panel of Table 3 shows linear estimates of the impact of years exposure to unilateral divorce, first on the probability of a woman having any children and then on the number of children that she has. The number of children regression *has not* been conditioned on the childbearing decision. We include women who have not had any children in this regression, with the dependent variable set equal to zero.

Exposure to unilateral divorce seems to increase both the likelihood of bearing a child, and the number of children born. An additional year spent in a unilateral divorce state raises the probability of childbearing by .38 percentage points; the average number of children increases by .009. Panel B presents results from our discrete treatment parameterization. While the non-linear effects do appear to be highly monotonic, it seems that the constraint imposed in the linear specification masks potentially important heterogeneity in the effects of unilateral divorce. In particular, 1-4 years of exposure to unilateral divorce laws reduces the propensity of individuals to bear children. High levels of exposure to unilateral divorce laws (particularly 9 years or more) increases the probability of child bearing.

Effects on Earnings

In Table 5, we provide evidence on the impact of unilateral divorce laws on earnings. Again, we begin with the full sample. An additional year of exposure to unilateral divorce laws as a child reduces weekly earnings by about .2 percent. This result is robust to the inclusion of both state of residence main effects and other covariates. In our non-linear specification, increased years of exposure to unilateral divorce lower earnings monotonically. Earnings are lower by 1.5 percent for those individuals who experienced 9 or more years of unilateral divorce. Adding covariates, some of which are arguably endogenous, such as education and marital status, reduces this estimate to 1.1 percent.[7]

When we split our sample by gender, a familiar pattern emerges. The impacts of unilateral divorce exposure are substantially more negative for women than for men. It appears

[7] Adding endogenous covariates generally biases our treatment effect estimates toward zero. (Angrist and Krueger, 2000).

that if unilateral divorce did have an impact on male children, that impact was small. Our estimate from the regression with full state of residence main effects and other covariates implies that an additional year in a unilateral divorce state reduces earnings by about .4 percent for women. Our non-linear treatment dummy specification shown in Panel B yields similar results. More exposure to unilateral divorce lowers earnings by a progressively greater amount. Women exposed to more than 9 years of unilateral divorce earned 4.2% less relative to women with no exposure to unilateral divorce. We graph the relationship from the regression in Table 5 in Figure 3.

Caveats

There are several important concerns with our empirical strategy, which must be carefully considered in order to interpret the estimates in our paper properly. In no particular order:

(1) Random or selective migration biases our measurement of childhood exposure to unilateral divorce laws for migrants.

(2) Unobserved interactions between year of birth and state of birth (not induced by unilateral divorce laws) will bias our estimates. One worrisome form of such interaction is *political endogeneity*. This arises if unilateral divorce laws are enacted in response to unobserved political, social, or economic factors that are changing differentially across time within states.

(3) Unilateral divorce laws may have had independent effects on the fertility or marital decisions of parents, which could lead to a sample selection problem.

(4) Unilateral divorce laws may have independent effects on the marital status of the children themselves, which could render the reduced form estimates difficult to interpret as the effect of *parental divorce* on future child outcomes.

A. The Role of Migration

The first concern is about the role of family migration during childhood. We assign exposure to unilateral divorce law based upon an individual's state and year of birth; if that person moved to a different state during childhood, we might mismeasure his exposure to unilateral divorce laws. If such migration happened randomly or at least independently of the child's future outcomes (e_j in equation 1), the consequence would be to introduce standard measurement error into the instrument. The reduced form coefficients would be biased toward zero – the effects of unilateral divorce on later outcomes would appear to be smaller than they truly are.

Even if migration were selective – parental migration was a non-trivial function of both their children's potential future outcomes and unilateral divorce law – the reduced-form estimates are unlikely to be seriously biased toward showing negative effects of unilateral divorce. This is because our measure of exposure is based upon state of birth, not upon the state where the child actually lived. In order for migration to bias our reduced-form estimates, parents would have had to migrate on the basis of unilateral divorce law and e_j *prior to the birth of their child*. Recall that fewer than 3% of our sample was born in a state with an existing unilateral divorce law, so migration on the basis of existing laws is unlikely to be a problem in this sample. Parents would have had to foresee future state divorce law changes, in order to base selective migration on divorce laws. This kind of selection mechanism seems particularly implausible. Moreover, migration itself is a surprisingly small piece in most individuals' life cycles.

B. State of Birth and Year of Birth Interactions

Secondly, attributing our reduced-form estimates to be the causal effect of unilateral divorce laws requires us to assume that there are no interactions between state of birth and year of birth (other than unilateral divorce laws) that affect the later outcomes.[8] This key identifying

[8] As our approach is conceptually similar to differences-in-differences and uses a similar kind of variation for identification, we must make the same identifying assumption as is typically made in differences-in-differences analysis.

assumption could be violated in many circumstances. We discuss several of the more plausible threats to this identification assumption, and the possible empirical implications.

The first set of circumstances may be broadly referred to as political endogeneity.[9] Perhaps states adopting unilateral divorce legislation enacted policies with a general disregard for children and family stability. Perhaps these states also allocated fewer resources to public schools, or less money for child health care. Along similar lines, states enacting unilateral divorce laws may have done because of the wishes of the electorate. If the preferences of the electorate had shifted independently toward easier marital dissolution, unilateral divorce laws may simply reflect a secular-increasing acceptance of divorce. If such secular trends themselves have effects on future outcomes of children, then our reduced-form estimates will exaggerate the effects of unilateral divorce laws.

While not every competing explanation may be ruled out, these “confounding stories” have empirical implications that can be examined in the estimated reduced form equation. For example, if states were implementing a menu of family-unfriendly policies, we might see “effects” of unilateral divorce in the reduced form before the actual implementation of unilateral divorce. Similarly, pre-existing shifts in the state-specific marital and family preferences would likely show up in terms of non-zero treatment effects on individuals who turned 18 a few years prior to the enactment of unilateral divorce laws. We test for such “pre-treatment effects”. Moreover, our variation at the level of state of birth provides us enough richness to estimate reduced-form equations with a full-set of region of birth times year of birth effects. Such models control for very general (non-linear) forms of interactions between year of birth and state of birth, parameterized at the level of region of birth. We would have reason to suspect that year of birth state of birth interactions were important if the treatment effects estimated from these general

[9] Friedberg (1998) discusses political endogeneity at length. There is evidence that lagged divorce rates can predict whether a state passes a unilateral divorce law. However, she further argues that there is no evidence that initial divorce rate is correlated with when a state adopts such laws.

models were very different (particularly if they were weaker) relative to the estimates from the simple models. Both parameterizations test the robustness of our results to interactions between state-of-birth and year-of-birth.

For our educational outcomes, we conduct specification tests in Table 6. Columns (1) and (2) repeat our baseline results. Notice the inclusion of covariates does not significantly change our results; in fact, they get somewhat bigger. In columns (3)-(4) we also include a series of region of birth times of year of birth interactions to control for state-age specific trends or interactions. Although our parameter estimates are smaller, they are still negative and significant. Finally, we test for the possible political endogeneity of unilateral divorce laws by including an additional dummy for "negative" years of exposure to divorce in columns (5) and (6). Here, we find that the effects are positive and insignificant; it seems unlikely that divorce trends before the passage of the laws could be driving our educational outcomes.

In Table 7, we show the specification tests for the earnings results; the first three columns present our previously reported baseline results. Including of year of birth-region of birth interactions does not change the results. In fact, in the non-linear treatment specification, the estimated wage impacts get larger relative to the baseline specification (more negative) for the first three treatment dummies.

Columns 7-9 of table 7 show the results from our “negative” treatment effects falsification exercise. There is no evidence for political endogeneity based upon the estimates for the group turning 18 prior to the enactment of unilateral divorce. All “false” treatment estimates are less than their corresponding standard errors, and one out of three estimates is positive. The "real" treatment effects remain negative and similar in magnitude to the baseline results. We would expect to see negative estimates prior to the treatment, if political endogeneity of unilateral divorce laws were driving our actual estimates of the treatment effect.

C. Unilateral Divorce Laws May Have Effects on Parental Marital and Fertility Decisions

If unilateral divorce laws had independent effects on the marriage and childbearing decisions of parents, this may change the distribution of marriages and children who are actually born. Thus, we would observe a different distribution of adult outcomes among children born in unilateral divorce states, and this would in fact be due to unilateral divorce laws. However, the observed difference would not be attributable to the changing distribution of marriages and births. We can easily reject such an interpretation of our estimates, however, because almost the entirety of our sample (97%) was already born when unilateral divorce laws had been enacted.

D. Independent Effects on Adult Marital Status

Finally, some concern might exist about the possibility of independent effects of unilateral divorce on the future marriage behavior of children. Since children as adults might also be exposed to differing regimes based upon their state of residence, it is possible that some of our estimates are picking up direct effects of unilateral divorce laws on own marriage behavior, and not the effect of the laws on the parents marriage behavior. We have run several specifications to test this possibility. First, we have run our standard specifications including first state of residence fixed effects, and then state of residence fixed effects *and* current marital status. The inclusion of these variables (particularly the state of residence fixed effects) has no qualitative impact on the estimates. Second, we have run the results in the sample of young adults in 1990 that all lived in a state with a unilateral divorce law by the time they were 18. For these individuals, the only variation in exposure to the laws occurred while they were children. Thus, any effects that unilateral divorce laws will have on our children, as adults would be controlled for as a result of including state of residence fixed effects. By the time they were adults, the years of exposure to unilateral divorce would be the same for all people of the same age. The results in this sample are consistent with our previous findings – unilateral divorce still has a significant and negative impact on education and earnings for women.

The Impact of Unilateral Divorce on Divorce Rates

Having established that unilateral divorce laws negatively impact future labor market outcomes of children, we explore the possible channels for this effect. First, unilateral divorce may raise divorce rates; second, unilateral divorce may impact bargaining power within the household. These mechanisms are not mutually exclusive as much of the previous literature treats them. We demonstrate that unilateral divorce likely effects both divorce rates and bargaining power of intact families.

To test the impact of unilateral divorce laws on parent's divorce rates, we use a sample of children under the age of 17 from the 1980 Census. This is the only sample in which we can reliably match children to their parents and thus measure the effects of unilateral divorce laws on the probability of a parental divorce.[10] For each child, we construct the number of years they lived in a unilateral divorce state from their state of birth and current age. For individual j born in state k:

$$YearsUN_{jk} = \min\{\text{age}_j, \max(0, 1980 - \text{Year Unilateral Divorce Enacted}_k)\} \tag{4}$$

Naturally, age limits each individual's exposure to unilateral divorce laws. We estimate this relationship with two principal specifications. For individual j born in state k in year t,

$$D_j = \sum d_k + \sum g_t + p YearsUN_{kt} \tag{5}$$

$$D_j = \sum d_k + \sum g_t + \sum p_i \mathrm{I}(i - 1 \le YearsUN_{kt} < i) \tag{6}$$

where D_j takes the value 1 if individual j's parents were divorced as of April 1, 1980, and 0 if they were not, and where the d_k are state of birth main effects and the g_t are year of birth main effects.

In Figure 4, we graph the age and state regresssion-adjusted relationship between unilateral divorce exposure and parental divorce. There is clearly an upward sloping relationship between unilateral divorce and the years of exposure, with the effect being quite precisely

[10] See Appendix C for details on our matching procedure.

measured for the first 6 exposure years. The impact of unilateral divorce laws are almost completely monotonic for the first 10 years of exposure to no-fault divorce, and then start to diminish slightly toward the upper-extreme of the treatment. This fanning out of the effect is perhaps not surprising given the small sample number of individuals who lived under unilateral divorce laws for long periods of time. Further, the probability that one's parents got remarried is more likely to be an issue for the group with the longest exposure to unilateral divorce.

In Table 8, we present regression results from two alternative specifications of the impact of unilateral divorce on parent's divorce rates: one with a linear unilateral treatment effect, and a second with dummies for three discrete levels of treatment. The baseline estimate for the full sample of children is .005, which implies that an extra year of exposure to unilateral divorce increases the probability that a child's parents are divorced by $5/10^{th}$ of a percentage point (column 1). The dependent mean on divorce is .203, which implies about a three-percent increase in the divorce rate. When we estimate the effects allowing the treatments to vary non-linearly by including treatment dummies, we find the same pattern as depicted in Figure 4; more exposure to unilateral divorce is positively correlated with divorce rates. The relationship is monotonic for years 1-12, and then starts to diminish thereafter (although, again, the estimate for 12 or more years of unilateral divorce exposure is less precise.) Table 8 also provides estimates from the sample split by boys and girls; the estimates are very close to each other, indicating no apparent differential effect of unilateral divorce by gender of child on divorce probabilities.

There are two serious limitations with our approach. First, some parents in the sample may be divorced and remarried. Given the coding of the census marital status question, these persons will appear as married in our sample. This type of misclassification will bias against finding a positive impact of unilateral divorce laws on divorce rates. To account for the remarriage problem, we coded individuals who report they are remarried as having been divorced. This solution may overstate the impacts of divorce, but clearly no individual can be remarried unless they have been married and separated previously.

Second, we can only observe whether a child's parents have divorced prior to April 1, 1980 – we have no way of knowing whether the parents divorce after this date (but before their child or children turn 18). Provided unilateral divorce laws increased the cross-sectional probability of divorce, our estimates of this relationship might understate the impact of unilateral divorce laws on the cumulative probability of divorce while the child is under age 18. This happens because our measure of divorce is right-censored relative to a complete record of childhood experiences with divorce. The impact of unilateral divorce on the overall probability of children experiencing divorce will be larger than the probability estimated in our sample. To test this possibility, we run maximum likelihood estimation of censoring models. Our methodology and results from the maximum likelihood are explained in Appendix D. Quantitatively, the estimates do not change when we implement this procedure.

If we interpret unilateral divorce as affecting children only through increased divorce rates, we can then compute an instrumental variable-like estimate of the effect of *divorce* on children.[11] A two-sample instrumental variables (2SIV) estimator can be used when data only exists on the outcomes and the instruments in one sample, and the endogenous regressor and the instruments in a second sample.[12] In an ideal setting, we would be able to match cohorts across our two samples. However, the cohort of the appropriate age in the 1990 sample is too old in the 1980 Census to be matched to their parents. Instead, we construct 2SIV estimates by combining the simple estimates from different cohorts. Although this approach is not ideal, it is the best we can do given our data constraints.

Results from our 2SIV procedure are presented in Table 9. Since the equation is just-identified, the 2SIV estimate is computed by dividing the reduced form coefficient by the first

[11] We caution the reader that, for reasons discussed below, this is *not* our preferred interpretation.

[12] See Angrist and Krueger, 1995 for a description of this methodology and Angrist and Evans, 1999 for an application to abortion laws.

stage coefficient. All standard errors are calculated using the delta-method.[13] The 2SIV estimates for women in column (3) imply that parent's getting divorced reduces the probability of a female completing high school by 15 percent, but implies earnings (as an adult) are reduced by 100 percent and lowers the probability of own divorce by 6 percent. Absent extremely large differences in the cohort examined in 1980 versus that examined in 1990 (which happen to cause us to strongly understate the effect of unilateral divorce laws on divorce probability), the results are too large in magnitude to support this strict instrumental variable interpretation. Rather the divorce regime must affect children other than through increasing divorce rates *per se*. Yet, this finding should not be surprising in light of recent research that highlights the importance of bargaining on the allocation of resources within the household.[14]

The Impact of Unilateral Divorce on Household Bargaining

Bargaining models imply that family decisions are made in strategic ways that depend on the relative outside opportunities of the partners, and the enforceability of the marriage contract.[15] How might the shift to unilateral divorce regimes affect bargaining power within the household, and ultimately, affect children in these families? One view of unilateral divorce laws is that they make the marriage contract unenforceable. As a result, the agreements reached in intact marriages may change, since each partner faces different outside opportunities. Since the threat points determine the range of mutually beneficial trades along the contract curve, the outcome of bargaining is generally altered if the outside opportunities shift (Pollak, 1985).

An extension of this bargaining model (e.g. Brinig and Crafton, 1994) suggests the possibility of expropriation by one partner at the expense of the other. Spouses will favor investment in transferable human capital at the expense of investments which loses their value in

[13] This is an application of the delta-method. We have a function of two regressors, and derive the standard error as the product of three matrices.

[14] See Lundberg and Pollak (1996) for an excellent discussion of these alternative models. Also, it is interesting to note that, in part, one of the motivations for the original paper by Peters was to distinguish between theoretical models of divorce behavior and information in the household.

case of divorce. For example, in an extremely strict regime allowing few divorces, one partner may be willing to work in order to fund the education of his or her spouse. When divorce is unlikely, or when accompanied by large alimony awards, this investment is likely to be repaid in higher *shared* future income and consumption. When divorce is made easy and alimony is independent of fault, the sacrifice of the working spouse can occur without any repayment. Investment specific to the marriage is *vulnerable* to expropriation in the unilateral divorce regime.

One form of vulnerable investment is a parent who stays at home caring for children, at the opportunity cost of acquiring market work experience. Provided that the marriage stays intact, parental investment in childcare can be rewarded through appropriate inter-spousal transfers. However, investment in stay-at-home childcare reduces the outside labor market opportunities available to the stay-at-home spouse, and in an easy-divorce regime, may not be fully compensated if the marriage fails. To the extent that unilateral divorce moved women fearful of expropriation from household production of childcare to market-based activities, there may be a substantial negative impact on children. This is particularly true if hired childcare does not provide a perfect substitute for care by a parent in the child's developmental process.

An empirical test of the impact of unilateral divorce on bargaining is whether women shifted from non-market to market activities. This exercise is similar in spirit to analyses by Peters (1986), Parkman (1992), and Gray (1998). However, unlike this previous work, we fully explore state-time variation in unilateral divorce laws rather than simple pre-post differences that rely only on one pre-unilateral and one-post unilateral year of treatment. We use data from the annual demographic files of the Current Population Survey (CPS) from 1964-1986. We construct state-year cells containing the average labor force participation rate for men and women age 20-58. We regress these cell means on state and year fixed effects and dummy variables indicating the presence of unilateral divorce laws. In the first specification, we include a unilateral dummy

[15] Examples include Manser and Brown, 1980; McElroy and Horney, 1981; Lundberg and Pollak, 1993.

which is for the entire post-unilateral treatment period. In the second specification, we look for impacts in years immediately following enactment of unilateral divorce laws. We weight all regressions by the inverse of the state-year cell sizes to correct for heteroscedasticity induced by using grouped data. As a specification check, we also estimate effects for years immediately prior to unilateral divorce enactment. This strategy is a difference-in-differences approach where the comparison is across states and years before and after the enactment of unilateral divorce.

In Table 10, we present results from these regressions. In the first three columns, we provide evidence for the sample of married adult women. The results show that female labor force participation appears to increase in the first and second year following the enactment of unilateral divorce, but the effect fades in the third year. Our estimates imply that labor force participation rates increase between 3 and 11 percent one year following the enactment of unilateral divorce, and between 1/2 to 9 percent in the second year. Changes in labor force participation of married women cannot be driven by divorce; the likely mechanism through which this change occurs is bargaining power. As a further specification check, in columns (4) through (6) we run the same results for the sample of single women. If the changes in labor force participation were driven by factors other than bargaining, we'd expect to see significant, positive changes in labor force participation for this group as well. In fact, the majority of the effects are negative, and the only positive effects are not statistically significantly different from zero.

It is also important to note that an alternative (but complimentary) theory of bargaining power might imply that individuals with more meager outside opportunities may engage in selfish behavior that benefits the injurer at the expense of the marriage. Tauchen et al. (1991) have used the bargaining framework of Pollak (1985) to model the decision of spouses to commit domestic violence against one another. The theoretical relationship between domestic violence and unilateral divorce is difficult to sign. The availability of easier divorce could reduce abuse, by allowing the abused partner to exit the relationship, or to threaten credibly to do so. Brinig and Crafton (1994) suggest the opposite relationship, pointing out that the threat of a costly divorce may be a significant deterrent to spousal abuse. Reducing the cost of divorce weakens this threat, perhaps leading to more spousal abuse.

Regardless of which effect dominates, it is conceivable that the amount and severity of domestic violence in households affects children directly. Two recent empirical papers have tested whether observable measures of domestic violence have responded to changing unilateral divorce laws. Both Stevenson and Wolfers (2000) and Dee (1999) found sizeable and statistically significant impacts of unilateral divorce implementation on spousal murders, self-reported domestic violence, and suicide. Curiously, despite examining similar outcomes and using identical data, the authors disagree on a principal finding. Dee calculated a 15% increase in the rate of husbands killed by wives, with no significant chance in the rate of wives killed by husbands. On the other hand, Stevenson and Wolfers concludes that women suffered substantially less domestic violence in the wake of unilateral divorce laws. The findings of both studies suggest a substantial change in the outside opportunities available to spouses, and in the level of domestic violence experienced in all families. Both are likely to have had direct impacts on children growing up with unilateral divorce.

Conclusions

Economists have been interested in the economic consequences of unilateral divorce since it was first enacted. Our results provide a contrast from the existing literature along several key dimensions. First, we focus on the long-term consequences for the children of unilateral divorce. Second, while we confirm that unilateral divorce increased divorce rates, we also provide evidence that bargaining power within the household was a key factor in affecting children. Many previous studies treat these channels as being mutually exclusive. Third, our identification strategy allows us to test, and ultimately reject, political endogeneity and other contaminating factors that could be responsible for the effects we find.

Our findings suggest that women earn lower wages and attain less education with increased exposure to unilateral divorce. We detect no such effects for men. However, we do find effects on both marriage propensity and probability of divorce as adults for both groups. Despite the evidence we provide about the mechanisms driving the effects of unilateral divorce on children, more detailed information about time allocation would further our understanding.

Given recent debates in Louisiana and Washington about repealing unilateral divorce laws, our evidence can inform the policy debate. We are cautious, however, about interpreting our results to mean that the repeal of unilateral divorce would automatically make children better off. Our study answers the question of how unilateral divorce affected past cohorts of children. This information, however, is merely suggestive of what similar experiments or repeals might accomplish today. A natural avenue for future research is to study the impacts of several recent legal innovations- such as the Florida mandatory divorce course or the covenant marriages in Louisiana and Arizona.

Table 1
Descriptive Statistics by Years Lived in a Unilateral Divorce State
1990 5% Census Sample

	Years Lived in a Unilateral Divorce State as a Youth				
	Full Sample (1)	None (2)	One-Four Years (3)	Five-Eight Years (4)	Nine-Twelve Years (5)
			Demographic Characteristics		
Age	29.66 (2.80)	29.61 (2.85)	32.75 (1.35)	30.01 (1.75)	26.75 (1.33)
Married	0.59 (0.49)	0.58 (0.49)	0.66 (0.47)	0.61 (0.49)	0.51 (0.50)
Divorced	0.09 (0.28)	0.08 (0.27)	0.12 (0.32)	0.10 (0.30)	0.08 (0.27)
White	0.87 (0.33)	0.87 (0.34)	0.88 (0.32)	0.88 (0.32)	0.87 (0.33)
			Schooling		
Years of Schooling	13.15 (2.25)	13.23 (2.27)	13.14 (2.25)	13.09 (2.23)	13.02 (2.21)
Less than 12 years schooling	0.14 (0.34)	0.13 (0.33)	0.13 (0.34)	0.14 (0.35)	0.16 (0.36)
High School Graduate	0.34 (0.47)	0.34 (0.47)	0.34 (0.47)	0.33 (0.47)	0.32 (0.47)
Completed Some College	0.31 (0.46)	0.30 (0.46)	0.33 (0.47)	0.33 (0.47)	0.33 (0.47)
College Graduate	0.22 (0.41)	0.23 (0.42)	0.21 (0.40)	0.20 (0.40)	0.19 (0.39)
			Employment Characteristics		
Labor Force Participation	0.87 (0.34)	0.87 (0.34)	0.87 (0.34)	0.86 (0.34)	0.87 (0.34)
Weekly Income	444.61 (595.85)	455.92 (580.11)	477.97 (624.41)	445.99 (669.25)	394.67 (547.49)
			Instrument		
Years in Unilateral Divorce State	3.25 (4.35)	N/A	2.94 (1.05)	6.48 (1.12)	10.23 (1.06)
Sample Size	1791793	926590	200650	323781	243713

Table 2a
Reduced Form Effects of Years Lived in a Unilateral Divorce State on Years of Education Completed

	Full Sample		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Linear Treatment Specification			
Years in Unilateral Divorce State	-0.0030 (0.0012)	-0.0032 (0.0012)	-0.0047 (0.0017)	-0.0048 (0.0017)	-0.0016 (0.0018)	-0.0016 (0.0018)
			B. Dummy Treatment Specification			
1-4 Years in Unilateral Divorce State	-0.0812 (0.0286)	-0.0840 (0.0285)	-0.1150 (0.0389)	-0.1100 (0.0387)	-0.0446 (0.0419)	-0.0559 (0.0417)
5-8 Years in Unilateral Divorce State	-0.0616 (0.0292)	-0.0654 (0.0291)	-0.0961 (0.0389)	-0.0916 (0.0397)	-0.0257 (0.0429)	-0.0381 (0.0427)
9-12 Years in Unilateral Divorce State	-0.0841 (0.0300)	-0.0876 (0.0299)	-0.1242 (0.0409)	-0.1200 (0.0407)	-0.0432 (0.0440)	-0.0546 (0.0438)
State of Residence Main Effects	No	Yes	No	Yes	No	Yes
Sample Size	1,791,630		941,435		877,195	

Notes: All entries are OLS estimates of regression equations for years of education (using the Park (1994) linear interpolation for years of education) on measures of childhood exposure to unilateral divorce laws. The details of our construction of childhood exposure to unilateral divorce laws are discussed in the text. All specifications include a full set of year of birth and state of birth main effects. Data are from the 1990 Census IPUMS 5% Sample.

Table 2b
Effects of Years Lived in a Unilateral Divorce State on Discrete Levels of Schooling

	Women's Education					Men's Education				
	Completed 10th Grade	Completed 11th Grade	Completed 12th Grade	Completed Associate's Degree	Completed Bachelor's Degree	Completed 10th Grade	Completed 11th Grade	Completed 12th Grade	Completed Associate's Degree	Completed Bachelor's Degree
					A. Linear Treatment Effects					
Years in Unilateral Divorce State	-0.0003 (0.0001)	-0.0004 (0.0002)	-0.0008 (0.0002)	-0.0092 (0.0003)	-0.0005 (0.0003)	0.0001 (0.0002)	0.0002 (0.0002)	-0.0005 (0.0002)	-0.0006 (0.0003)	-0.0001 (0.0003)
					B. Dummy Treatment Effects					
1-4 Years in Unilateral Divorce State	-0.0036 (0.0032)	-0.0072 (0.0043)	-0.0114 (0.0051)	-0.0283 (0.0083)	-0.0223 (0.0074)	-0.0064 (0.0036)	-0.0077 (0.0047)	-0.0164 (0.0057)	-0.0045 (0.0083)	-0.0044 (0.0076)
5-8 Years in Unilateral Divorce State	-0.0031 (0.0033)	-0.0055 (0.0044)	-0.0101 (0.0053)	-0.0262 (0.0085)	-0.0198 (0.0076)	-0.0067 (0.0036)	-0.0063 (0.0048)	-0.0170 (0.0058)	-0.0006 (0.0085)	-0.0003 (0.0078)
9-12 Years in Unilateral Divorce State	-0.0042 (0.0033)	-0.0076 (0.0045)	-0.0138 (0.0054)	-0.0323 (0.0087)	-0.0233 (0.0078)	-0.0056 (0.0037)	-0.0062 (0.0050)	-0.0197 (0.0059)	-0.0063 (0.0087)	-0.0032 (0.0080)
Dependent Variable Mean	.966	.938	.907	.325	.227	.960	.927	.890	.303	.229
Sample Size			941435					877195		

Notes: All entries are OLS estimates of regression equations for probability of completing levels of education as a function of exposure to unilateral divorce laws. All estimates are weighted by the Census Sample Line Weights. The details of our construction of childhood exposure to unilateral divorce laws are discussed in the text. All specifications include a full set of year of birth and state of birth main effects. Data are from the 1990 Census IPUMS 5% Sample.

Table 3
Effects of Years Lived in a Unilateral Divorce State on Adult Marital Status

	Women						Men					
	Married		Divorced		Never-Married		Married		Divorced		Never-Married	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	A. Linear Treatment Specification											
Years in Unilateral Divorce State	0.0028 (0.0004)	0.0027 (0.0004)	-0.0003 (0.0002)	-0.0003 (0.0002)	-0.0031 (0.0003)	-0.0030 (0.0003)	0.0036 (0.0004)	0.0036 (0.0004)	-0.0009 (0.0002)	-0.0009 (0.0002)	-0.0030 (0.0004)	-0.0028 (0.0004)
	B. Dummy Treatment Specification											
1-4 Years in Unilateral Divorce State	-0.0199 (0.0086)	-0.0203 (0.0086)	0.0034 (0.0053)	0.0029 (0.0053)	0.0098 (0.0075)	0.0104 (0.0074)	-0.0044 (0.0089)	-0.0034 (0.0089)	-0.0003 (0.0048)	-0.0003 (0.0048)	0.0083 (0.0084)	0.0074 (0.0083)
5-8 Years in Unilateral Divorce State	-0.0097 (0.0088)	-0.0104 (0.0088)	0.0053 (0.0054)	0.0045 (0.0054)	-0.0024 (0.0077)	-0.0012 (0.0076)	0.0038 (0.0091)	0.0043 (0.0091)	-0.0015 (0.0049)	-0.0017 (0.0049)	0.0012 (0.0086)	0.0010 (0.0085)
9-12 Years in Unilateral Divorce State	-0.0027 (0.0091)	-0.0037 (0.0090)	0.0023 (0.0056)	0.0013 (0.0057)	-0.0095 (0.0079)	-0.0079 (0.0078)	0.0196 (0.0093)	0.0199 (0.0093)	-0.0064 (0.0051)	-0.0068 (0.0051)	-0.0112 (0.0088)	-0.0112 (0.0088)
State of Residence Main Effects?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Dependent Variable Mean	.596		.100		.247		.531		.076		.349	
Sample Size	914435						877195					

Notes: All entries are OLS estimates of regression equations for probability of attaining certain adult marital statuses as a function of exposure to unilateral divorce laws. All estimates are weighted by the Census Sample Line Weights. The details of our construction of childhood exposure to unilateral divorce laws are discussed in the text. All specifications include a full set of year of birth and state of birth main effects. Data are from the 1990 Census IPUMS 5% Sample.

Table 4
Effects of Exposure to Unilateral Divorce on Child Bearing
Women Only Sample

	Any Children?		Number of Children	
	(1)	(2)	(3)	(4)
	A. Linear Treatment Specification			
Years in Unilateral Divorce State	0.0038 (0.0004)	0.0037 (0.0004)	0.0086 (0.0012)	0.0086 (0.0012)
	B. Dummy Treatment Effects Specification			
1-4 Years in Unilateral Divorce State	-0.0029 (0.0083)	-0.0046 (0.0082)	-0.0082 (0.0287)	-0.0154 (0.0284)
5-8 Years in Unilateral Divorce State	0.0064 (0.0085)	0.0046 (0.0084)	0.0163 (0.0292)	0.0093 (0.0291)
9-12 Years in Unilateral Divorce State	0.0185 (0.0087)	0.0167 (0.0087)	0.0371 (0.0300)	0.0301 (0.0299)
State of Residence Main Effects	No	Yes	No	Yes
Dependent Variable Mean	0.643		1.947	

Notes: All entries are OLS estimates of regression equations for (1) a dummy indicating whether a woman has born any children, and (2) how many children she has had on measures of childhood exposure to unilateral divorce laws. The details of our construction of childhood exposure to unilateral divorce laws are discussed in the text. All specifications include a full set of year of birth and state of birth main effects. All regressions have 914,435 observations. Data are from the 1990 Census IPUMS 5% Sample.

Table 5
Reduced Form Effects of Years Lived in a Unilateral Divorce State on 1989 Log Weekly Wages

	Full Sample			Women Only			Men Only		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				A. Linear Treatment Specification					
Years in Unilateral Divorce State	-0.0020 (.0005)	-0.0020 (0.0004)	-0.0019 (0.0004)	-0.0054 (0.0007)	-0.0054 (0.0007)	-0.0044 (0.0006)	0.0010 (0.0006)	0.0011 (0.0005)	-0.0001 (0.0005)
				B. Dummy Treatment Specification					
1-4 Years in Unilateral Divorce State	-0.0060 (0.0104)	-0.0067 (0.0103)	-0.0018 (0.0094)	-0.0147 (0.0156)	-0.0099 (0.0154)	-0.0005 (0.0145)	-0.0069 (0.0128)	-0.0100 (0.0127)	-0.0004 (0.0118)
5-8 Years in Unilateral Divorce State	-0.0111 (0.0107)	-0.0116 (0.0105)	-0.0090 (0.0097)	-0.0267 (0.0159)	-0.0223 (0.0158)	-0.0118 (0.0147)	-0.0052 (0.0131)	-0.0078 (0.0130)	-0.0066 (0.0121)
9-12 Years in Unilateral Divorce State	-0.0149 (0.0109)	-0.0153 (0.0108)	-0.0111 (0.0098)	-0.0418 (0.0164)	-0.0372 (0.0162)	-0.0229 (0.0152)	0.0011 (0.0134)	-0.0017 (0.0134)	-0.0028 (0.0124)
State of Residence Main Effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Other Covariates	No	No	Yes	No	No	Yes	No	No	Yes
Sample Size		1,546,150			725,970			800,328	

Notes: All entries are OLS estimates of regression equations for log weekly wages on measures of childhood exposure to unilateral divorce laws. All estimates are weighted by the Census Sample Line Weights. The details of our construction of childhood exposure to unilateral divorce laws are discussed in the text. All specifications include a full set of year of birth and state of birth main effects. “Other covariates” include educational attainment, race, and current marital status main effects. Data are from the 1990 Census IPUMS 5% Sample.

Table 6
Extended Results – Effects of Exposure to Unilateral Divorce on Education
Women Only Sample

	Baseline Results		Including Year-of-Birth times Region-of-Birth Effects		Falsification Exercise – Treatment Dummy for "Negative" Years Exposure	
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Linear Treatment Specification			
Years in Unilateral Divorce State	-0.0047 (0.0017)	-0.0048 (0.0017)	-0.0037 (0.0020)	-0.0035 (0.0020)	N/A	N/A
			B. Dummy Treatment Specification			
Negative 1-4 Years in Unilateral Divorce State	N/A	N/A	N/A	N/A	0.0568 (0.0340)	0.0677 (0.0339)
1-4 Years in Unilateral Divorce State	-0.1150 (0.0389)	-0.1100 (0.0387)	-0.1135 (0.0418)	-0.1041 (0.0416)	-0.0598 (0.0511)	-0.0441 (0.0509)
5-8 Years in Unilateral Divorce State	-0.0961 (0.0389)	-0.0916 (0.0397)	-0.1065 (0.0434)	-0.0972 (0.0433)	-0.0404 (0.0519)	-0.0253 (0.0517)
9-12 Years in Unilateral Divorce State	-0.1242 (0.0409)	-0.1200 (0.0407)	-0.1191 (0.0448	-0.1087 (0.0446)	-0.0674 (0.0532)	-0.0523 (0.0530)
State of Residence Main Effects	No	Yes	No	Yes	No	Yes

Notes: All entries are OLS estimates of regression equations for years of education (using the Park (1995)) linear interpolation for years of education) on measures of childhood exposure to unilateral divorce laws. The details of our construction of childhood exposure to unilateral divorce laws are discussed in the text. All specifications include a full set of year of birth and state of birth main effects. All regressions have 941,448 observations. Data are from the 1990 Census IPUMS 5% Sample.

Table 7
Extended Results – Effects of Exposure to Unilateral Divorce on 1989 Log Weekly Wages
Women Only Sample

	Baseline Results			Including Year-of-Birth times Region-of-Birth			Falsification Exercise – Treatment Dummy for "Negative" Years Exposure		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				A. Linear Treatment Specification					
Years in Unilateral Divorce State	-0.0054 (0.0007)	-0.0054 (0.0007)	-0.0044 (0.0006)	-0.0035 (0.0008)	-0.0035 (0.0008)	-0.0026 (0.0007)	N/A	N/A	N/A
				B. Dummy Treatment Specification					
Negative 1-4 Years in Unilateral Divorce State	N/A	N/A	N/A	N/A	N/A	N/A	-0.0012 (0.0133)	0.0038 (0.0131)	-0.0086 (0.0123)
1-4 Years in Unilateral Divorce State	-0.0147 (0.0156)	-0.0099 (0.0154)	-0.0005 (0.0145)	-0.0311 (0.0166)	-0.0253 (0.0164)	-0.0170 (0.0154)	-0.0173 (0.0202)	-0.0076 (0.0199)	-0.0095 (0.0187)
5-8 Years in Unilateral Divorce State	-0.0267 (0.0159)	-0.0223 (0.0158)	-0.0118 (0.0147)	-0.0427 (0.0173)	-0.0379 (0.0171)	-0.0269 (0.0160)	-0.0264 (0.0205)	-0.0169 (0.0203)	-0.0182 (0.0190)
9-12 Years in Unilateral Divorce State	-0.0418 (0.0164)	-0.0372 (0.0162)	-0.0229 (0.0152)	-0.0451 (0.0178)	-0.0400 (0.0176)	-0.0265 (0.0167)	-0.0428 (0.0210)	-0.0329 (0.0208)	-0.0299 (0.0195)
State of Residence Main Effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Other Covariates	No	No	Yes	No	No	Yes	No	No	Yes

Notes: All entries are OLS estimates of regression equations for log weekly wages. The details of our construction of childhood exposure to unilateral divorce laws are discussed in the text. All specifications include a full set of year of birth and state of birth main effects. "Other covariates" include educational attainment, race, and current marital status main effects. Data are from the 1990 Census IPUMS 5% Sample. All regressions contain 719,434 observations.

Table 8
Effects of Years Lived in a Unilateral Divorce State on Parent's Divorce, 1980 5% IPUMS Sample

	Full Sample		Boys		Girls	
	(1)	(2)	(3)	(4)	(5)	(6)
			C. Linear Treatment Specification			
Years in Unilateral Divorce State	.0053 (.0002)	.0060 (.0002)	.0053 (.0002)	.0059 (.0002)	.0054 (.0003)	.0059 (.0003)
			D. Dummy Treatment Specification			
1-4 Years in Unilateral Divorce State	.0154 (.0022)	.0177 (.0022)	.0177 (.0031)	.0200 (.0031)	.0131 (.0031)	.0151 (.0031)
5-8 Years in Unilateral Divorce State	.0320 (.0020)	.0367 (.0021)	.0331 (.0029)	.0379 (.0028)	.0308 (.0030)	.0356 (.0029)
9-12 Years in Unilateral Divorce State	.0564 (.0022)	.0613 (.0022)	.0592 (.0031)	.0643 (.0031)	.0535 (.0032)	.0583 (.0032)
State of Residence Main Effects and Other Covariates	No	Yes	No	Yes	No	Yes
Sample Size	2,825,173		1,445,838		1,379,335	

Notes: All entries are OLS estimates of regression equations for years of education (using the Park (1994) linear interpolation for years of education) on measures of childhood exposure to unilateral divorce laws. The details of our construction of childhood exposure to unilateral divorce laws are discussed in the text. All specifications include a full set of year of birth and state of birth main effects. Data are from the 1980 Census IPUMS 5% Sample.

Table 9
Implied Two Sample Instrumental Variables Estimates

Outcome	Women			Men		
	Reduced Form	1st Stage	2SLS	Reduced Form	1st Stage	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Years of Education	-0.0047	0.0054	-0.8704	-0.0016	0.0053	-0.3019
	(0.0017)	(0.0003)	(0.3153)	(0.0018)	(0.0002)	(0.3399)
Completed High School?	-0.0008	0.0054	-0.1481	-0.0005	0.0053	-0.0943
	(0.0002)	(0.0003)	(0.0371)	(0.0002)	(0.0002)	(0.0378)
Completed College?	-0.0005	0.0054	-0.0926	-0.0001	0.0053	-0.0189
	(0.0003)	(0.0003)	(0.0556)	(0.0003)	(0.0002)	(0.0566)
Log Weekly Wages	-0.0054	0.0054	-1.0000	0.001	0.0053	0.1887
	(0.0007)	(0.0003)	(0.1298)	(0.0006)	(0.0002)	(0.1133)
Married?	0.0028	0.0054	0.5185	0.0036	0.0053	0.6792
	(0.0004)	(0.0003)	(0.0742)	(0.0004)	(0.0002)	(0.0755)
Divorced?	-0.0003	0.0054	-0.0556	-0.0009	0.0053	-0.1698
	(0.0002)	(0.0003)	(0.0371)	(0.0002)	(0.0002)	(0.0378)
Never Married?	-0.0031	0.0054	-0.5741	-0.003	0.0053	-0.5660
	(0.0003)	(0.0003)	(0.0556)	(0.0004)	(0.0002)	(0.0755)
Any Children?	0.0038	0.0054	0.7037			
	(0.0004)	(0.0003)	(0.0742)			

Notes: Standard errors are in parenthesis. All reduced form coefficients and 1st stage coefficients were previously reported in tables 3-8. Implied 2SLS coefficients were calculated by dividing the reduced form coefficient by the 1st stage coefficient. An asymptotic standard error of this ratio is calculated by the delta method.

Table 10
Differences in Differences Result, Female Labor Force Participation
March Current Population Survey 1964-1986, Mare-Winship Uniform Data

	Female Labor Force Participation Married Women Only			Female Labor Force Participation Single Women Only		
	(1)	(2)	(3)	(4)	(5)	(6)
	A. Single Post-Unilateral Law Specification					
Unilateral Law	0.0021 (0.0077)	0.0007 (0.0076)	-0.0067 (0.0121)	-0.0506 (0.0149)	-0.0499 (0.0144)	-0.0470 (0.0247)
	B. Time-Varying Treatment Effect					
3 years prior	0.0112 (0.0179)	0.0109 (0.0178)	0.0031 (0.0159)	-0.0538 (0.0346)	-0.0578 (0.0342)	-0.0495 (0.0301)
2 years prior	0.0231 (0.0195)	0.0236 (0.0192)	0.0090 (0.0173)	-0.0426 (0.0388)	-0.0480 (0.0382)	-0.0281 (0.0337)
1 year prior	0.0376 (0.0181)	0.0410 (0.0179)	0.0225 (0.0165)	-0.0902 (0.0363)	-0.0819 (0.0360)	-0.0698 (0.0323)
year enacted	0.0322 (0.0176)	0.0355 (0.0174)	0.0149 (0.0164)	-0.0873 (0.0362)	-0.0776 (0.0358)	-0.0793 (0.0330)
1 year post	0.1182 (0.0170)	0.1124 (0.0172)	0.0303 (0.0189)	-0.0041 (0.0350)	0.0172 (0.0348)	-0.0006 (0.0374)
2 years post	0.0853 (0.0174)	0.0778 (0.0174)	0.0047 (0.0193)	-0.1945 (0.0325)	-0.1896 (0.0328)	-0.1964 (0.0373)
3 years post	0.0165 (0.0188)	0.0156 (0.0186)	-0.0597 (0.0196)	-0.1530 (0.0361)	-0.1493 (0.0356)	-0.1544 (0.0392)
State Effects	yes	yes	yes	yes	yes	yes
CPS Year Effects	yes	yes	yes	yes	yes	yes
State*Year Trends	no	no	yes	no	no	yes
Other Covariates	no	yes	yes	no	yes	yes
Male LFP	no	yes	yes	no	yes	yes

Notes: Each regression is run on 862 state-year cells, for average labor force participation rate. The averages in each state-year cell were computed from individuals aged 20-58. Other covariates are the average age and average education in the state-year cell.

Appendix A:
Evolution of Unilateral Divorce Laws by Year

Year	States
1953	Oklahoma
1963	Alaska
1970	California, Kansas, Texas
1971	Alabama, Florida, Idaho, North Dakota, Oregon
1972	Colorado, Kentucky, Michigan, Nebraska, New Hampshire
1973	Connecticut, Georgia, Hawaii, Indiana, Iowa, Maine, Nevada, New Mexico, Washington
1974	Arizona
1975	Montana
1976	Rhode Island
1977	Wyoming
1978	Delaware
1985	South Dakota
1986	Louisiana, Wisconsin
None	Arkansas, District of Columbia, Illinois, Maryland, Massachusetts, Minnesota, Mississippi, Missouri, New Jersey, New York, North Carolina, Ohio, Pennsylvania, South Carolina, Tennessee, Utah, Vermont, Virginia, West Virginia

Source: Friedberg, 1998, Brinig and Buckley, 1998

Appendix B:
Implementation of Unilateral Divorce Laws by State and Years of Exposure

(Percentage of Individuals from State in Treatment Group in Parenthesis)

No Years of Exposure to Unilateral Divorce

New York (15.6 percent) Pennsylvania (10.6 percent) New Jersey (9.8 percent) 24 Other States (64 percent)

Other States: Arizona, Arkansas, Delaware, District of Columbia, Illinois, Louisiana, Maryland, Massachusetts, Minnesota, Mississippi, Missouri, Montana, North Carolina, Ohio, Rhode Island, South Carolina, South Dakota, Tennessee, Utah, Vermont, Virginia, West Virginia, Wisconsin, Wyoming.

One Year of Exposure to Unilateral Divorce

Indiana (19.8 percent) Georgia (18.5 percent) Iowa (12.0 percent) 11 Other States (49.7 percent)

Other States: Arizona, Connecticut, Delaware, Hawaii, Maine, Montana, Nevada, New Mexico, Rhode Island, Washington, Wyoming.

Two Years of Exposure to Unilateral Divorce

Michigan (21.2 percent) Indiana (12.3 percent) Florida (11.1 percent) 16 Other States (55.4 percent)

Other States: Arizona, Colorado, Connecticut, Georgia, Hawaii, Iowa, Kentucky, Maine, Montana, Nebraska, Nevada, New Hampshire, New Mexico, Rhode Island, Washington, Wyoming

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Three Years of Exposure to Unilateral Divorce

Michigan (17.2 percent) Georgia (8.5 percent) Florida (7.6 percent) 21 Other States (66.7 percent)

Other States: Alabama, Arizona, Colorado, Connecticut, Delaware, Hawaii, Idaho, Indiana, Iowa, Kentucky, Maine, Montana, Nebraska, Nevada, New Hampshire, New Mexico, North Dakota, Oregon, Rhode Island, Washington, Wyoming.

Four Years of Exposure to Unilateral Divorce

California (17.5 percent) Texas (14.2 percent) Michigan (11.5 percent) 24 Other States (56.8 percent)

Other States: Alabama, Arizona, Colorado, Connecticut, Delaware, Florida, Georgia, Hawaii, Idaho, Indiana, Iowa, Kansas, Kentucky, Maine, Montana, Nebraska, Nevada, New Hampshire, New Mexico, North Dakota, Oregon, Rhode Island, Washington, Wyoming.

Five Years of Exposure to Unilateral Divorce

California (18.1 percent) Texas (14.1 percent) Michigan (10.6 percent) 24 Other States (57.2 percent)

Other States: Alabama, Arizona, Colorado, Connecticut, Delaware, Florida, Georgia, Hawaii, Idaho, Indiana, Iowa, Kansas, Kentucky, Maine, Montana, Nebraska, Nevada, New Hampshire, New Mexico, North Dakota, Oregon, Rhode Island, Washington, Wyoming.

Six Years of Exposure to Unilateral Divorce

California (18.7 percent) Texas (14.3 percent) Michigan (10.9 percent) 23 Other States (56.1 percent)

Other States: Alabama, Arizona, Colorado, Connecticut, Florida, Georgia, Hawaii, Idaho, Indiana, Iowa, Kansas, Kentucky, Maine, Montana, Nebraska, Nevada, New Hampshire, New Mexico, North Dakota, Oregon, Rhode Island, Washington, Wyoming.

Seven Years of Exposure to Unilateral Divorce

California (18.6 percent) Texas (13.9 percent) Michigan (11.1 percent) 22 Other States (56.4 percent)

Other States: Alabama, Arizona, Colorado, Connecticut, Florida, Georgia, Hawaii, Idaho, Indiana, Iowa, Kansas, Kentucky, Maine, Montana, Nebraska, Nevada, New Hampshire, New Mexico, North Dakota, Oregon, Rhode Island, Washington.

Eight Years of Exposure to Unilateral Divorce

California (20.6 percent) Texas (15.0 percent) Michigan (10.1 percent) 21 Other States (54.3)

Other States: Alabama, Arizona, Colorado, Connecticut, Florida, Georgia, Hawaii, Idaho, Indiana, Iowa, Kansas, Kentucky, Maine, Montana, Nebraska, Nevada, New Hampshire, New Mexico, North Dakota, Oregon, Washington.

Nine Years of Exposure to Unilateral Divorce

California (20.9 percent) Texas (14.9 percent) Michigan (10.2 percent) 20 Other States (54 percent)

Other States: Alabama, Arizona, Colorado, Connecticut, Florida, Georgia, Hawaii, Idaho, Indiana, Iowa, Kansas, Kentucky, Maine, Nebraska, Nevada, New Hampshire, New Mexico, North Dakota, Oregon, Washington.

Ten Years of Exposure to Unilateral Divorce

California (21.2 percent) Texas (14.7 percent) Michigan (10.4 percent) 19 Other States (53.7 percent)

Other States: Alabama, Colorado, Connecticut, Florida, Georgia, Hawaii, Idaho, Indiana, Iowa, Kansas, Kentucky, Maine, Nebraska, Nevada, New Hampshire, New Mexico, North Dakota, Oregon, Washington.

Eleven Years of Exposure to Unilateral Divorce

California (29.4 percent) Texas (20.3 percent) Michigan (14.0 percent) 11 other States (36.3 percent)

Other States: Alabama, Alaska, Colorado, Florida, Idaho, Kansas, Kentucky, Nebraska, New Hampshire, North Dakota, Oregon

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Twelve Years of Exposure to Unilateral Divorce

California (40.3 percent) Texas (27.3 percent) Florida (12.2 percent) 6 Other States (20.2 percent)

Other States: Alabama, Alaska, Idaho, Kansas, North Dakota, Oregon.

<u>Appendix C:</u>
Matching Procedure

We used the following matching and variable construction procedure to examine parental divorce. Every individual in the 1980 IPUMS is uniquely identified by a household identification number (IPUMS variable "SERIAL") and a person-line number (IPUMS variable "PERNUM"). In addition, the variables MOMLOC and POPLOC provide the person-line number of each individual's mother and father, respectively, provided that the parent and child live in the same household. We first limited the 1980 IPUMS to individuals younger than 17 (hereafter referred to as "the child sample"); then we matched the child sample first to female adults and then to male adults, on the basis of household and person line numbers. For example, a mother and child match exists if the child's SERIAL equals SERIAL for an adult female ***and MOMLOC from the child's record equals PERNUM for the same adult female.*** An analogous method was used to match children to their fathers.

Finally we constructed a dummy variable to indicate parental divorce, based upon the marital status of the mother or the father as found in the matched sample. If a child was found to be living with only the mother or the father, our divorce variable takes a value of one if that parent was divorced. If the child was matched to both a father and a mother, we looked at the marital status of the mother to assign the divorce variable.

Appendix D:

Maximum Likelihood Estimation

The event under consideration is parental divorce. We will assume that parents are married at the child's birth, which is t=0. The duration in this situation is how long the parental marriage remains intact. Suppose that the unconditional probability of marital dissolution at time t for individual i is given by $f(t \mid X_i, \beta)$, where X_i represent a vector of individual specific covariates, and β is a parameter vector common to all individuals. Each individual's exposure to unilateral divorce laws is included in X_i. The probability of a divorce occurring at anytime prior to time T would be given by:

$$F(T) = \int_0^T f(t \mid X_i, \beta)\,dt$$

and the probability of no divorce occurring prior to time T equals $1\text{-}F(T)$.

Our sample from the 1980 Census allows to observe whether divorce occurs prior to the age of each individual child. Denote the age of each child in the sample as a_i, and D_i as an indicator variable for whether or not divorce has occurred as of age a_i. Then a general form for the likelihood of observing our sample is:

$$L(\mathbf{x} \mid \hat{\mathbf{a}}) = \prod_i F(a_i)^{D_i}(1 - F(a_i))^{1-D_i}$$

Notice that both β and x_i are implicit in $F(a_i)$.

We would like to capture the impact of unilateral divorce laws on the cumulative probability of divorce prior to a child's turning 18 years old, denoted as $F(18)$. To compute the effect of unilateral divorce laws on $F(18)$, we must estimate β from our sample; a parametric form of $F(a_i)$ must be specified to do the estimation. We use several different specifications common in the econometric literature on duration models, including the exponential, the Weibull, and the lognormal.

The appropriate cumulative distribution functions are:

$$F(a_i) = \left\{ \begin{array}{ll} 1 - e^{-\lambda_i a_i} & \text{if exponential model} \\ 1 - e^{-(\lambda_i a_i)^{\rho}} & \text{if Weibull model} \\ 1 - \Phi[-\rho \ln(\lambda_i a_i)] & \text{if lognormal model} \end{array} \right\} \text{ in each case, } \lambda_i = \exp(-x_i\beta).$$

Covariates enter the cumulative distribution function through λ_i, which represents the individual-specific hazard rate – the probability of divorce occurring at time t, conditional on divorce having not happened before time t. On *a priori* grounds, we have few reasons to prefer one specification of the cumulative distribution function to another. A point in favor of the Weibull or lognormal specifications is that they allow for a hazard function that declines or increases with the child's age.[16] By contrast, the exponential distribution constrains the hazard rate to be constant.

Decreasing hazard functions, said to exhibit "negative duration dependence", indicate that the longer a marriage has survived the less likely it is to end in divorce. We highlight negative duration dependence because it is commonly found in demographic studies of the divorce probability as a function of the age of children (Bumpass 1984). Divorce is most likely to occur when the child is young, with a diminishing hazard rate as the child ages. The Weibull and lognormal distributions allow for this observed pattern.

We estimate β and ρ (where appropriate) for each specification using an iterative maximum likelihood procedure. The estimates of β and ρ themselves are difficult to interpret and are not of intrinsic interest. We use β and ρ to compute $F(18)$ for each specification. All covariates are set equal to the sample means. Then to simulate the impact of changing the individual's exposure to unilateral divorce laws on the divorce probability, we make two separate computations for each specification:

a. We increase the value of YRSUN by one, and compute the change in value of $F(18)$.
b. We compute the derivative of $F(18)$ with respect to YRSUN and evaluate it at the sample means.
c. We compute F(1), F(2), …, and F(18). Then we compute an implied regression-weighted average of these values. [I probably need to explain what the hell this means.]

The qualitative findings are similar regardless of whether calculation method a, b, or c is used. Moreover, the results are relatively robust to the specification of the likelihood function. The probability of parental divorce (prior to the child reaching maturity) increases by .19-.33 percentage points with an additional year of exposure to unilateral divorce.

[16] The lognormal hazard function first increases then decreases; the overall shape is a function of the parameter ρ.

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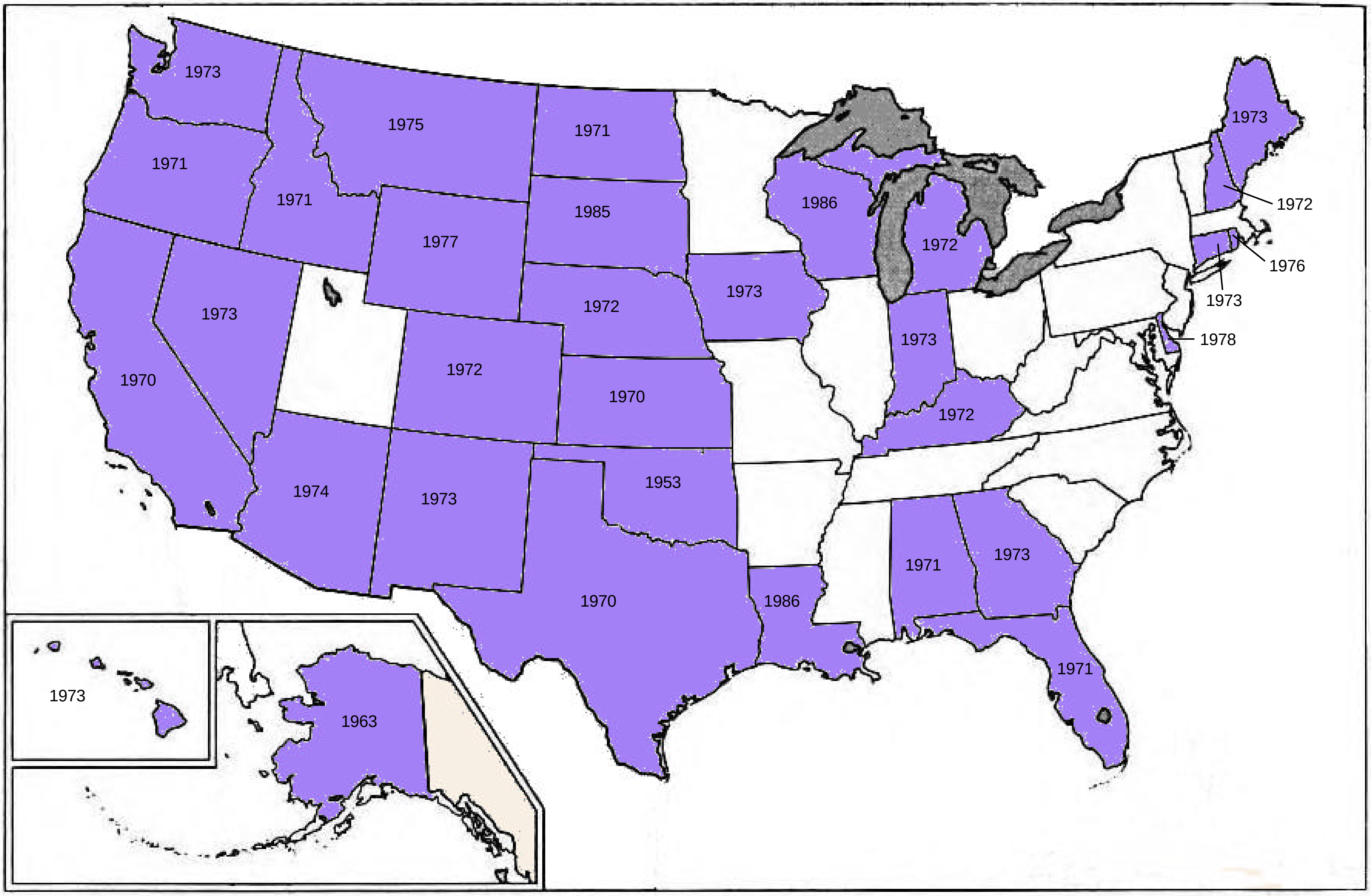

Figure 1. Adoption of Unilateral Divorce Laws by State.